

The Cliometric Society

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TWO CLIOMETRICIANS WIN ECONOMICS NOBEL

(Oxford OH) On October 12th the Royal Swedish Academy of Sciences announced that two of the earliest practitioners of cliometrics, Robert W. Fogel of the University of Chicago and Douglass C. North of Washington University in St. Louis, are jointly to be awarded the Bank of Sweden Nobel Memorial Prize in Economic Sciences for 1993, in recognition of their contributions to 'having renewed research in economic history.' The news was greeted enthusiastically, not only by economic historians and economists but also, with some dissent, by the press.

For economic historians – cliometricians in particular – the news was most heartening. Interviewed by phone for *The Washington Post* (10/13), North said this year's award is 'of immense significance,' showing that 'the field of economic history has come of age.' Richard Easterlin (USC) seconded the motion in *The Boston Globe* (10/13): 'The new economic history has been around for more than 30 years. It's good to finally have some recognition.' Their views were echoed by economists. Milton Friedman, a former Chicago colleague of Fogel, told the *Los Angeles Times* (10/13), 'This recognizes that [economic history] ... has finally made the grade.' The regular column for the *Sunday Atlanta Journal-Constitution* by Donald Ratajczak (Georgia State) was entitled 'Economic history was overdue to get Nobel recognition.' 'It is a good day for economics,' stressed Donald McCloskey (Iowa) in *The Boston Globe* (10/13); 'This is a prize for empirical work,' not, as has usually been the case, for 'blackboard' economics. Claudia Goldin (Harvard) told the *Washington Post* (10/13) that 'the award is a triumph for a discipline peopled by "clever little rodents" who discover ingenious methods for unearthing historic truths.'

Similar sentiments appeared in the international press. Roderick Floud (London Guildhall University) stated for the *Financial Times* (10/13), 'It's a tremendous decision. [Their] emphasis on quantitative techniques has transformed economic history and stimulated greater numeracy in other branches of history.' Gabriel Tortella (Universidad de Alcalá) wrote in *El País* (10/13) that the award '*Es una gran noticia para la historia económica*', while Ilkka Nummela (University of Jyväskylä) explained the importance of '*cliometrikko*' in two articles for the Finnish daily, *Keskisuomalainen* (10/16; 10/24). At the University of York in England, a notice quickly appeared on bulletin boards, headed Economic and Social History at York: Your Route to a Nobel Prize?, reproducing Will Hutton's article for *The Guardian* (10/13), 'Two who transformed economic history.'

The laureates' home town papers were obviously proud. The front page of *The Chicago Tribune* (10/13) crowed 'Another Nobel day for U. of C.,' reminding readers that a Chicago economist had won or shared the Nobel Prize for Economics in each of the past four years. Thus, as the newly-installed President of the University, Hugo Sonnenschein (economist and former student of Lance Davis and Jon Hughes at Purdue) remarked, they have outdone even the Bulls with a 'four-peat.' Having arisen at his usual 4 AM CDT, Fogel was already at

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Executive Director's Notes

Trustees' Meeting Report

The Trustees of The Cliometric Society met in Tucson, Arizona, on October 1, 1993.

This was the first Trustees' Meeting since the By-Laws were ratified, so several agenda items related to those policies. The Executive Director made the following reports to the Trustees:

Membership: Our 1993 membership consists of 385 members in 23 countries, a 10% increase over last year. The largest numbers of members are in the USA, Canada, the United Kingdom, and Spain, with 20 Student members, 65 Sustaining members, and 290 Regular members. 245 members subscribed to *Explorations in Economic History* through the Society; 115 members chose our dual membership option to join the European Historical Economics Society.

Finances: The Trustees accepted the financial report submitted, determining that the Society has a balanced budget. We have scheduled an audit, with an annual financial statement to appear in the February *Newsletter*.

No dues increase for 1994 is necessary; however, since Academic Press raised *EEH* subscription rates, prices for combined dues and subscriptions will increase. The Executive Director announced that he has been informed that the NSF has approved funding for the 1994-96 Cliometrics Conferences.

Newsletter staff: Sam Williamson was appointed Editor for the four-year term outlined in the By-Laws. In turn, he appointed John Lyons and Lou Cain Associate Editors, and Debra Morner Assistant Editor. Advertising prices were discussed and established for half page, full page, and "trade-out" advertisements, but policy and prices for "classified" ads remain at the Editor's discretion. Current rates are now \$80 for a full page and less for smaller ads and "trade outs." All members are encouraged to have their publishers advertise in the *Newsletter*.

Milan: The program for the Society's C Session at the International Economic History meetings in Milan has been completed. Selection Committee members Jeremy Atack, Lance Davis and Sam Williamson chose six papers from a total of 13 submitted. Further announcements about this session will be in the *Newsletter* next year.

ASSA: 1994 Program Chairs John C. Brown and Martha L. Olney did a wonderful job organizing Society sessions for this year's meeting in Boston, Massachusetts, January 3 - 5. This year we are co-sponsoring two sessions, one with AEA and one with AREUEA. [See Insert for more information.]

To avoid confusion in the future, it was agreed to centralize organizational work for The Cliometric Society sessions at the ASSA. From now on, paper proposals will be sent to the Society by May 15, then routed to the Program Chairs for selection. All correspondence with authors (letters of acceptance, style guides) will originate from the Society office. Deadline dates will remain the same from year to year to assist authors in planning. The Call for Papers will appear in the February *Newsletter*. Barbara Sands and John James have agreed to serve as Program Chairs for the January 1995 ASSA meetings in Washington, D.C.

The final item of business was a discussion of The Society's Listserv and gopher. [See article on Page 21.]

Samuel H. Williamson

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To Our Fellow Cliometricians

Receiving the Nobel Prize with Bob has inevitably brought back wonderful memories of the long road we have all traveled since 1960 in Lafayette, Indiana, when the first meeting of cliometricians took place. There are fond memories of Jon Hughes, Lance Davis, Nate Rosenberg and others who were at Purdue; of Bill Parker, Dick Easterlin, Bob Gallman who were there at the beginning; of the six years Bill and I ran the *Journal* and got into hot water with the Trustees for running Bob's article on railroads and promoting the New Economic History; of the wonderful students I have had who have joined us since then; and of the many others, too numerous to mention, who have joined the ranks over the past 33 years.

We have always been more than a group of scholars united by a common cause; we have formed friendships that have enriched and shaped our personal lives. For all these memories and much, much more, I am indebted to all of you for having made these many years such a rich experience.

Doug North

The official citation of the Royal Swedish Academy of Sciences makes it clear that the 1993 Nobel Memorial Prize was intended not only to recognize the work of two particular scholars but also to recognize that economic history was a major subfield of the discipline of economics (and, I might add, of history). As the news report in *Science* put it, the rise of cliometrics has moved economic history 'from the fringe to center stage'.

All those who know the history of cliometrics know that the annual Cliometrics Conference and this *Newsletter* have been central forces in making the new economic history attractive to the many bright young scholars who have entered the discipline over the past several decades.

Bob Fogel

The Citation for the Nobel Memorial Prize, 1993

The Sveriges Riksbank (Bank of Sweden) Prize in Economic Sciences in Memory of Alfred Nobel

The Royal Swedish Academy of Sciences has decided to award the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel for 1993 jointly to

Professor **Robert W. Fogel**, University of Chicago, USA
Professor **Douglass C. North**, Washington University, St. Louis, USA

for having renewed research in economic history by applying economic theory and quantitative methods in order to explain economic and institutional change.

This year's prize winners are leading figures within the field of "new economic history".

Modern economic historians have contributed to the development of economic sciences in at least two ways: by combining theory with quantitative methods, and by constructing and reconstructing databases or creating new ones. This has made it possible to question and to reassess earlier results, which has not only increased our knowledge of the past, but has also contributed to the elimination of irrelevant theories. It has shown that traditional theories must be supplemented or modified to enable us to understand economic growth and change. Economic historians often consider far reaching problems, the estimation of which demand an integration of economics, sociology, statistics, and history. Robert Fogel and Douglass North are the economic historians that have come furthest in such a scientific integration. They were pioneers in the branch of economic history that has been called the "new economic history" or cliometrics, i.e. research that combines economic theory, quantitative methods, hypothesis testing, counterfactual alternatives and traditional techniques of economic history, to explain economic growth and decline. Their work has deepened our knowledge and understanding within fundamental areas of research, as to how, why and when economic change occurs.

Robert Fogel's foremost work concerns the role of the railways in the economic development of the United States, the importance of slavery as an institution and its economic role in the USA, and studies in historical demography.

Douglass North has studied the long term development of Europe and the United States, and has in recent work analysed the role institutions play in economic growth.

Citation(continued)

Robert W. Fogel's scientific breakthrough was his book on the role of the railways in the American economy. Joseph Schumpeter and Walt W. Rostow had earlier, with general agreement, asserted that modern economic growth was due to certain important discoveries having played a vital role in development. Fogel tested this hypothesis with extraordinary exactitude, and rejected it. The sum of many specific technical changes, rather than a few great innovations, determined the economic development. We find it intuitively plausible that the great transport systems play a decisive role in development. Fogel constructed a hypothetical alternative, a so called counterfactual historiography; that is he compared the actual course of events with the hypothetical to allow a judgement of the importance of the railways. He found that they were not absolutely necessary in explaining economic development and that their effect on the growth of GNP was less than three per cent. Few books on the subject of economic history have made such an impression as Fogel's. His use of counterfactual arguments and cost-benefit analysis made him an innovator of economic historical methodology.

Fogel's painstaking criticism of his sources, and his use of the most varying kinds of historical material, made it difficult for his critics to argue against him on purely empirical grounds. As Fogel has stressed, it is the lack of relevant data rather than the lack of relevant theory that is often the greatest problem for research workers. Fogel's use of counterfactual analysis of the course of events and his masterful treatment of quantitative techniques in combination with economic theory, have had a substantial influence on the understanding of economic change.

Fogel's second work of importance, which aroused great attention and bitter controversies, treated slavery as an institution and its role in the economic development of the United States. Fogel showed that the established opinion that slavery was an ineffective, unprofitable and pre-capitalist organisation was incorrect. The institution did not fall to pieces due to its economic weakness but collapsed because of political decisions. He showed that the system, in spite of its inhumanity, had been economically efficient.

His exceedingly careful testing of all possible sources and his pioneering methodological approach have allowed Fogel to both increase our knowledge of an institution's operation and disintegration and to renew our methods of

research. Both his book on the railways and that on slavery have forced researchers to reconsider earlier generally accepted results, and few books in economic history have been scrutinised in such detail by critical colleagues.

Fogel's third area of research has been economic demography, and in particular the changing rate of mortality over long periods of time and its relation to changes in the standard of living during recent centuries. This project is less controversial than the other two, and is both interdisciplinary and international, with fellow workers from many countries. His conclusion is that less than half of the decrease in mortality can be explained by better standards of nourishment, before the breakthroughs of modern medicine. This leaves the greater part of the decline unexplained. According to Fogel, a systematic analysis demands an integrated study of mortality rates, morbidity rates, food intake and individual body weights and statures. A combination of biomedical and economic techniques is required to achieve this, something that he has at present set about accomplishing. It is already apparent that his analyses will affect research in economic history at many levels.

Douglass North presented in 1961 an explanatory model for American economic growth before 1860, that came to affect the direction of research not only in the USA. Starting from an exportbase model he had previously formulated himself, North analyses how one sector (the cotton plantations) stimulated development in other branches, and led to a specialisation and interregional trade.

In 1968 North presented an article on productivity in ocean shipping, which has become one of the most quoted research works in economic history. In this article he shows that organisational changes played a greater role than technical changes. North has more and more pointed out that economic, political, and social factors must be taken into account if we are to understand the development of those institutions that have played a role for economic growth, and how these institutions have been affected by ideological and non-economic factors. North maintains that if political economics is a theory of choice under certain specific assumptions and restrictions, then the purpose of economic history is to theorise about the development of these. North has pointed out that there is a risk that economic analyses may become ahistoric if the

time factor and the conflicts in society are not taken into account. A systematic reintroduction of institutional explanations in the historic analysis is an attempt to correct this deficiency.

In a number of books, North has demonstrated the role played by institutions, including property rights. He is one of the pioneers in 'the new institutional economics'. Putting it simply, North maintains that new institutions arise, when groups in society see a possibility of availing themselves of profits that are impossible to realise under prevailing institutional conditions. If external factors make an increase in income possible, but institutional factors prevent this from happening, then the chances are good that new institutional arrangements will develop. North tested his hypotheses on development in the USA during the nineteenth century, and showed how agricultural policy, banking, transport, etc. could be explained by the institutional arrangements. In a following book, he considered the economic development of Western Europe from the middle ages to the eighteenth century, and showed that economic incentives, based upon individual property rights, were a prerequisite for economic growth. Changes in relative prices and fluctuations in population growth led to institutional changes. The speedier industrialisation in England and the Netherlands depended upon the fact that certain conservative institutions, such as the guilds, were weak. Private property rights were also guaranteed in these countries, as opposed to the case of Spain where the lack of institutional innovation led to a century long stagnation. Innovations, technical changes and other factors that are generally regarded as explanations, are not considered to be sufficient by North. They are themselves a part of the growth process and cannot explain it. Effective economic organisations are the key to economic change. 'Institutions are sets or rules, compliance procedures, and moral and ethical behaviour of individuals in the interest of maximizing the wealth or utility of principals.'

In his latest book, North poses the fundamental question of why some countries are rich and others poor. 'Institu-

tions provide the basic structure by which human beings throughout history have created order and attempted to reduce uncertainty in exchange. Together with the technology employed, they determine transaction and transformation costs and hence the profitability and feasibility of engaging in economic activity.' Greater institutional changes occur slowly, since institutions are the result of historical change, which has moulded individual behavior. The greater the institutional uncertainty, the greater become the transaction costs. The lack of opportunity of entering binding contracts and other institutional arrangements is a cause of economic stagnation, both in today's developing countries and the former socialistic states. North has tried to explain the difficulties that meet these countries by focusing his analysis on the political and legal framework for economic growth. In his book he poses fundamental questions concerning the connection between economic change, technical development, and institutional conditions. He shows both the difficulties that neo-classical theory has had in explaining growth, and the strength of using this theory in combination with the approaches he has proposed. North has forced economists to rethink, to be conscious of when economic 'laws' are sufficient as an explanation of a given problem, and of when other factors must be taken into account.

North has, like Fogel, inspired a large number of research workers. His persistent stressing of the importance of stringent theory, together with his emphasis on the role of institutions, has influenced not only economic historians, but also economists and political scientists. Fogel is an empiricist, who never leaves any sources unexplored. North can be compared to those prize winners who have previously received the prize for purely theoretical works. North is an inspirer, a producer of ideas, who identifies new problems and shows how economists can solve the old ones more effectively.

Fogel and North have thus in different ways renewed research in economic history, by making it more stringent and more theoretically conscious.

**KUNGL.
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12 October 1993

AN INTERVIEW WITH DOUGLASS C. NORTH

Editors' Note: Douglass C. North has been Henry R. Luce Professor of Law and Liberty and Professor of Economics and of History at Washington University in St. Louis since 1983. He taught at the University of Washington (1950-79; Chairman of the Economics Department, 1967-79). Before moving to St. Louis, he was also Peterkin Professor of Political Economy at Rice and Pitt Professor of American Institutions at Cambridge University. He was Co-editor of the *Journal of Economic History* and Trustee of the Economic History Association (1961-1967), member of the Board of Directors of the National Bureau of Economic Research (1967-86), President of the Economic History Association (1973) and of the Western Economics Association (1976).

Gary Libecap (University of Arizona) was the primary interviewer for the *Newsletter*; his contribution was built from a series of letters and telephone conversations during the summer of 1993. On October 2nd at the meetings of the Economic History Association in Tucson, Doug North agreed to do some more reminiscing about the early days of the 'New Economic History' and to respond to some follow-up questions put by Sam Williamson and John Lyons. In the text that follows, North's comments that day have been merged with his conversations with Gary Libecap. Although his recorded words have been edited, we attempt to present his views in conversational rather than formal mode; both questions and replies have been ordered 'logically' rather than chronologically.

To introduce the interview, Gary Libecap writes: My personal contact with Doug North's work began in the fall of 1972 when I was contemplating my upcoming departure from the Air Force. Its call had interrupted my graduate training at Penn in 1969 and, having been away for so long from economics, I was not sure I wanted to return to graduate school. I decided to read a variety of books in economics to see if they would spark my interest; Bill Whitney at Penn sent me a reading list. Toward the top was a new book that caught my eye, *Institutional Change and American Economic Growth*, by Lance Davis and Douglass North. Their use of neoclassical theory to analyze the development of important legal and political institutions was intriguing, and I began to read other material on linkages between institutions and economic behavior. The subject remains of central concern to me and, in a very real sense, I owe much to the work of Doug North, his colleagues and students.

I was thus interviewing a person I greatly admire and whose work has played a vital role in the development of modern economic history as a discipline. Since the early 1970s, Doug North has been foremost in insisting that much more attention be directed to the institutional structure of a society in general, and its property rights arrangements in particular, to explain differences in economic performance across societies and over time. North has called on economists and economic historians

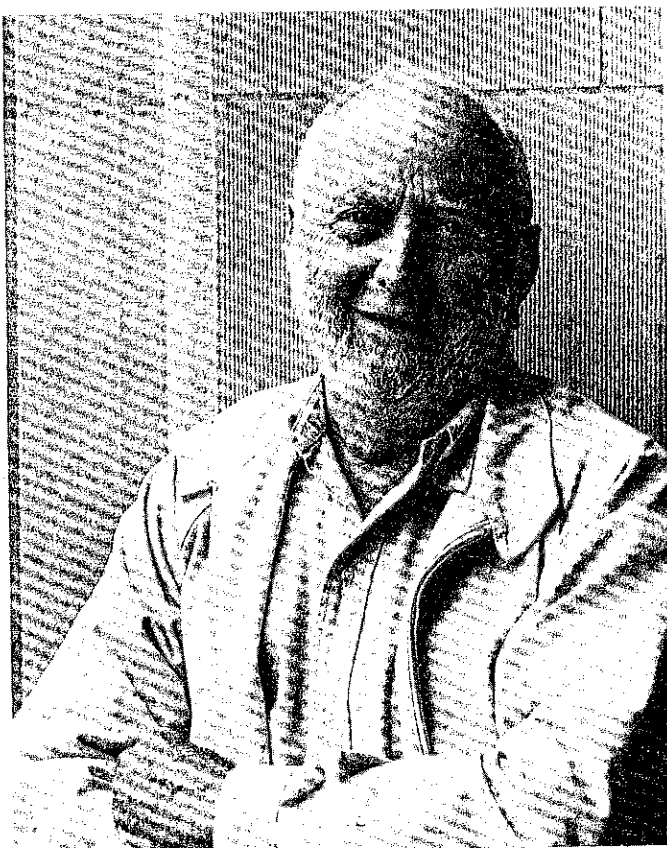


Photo: Herb Weltman, Washington University

to engage in more systematic analysis of institutions and of the opportunities and incentives they provide for economic decisions and performance. He has urged us to relax our self-imposed devotion to the constraints of a strict reading of neoclassical theory and to broaden the scope of our investigations to include analyses both of the role institutions play in economic growth and of the process of institutional change. More recently, he has called for consideration of the elusive concepts of ideology, fairness, and path dependence in attempting to explain why some societies have been successful in economic development, while for others sustained economic growth remains a distant goal.

What gave the 'New Economic History' its early drive?

The 'New Economic History' revolution – and that's what it was – was already in the wind by the second half of the 1950s. Walt Rostow was an early influence but, of course, Simon Kuznets was most influential, as was all the activity going on in the field of development. The NBER also played an important role. I was invited to be a Research Associate at the Bureau in 1956-57, and

Solomon Fabricant (then Director of Research) went out of his way to provide encouragement – sending me down once a week to spend the day with Kuznets in Baltimore. Both Dick Easterlin and Bob Gallman were there as well. The culmination of that year, 1957, was the EHA-NBER Income and Wealth Conference at Williams in the fall. At the time we were convinced we could overturn old, obsolete dogmas and remake the field of economic history. But more fundamental was the inspiration we got, not only from Kuznets, but also from a broad array of economists who were deeply interested in what we were up to. Of course, it was Jon Hughes and Lance Davis at Purdue who got us all together in 1960 for what became the annual frozen February trek to Lafayette.

Tell us about those early meetings.

The early Clio meetings were heady affairs, usually with a mixture of economists and econometricians thrown in with us economic historians. They tended to be pretty much no-holds-barred discussions. (One participant got so mauled that he locked himself in his room for a spell and never returned.) It was very exciting. First of all, there wasn't such a distinction in those days between economists and economic historians, so that all of us, the economists included, were really excited about what we economic historians were trying to do. So economists attended the early Clio's, providing a powerful encouragement. In fact, they egged us on in a way. That was very important. There were a lot of them – not just at the first meeting, but at *most* of the meetings. We spent a lot of time arguing – disagreeing – about how we could use our economic theory correctly.

A while back I went through the old Purdue volumes where they list the participants and I was amazed at the people who did attend whom I had forgotten about. What I do remember is that it was exciting; it was *fun*; we really had a good time. Dick Easterlin came – and I don't know whether it was the first or second session – but part of it was on my first book, *Economic Growth of the United States*. Dick Easterlin had written a 22-page book review for the *Journal of Economic History* and Bill Parker and I were the editors. He was critical; Dick didn't have a good word to say about my book in 22 pages, so I said, 'Well, that's all right, but isn't it a little long?' And I said to Bill, something like 'You handle it, but I think I'd cut it down to about six pages.' We really *did* give each other the business, and it was very good as a result. I felt wounded a lot of times, but then we *all* did, and that's the way it should have been. I've never been to a set of

meetings that I looked forward to – even including the lousy fare in Lafayette – as much as I did those things; they were really exciting.

There is this story – and I don't know if you've ever put it in print – about you standing in the airport on the way to the first meeting – how does that story go?

The one about Bob Fogel? Well, that's a true story. Bill Parker and I had met at the airport and we were standing there and I knew everybody else – we were all *old* friends – and I said, 'Who's this guy Fogel who's supposed to come to this? I never heard of such a guy.' And then this great big bear of a form, you know, turned around and said 'I'm Bob Fogel.' But that's the way I met Bob Fogel. Sitting there in the airport.

In Chicago?

In Chicago, on our way down to getting snowbound that year. There had been a lot of preparations and we all not only knew who was going, but we'd known a lot about it, we'd had a lot of talk. Since Lance and Jon, you know, were formerly my students, we'd spent a lot of time talking about who should be there and everything. So Fogel was a surprise – I'd never heard of him.

What was the reaction of historians and other economists to the 'new' approach?

Initially the reaction of historians was very hostile. Economists were generally enthusiastic; the demand for 'new economic historians' by economics departments in the 1960s and early 1970s was ample testimony to that. Surprisingly, I think the long-run effect has been the reverse. Cliometrics has had more influence on historians than on economists. Historians, even while protesting about cliometrics, have become much more self-conscious about quantitative methods and at times even tend to be uncritical in accepting the theory we employ. But economics departments have largely reverted to thinking of economic historians as marginal to department needs. The reason, I believe, is that we do not add any particular dimension to economics. We just use their tools to explain the past.

So cliometrics hasn't lived up to its early billing?

No, I don't think it has. It was a real revolution in the beginning, and everyone in economics was caught up in what was going on. But the limitations of neoclassical

theory as a tool kit are today more appreciated by many in economics – where I think a revolution is going on – than in economic history, which tends to be more reactionary in terms of theoretical innovation than economics. And until economic historians break out of the strictures imposed by neoclassical theory, cliometrics will remain a relatively uninteresting field.

With no useful policy implications?

Nothing is a more telling indictment of economic history than its failure to play an important role in understanding economic development and in providing policy guidelines for development of Third World countries and, now, Eastern European economies. We should have been in the very forefront of the development field. I think it is promising that those economic historians who are breaking new ground are in substantial demand, because they do have something to contribute to the development field.

Nevertheless, there were some major achievements?

Yes, our primary achievement was the rigor of analysis that came with systematic use of neoclassical theory and quantitative methods – the economic way of reasoning. Equally important was the development of quantitative data on the past performance of economies. The result was to overturn a lot of accepted explanations in economic history, none more spectacular than Bob Fogel's attack on the indispensability of the railroad. But we were more successful at demolishing existing explanations than in constructing new ones.

What we did then was impressive enough to be called a revolution, but the failure to go on to deal with the two major shortcomings of neoclassical theory applied to history have aborted the revolution. One is to model the frictions in economies that result in imperfect markets – political and economic – and produce very diverse performances across economies. The second is that economic history is about *change through time*, and economic historians have simply not addressed that difficult but essential problem. Here and there some work is starting to be done, the exploration of path dependence, for example. I don't mean to sound too bleak because there is an increasing number of very bright younger people (and others like Paul David and Bob Fogel), who are integrating new developments from the social sciences with economic history to produce some exciting results – and the ongoing revolution is much broader than just in economics. It's just that one would have thought that eco-

nomics historians, with their revolutionary tradition, would have taken the lead instead of being laggards.

Looking back, do you think you all quite realized the significance of what you were doing in those days?

Well, Jon Hughes came to me early on and said, 'You know, the new economic history isn't really going to dominate the profession in my lifetime.'

Is that right?

Yes. We didn't think it would – it was not the economists, the economists loved it – because in those days, four-fifths of economic history *at least* was being taught in history departments. So the idea that economists would ever come to dominate it, that it would move more into economics departments, seemed to be very, very remote, but then what happened was that economists made room in departments all over the place. In the 1960s, if my students didn't have six job offers, I thought they were really doing terribly. So, it was a great time. But the historians did dominate it then – I don't know quite how we ever really infiltrated into economics; I think mostly it was just that economics departments opened up to economic history in those days. Jon Hughes said that, I remember, in 1958, when we were both at the meetings in Toronto. About 10 years later, I remember bugging him and saying 'Look! Look what's happened!' and Jon expressed amazement, he couldn't have believed there'd have been such a rapid change. So it was an extraordinarily rapid revolution, and it was a revolution, you're darned right it was a revolution.

What led you to become an economic historian?

I finished undergraduate work at Berkeley in '42 and went into the war. I was going to become a lawyer, and war came along, so I went off to war and had four years in the Merchant Marine, running around the world,* doing – well, reading, and by the time I got through – and I was a good radical, I was a Marxist – I'd decided that I'd like to change the world. And I asked myself 'How can I change the world?' And I said, 'Well, economics is the way to change the world.' And then, 'Well, what kind of economics?' And finally, 'Understanding how economic change takes place has got to be the key to what you need to do to change the world.' I have not changed my view about that in the last 50 years; I'm still trying to figure out how economic-societal change takes place. So, in a way, I've got a single-minded objective. I started out with that

view in 1944, and I still have it today; it's a guiding factor that is still shaping the way I'm trying to evolve.

So you went to graduate school...

I went back to the only place that would take me – I had about a C- average as an undergraduate because I led this little 'left' protest at Sather Gate in Berkeley in 1940-42 – Berkeley took me only as a provisional student. They said, 'We'll give you one semester' and I got all A's and then went on.

Who was influential on the faculty?

Well, most of the left wing: Robert Brady, who was a leftist, and M. M. Knight – and Leo Rogin, who was the biggest influence on all of us graduate students at Berkeley, a wonderful influence – very bright guy, taught history of thought. But when I got out, I didn't believe any neoclassical economics. In fact, I got Distinguished in the writtens; then when I went to the oral exam, where somebody asked me a simple sophomore-level question in economic theory, and I couldn't answer it because I'd just memorized all this stuff (which you could do in the writtens). They had a long debate whether to pass me or not. They should have flunked me but they didn't: it's hard to flunk a guy who's gotten Distinguished in the writtens and couldn't answer any of the orals and not to admit that your writtens were all a big mess. So, I didn't learn any economics 'til I got out of graduate school and went to my first job at the University of Washington with a guy named Don Gordon, who was – is still – one of the best theorists I've ever known in my life. He and I played chess every day from 12 to 2, every single day, and during those four years we fought over economics, and I learned neoclassical economics from Don.

Over a chess board.

Over a chess board! So then I became holier than the Pope, you know; I was a Chicago School neoclassical theorist, and that's when I became one of the founders of cliometrics. And I was gung ho on neoclassical economics. It wasn't until much later that I began to say 'Huh! This theory can't seem to provide me with tools to deal with a lot of the problems,' not so much when I was in American economic history as when I shifted over in the late '60s to European economic history. And it was clear: the tools weren't there to be able to make sense out of history. Then I began this long evolution that I've been going through ever since.

Some of your first students at Washington have gone on to become –

Yeah, lots of them. Well, I haven't counted them, but I think there are somewhere between 40 and 50 Ph.D.'s that I've turned out.

And undergraduates?

Lance, and Jon – Lance and Jon were in my first class.

Anybody else?

Oh, Willie Wolman who is now editor of *Business Week*, and Irwin Unger – who got the Pulitzer Prize in history. It was an *incredible* first seminar, and they all thought that I was (whistles) – incompetent. They were right; I was incompetent. But they were a terrific group. I've never had a seminar like it; it was just by happenstance that I had them. And we had a wonderful time. By the end of the semester, we had all established rapport and afterwards Lance and Jon went on to do their own thing.

And later, Richard Sutch was a student who went on to MIT. Except for those three, all the rest stayed on. Roger [Ransom] stayed on to get his Ph.D. – Roger, Terry Anderson – oh, god, I can't remember all the names – every once in a while, one pops up I've forgotten about. But it's a large number – there must be at least 20 or 25 still active, and a lot of them went off into development. Half a dozen of my students are at the World Bank, and some went into business and so on – so they're all over the place – but a lot are economic historians.

It's hard when you're a starting professor, and you get a class like that, to realize how exceptional it is.

Yeah, I didn't know it at the time. What a class! What a recalcitrant bunch of bastards they were! There's a wonderful story: Irwin Unger, in about our fourth session when I was talking about the Mechanics' Lien Law, asked me a question about it and I didn't know the answer. Irwin Unger leaned over and said, 'Anybody who doesn't know *that* about the Mechanics' Lien Law is incompetent to teach a graduate seminar in economic history.' How would you like to have a class that began like that? Whew! It was a tough class.

When you and Bill Parker were editors of the *JEH* in the '60s, do you think that you were drawing the 'new economic history' out and presenting it to the world,

or were you overseeing something that was happening anyway – just selecting what you saw as most worthy at the time?

In a way, we were sort of pawns in a big game that we didn't really quite know. Did you know that we got impeached?

I hadn't heard that.

I'm not sure that many people did – but midway through Bill's and my tenure as editors, why, the trustees voted to impeach us; that is, they voted to examine whether we should stay on as editors. The basis was complaints (by some people I'll leave nameless) that we were incompetent. So the trustees then demanded that we explain ourselves. Well, Bill was very nice and cooperative and went before them – and I wrote them a nasty letter saying 'Go jump in the lake. I won't have anything to do with it.' I said, 'You've made us editors; I think we're doing a good job, and that's that.' Now Bill and I disagree about everything, you understand – in fact Bill and I almost came to blows over Bob Fogel's railroad article, which he didn't want to publish as Bob had framed it and I did. But we did compromise – we tried very hard to take account of all the criticism. But we were pawns in a big game: economic history was changing, we were starting to get articles that mirrored this, like Bob's article, and at the same time, why, there was lots of tugging that this was a terrible thing. Fritz Redlich was denouncing us and lots of other people were, too – and there we were, trying to walk this tightrope which we didn't walk well – I didn't walk it as well as Bill. Bill was much more diplomatic and very good about it, and I tended to say, 'damn the torpedoes and let's go on'. Between us, however, I think what we did was a landmark for the *Journal*. It's true people complained about things, but the fight was over – that is, that this sort of economic history had an important place and was really part of what economic history was going to be. What proportion it should be, and how it should be done, and things like that, were still controversial, but not that it shouldn't be done and that it shouldn't be a part of the profession. I might add, we got impeached but we didn't get fired; finally, they went back and agreed to continue us, even though with some reluctance on quite a number of the trustees' parts.

How did you get interested in the study of institutions?

In 1966-67, I went to Geneva on a Ford Fellowship and decided to retool and become a European economic

historian. It didn't take me very long to become persuaded that we couldn't make sense out of European economic history without explicitly modeling institutions, property rights, and government. The studies with Lance Davis [*Institutional Change and American Economic Growth*] and Bob Thomas [*The Rise of the Western World*] were both pioneering efforts to apply an institutional framework to American and European history. The underlying assumptions were from neoclassical theory, but there were too many obvious loose ends that didn't make sense, such as the notion of institutions being 'efficient', however defined. Ignoring politics and the consequences of politics for economic performance was an enormous hole in our research. Moreover, it just wasn't possible to explain long-run persistent poor performance of some economies in a neoclassical model. So I gradually began to explore what was wrong. Individual beliefs are obviously important to the choices people make, and it is only the extreme myopia of economists that prevents them from understanding that ideas, ideologies, prejudices matter. Once you recognize that, you are forced to examine the rationality postulate critically. In turn, that leads to the very exciting field (in terms of its implications for social science theory) of cognitive science. Political economy research has finally become an accepted sub-discipline. The notion of path dependence was developed by Brian Arthur and Paul David to explain technological change, but it seemed to me that it has important applicability to institutional change, although the explanation I have is somewhat different from Brian's and Paul's. But I should emphasize that I still consider myself a neoclassical economist. What I want to do is modify the discipline, not to start all over again. The economic way of reasoning is a very powerful tool of analysis.

Our ability to address and come to grips with the central problems of economic history will continue to improve, I think; not only will this provide fundamental new insights, but it will make economic history a vital and essential part of economics. Our job is to model economic and other kinds of change through time – not just institutional change, but also demographic and technological change. The difficulties are to develop useful theory in these areas and then do the empirical work to demonstrate the usefulness or limitations of the theory. It's nice to be able to note that exciting work is going on now in all three areas; for example, your [Gary Libecap's] work on institutional change; Bob Fogel's on demographic change; Nate Rosenberg's on technological change.

Just now you merely mentioned your work with Lance

Davis and Bob Thomas; recently you've done some work with others. Tell me, how do you collaborate?

Well, collaboration is a very personal psychological thing, and I don't collaborate well with a lot of people. Lance and I, for example, had some difficulties collaborating. On the other hand, Bob Thomas was a bit easier to collaborate with: we were very different, but we complemented each other. Barry Weingast and I collaborate very well. I'm working now with Art Denzau, who is a very smart guy and a very good theorist. It's another difficult collaboration, because he gets an idea a minute and he goes in all directions, and my job is doing the reverse of what I usually do: often I'm the idea person but now I'm always constraining him just because we can only write one article at a time. So, collaborating is hard but I like to collaborate because I'm not a high-powered theorist; in fact, I flunked plane geometry in high school and that's the last math I ever had. I don't know – I'd flunk every prelim now that we give in our field. On the other hand, I think I have good instincts for issues and good intuition, and I have a solid sound sense about economic theory. But these days, if you're going to do a lot of work, you really need to collaborate with people who are better trained and better organized and know the modern discipline and all that jazz. Or game theory: I did some work with Paul Milgrom and Barry, and that's been wonderful, and terribly important. In fact, you can't be my age and be able to keep up with all the new stuff. So, if you're really going to keep up with things, to say something interesting, you do need to collaborate on occasion.

How did you and Lance get together to write your book on institutions and manage to overcome those 'difficulties'?

Lance and I just got talking when I got back from Geneva in 1967, and with my dissatisfaction with the state of economics – I just began to get interested in institutions and transactions costs. Lance was interested, too, and so we started collaborating. I think it was a good book that came out of it, but it wasn't all that simple to get it done. That's probably as much an indictment of me as anything – it's just that Lance and I didn't find it easy to integrate the ways we think about problems.

But there was some positive feedback?

Oh, yes, I think it we had strong positive feedback, and all that, and despite our struggles at the time, why, we're still best friends. I guess I'm as close to Lance as I am to

anybody in the field at this point in my life. And I have been for, well, 40, 43 years – since we started in 1950.

To some people it appears that you have moved well away from the old 'new economic history' as you have moved into institutional economic history.

Right! Now I'm sometimes looked on as a traitor to my cause, you know, because I'm attacking cliometrics in many ways.

What do you mean by that?

I think the tools that we get out of neoclassical economics are inadequate to do what we ought to be doing as economic historians. There are two critical things here: one, economic history is about why markets *don't* work and two, it's about time and I think those are both missing from the theory. Neoclassical theory isn't about time – neoclassical theory is statics, comparative statics, and there is no way time gets incorporated into the argument at all. If you really wanted to be hubristic, you'd say what we're trying to do is evolve a dynamic theory of change, but that has escaped our profession as economists. We certainly want to be so self-critical that our models attempt to do that, but we haven't – we really futz around, and most of the program today futzes around, with looking at little details at a moment of time. If we look over time, we can make the connection from one time to the other.

You seem to be defining cliometrics rather narrowly. Is 'cliometrics' the wrong word? Don McCloskey wants to call us historical economists, and Phil Mirowsky prefers 'bad econometricians'. Yet the comment you, perhaps more than anyone, originated, was that cliometricians were going to bring some economic theory to historical questions and see if that could add to the story. For example, you said 'How can you talk about the Navigation Acts without knowing something about elasticity of demand?'

I wouldn't change that at all. I heartily agree. Look, I'm still a neoclassical economist; I think of myself first, last, and always as a neoclassical economist so, unlike Mirowsky, who thinks it's all physics or whatever, I think that price theory and opportunity cost – the economic way of reasoning – is the most powerful tool of analysis in all the social sciences, and you don't give that up. It's *the* essential tool. It's what makes any of us able to walk

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Report on the ESRC Quantitative Economic and Social History Conference

by

Katherine Watson (University of York)

(York UK) The 1993 conference was held on 8th-9th September at the University of York. Regrettably, some regular participants could not be present, but compensation for their absence came in the form of a welcome injection of 'new blood', including some graduate students attending their first conference. Financial support for the meeting was gratefully received from the ESRC and from the British Academy.

Peter Wardley (West of England) employed Bristol probate inventories to explore the diffusion of Hindu-Arabic numbers in Britain. Since this system combines the advantages of place-value, a restricted character set, and the use of a zero, its adoption facilitated more complex calculations. Wardley argued that the diffusion of Hindu-Arabic numbers should be regarded as an example of technological change, and could be modeled by a logistic growth curve. He concluded that larger studies of probate inventories would provide useful insights into regional differences in numerical representation. It might then be possible to complement the existing historiography on the growth of literacy by mapping at least some aspects of numeracy diffusion.

Simon Smith (York) was sceptical of attempts to quantify popular numeracy since it was extremely difficult to establish what constituted numeracy. The existence of informal numeracy strategies implied that the diffusion of formal number systems would not capture fully the diffusion of numeracy. He also noted that it was necessary to explain why the English were relatively slow in adopting Hindu-Arabic numerals: was the delay in changing from Roman clock faces another example of English institutional conservatism? Bill Sheils (York) suggested that numerical change was likely to be related to the application of multiplication in surveying methods. Frank Lewis (Queen's, Ontario) argued that if the diffusion of Hindu-Arabic numbers was regarded as an example of technological change, then it implied there were net benefits from their adoption, and Wardley should be able to observe larger aggregate numbers appearing in documents as diffusion occurred.

Martin Weale presented a paper written jointly with Sara

Horrell and Jane Humphries (all of Cambridge), constructing an input-output table for 1841. One of their innovations, facilitated by the use of household accounts, was the separation of the food, drink and tobacco sector from agriculture. Food, drink and tobacco proved to be the second largest manufacturing industry after textiles and clothing, contributing almost 9% of national income. Although there was considerable industrial interdependence in the early Victorian economy, there was nevertheless a division between those industries based on agriculture and those on minerals, the latter having less significant backward linkages and lower productivity than the former. Weale argued that the contribution of food processing had hitherto been understated, and that backward linkage effects to agricultural production were significant.

Nick Horsewood (Birmingham) queried whether the claim that the early Victorian economy was block triangular might be weakened by the inability to separate distribution and transport from other sectors. He also questioned whether the significance attributed to food, drink and tobacco had been distorted by reliance on budget studies not representative of all regions and classes. Horrell denied that this was the case. Having confirmed that the estimates offered were for the UK including Ireland, Ron Weir (York) suggested that Weale adopt Cormac Ó Gráda's revised estimates for agriculture rather than rely on Deane and Cole. Bob Millward (Manchester) suggested that value added *per capita* reflected relative prices rather than changes in productivity; Weale responded that an underlying assumption was that industries sold at competitive prices.

Steve Broadberry (Warwick) was concerned that reliance on input-output tables alone understated the significance of certain industries. Even if metals did not contribute greatly to output, they might have been vital to facilitating development in other industries. Similarly, the comparative advantage in international trade offered by textiles suggested that this sector was more important in the industrialisation process than had been implied. Weale agreed that a different impression of the significance of individual industries might be gained from investigating

linkages in a dynamic rather than a static context.

Tony Corley (Reading) re-examined estimates of Britain's foreign direct investment before 1914. As a proportion of total investment overseas, foreign direct investment increased from 43.9% in 1907 to 45.3% in 1913. By 1910, 56% of this direct investment was destined for the British Empire and 44% for other foreign countries. It was much more common for British multinationals to be engaged in resource-based industries than it was for them to be market-based, reflecting British demand for the primary products on which the 'Workshop of the World' was founded. Since the 1970s, increased recognition of the significance of direct investment relative to portfolio investment has resulted in a reassessment of explanations of the growth of overseas investment: rather than being pushed abroad by the lack of profitable opportunities at home, a large share of capital exports was generated by entrepreneurial initiatives. As free trade evaporated and Britain's share of world trade declined, entrepreneurs substituted direct investment abroad for visible trade.

Millward suggested that in order to explore the hypothesis that increased direct investment was entrepreneurial, it would be worth comparing rates of return on portfolio and direct foreign investment. He also proposed further investigation of the increase in overseas companies raising capital in Britain without domestic management to see whether this was largely a British phenomenon related to associations with the Empire. Broadberry was sceptical of Corley's resuscitation of British entrepreneurs: the entrepreneurial failure debate focused on manufacturing, and only a small proportion of British overseas companies was from this sector.

Larry Neal (Illinois) presented evidence suggesting that the London stock market crash of 1825 was not due primarily to the collapse of a speculative bubble. Rather, during the early 1820s a great variety of new issues was floated, including Latin American debt which was especially difficult to evaluate. This exacerbated the asymmetric information problem facing investors. Symptomatic of these uncertainties was the divergence in share prices for individual companies within the same sector, and also the peak in bankruptcies following the crash. However, crisis induced legislative change, including promotion of joint stock banking and bankruptcy law reform. It was these institutional innovations which provided Britain with a more secure foundation for the extension of her influence on international finance.

Corley suggested that Neal might have exaggerated the role played by Latin American debt in stimulating the crisis, since Latin American mines constituted only a small proportion of total capital. Broadberry commented that it was easy to accept that large flotations of new issues created asymmetric information problems which could generate a crash, but the really interesting problem was why these instruments were floated at the time.

Christopher Green, Alistair Bowen and Paulo Maggioni (all of Cardiff Business School) introduced new data on British long-term interest rates for the period 1866-1992. The series provides monthly observations of prices and yields for three groups of bonds: consols, local authority bonds and home industrials. Their paper forms part of a larger research project which intends to employ these data in a reappraisal of evidence on the determinants of long-term interest rates, focusing on monetary policy and the Fisher effect. Preliminary analysis of spectral density functions for monthly returns revealed a consistent cycle of about four years in long-term interest rates, but the authors were reluctant to speculate on the implications of this observation at this stage. Ashraf Mahate (City University Business School) suggested that it would be useful to extend the series for local authority bonds to include public utilities. Watson queried whether the new data offered such a significant improvement on existing series, given that the sample for each index was very limited. Mahate recommended that future analysis should employ co-integration to examine the Fisher hypothesis.

Brewing investment booms during the late 1880s and 1890s were regarded as manias by contemporaries. Katherine Watson (York) attempted to assess whether brewing share prices for this period were consistent with market fundamentals. Unit root tests on brewing equity and debenture prices, combined with analysis of the distribution of price innovations, suggested that share prices followed a random walk. Watson argued that the evidence did not support contemporary opinion that brewing investment was motivated by speculation. By implication, the financial crises facing brewers during the early 20th century were not due to the crash of a bubble, but rather to deterioration in the commercial environment facing the industry.

Weir expressed concern that, in rejecting the importance of speculation, Watson's findings by extension also rejected the orthodox literature on the drinks trade, and asked whether monthly data were sufficiently sensitive to reveal bubbles. Several participants argued that more

direct assessment of market fundamentals might be possible by examining, for example, the value of licensed property, property share prices and unemployment. Lewis suggested that Watson examine the discounted stream of future dividends over a longer period and see whether the *ex-ante* and *ex-post* stories were consistent. Neal wondered whether analysis of share prices for individual companies might simply reveal another example of a market for lemons.

Bob Millward and Sally Sheard (both of Manchester) employed Local Taxation Returns for 25 towns in England and Wales to investigate the growth in local authority expenditure on social overheads and infrastructure during the period 1870-1914. Expenditure on roads dominated local authority budgets, with current expenditure on police and public health barely keeping pace with population growth in many towns. Urban boroughs were revealed to be more dynamic than rural boroughs in responding to financial constraints, relying increasingly on trading profits to supplement rates as a source of income. Furthermore, rural boroughs proved less efficient than their urban counterparts in providing public services.

Their paper stimulated a lively discussion, especially since it raised issues familiar to the audience, many of whom were still reeling from recent upheavals in local government prerogatives and finance. With such contemporary questions in mind, Peter Scott (Oxford) asked whether more explicit political factors, such as the party in control of a council, influenced the distribution of public expenditure. Broadberry doubted the validity of the reported demand and cost elasticities since both equations were largely based on the same variables. Bronwyn Croxson (Cambridge) wondered whether differences between rural and urban councils could be attributed to effects of stronger urban civic pride. Bernard Harris (Southampton) thought that the significance of roads might be due in part to classification problems in the tax returns, so that, for example, expenditure on sewers, although related to growing concern about public health, might be included with expenditure on road construction. He also argued that infant mortality provided a better proxy for public health than adult mortality. Weir speculated whether this analysis of local authority expenditure could shed any light on the debate about the operation of the capital market during this period, and, in particular, on the view that overseas investment deprived the domestic economy of funds for social overhead capital.

Francesca Carnevali (LSE) took another theme with obvious contemporary parallels, examining the relative efficiency of public and private enterprise in the British gas industry during the 1930s. Her assessment was based on a comparison of costs, prices, technology and profits for municipal and corporate undertakings estimated from the 1935 returns reported in *Gas World*. On average both costs and prices were lower for municipal corporations than for private companies, and profits were higher. This relative efficiency was attributed to a greater commitment to long-term investment displayed by municipalities. By contrast, private undertakings were reluctant to divert profits from dividends into investment, especially in a period when the competitive threat from the electrical industry was very real.

Max Schultz (Swansea) was unhappy about the estimates of running costs and queried whether there was multicollinearity between capital intensity and other explanatory variables. Broadberry observed that it might be better to retain old technology in a declining industry, and Lewis suggested expanding the data set to address long-term efficiency and to examine returns on capital. Millward wondered whether, in addition to the threat from the electrical industry, a fear of nationalisation also deterred private gas companies from undertaking investment. Neal asked if this were yet another example of a lemon, with municipalities securing the most efficient plant and the private companies being left with expensive technology.

Amanda Berry (Oxford) drew on case studies of three English provincial voluntary hospitals to examine patronage between 1765 and 1815. Individual subscriptions clearly dominated hospital receipts but, despite remaining relatively limited, corporate patronage increased markedly over the period. Hospital income from all sources was vulnerable to economic fluctuations, with subscriptions varying inversely with consumer prices. Regional differences in patterns of hospital patronage reflected the peculiar characteristics of each institution and local economic and social conditions. Horrell suggested investigating the importance of local diversity by examining a larger range of towns, advocated analysing whether philanthropy was linked to a desire for social control, and asked whether patronage and certain diseases were connected. Watson wondered if 18th-century patronage patterns were parallel to varying perceptions of health status between wealthy and poor provinces in India, as Sen has observed. If wealthy towns were less

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Cover Story (Continued from page 1)

his desk at 5 when the call came from the Academy. Later in the day, he found he had to respond anew to the debate sparked by his and Stanley Engerman's still controversial book, *Time on the Cross* (1974). Re-emphasizing that to find slavery was economically efficient is *not* to endorse the system, Fogel said, 'It would be nice if efficient processes were moral. I don't think that's been the history of the 19th and 20th centuries. There is such a thing as morality, and morality is higher than economics.'

In St. Louis, a banner headline on the front page of the *Post-Dispatch* (10/13) announced 'Unassuming St. Louisan Wins Nobel Prize.' North said he had been awakened at 5:30 AM CDT by a phone call from the Swedish Academy, and that 'Somebody I didn't know formally announced to me that Bob Fogel and I had won the prize.' The article continued, 'Colleagues gathered in his office all morning' in celebration, but later North 'stopped taking phone calls so he could concentrate on his 11 a.m. class.' At a news conference yet later in the day, when asked if the prize offered vindication for his past iconoclasm, North replied, 'You're darn right it does.' Jeffrey Sachs (Harvard), told the *Washington Post* that North's work 'has profound implications for developing economies.' North's colleague, John Nye, captured one such implication for *Science* (10/22): 'You cannot just leave development to markets.' When asked if he should be a presidential advisor, North replied, 'No, I don't believe economists should shoot off their mouths when they don't know what they are talking about.'

Editorial opinion on the award, where it appeared at all, was diverse. *The Wall Street Journal* (10/13) announced 'Chicago Rules', citing 'the intellectual success that free-market ideas have enjoyed in the former Soviet bloc,' due in part to 'Chicago' economists whose works are represented by those of Fogel and, evidently by adoption, of North. At the opposite pole, *The Nation* (11/1) denounced the award to Fogel for the same reason: 'the purpose of the Nobel Prize in Economics is not to foster innovative ways of thinking about the world's economic problems, but to reinforce the hegemony of market ideology.'

Firm endorsement of the award was supplied by *The Economist* (10/16). 'Breaking with the prize's 24-year history, it has been awarded not to economic theorists or econometricians but rather to economic historians. This is good news. Economic history has frequently been marginalised by the more scientific and mathematically inclined professional mainstream, which has tended to

pooh-pooh historical processes. Now that economists are more prone to admit that social institutions play a significant role in economic growth, historical analysis is finally getting its due.' For Fogel's and North's methods of analysis, for their conclusions – whether accepted or not – and for their inspiration to a younger generation of economists, 'their prize is richly deserved.'

Frequent notice was given to the work and personal influence of both winners. Goldin, once a student of Fogel, sees the two as complementary. North's style, she said, is 'to discover a fascinating theory, then fill it in, and show how it helps us understand history.' Fogel's aim is to be known 'as the supreme empiricist, the great numbers person, the person who establishes what a fact is,' even if 'the facts he establishes are rather controversial.' (*Washington Post*, 10/13, and *New York Times*, 10/13, respectively.) A current colleague of Fogel, James Heckman, referring in *Science* (10/22) to North, Fogel, and other pioneers of the new economic history, said 'These people came into history carrying the weapons of science ... and like those bringing gunpowder to Europe, they were not always liked.' Lance Davis (Cal Tech) described North for *The San Francisco Chronicle* (10/13) as 'an exciting mentor who hooked him on economic history', who 'would throw an idea out and we'd yell and scream for hours' in 'one of the craziest economics seminars ever put together.' In the same article Richard Sutch (California-Berkeley), a major contributor to the heated scholarly critique of *Time on the Cross*, noted that the debate with Fogel and Engerman had 'focused a lot of attention on the potential contributions of scientific economic history. In the end it helped us all.' A summary view was provided by David Landes (Harvard) to *USA Today* (10/13), 'In order to understand today's problems, you have to understand how we got where we are – you need work that has a vertical dimension, extending through time... Robert Fogel and Douglass North are members of the first generation that brought this discipline to flower.'

Elsewhere in this issue can be found more coverage of the Nobel story: the full text of the Nobel citation from the Swedish Academy, an insert containing numerous responses from members of the Cliometric Society, and the tenth in our series of interviews with pioneering cliometricians. The apparent wisdom and foresight of your editors in selecting an interview schedule is ascribable only to sheer luck. It is therefore our great pleasure to present in this issue, to accompany our July 1990 interview with Nobel laureate-to-be Robert W. Fogel, our interview with Nobel laureate Douglass C. North.

THE JONATHAN HUGHES PRIZE Announcement

Jonathan Hughes was an outstanding scholar and a committed and influential teacher of economic history, who left a rich and varied legacy of excellence in teaching, from the many students he inspired to his highly successful textbooks. At their October 1993 meeting, the Trustees of the Economic History Association established 'The Jonathan Hughes Prize for Excellence in Teaching Economic History', commemorating Hughes's work both as scholar and as teacher of economic history. The prize, which includes a \$1,000 cash award, will be administered by the Committee on Education and Teaching in Economic History of the Economic History Association, whose members for 1993-4 are David Mitch, Chair, Samuel H. Williamson, and Mary Schweitzer. The winner of the first of these annual awards will be announced at the 1994 EHA meetings in Cincinnati, Ohio.

While many people contributed to establishing the award, we make special mention of the energetic leadership of Joel Mokyr and the highly successful fund-raising effort headed by Louis Cain. We also thank everyone who contributed to the prize fund.

The Committee anticipates that selecting the first winner will be an educational process. We look forward to learning of the characteristics thought to contribute to teaching excellence in economic history, as well as about individuals embodying those characteristics. Winners will be asked to describe their approaches to teaching in statements on 'The Rewards and Challenges of Teaching Economic History'.

The deadline for nominations is March 20, 1994. Nomination and selection procedures for the first award are stated in an accompanying notice in this *Newsletter*. The Committee wishes to ensure wide distribution of the Call for Nominations for the Hughes Prize, and has placed notices in a variety of publications and E-mail networks. We ask Society members to bring this notice and the Call for Nominations to the attention of others, and to suggest other publications in which a Call for Nominations might quickly be placed. Comments or suggestions may be sent to any committee member.

Committee on Education and Teaching Economic History Association

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THE JONATHAN HUGHES PRIZE Call for Nominations

The Economic History Association has established an annual award for excellence in teaching economic history in memory of Jonathan Hughes, who taught at Northwestern University from 1966 until his death in 1992. The first Hughes Prize, which includes a \$1,000 cash award, will be presented at the 1994 Economic History Association meetings in Cincinnati, Ohio. Each winner will be selected by the Committee on Education and Teaching of the Economic History Association. For 1993-94, the Committee consists of David Mitch, Chair, of the University of Maryland Baltimore County, Mary Schweitzer of Villanova University, and Samuel H. Williamson of Miami University.

The Committee is now calling for nominations for the 1994 Hughes Prize. Anyone may submit a nomination. Teachers are eligible from any institution of higher education, from community colleges to institutions with graduate programs. The Committee on Education and Teaching will rely *exclusively* on nomination statements to select a list of finalists from all nominations submitted. Statements nominating outstanding economic history teachers will be judged by their clarity, cogency, and insight into the teaching character of the nominee. Please provide a full profile of the nominee's contributions to all aspects and levels of teaching economic history. We emphasize that nominees teaching economic history at only one level of instruction will receive the same consideration as those who teach at multiple levels. Focus should be on teaching economic history, not on the nominee's other subject areas. The Committee may also solicit information directly from one or more of the people nominated.

Nomination statements should describe how the nominee exemplifies excellence in economic history teaching. There are no restrictions on format or topics covered, but we offer these considerations.

Teaching can be regarded as having three components, the teacher, the subject matter and the students. A statement might describe how the nominee excels in each component, discussing:

- the qualities of character and personality, both professional and personal, the nominee exhibits,
- how the nominee's teaching offers insight into the discipline of economic history, and
- how the nominee has recognized potential and has motivated superior performance among students.

Where possible, please provide specific examples of teaching excellence, describing, *e.g.*, students influenced significantly by the nominee's economic history teaching, a creative classroom presentation on a specific topic, or a particularly stimulating assignment or examination question.

Questions about preparing nomination statements may be addressed to any member of the Committee.

Deadline for nominations is March 20, 1994.

Three copies of nominating statements should be sent to:

Professor Mary Schweitzer
Department of History
Villanova University
Villanova, Pennsylvania 19085

Report on the Franco-American Conference

by Margaret Levenstein (University of Michigan) and
Philip Hoffman (California Institute of Technology)

(Cambridge MA) On July 15th and 16th The Franco-American Foundation sponsored a conference, in cooperation with the National Bureau of Economic Research, bringing together French and American economic historians. The conference was one of a series involving French and American scholars, organized by the Bureau and supported by The Franco-American Foundation. Its purpose was to introduce historical scholars to research underway on either side of the Atlantic using large bodies of quantitative evidence. The authors of this report unfortunately did not realize while attending the conference they would later share its events with the readers of the *Newsletter*. As a result, summaries of the discussion are based on the limited and faulty memories of Professors Hoffman and Levenstein. We of course assimilated the most brilliant comments into our own understandings of the papers and therefore do not credit the original proponents. Please forgive us.

Pierre-Cyrille Hautcoeur (École Normale Supérieure) assembled a data set for 1,000 firms listed on the Paris stock exchange between 1890 and 1939, and uses these data to ask whether agency theory can explain which firms had access to funds. Stock market issues, especially those of industrial firms, grew rapidly during this period despite the lack of government or industry standards which would inform stockholders of a firm's financial condition or protect them from opportunistic behavior by managers. He concludes that certain financial instruments, such as multiple voting shares, which facilitated such malfeasance, did not generally decrease a firm's ability to raise funds in the stock market, challenging applicability of the Jensen model to the French capital markets of this period. Discussant Peter Temin (MIT) commended Hautcoeur on his attempt to confront modern theoretical research with historical data, but argued that other theoretical approaches, such as those focusing on the life cycle of the firm, might have been a better starting place for the project.

Lance Davis (Cal Tech) and Robert Gallman (North Carolina-Chapel Hill) have compiled a data set of foreign stock and bond issues on the London City Exchange for 1870-1910 and use it to compare the volume and type of issue on the New York Stock Exchange with the issues of American firms in London. They show the London stock exchange provided more and earlier finance for non-

governmental, non-railroad interests than the New York Stock Exchange, and argue the difference resulted from a greater sophistication of British investors and from lower costs of issuing stocks and bonds on the London exchange. Flows of investment funds from Britain to Argentina, Australia, Canada, and the United States are contrasted: the US relied much less on British investment than the other three countries. The direction of the flow of British investment among the four shifted in response to changes in national demands for investment funds, while changes in the supply of funds from Britain were less important in determining the direction of investment. The discussion questioned their definition of 'sophisticated investors'. Did British investors earn higher rates of return than Americans? Was it necessarily more sophisticated to invest in newly-emerging regions and industries, as the British did? Others asked the authors to clarify the reasons for the higher cost of listing in New York. Was this due primarily to a more cartelized institutional structure of the New York exchange or to the higher costs of informing ignorant American investors?

Naomi Lamoreaux (Brown) and Kenneth Sokoloff (UCLA) discuss changes in regional and local patterns of inventive activity between 1840 and 1914. Analysis of the annual reports of the US Commissioner of Patents shows that New England had higher rates of patenting throughout the period. Patenting rates were highly correlated with relative *per capita* income, and, except in New England, patenting rates increased throughout the period. The precipitous decline in New England after 1890 led to a fall in the national patenting rate by 1911. A new sample of individual patents for 1870 to 1911 reveals a trend toward specialization and urbanization of inventive activity. Specialization in inventing, however, did not exceed specialization in production. They also reported an active market for new technology by 1870, evidenced by high assignment rates (frequently from individuals to companies) particularly in highly inventive regions. Finally, they explore the effect of the growth of large firms and new industries on patenting rates and on the location of patents and patent assignments.

Didier Blanchet (INSEE) and Denis Kessler (EHESS) reported on the results to date of the Seven Generation Survey (or Enquête TRA). This survey follows 3,000 individuals whose surnames begin with 'Tra' and who

married between 1803 and 1832. The survey follows their marriages, births, deaths, bequests, inheritances, and those of their descendants with the same surname (descendants of females are dropped), up to the present. The data set also includes information on residence, occupation, age, and the value of contracts at birth, marriage and death for each individual and his descendants. The central motivation of the study is to compare the characteristics of migrants, who have been ignored in most previous studies of French demography, mobility, and inequality, with those peasants who 'acted like peasants' and stayed put. Preliminary results, based on a sample of 46,000 marriage records from the 19th century, show higher than expected rates of mobility. The previously-reported increase in mobility during the course of the century is confirmed. Increasing rural to urban mobility is also confirmed, but a surprisingly high rate of urban to rural mobility (almost one-third) is now observed. Finally, for another sub-sample, following 31 dynasties in the departement of la Sarthe, they report a general increase in average wealth, an increase in wealth inequality, and no evidence of life-cycle savings patterns.

Kenneth Snowden and Nidal Abu-Saba (both of North Carolina-Greensboro) seek explanations for the failure of mortgage banking in the United States in the records of one of the largest mortgage brokers and bankers of the late 19th century, J. B. Watkins Company. Snowden and Abu-Saba hypothesize that informational asymmetries may have led Watkins to assign lower quality loans to mortgage-backed bonds rather than to broker them to individual investors, whose acceptance of mortgages was contingent on their approval of information about properties and borrowers. To their surprise, no evidence suggests that Watkins used his informational advantage to earn a higher rate of return on mortgage-backed bonds. Similarly, they found no evidence that mortgages marketed through bonds had higher default rates than those brokered individually to investors. Mortgage banking thus increased during booms, not to take advantage of uninformed investors, but to take advantage of economies of scale in the transactions costs of providing mortgage funds. The drastic failures of mortgage lending (60% of the loans in the sample were defaulted) resulted not from asymmetric information but from conscious 'overlending' on the part of mortgage companies. In support of this last contention, they provide evidence that when a mortgage was made, the risk premium received by Watkins (but not by investors, individual or institutional) was positively correlated with the likelihood of foreclosure. Discussion questioned the authors' assumption that an asymmetric information model implies that Watkins

would have been better off 'hiding bad loans' behind mortgage bonds. If one considers the importance of reputation, the opposite may have been true (supporting Jean-Laurent Rosenthal's contention that reputation models can be used to justify *any* behavior). Others asked whether Watkins' record keeping would even have allowed him to pool mortgages in the manner implied. Richard Sylla (NYU) commended Snowden and Abu-Saba for being willing to reject their initial hypotheses on the basis of empirical findings.

Philip Hoffman (Cal Tech), Gilles Postel-Vinay (I'INRA), and Jean-Laurent Rosenthal (UCLA) use a data set, originally compiled by the French government for genealogists, of 59,000 notarized contracts signed in Paris in 1751. These contracts include marriage contracts, wills, transfers of real property, and the purchase of annuities; the last group is the primary focus of this study. The authors find that an active, though not centralized, financial market allowed individuals to borrow and lend and to readjust their portfolios as desired, despite the absence of organized financial intermediaries. Notaries did not simply provide verification for enforcement of contracts; they also provided services (information matching) to potential borrowers and lenders, thus reducing the otherwise high search costs of contracting in such a decentralized market. Individuals with differences in wealth, family status and occupation relied on different kinds of savings instruments. Those with strong bequest motives, primarily widows and members of the aristocracy, tended to purchase privately-issued perpetual annuities. Those with a lesser interest in bequests, who were fairly risk-averse or whose incomes were already highly correlated with the government's financial situation, preferred private life annuities. The wealthy bourgeoisie displayed the greatest willingness to purchase government-issued life annuities. Finally, the authors argue that the high degree of loyalty of individuals to their notaries (70 to 80% returned to the same notary) can be explained as a mutual bonding mechanism to discourage opportunism and increase information sharing, both between customer and notary, as well as among notaries.

Patrick Verley (Paris) examines the demand for textiles in France and its effect on industrial growth. The bulk of his paper is taken up with apportioning national income among five classes of textile consumers for each decade between 1830 and 1910: peasants, workers, artisans and shopkeepers, white collar employees, and professionals and property owners. Verley estimates the income share that each class devoted to textiles and uses the result to

(continued on page 22)

Cruising the "Nets"

by Samuel H. Williamson

Readers who spend time browsing through the internet know that there is a burgeoning amount of information available on the "nets," and that the medium has both the blessing and the curse of being virtually instantaneous. It is now possible to give and receive immediate reaction to almost everything and to 'publish' with almost zero marginal cost — it is as if we are communicating in a daily newspaper. Last week's story is lost, buried in messages piled up since you last logged on. Many already regard the system as electronic junk mail, and have stopped subscribing to lists because they do not have time to read all the messages received. To find information on electronic resources to share with readers, I subscribed to several listservs dealing with history and economics, and some dealing with listservs themselves. I feel a sense of rapidly diminishing returns, as it could take an hour a day for me to read every message I received.

I still firmly believe this is *the* medium of the future, and that the system will continue to evolve and become easier to use more effectively. I think the people actively developing this medium are dedicated and hard-working, but those of us who will benefit from their efforts need direction. It is increasingly difficult to keep up with the growing number of resources and to learn how to access them. This article answers questions frequently asked about listservs, then presents a "primer" of Cliometric Society offerings and others which may interest readers.

What is a listserv?

A listserv is an electronic bulletin board. (In Europe, listservs are sometimes called "conferences".) People subscribe to the list *via* the internet; when any subscriber sends a message to the list, it is forwarded to all other subscribers.

What is a monitored listserv?

Some listservs forward all messages immediately. They are unmonitored. Others have an editor who checks messages and often looks for announcements to keep the list active.

What is Econhist?

ECONHIST is a monitored listserv for those interested in teaching and economic history. I am the current editor.

How do I register for Econhist?

To register for Econhist, please send the message SUBSCRIBE ECONHIST then your first and last name to Listserv@miamiu.acs.muohio.edu

What is the future of Econhist?

Econhist is our first listserv, and we are planning more listservs as demand develops. The H-Net listserv (see below) which started in February now has 21 lists with 5,000 subscribers, an example of this medium's potential. We plan to proceed with care to make our lists a useful academic tool and not electronic "junk mail".

How can I be more involved in the Econhist lists?

Subscribers might volunteer to edit new lists in economic history. Editors monitor postings to the list from subscribers, forward relevant announcements from other lists, and in some cases, solicit contributions of interest to the group. To maintain quality, an editorial board will advise the list editors. Co-editing is welcome. The only requirement for being an editor is to have an internet address. No technical knowledge is necessary; we have the staff to help with any problems. Graduate students and new faculty might find this an exciting way to network in the field. Readers may want to recommend current or former students. If you are interested in editing a list or wish to suggest a possible editor, please contact me at shwillia@miamiu.acs.muohio.edu

Clionet Gopher

As announced in our July issue, The Cliometric Society also operates a gopher server with a variety of information and files of interest to economic historians. We regularly add more files that you may find useful. The files include approximately 50 syllabi from economic history courses, abstracts of 1990-1994 ASSA sessions sponsored by The Cliometric Society, and data directories. We now have directories of members in the Cliometric Society, the Economic History Association, and the Business History Conference. Within seconds you can look up a member's email address and other pertinent information. We want to create the best and most up-to-date directory of economic historians. When you are so inclined, please check your own listing. If you find an error or if your information has changed, or if you want anything in your listing to be confidential, please let us know.

For new users, the address of the gopher is clionet.cas.muohio.edu. You can access our gopher using gopher client software or through Telnet, typing in "gopher" for the login prompt. See the July issue of the *Newsletter* for more information on using our gopher, or send an e-mail message to

Cliomet@sba-laws.sba.muohio.edu

Readers of earlier *Newsletters* know that we plan to add to the Society's electronic resources. The National Science Foundation has provided support for new hardware and software so we can expand our server capabilities. My most important goal is to make this server easy to navigate, one that helps users give and receive information, and is nonintrusive. I anticipate funding before the end of the year, so please watch your e-mail for announcements. Your ideas on how best to develop The Society's electronic resources in economic history will be greatly appreciated. Please send me your comments or suggestions.

Other internet sources

H-NET

Richard Jensen (University of Illinois-Chicago History Department) runs the very successful "History On-Line" ("H-Net") server, with 21 listservs devoted to history subtopics such as "Ethnic and Immigration History", etc. The lists have editors actively pursuing information, including course syllabi, bibliographies, book reviews, and other material useful to academic historians. I subscribe to the "Labor" list and receive 10 to 15 fairly long listings each week; many are messages which have been sent to all the lists. Econhist and H-Net will continue to maintain a close relationship, sharing announcements and material of mutual interest. To find out more about H-Net,

send the message GET HISTORY E-LISTS to Listserv@uicvm.uic.edu

Mailbase

Donald A. Spaeth (University of Glasgow) publishes a set of six Listservs on history subjects, oriented to European issues. One list is "History-Econ", similar to Econhist; others include "History-Teaching", concerning instruction, and "History-Methods" on research methodology. Like H-Net, subscribers to one list often receive messages sent to other lists as well. To subscribe, send the one-line message JOIN HISTORY-ECON to mailbase@mailbase.ac.uk then type in your first and last names.

Economics Working Paper Archive

Bob Parks at Washington University, St. Louis, is building a data base of working papers in economics that can be accessed electronically. His announcement says "econwpa.wustl.edu is an electronic working paper archive available to all economists with internet access of any kind. The archive provides an automated system for archival and distributing working papers in all areas of economics." For more information send an e-mail message to econ-wp@econwpa.wustl.edu with the subject GET ANNOUNCE

Franco-American Report (continued from page 20)

explain peculiarities of demand for textiles in France and French industrial growth. In particular, he argues that in the late 19th century the French textile industry suffered not only from a collapse of foreign demand, but also from weak domestic demand, caused by stagnant incomes among those who had the highest propensity to consume cloth: workers, artisans, shopkeepers, and white-collar employees. Discussion of Verley's paper focused on his method. Both commentator Hoffman and others in the audience worried that textile demand of professionals and property owners was estimated simply by making it a residual category. They urged Verley to seek other sources and to subject his results to sensitivity analysis. Pierre Sicsic (Banque de France) and others wanted Verley to frame a more precise counterfactual. Was low income among workers and white-collar employees the issue, or was it the somewhat vaguer notion of a 'consumer revolution' that social historians have bandied about?

Joseph Ferrie (Northwestern) analyzes the occupational mobility of European immigrants to the United States. His concern is the relationship between mobility and the immigrant's occupation before arriving to the United

States, a topic that historians have normally been able to examine only for narrowly circumscribed groups or geographic areas. Ferrie traces roughly 24,000 male immigrants who arrived in New York between 1840 and 1850 from passenger lists to the 1850 and 1860 Federal Censuses. He gathered a variety of information, including occupation and age at arrival and occupations in 1850 and 1860. Literate immigrants and those who moved to rapidly-growing cities were the most successful at moving up from unskilled jobs in Europe to skilled and white-collar jobs in the United States. Among ethnic groups, the British did better than the Germans, while the Irish trailed far behind, even if the personal characteristics of Irish immigrants are taken into account. The discussant Denis Kessler (Association Francaise des Sociétés d'Assurance) worried about the large number of Ferrie's 24,000 immigrants who could not be located in the censuses. Only 1,544 immigrants could be traced from passenger lists to the 1850 census, and only 1,192 to the 1860 census. He and others were concerned about biases that might have been introduced into the results by the loss of so many individuals. Ferrie responded that most losses were due to uncertain identifications; he did not think that his results would be affected.

In one of several papers on labor markets, Jean-Marie Chanu (CREST), Jean Heffer (EHESS), Jacques Mairesse (EHESS), and Gilles Postel-Vinay (I'INRA) look at wage disparities in 19th-century France. Their evidence comes from agricultural censuses of 1852, 1862, and 1892, industrial censuses of 1839-47 and 1860-65, and government investigations of 1892. The authors discover wide nominal wage disparities between regions and sectors; much of the difference in wages can be explained by cost-of-living differentials and personal characteristics of workers. Evidence from mid-century suggests that while labor markets may not have been well integrated at the national level, they did seem so at the regional level. Interestingly, there was no sign of increasing labor market integration over time. Finally, it was not until the end of the 19th century that women seemed to specialize in industrial employment both paying more than agriculture and rewarding education. The spirited comments of Gerald Friedman (Massachusetts-Amherst) focused on the political implications of the paper and on the wage regressions that formed the basis of the paper's conclusions, with Friedman even producing his own set of regressions for comparison. He and others questioned the rationales for including certain variables, and Friedman hoped that the authors eventually would add to the explanatory variables such items as union membership.

Labor markets were also the focus of a paper by Pierre Sicsic (Banque de France) on internal migration in 19th-century France. Using figures on mobility between French départements in the period 1831-1911, Sicsic estimates the wage elasticity of internal migration in France, finding that internal migration in France did respond to wages. Indeed, his claim is that migration was just as responsive to wages in France as it was in England. The target here, as in the other papers on French labor markets, is the old argument that industrialization in France was choked off by a shortage of labor – an argument that seems to be collapsing under the attack.

In his paper on educational opportunity on the urban frontier, David Galenson (Chicago) looks at migrants of a different sort: the Irish and Germans who flocked to the new metropolis of Chicago. Galenson's prime objective is to explain low rates of school attendance among Irish and German children. Using evidence drawn from the 1860 population Census, Galenson regressed school attendance rates for children on ethnicity, father's occupation, and family wealth. The regressions revealed that the direct effects of ethnicity were unimportant: once wealth or parental occupation is taken into account, German and Irish children were not less likely to attend school. Immi-

grants thus did not place a lower value on education, as some historians have argued, nor did they seem to hold education in lower esteem. Wealth determined whether and how long a child attended school, both for immigrants and natives. The paper concludes with speculation about the economic consequences of Chicago's commitment to mass education – a commitment that startled only the non-Midwesterners in the audience.

Clayne Pope (Brigham Young) examines the wealth and income of migrants further west: the northern Europeans who journeyed to Utah in the 19th century. These immigrants, converts to the Mormon Church, enjoyed the advantages of subsidized passage, gifts of land, and social acceptance. By 1870 such foreign-born households were in the majority in Utah. While Pope found that occupational distributions in Utah did not vary by nativity, the foreign-born appear to have been poorer than the US-born, with only two-thirds as much wealth on average. Controls for age, occupation and duration in Utah do not eliminate the wealth disadvantage of the foreign-born, but they had higher incomes than natives, controlling for the same variables. Where immigration and economic mobility are concerned, the Utah experience highlights the importance of local conditions and the significance of the distinction between wealth and income.

Taking a swing at generalizations of social and economic historians alike, Gilles Postel-Vinay and David Weir (both of I'INRA) examine the myths and realities of agricultural labor in 19th-century France. They too blast away at the assumption that agricultural labor markets withheld labor from French industry. They begin by noting that nominal wage gaps between city and countryside were low in France until the last third of the century. A rising gap late in the century and evidence about seasonality militates against any uniform trend toward market integration. One might still worry about earnings of peasants who farmed their own land and did not participate in the rural labor market, but evidence from agricultural and population censuses shows that such peasants earned as much in 1862 as urban artisans – yet another sign that the economy was not burdened with excess labor in agriculture. Like agricultural wages, peasants' earnings did decline late in the 19th century, a trend that the authors suggest was tied not to a peasant mentality but to the rise of government trade protection.

Festivities at the conference included a clam and lobster bake at the Harvard Faculty Club and many trips to the used book stores in Harvard Square.

North Interview (Continued from page 12)

into a room of other scholars – and I've done this so many times in my life now that it's ridiculous – and after a while we always dominate the conversation, not necessarily because we're loud, but because as a way of reasoning, we have a very powerful tool. You get to focus on the core of a problem right away, and that's the heart of what we ought to be doing. It's just that it's an inadequate tool unless you radically modify some of the assumptions of neoclassical theory. One is the rationality assumption, and the other one is that you've got to think about time, and that's a tough subject. It's what I'm working on now – I'm trying to work on time conceived as a theory of learning. What we should mean by 'time', I think, is how individuals and groups and societies evolve in terms of the way they perceive the world. That's really where time comes in. And then you're able to start to plug that into models which then can evolve and provide some context for why we're changing. The most exciting question in economic history is 'How do we get from there to here?' And we don't do a good job of answering that question.

Why can't cliometrics be seen also as including path-dependence and –

Oh, no, I think it should be. No, that's why I say I'm *still* a neoclassical economist; I don't want to abandon it – a lot of cliometricians think I've deserted them, but I don't think I've deserted them. I'm trying to drag them off into doing these things.

As is Paul David.

Yes, that's exactly what we want to do. We don't want to abandon all the things that made the cliometric revolution a revolution, which it *really* was. What we want to do now is to keep on extending its horizons to encompass problems that we didn't think of before. What's made me really change my view of the world has been the last six years, when I've become deeply involved in problems of development, where I've wandered around in the formerly socialist economies. I've gone to Moscow for the Soviet Academy of Science; I advised Vaclav Klaus in the Czech Republic at the time of privatization. I've just learned that we haven't really come to grips with things we should have had handles on. We should have been able to tell people how economies work over time, which, in turn, would give you a handle on how to make them work better. We have really not done that and, boy, it makes you very self-critical once you become deeply involved in problems of development, at least if you're an economic

historian. And that's what – more than anything else – really changed my whole approach to economic history, trying to understand the importance of property rights and other institutions in the countries in Eastern Europe. And a lot of the work I'm doing now is little incremental steps in that direction.

Continuing the cliometric revolution the way you suggest requires not only new questions and theory, but also what we might call continuous time data. Does an excess of comparative statics in economic history come from economizing on data collection?

Well, probably – in small part, it's a problem of getting different data. But even after that, I'd say if we get all the *different* data that I would like to have us gather, there's still have a big problem when you talk about time – and the way people learn. Learning is what makes changes in the choices people make, and the choices people make are what determine how economies work and, therefore, how they evolve. So learning is the key. True, you could say, well, it's exogenous forces – you know, earthquakes and so on – that, first of all, work through people's perceptions, but people mostly, to use an old hackneyed phrase, 'make their own history'. And they make their own history because they perceive the world differently through time. And *why* they perceive it differently and *how* they perceive it differently is what I would like us to get a handle on. Now that really doesn't answer your question very well, because then what kinds of data really get at that? Well, *some* can. Here's one that does indirectly, for example: I try to measure, as I'm doing in some Third World countries, transaction costs in particular markets through time, and then you go back and say, 'Okay, what made it so that transaction costs changed during this time period?' And then you look at the institutions and the things that changed, and then you ask yourself the still further dirty question, 'What made them change?' So it's sort of an infinite regress, but it gets you at the problem. But there's no easy way to answer that; that's a hard question to answer directly, I think.

Aside from yourself, aren't there a lot of people trying to break out of static neoclassical analysis, in addition to people like Paul David and Brian Arthur?

Yes, that's right. In fact, I said earlier that I really wasn't that bleak about it, but I thought that there were signs of younger people coming along who were doing interesting work in addition to old people (well, semi-old), like Paul David and Bob Fogel and Gary Libecap. All of them are

doing things that I think are really interesting. No, to name some, I think Jean-Laurent Rosenthal and Avner Greif and my colleague John Nye are real stars coming up in the profession, who have a real grasp of interesting issues and write well and are going really to be major contributors to changing the shape of the field. So I'm really quite optimistic about what's happening. What bothers me is that economics has been changing before economic cliometrics has. Economics is a very exciting place these days, at least wherever I seem to be invited, anyway, which is maybe exotic parts of economics. I was at a conference in Lund on law and economics that was just full of interesting stuff, and two weeks ago I was at a conference in London at which there were several good papers on Third World development problems. So there's a lot of work going on, most of which revolves around trying to deal with the problem of frictions, *i.e.*, the problem of why markets don't work well – political and economic – or they're trying to deal with the issue of rationality or they're trying to develop dynamic models of change, whatever a dynamic model of change is. But something in which you incorporate time – really, what makes economic history economic history, is that we're trying to explain things through time. And we didn't do that for a long while, and now I think we *are* starting to do it. So, no, I'm really quite optimistic about the field. But it has been very slow. You'd think that since we were the revolutionaries, we'd have *stayed* revolutionary, but we didn't. We made a revolution and then everybody sat back and spent at least 25 or 30 years saying, 'Wasn't this great?' and patting themselves on the back. Now McCloskey and I disagree. He thinks we're still revolutionary. I don't think so; I think we quit being revolutionary some time ago, and I think the proof of the pudding is that in economics departments the demand for economic historians has really dried up. I hope it's coming back again, but it did dry up for a while.

Wasn't the environment of the '60s so expansive anyway that an economic historian, with all the appropriate econometric and theoretical tools, was a nice 'consumption good'? But did the people who hired the economic historians in the '60s – the economists who did that – really think then it was important to have economic history and have since *lost* that view?

McCloskey would have the latter view; I think that we lost, and I think that cliometrics really was a very boring field for a while; there were a just lot of people running around testing hypotheses about the past, with not such good data, and doing the same thing economists were

doing. And I think economists said, 'Well, that's fine, it's nice to have one around if you don't have anything better to do, but they don't add a new dimension.' The dimension we're supposed to add to economics is that we're supposed to do things they *don't* do. And that's looking at the things they hold constant – that's why what Nate does in technology is so important, what Bob and Paul do in demography is so important, and all the institutional work, because it's adding a whole dimension that economists don't have, and I think that's all very exciting stuff.

But a lot of what is exciting in economics, as opposed to economic history, takes an awfully long time to filter down to the ordinary folk teaching economics and –

Oh, I think that's right. There's another thing I'm impressed with, too, which is that the other social sciences are becoming really interesting – political science, in particular. Now, political science in a way started to ape economics when they adopted rational choice models and all this jazz, but they are now much more receptive to change, because they don't have a paradigm like economics, where you develop formal mathematical models which are hard to break away from. Some of them have that now, and I think they're exciting – sociology and anthropology are full of interesting people. The Political Economy Center we have in St. Louis is just a very exciting place – we have people in law, business, finance and anthropology and so on – all of whom are doing very interesting work. And that combination makes it exciting. And I think that's another facet of economics – you can't be a good economic historian and just be an economist; all of economic history is really a mixture of political science and sociology and economics and law and anthropology, and you've really got to know your way around those disciplines. And that's asking a lot.

Sure. But can you be a good economist without knowing all that stuff too?

Well, okay, I'd agree with you there, I think you can't be a good economist either. But it's particularly true in economic history: it's more glaring, there are some things in economics – I can see where you can study financial markets, or other things, where the degree to which you have to go out of the field is quite limited, but in economic history, there's really no major topic you can find that isn't going to take you to the edges of other disciplines. And it should – and that's what makes it really the most fun discipline I think it is; it's much more challenging, it's exciting. *Still* exciting.

Since you've moved into institutional economic history, you've tended to publish your major ideas in a series of books. Which are you most pleased with?

I'm always most pleased with the most recent work, since it does represent an evolutionary development from my earlier work. But if I had to pick one that I think most completely and effectively put it all together at one moment in this evolutionary process, it would be *Structure and Change in Economic History*.

Why do you say that?

Well, I think I did two things there: I tried both to develop some theory and to apply it, to illustrate how useful it was. In my newest book, *Institutions and Institutional Change*, I was most concerned to develop the theory, and there isn't much history in it, even though it's developed because of history. And so *Structure and Change* has been very satisfying on that score. It's not that I don't like my new book – I like it a lot, but it's just that *Structure and Change* satisfied me a great deal at the time. One thing that turned out to be very interesting is that *Structure and Change* is starting to have some impact on economic history – there's always a lag of about eight or 10 years with books of mine – but the new book is already having a big influence outside economic history; the new book is now in its fourth printing and is being sold like wildfire amongst economists, political scientists, sociologists. I get stuff from all over the world about the new book, so it has had a completely different effect, but not much amongst economic historians; I don't really think that economic historians have paid much attention to it. But *Structure and Change* is starting to have some impact; even though I think that in *Structure and Change* the theory is very incomplete – it's much better developed later on – but it nevertheless had enough illustrations of the implications for history, so I thought it turned out better.

In *Structure and Change*, and in much of your other work in the past 25 years – wouldn't you say you've specialized in 'grand theorizing'?

Yeah, that's certainly the right way to put it. It is grand theorizing...

And Eric Jones has tried to do the same thing. Do you see an overlap of your work with his *European Miracle* or *Growth Recurring*?

Well, I like his stuff. I'm more of an economist than Eric, so I tend to think – to try to frame the issues – in economic terms, so that it's congruent to economic theory. I think that if we're to do our job right, we've got to make it so that we can tie in with what economists can do and understand. And Eric is a little less of an economist. On other hand, in some ways he's more imaginative than I am, he's more creative in the sense that he looks at things that I never would have thought to do, I mean, all this stuff on agriculture and a lot he's played around with are wonderfully exciting things, or on catastrophes. He's very good and a real important addition. You know, we need people like that. We need people who are going to do the grubbing but we also need people to try to provide some vision on the large issues. So I think he's fine.

How should a young person just entering the field begin? What path of research do you recommend?

I think every young economic historian should begin by doing empirical work and making a contribution to our stock of historical data. I would, of course, most like them to contribute data to flesh out the new theoretical developments that are occurring. Second, I'd urge them to carve out an important aspect of historical change, and dig in; that is, to undertake the ongoing and exciting task of the give and take between developing theory, and then empirical work, and so on. There's no substitute for learning by doing. Third, I would try to persuade them that economic history is no better than the theory we possess, and that the theory is so far woefully inadequate. Young scholars should not only be up-to-date on innovations in economic theory that appear useful, but also in the related social sciences as well. As I said, you can't be a good economic historian just by knowing economic theory; you must also have knowledge in depth of the history relevant to your research. That's an awesome set of requirements, but it's an awesomely challenging field of scholarly research.

You recommend knowing a lot – the theory, the related fields, and collecting the data. Should an economic historian cut her teeth doing the data grubbing? Perhaps that's not professionally wise these days?

Oh, I think that is professionally wise. I think you should get tenure before you go out and do crazy things; and getting tenure means you do things that the discipline, in this case economics, is going to buy and accept in journals. And that's pretty conservative stuff. Avner Greif is probably the most interesting exception that I've seen.

I've never seen anybody else who could go out – he's writing about belief systems and culture and all kinds of things – I think they're great but they're also terribly dangerous, but he is smart enough so I think he'll get away with it. But most young people, you know, should cut their teeth on providing some empirical foundations. That shouldn't be dull: there's no reason they can't be somewhat imaginative, but it's got to be within the frame that can get published.

For empirical work you have to go into the raw materials of history, maybe an expensive proposition in terms of career. When 'straight' economists say 'empirical' they often mean things (like running regressions) that don't mean anything like that to a historian.

No, I'm very old fashioned here. By 'empirical' I mean data-gathering; I mean going to the public records office or whatever, because there is no substitute for getting really queasy about understanding what a number means, and you don't understand what a number means until you've tried to gather them, and, boy, that's a sobering experience. When I did the balance of payments for the United States, I found that just a very tiny change in the initial assumptions I made about the indebtedness of the United States, 70 years later at the end of the period, would have made a change of *astronomical* proportions. So, number-crunching and number-gathering and putting that in historical context is something I think everybody ought to do, just so they see what they're doing. You can't theorize or do anything else grand without first of all understanding some real empirical work, and by that I mean qualitative as well as quantitative.

Counting is useful, but knowing *what* you're counting—

Knowing what you're counting, and whether what you're counting is *really* what you're counting – those are all critical parts of the whole story.

That's surely true for economic history, but empirical work, defined as you just did, doesn't cut much ice in some economics departments, does it?

Well, I think that the direction economics has gone, to formal theory and mathematics, is ridiculous: it's become more and more sterile, concerned less and less with anything that has any possibility of being applied to empirical work, and it therefore has become less tied to

empirical work. If economics loses sight of that link, it loses sight of everything that's valuable now. Even at MIT or Minnesota or Stanford, where they still have high-powered mathematical theorists, there's still a *big* blooming of other people who realize you must have close ties to empirical work, to the real world, trying to solve real problems. And I think that's happening and I think that's very salutary. And so I disagree with lots of people who want to go in other directions. I fight about that endlessly in my own department. Lance was grouching to me over dinner the other night about how all the formal game theorists are dominating, and I agree – I like game theory, and I use it – but formal game theory is as sterile as high-powered mathematical economics.

Yes, but it's interesting that the basis of what you like in Avner Greif's work is formal game theory.

Yes, and Avner and I were just arguing about this, and I said, 'All right, you show me, I'm perfectly willing – if you can show me, provided that it adds an important dimension to the explanation of how economies evolve, I'll buy it.' He thinks he can. I think the power of game theory – and it's the way I've used it – is that it makes you structure the argument in formal terms, in precise terms, so you use it just like neoclassical theory: it's a foil against which to think properly about the issues. But after it's done that, it doesn't seem to me that game theory adds a lot. Now Avner thinks it does add more than that, particularly since I think that it doesn't lend itself to dynamic change, that is, to change through time, and he does. So I said, 'Fine, then show me.' Since he's doing a book for me, we're going to see. You know, I've been wrong a lot about this – every once in a while when I tell a good theorist that, well, there's a lot of baloney in what you're doing, they'll show me that in some dimension I'm wrong, that the theory is useful. So I remain open – at least I hope I remain open – to those things. And I think that's important. I think of myself as a good neoclassical economist, but as a neoclassical economist who really wants to widen, open up new boundaries, modify the rationality assumption, introduce transaction costs and imperfect information and all these kinds of things, and then we've got an exciting body of theory.

Learning, too –

And particularly learning. That's where I'm going now, trying to figure out how people learn and what that does to our theories. Now I keep quoting (and indeed I got the John R. Commons Award a couple of years ago, and in

that I quoted) Frank Hahn from a 1990 issue of the *Economic Journal* where they asked leading economists to forecast the next hundred years. He said, 'This is the end of high-powered theory.' He said that the future of economics is going to be less precise; it's not going to make formal mathematical economists happy, but it's going to be more historical, more concerned with, related to the other social sciences—I don't know, he went on and on. When Frank first said this, I was at the Center [for Advanced Studies in the Behavioral Sciences in Palo Alto]—and Frank I know from the days when we were at Cambridge together. He gave a talk to the Stanford Economics Department in 1987 in which he said 'Mea culpa—I think I've come to a dead end...General equilibrium theory and all this, it has nowhere to go.' And he said, 'The future rests with institutional economics.' He said that. And Paul Milgrom in the back of the room asked 'Why don't you talk to Doug North?' and Frank said, 'I have!' But it's true, I think that that's exactly where it's going—and somebody that bright, like Ken Arrow, sees the same thing. There are a lot of people who are very smart and realize that the future really lies in developing these other areas. The trouble is, of course, that we don't attract economists because we can't develop a lovely and neat body of theory which you can formalize in mathematical terms. We may never be able to do it. And I can see where that would be terribly frustrating—if I had all my human capital invested in a body of theory that I could form into a set of equations, I'd love it. But I can't—and I doubt that we ever will. Now, to the extent we can, we should. And you know, people like Paul David—they might be able to do it, because certainly I'll never be able to do it.

But you know, it's very exciting. Somebody asked me the other day, if I could start all over again, what would I do different? And I said, well, I might like to do it better, but I said I wouldn't change a thing. It's been a very satisfying 40-some-odd years. Very, very satisfying.

Including the Merchant Marine?

Yeah. The Merchant Marine was fun; I wouldn't possibly have ever done it if I hadn't had those four years to sit and think about what the hell to do. Oh, no, that was very, very good.

Note: A summary of North's work and of the evolution of his views on major issues in economic development and history can be found in Gary Libecap, "Douglass North", in Warren J. Samuels (ed.), *New Horizons in Economic Thought: Appraisals of Leading Economists*, Edward Elgar, 1992.

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willing to tolerate certain diseases, this might result in larger gifts being offered to hospitals; donations might in some sense reflect civic pride. Croxson raised the question of hospital admission policies as a factor influencing patronage: if admission were granted only to local patients, philanthropists seeking to sponsor individual patients would also tend to live nearby. Neal argued that the analysis be based on nominal subscriptions, since flaws in the Schumpeter-Gilboy price index would produce spurious spikes in a series of 'real subscriptions'; Weir thought that subscriptions might well be less volatile than prices due to a permanent income effect on charitable giving.

Using a principal-agent framework, Bronwyn Croxson (Cambridge) analysed admissions to the Middlesex hospital between 1745 and 1900. The primary objective of the hospital was to maximise the number of successful treatments, with the result that sick people were not always admitted, or were discharged prematurely if they were thought incurable. However, benefactors were also allowed to sponsor individual patients who otherwise might not have received treatment. Although recommendations were monitored by a Weekly Board and by medical staff, conflicts still arose between personal sympathies and maintenance of a cost-effective admissions policy. Despite lobbying by medical staff, abolition of letters of recommendation was resisted due to fear that this might inhibit fundraising. Nevertheless, as medicine developed, medics were able to screen potential patients more effectively, so that by the end of the 19th century the role of Governors' letters had diminished in importance in rationing health care.

Dudley Baines (LSE) asked for the data to be classified by sex to reveal whether there were different attitudes to the treatment of male and female patients. He also regretted that more general epidemiological data are unavailable, since they would allow a direct assessment of the extent to which hospitals were responding to local health needs. Although Harris accepted the benefits accruing from such a long-term study, he cautioned that changes in hospital objectives, and changes in acceptable responses to these objectives, might cause problems for the analysis, urging Croxson to consider exercise of social control among the motivations of patrons. Finally, he observed that morbidity data from Southall's study of medical insurance records might provide useful evidence on non-fatal diseases. Neal wondered whether there was a parallel between patronage in the US Federal Government and the story described here: political favours be-

came increasingly costly for Senate, Congress and the Presidency to dispense, so administrators took over. Similarly the influence of medics on real decision-making increased with the professionalisation of medicine, and letters were retained purely to sustain subscriptions via the 'feel good factor'.

Bernard Harris (Southampton) presented a review of recent work in anthropometric history, indicating the contribution to established debates in economic and social history offered by research of this kind. He drew attention to the large literature about the impact of economic and social variables on height, and on using height as an indicator of changes in living standards. Komlos' attempt to integrate anthropometric study into an analysis of the causes of industrialisation in the Habsburg Empire was also discussed. Harris suggested that future research should be directed towards improving our understanding of sex differences in height, and also towards the question of intergenerational influences on physiology. In addition, he called for research into changes in the timing of physiological development, such as for example the ages at which different generations have matured.

With reference to Fogel's and Engerman's argument that slave owners did not seek to maximise the number of children born to slave women, Watson asked whether evidence suggested that on average a slave woman gave birth to her first child later than other women did and, if so, whether this might indicate a desire on the part of slave-owners to delay pregnancy and avoid impairing the efficiency of highly productive young women. She also drew attention to recent reports about the Human Genome Project suggesting that in the future many individuals might be able to predict their age and cause of death from genetic information. This raised questions about the extent to which anthropometric studies were affected by the variability of individuals' genetic endowments. Horrell welcomed awareness of the need to investigate gender differences in height, citing Nicholas's study of convicts, which showed that rural women seemed to have suffered more from the effects of industrialisation than urban women, since their employment opportunities were reduced. Anthropometric research could therefore illuminate study of family budgets. Croxson requested elaboration of the relationship between height and social status, thereby stimulating a lively discussion about Social Darwinism and drawing out the insecurities of all the 'small' people in the room.

Ashraf Mahate (City University Business School) began

his paper by seeking to identify the causes behind the failures of Overend Gurney Co., the City of Glasgow Bank, and Baring Brothers. He then employed capital market data to calculate abnormal returns and test for contagion effects from these failures. Mahate concluded that contagion effects operated in England but not in Scotland following the failure of Overend Gurney, but that the pattern was reversed with the City of Glasgow Bank failure. The failure of Baring Bank was contagious neither in England nor Scotland. Neal observed that interpretation of Mahate's results would be eased considerably if the estimates of cumulative abnormal returns were presented in annualised form, and suggested that the distribution rather than the mean of abnormal returns might also warrant analysis. He then asked whether the late 19th century was the best period in which to study contagion effects, since by contrast with the US, banking in the UK was then stabilised by the Bank of England acting as a lender of last resort.

Paul Turner and Sue Bowden (both of Leeds) constructed a model to investigate the impact of stop-go policies on the British car industry during the 1950s and '60s. They argued that this sector was especially sensitive to the stop-go cycle because of the direct influence of changes in taxation and hire-purchase conditions on domestic demand for cars. Fluctuations in domestic demand then impeded the long-term development of export markets. Given the industry's inflexible production technology, its long-term investment was also inhibited by market uncertainties. Furthermore, the burden of adjustment to fluctuations was carried largely by labour rather than capital, exacerbating the industrial relations problems for which the British car industry became notorious. Ultimately, stop-go policies contributed significantly to the competitive weakness of UK car manufacture.

Broadberry was not persuaded that the uncertainties facing the postwar British car industry were any more severe

than during the interwar period, adding that fluctuations in motor vehicle production for the period 1955-1970 were greatest for two of the most successful car manufacturing countries, Japan and the USA. He also doubted there had been underinvestment in the British car industry: since investment remained a constant proportion of income, perhaps over-manning gave the impression of inadequate capital. He then argued that the model presented should allow for damage from declining production to exceed benefits from increasing production. Finally, Broadberry suggested that an examination of overseas demand and Empire effects might provide a more fruitful source of demand-side explanations for problems in the British car industry. Weir queried whether unemployment was the best indicator of income uncertainty in their model of the long-run desired vehicle stock; he proposed including the trade balance instead. Millward suggested they consider the impact of stop-go policies on domestic industry in general. In a valiant attempt to boost the morale of the presenters after the Broadberry onslaught, Croxson argued that there might not be an inconsistency in the paper if demand-side factors had initiated supply-side problems.

The conference drew to a close with a discussion of how to fund future meetings. Whilst academic commitment to these workshops was unreserved, the ESRC has unfortunately withdrawn financial support. Participants were asked to contact Katherine Watson (York) or James Foreman-Peck (St. Antony's, Oxford) with suggestions for future funding and venues. Meanwhile the arms of the Cambridge contingent were gently being twisted with a view to hosting the 1994 conference in a university which might be more likely than most to have spare resources. Since the end of the conference, we have received two pieces of encouraging news: Solomos Solomou (Peterhouse, Cambridge) has agreed to organize the meeting, and the Royal Economic Society has promised a portion of the desired financial support.

Happy Birthday  Doug and Lancel

The NBER/DAE 1993 Summer Institute

(Cambridge MA) The Development of the American Economy Program (DAE) of the National Bureau of Economic Research (NBER) held its annual Summer Institute July 12-14, 1993. Organizers were Jeremy Atack (Vanderbilt) and Charles Calomiris (Illinois), who will also organize the 1994 Summer Institute. Ten papers and two works-in-progress were presented.

Monday, July 12:

Joshua Rosenbloom (Kansas and NBER): "Looking for Work, Searching for Workers: US Labor Markets after the Civil War."

Mary Eschelbach Gregson (Wabash): "Population Dynamics in Rural Missouri, 1860-1880."

Christopher Hanes (Pennsylvania): "Changes in the Cyclical Behavior of Real Wage Rates, 1870-1990."

Robert Margo (Vanderbilt and NBER): "Explaining Black - White Wage Convergence in the 1940s: The Role of the Great Compression."

Tuesday, July 13:

Sukkoo Kim (UCLA, now Washington): "Expansion

of Markets and the Geographic Distribution of Economic Activity: Trends in US Regional Manufacturing Structure, 1860-1987."

Mario Crucini (Ohio State): "Tariffs and Economic Activity: A Dynamic General Equilibrium Approach."

Richard Grossman (Wesleyan): "The Shoe That Didn't Drop: Explaining Banking Stability During the Great Depression."

Hugh Rockoff (Rutgers and NBER): "The Meaning of Money in the Great Depression."

Wednesday, July 14 :

Michael Haines (Colgate and NBER): "Mortality in Massachusetts."

Richard Steckel (Ohio State and NBER): "Long-Term Trends in the Distribution of Wealth: Evidence from Ohio and Massachusetts, 1820-1910."

Alan Green (Queen's and NBER): "Immigration into Canada in the 1920s" (Work-in-progress).

Claudia Goldin (Harvard and NBER): "Measuring Educational Attainment" (Work-in-progress).

Copies of papers may be obtained from the authors.

Two Pioneers of Cliometrics

The Cliometric Society has published a commemorative volume to celebrate the award of the 1993 Nobel Prize in Economic Sciences to Robert Fogel and Douglass North. The five reprinted essays provide a perspective on the achievements in economic history recognized by the Nobel Committee.

Two Pioneers of Cliometrics, available at cost, can be an inexpensive way to introduce students in economic history or historiography to the field and two of its founders. Multiple copies may be ordered for class use; ask the Society about postage rates for such orders.

The volume contains: "Douglass North as a Teacher" by Jonathan Hughes, "Douglass North and the New Economic History" by Richard Sutch, "Robert William Fogel: An Appreciation by a Co-author and Colleague" by Stanley L. Engerman, "Robert William Fogel: An Appreciation by an Adopted Student" by Donald N. McCloskey, and "The History of Cliometrics" by Samuel H. Williamson.

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Members Comment on the 1993 Economics Nobel Prize

This special insert contains the thoughts of Cliometric Society members on the Nobel Prize. These thoughts are in response to fax or e-mail requests sent to each member of the Society for whom we had either type of number. We hoped to contact every member, but we found that some fax or e-mail numbers were incorrect, unknown to us, or nonexistent. If you did not receive a request, please accept our sincere apology.

Some statements have been edited for length.

I'm overwhelmed. In awarding Nobels to Bob and Doug, I think the committee is rewarding the best traditions of economic history: the search for facts about our economic past on a grand scale, and the search for stories to tie the facts together, also on a grand scale.

I went to Harvard thinking I would become a mathematical economist, but I was soon won over by the ideas, and more importantly, the people who gravitated to Bob Fogel's workshop. There really is an extended Fogel family, and I am grateful every day to be part of it. I know Doug's students feel the same way about him.

The awards can only help increase interest in economic history.

Robert A. Margo, Vanderbilt University

I was Doug's RA for two years and he was my dissertation adviser. When I talked to him I told him that I could scarcely contain my excitement for him. I said that I was thrilled that I had the opportunity to work and study with a Nobel laureate, but that I could not even imagine what the thrill must be like to actually be a Nobel laureate. His reply was that the past two days had been pretty hazy, especially since sleep was nearly impossible, with the phone ringing literally 24 hours a day.

Mike Hauptert, University of Wisconsin - La Crosse

The Nobel Prize award to Bob and Doug celebrates their achievement and also the achievement of cliometrics. The first generation of new economic historians, of whom Bob and Doug are preeminent members, brought economic history to the mainstream of modern economics. We who follow in their footsteps can only applaud their work. The only fly in the ointment is that the prize could not be awarded to the group as a whole.

We should remember that the Nobel committee has awarded the prize in this and past years to economists who have broadened the field. Economic history should not be simply applied economics; we need also to insist on the importance of the history in economic history. The Nobel Prize therefore reminds us as we remind our students that we need to be both good economists and good historians. We congratulate Bob and Doug and try to learn from their honor as we have learned from their work.

Peter Temin, MIT

At the Harvard Economic History Workshop we celebrated the event, and everyone was excited. It was absolutely marvelous for economic history and cliometrics. The halo effects are significant, and it has had a big effect on Harvard graduate students in the pipeline, as well it should. All the rumors and leaks had been pointing in this direction since the 1991 events at Lund/Stockholm, so I wasn't at all surprised by the choice; it was only a matter of time. Three cheers for Bob and Doug. They deserve it!

Jeffrey Williamson, Harvard University

Viewed from economic history, of course, the prize marks the accomplishments of cliometrics, or 'historical economics' as it might be called. But as North would himself point out, historical economics had deeper roots. Brinley Thomas, Alec Cairncross, T. S. Ashton, and others were writing economic history informed by modern economics in the 1940s. W. W. Rostow, it is often forgotten, wrote cliometrically in the 1940s, too. I like to torture economists by pointing out that their beloved technique of total factor productivity, in the price dual form, was invented by G. T. Jones, a student of Marshall, and applied to historical questions in 1933. Eli Heckscher, who in 1919 had most of the ideas that constitute modern trade theory, was a historical economist. In part the revolutionary tone of American cliometrics is a result of the backwardness of American economic history. Applications of economics that Jack Fisher or Ashton at the LSE would find routine were shocking to a field still thinking of itself as a branch of American institutionalism, a trans-Atlantic version of the German historical school, in opposition to British economics.

Viewed from economics - after all, it is a prize in economics - the prize marks, I hope, the beginning of

the end for blackboard economics. Well, maybe just the end of the beginning. Few of the Nobel prizes have been awarded for empirical work. Most of them have been awarded for blackboard economics. Most economists, and startlingly even some economic historians, believe that empirical issues can be settled on the blackboard with no recourse to the world. That is to say, many economists since Hobbes have believed that great social questions can be solved by staring at a blackboard. Not all economists think this way, of course – two who do not are Douglass North and, most passionately, Robert Fogel. The blackboard optimism in fact violates the principles of economics: it can't be so easy – it is a free lunch – to answer serious questions.

So some of us hope that increasingly in future the Nobel Memorial Prize in Economic Science will be awarded for economic science rather than for economic mathematics. We hope that the prize to North and Fogel is the beginning of a trend.

Donald McCloskey, University of Iowa

I am sure I speak for a great many economic historians working in Europe when I say that we warmly applaud the award of the Nobel Prize to Bob Fogel and Doug North. It has given us renewed pride in our subject and great optimism for its future.

Timothy J. Hatton, University of Essex

When I learned of the award I was reminded of a line that Paul Samuelson wrote years ago with regard to being a young scholar at Harvard in the late 1930s. I sent the following as part of a letter congratulating Doug on the prize:

"To have been born an economist before 1936 was a boon – yes. But not to have been born too long before!

"Bliss it was in that dawn to be alive,
But to be young was very Heaven!"

[Wordsworth]

And bliss it was to be young in Seattle before 1960 as we watched you invent cliometrics – or the New Economic History as we called it then.

I guess it's not the new kid on the block any more.

Roger Ransom, University of California-Riverside

Hurray for Bob and Doug! Their triumph, much belated, is a reflection on all of us, who have been working in the trenches of economics for decades.

Joel Mokyr, Northwestern University

Aside from being extremely happy about the Nobel awards and feeling that this recognition was long overdue and well-deserved, we should all be grateful to our two Nobel Laureates for the elevated status that they have generated for our field.

Jeremy Atack, Vanderbilt University

I count myself fortunate to have been a student of both Doug North and Bob Fogel. In the late 1970s as an undergraduate at the University of Washington I took two of Doug North's graduate seminars. Doug captured your mind and soul through dialogue and debate. Resistance was useless. No matter how much you might argue and disagree, Doug never gave up. You were always his student and ultimately you would learn and appreciate his lessons (sometimes not until years later).

In the early 1980s as a graduate student at the University of Chicago I had the good fortune to take classes and sit in on seminars with Bob Fogel. I was not one of the graduate student foot-soldiers in the Fogel research empire. And peering in from the outside, Bob was somewhat of an enigma. As students we learned more through observation and emulation than through dialogue and debate. When a historical problem was raised, Bob would amaze us by doing some quick calculations which produced what seemed to be a definitive answer. Most of us would have been content to stop with that answer; then we realized that to Bob, these quick calculations were just a prelude, and that 10 years' commitment to a detailed research program was called for to substantiate the answer. As students he imparted to us the joy and rewards of pursuing our research in a serious and meticulous manner.

The reaction of colleagues in my Economics Department was mixed. Some were not sure who Bob and Doug were. The Nobel Prize imparted legitimacy to economic history for a day, maybe a week. Others were not surprised by the award and believed the award was richly deserved. The work and reputation of Bob and Doug had made substantial and serious penetration into their fields, from public choice to labor to industrial organization. The best reaction I heard, however, was from my 70-year-old mother. She told her Senior Citizen's

Center bridge club group all about her meeting with Bob in 1982 and showed them the picture of him she had taken where he was licking the frosting off his cake plate!...I guess to prove that the great are human, too.

Farley Grubb, University of Delaware

I am delighted by the Nobel Committee's recognition of the new economic history, and of Bob and Doug. Cheers to the Nobel Laureates!

Bob Gallman, University of North Carolina

My unconsidered view – bloody great.
Peter Wardley, University of the West of England

Economics is richer and more exciting to those who take economic history seriously, and without doubt, economic history in this generation is made more meaningful and exciting by the imaginative contributions of Doug North and Bob Fogel. As a student and friend of both, my own work has benefited greatly from their guidance and encouragement. In a real sense their reward is a reward for all of us who accept the challenge of writing economic history. Recognizing these pioneering scholars as Nobel Prize winners in economics underlines the essential role that economic history plays in economic understanding.

Trevor Dick, University of Lethbridge

Although I was not a student of Bob's, I have long admired his work and taught it to my students. The controversies surrounding the work have served to increase interest in the work that economic historians do, and it makes it easier to teach economic history. More recently, however, I have been participating in Bob's research on health, longevity, mortality, and morbidity. The work is daunting in its data requirements, and it is largely the energy and determination of Bob which has pushed it forward. I believe that we will learn a great deal about historical health and about disease processes while, at the same time, finding how to use and interpret some difficult historical documents (namely, medical records). I hope that the Nobel Prize provides further incentive to our discipline to continue in those sometimes risky and lonely inquiries which constitute our research.

Once again, congratulations to Bob and Doug. Well-deserved recognition!

Michael R. Haines, Colgate University

The power of tradition in governing academic matters hereabouts notwithstanding, the awards of the Nobel Prize in Economics this year and their characterization by the press as 'tradition-breaking' have been welcomed with public and private expressions of great satisfaction among British economic historians of cliometric persuasions. *The Economist* newsweekly for 16th September began its account of the event thus:

In choosing this year's Nobel prize winners for economics, the Royal Swedish Academy of Science has perpetuated some traditions and broken others. As often before, it would be wrong to describe 1993's laureates, Robert Fogel of the University of Chicago and Douglass North of Washington University in St. Louis as famous [sic]. Yet breaking with the prize's 24-year history, it has been awarded not to economic theorists or econometricians but rather to economic historians.

This is good news....Messrs Fogel and North won the prize for their pioneering work in 'new economic history' – applying economic theory and statistical methods to the discipline of history.

And so all say: Amen.

Of course, *The Times* [of London] in its story could not resist quoting Hemingway's remark to the effect that nobody who had received the Nobel Prize [for Literature] ever had written anything worthwhile thereafter. But, can we not trust that if such is the tradition, it too will be shattered by this year's heroes?

Paul A. David, All Souls College, Oxford, and
Stanford University

My first reaction was – YESSSSSS! And naturally I was happy to see two outstanding economic historians win the prize. As I have told my colleagues, however, these were not the first economic historians to win. I suspect that both Fogel and North would recognize Simon Kuznets as having done some very good work in economic history. So I see this as two fine economists, who happened to work in economic history, walking off with the prize this year. I am very happy that they did work in economic history as they have made it an interesting, enjoyable, and exciting area in which to work. I presume the same can be said for previous winners, and it will no doubt be true of the next economic historians who win.

Tom Weiss, University of Kansas

This year's Nobel Prize in Economics recognized two great practitioners of the best traditions in

economic history: the collection and careful sifting of evidence, and the skillful interpretation of results within a theoretical framework. The awards told the world the importance and relevance of the field of economic history and it has been a personal pleasure to discuss Bob's and Doug's contributions with colleagues and friends.

Richard H. Steckel, The Ohio State University

In recent years, the Nobel Prize for Economics has often gone to recognized leaders in sub-disciplines of economics. But simply being a 'leader' has not been sufficient for the prize: the award has gone to those who have arguably started a major school of thought within economics. Public choice, finance, and even econometrics are examples. The award to Robert Fogel and Douglas North continues this tradition. Cliometrics is an approach to economic history so radically different in methodology from the 'old' economic history that it now represents a new school of thought. Robert Fogel and Douglas North were recognized for their parts in starting this new school of thought. But part of the excellence in Fogel and North's work is the fact that they carefully tie the lessons they learned from economic history to modern day. The economic past is relevant to the economic present. This Nobel Prize for Economics signals to the rest of the economics profession that economic history can greatly inform us about the major economic questions we face today. Congratulations to Robert Fogel and Douglass North!

Rick Sullivan, College of the Holy Cross

When I heard on the car radio that two Americans had won the Nobel Prize in economics for their work in economic history, I knew immediately that it had to be Fogel and North. The only surprise, I suppose, was that the award had gone to economic history in the first place. That it had was of course an immense delight for all the rest of us, who can bask in Fogel and North's glory and tell neighbors that we do the same thing. As for the reaction of colleagues, the prize seems to signal economic history's acceptance within economics. One of our own game theorists told me that Doug's most recent book is one of the best things he'd ever read; for him, it fits in perfectly with the current agenda in economic theory. I wonder, though, what the reaction will be in history departments: Will the prize get historians to read economic history again?

Philip T. Hoffman, California Institute of Technology

I have many thoughts about Doug and Bob. My formative years, four in graduate school and two as a post-doctoral fellow, were spent with the two of them. Their hospitality, guidance, and support have made the difference in my career at several critical junctures, and I know that I am not the only person who can say that.

I met Doug 19 years ago, when I was an undergraduate at the University of Washington. He was my honors independent study advisor, my dissertation chair, and co-author on several papers. I am an economic historian because of Doug, and what attracted me was what he thought we should be about as scholars. One afternoon, shortly after Doug had moved to St. Louis, I was visiting for the weekend to work on a paper. We were upstairs talking, smoking cigars, and listening to music. Doug got up to change a CD and said, 'The one thing I would most like to do in this world is write a symphony, but I can't, so I am going to try to explain how society works instead.' Doug has always asked the most interesting questions.

My closest association with Bob came during my two years as a post-doctoral fellow at Stigler's 'Center for the Study of the Economy and the State.' The only condition Stigler put on my fellowship was that I not come to Chicago to study with Fogel, a measure of George's respect for Bob. One thing stands out from many things I learned from Bob. He gave a seminar here in Washington, D.C., and presented a draft chapter from *Without Consent or Contract*. I spent the day talking with him, and at about midnight, as I dropped him at his hotel, he said, 'The goal in this profession is to influence how other people think.' Bob and Doug are the two economic historians best at influencing how other people think.

I know I have said too much, but just one more point. One of the major pleasures of being an economic historian is the company of other economic historians. Doug's contribution to our social interaction is fundamental. *The New York Times* called me on the day the prize was announced to get my perspective on Doug. The reporter asked if Doug was a curmudgeon. I told her, no, Doug wasn't a curmudgeon, but that he was a smart-ass. Doug's irreverence, his persistent challenge to accepted authority and accepted fact, his enjoyment of pointing out who has no clothes on, is not only his characteristic, it is characteristic of us as a profession. Doug's irascibility, though, is balanced by his genuine care for people, his wide and accepting circle of friends, and his delight in all the good things of the world. This was made painfully aware to me when we co-authored our transaction sector paper in Volume 51. We were pilloried in Lance Davis's

comments and Bill Parker's *JEH* review. I realized that Doug's most violent critics were also his best friends! You can disagree vehemently with Doug for hours on end, then he grins that Doug North grin, you have a Jack Daniel's, and the vehemence is gone. Doug embodies the contradictory spirits of fractiousness and friendship that make cliometrics vital fun. Doug's long ago University of Washington economic history seminar, with Lance Davis and John Hughes as students, may have launched the cliometric style, if not the cliometric revolution.

John Wallis, University of Maryland

In Finland we have been very positively minded for it. I'm working in history department, and all the people has seen the prizes as gold medals for economic history. In the economics department the situation was not the same because cliometrics has no place there. I suppose that things will be changed very soon there. I send with fax copies of two articles I was asked to write for the local daily newspaper *Keskisuomalainen*, which is one of the most influential newspaper in this country. The text are in Finnish, which is not so easy to learn.

Ilkka Nummela, University of Finland

In addition to my admiration of the Prize committee for having selected researchers whose contributions are so deserving, I also feel gratified that the committee is acknowledging the importance of our style of applied economic analysis. First Gary Becker and then – in the very next year – economic history. Can it be possible that this is evidence of a trend in the profession at large?

Carmel Chiswick, University of Illinois-Chicago

The Nobel Prize announcement was really great and sensational news for Russian historians and economists. As a cliometrician, I would like to emphasize the importance of the award toward raising the consciousness of our economists as to the significance of cliometrics. This news has been enthusiastically appreciated by all Russian cliometricians.

I consider the awarding of the Nobel Prize to Robert Fogel and Douglass North as a very important event for developing cliometrics in Russia – the prestige of the Nobel Prize is very high here. I prepared an article about this event for the *Russian Bulletin of Quantitative History* and for some other academic journals. Both

persons awarded are very well known in Russia; their works have been discussed in the '70s and '80s in a number of articles and books published here (including text books for students-historians). It concerns especially Fogel's works which have been criticized intensively, but as a result all the students of historical departments here considered his works on the contrafactual simulations as very interesting and intriguing. His works gave an impulse for developing of simulations in economic history research here. Illustrating the importance of the field here, three years ago Dmitry Levchik presented his PhD thesis on American Cliometrics to the Institute of General History of the Russian Academy of Sciences and today's Nobel Prize recipients were in the center of consideration.

As to my personal recollections – I met Robert Fogel three times in my life (in Moscow, in Spain and in London). In all cases I was impressed by his modesty, the wide range of his academic interests and inquisitiveness. These features reflected distinctively in his interview published in *The Newsletter of The Cliometric Society* three years ago. I translated it for the *Russian Bulletin of Quantitative History*, and after its publication I received a lot of responses which reflected enchantment and appreciation of Fogel's academic opinions and "life philosophy".

Leonid Borodkin, Moscow State University

When I took up my appointment at Melbourne, I mentioned to a meeting of research students that the Nobel Prize had been won by economic historians for the first time. Only one out of six knew either Fogel or North! Now all research students do a subject called Theory and Method in Economic History where they read Fogel on slavery and North on institutions.

Stephen Nicholas, University of Melbourne

I was elated when I heard the news. My applause woke up the family. However, I was not entirely surprised. About three years ago at some conference, I remember discussing the possibility of an economic historian winning the Nobel Prize. Somebody, I think Bob Margo, matter of factly said that some day Fogel and North would jointly win the prize. The prize is very significant because there are disbelievers out there who do not take Cliometrics seriously and who think it was a passing fad. In fact, there is no alternative to Cliometrics. How could anyone argue against rigorously testing his-

torical hypotheses by examining empirical data in the light of widely accepted economic theory? Cliometrics is the essence of the scientific method and it is a well established discipline.

On Wednesday after the Nobel Prize announcement, I spent about 15 minutes discussing Fogel's and North's work and Cliometrics in my Principles of Economics class. The next week I gave the midterm. One of the identifications was 'Cliometrics.' One student got things a bit confused. I enclose her definition: 'Cliometrics – the measurement of the musical scale. This application was used by the Nobel Peace Prize winner Roeggel in order to study economics of today.'

Robert Whaples, Wake Forest University

The Nobel Prize in Economics for Bob Fogel and Doug North is not only well-deserved but long overdue. Now it is up to the rest of the economic history profession to make sure this is a breakthrough for our specialty and not simply a farewell gesture.

Larry Neal, University of Illinois

Here at Washington, Lee Benham called me at 6:00 am in the morning to let me know that Bob and Doug had won the Nobel Prize. Amazingly enough I got through to Doug on the phone. He said he'd been awakened before 5:30 by the call from Sweden and had already given 15 interviews including one tv interview before I phoned him at about 6:10 am. I also called Joel to let him know. The chairman of our department then called me to see if I'd found out and we rushed to school that morning to prepare for the day. I think that for all of us Cliometricians, this has been powerful recognition of the seriousness and importance of the work we've been doing collectively for the last 30 years. Economic history has been a strange sort of stepchild in economics and having Bob and Doug win the award was a vindication of our life's work. At another level, we felt that Doug's sharing the prize was also a vindication of his more recent work (the last two decades or so) which we have all supported at Wash U. in which institutional and political concerns are blended with standard neoclassical theory. Doug, and to some extent most of us at Washington U., have tried to work within a slightly broader framework than standard theory dictates without abandoning most of the core precepts of that theory. Doug has been playing around with loosening individual rationality of late, but even he admits that's a tough nut to crack. For the most

part, what he and many other members of the profession realize, is that empirical work that focuses on the boundaries of markets and the institutional constraints in history is an important source of new knowledge that's important for the future of economics and history. It's still grounded in the theory we know, but speculate about other aspects that theorists can't quite formalize yet. That makes the work less tight than standard neoclassical work, but not so far removed from the mainstream that we reject the core that we know so well. As a colleague, Doug has been a real inspiration, both as founder and former director of the Center for Political Economy. His seminar series brought in the widest possible range of speakers from cliometrics, political science, game theory, public finance to anthropology and philosophy. Our department has made economic history a priority area of research partly because of Doug's success in convincing all his economic colleagues of how exciting broad-minded social science research can be. When North addressed the conference held in St. Louis over the weekend organized by R. Gephardt to discuss the problems of business and other exchange with the Newly Independent States that included dozens of politicians, business leaders, and representatives from the U.S. and the former Soviet Union, cliometricians will be pleased to note that only North received a standing ovation. In some ways, economic historians are best placed to give policy advice since our work is so inherently applied. And we would do well to heed Doug's advice to pay more attention to linking our work in cliometrics with the needs of development economics.

John V.C. Nye, Washington University

My associations with and memories of both Bob and Doug are fresh and warm. They have both been extraordinarily helpful to me over the years, as they have been to many in our profession. They deserve their award because of their impact but their impact has to be measured by more than the cogency of their ideas. They have been humanly – and humanely – a potent force in economic history that offers a lesson to us all. I am immensely proud to know them both and pleased that their contribution has been so stunningly rewarded.

You asked for recollections as well as reflections and reactions. My best memory of Bob Fogel is as his student taking his research seminar in economic history at the University of Rochester in the spring of 1963. (It was my last course in pursuit of my master's degree. Fogel gave me an 'A' for my efforts – but that is beside

the point, even if I treasured it as an act of unwarranted generosity.) The part that struck me most forcefully then and that has stayed with me ever since was his distribution of the chapters of his first book, the one on the Union Pacific Railroad with the assignment that we write critiques of the book. Full of student hubris, I questioned his logic and his data. Kindly and politely, but forcefully, he concluded the session in which we discussed my paper by telling the class that he had so structured his calculations, by weighting them against his argument, so as to subsume my objections. It was a powerful lesson. I have spent the rest of my days trying to get my data better so as to avoid ever again being subsumed!

Fortunately he took no offense from an over-eager graduate student and was, that same semester, instrumental in my obtaining admission to and financial assistance from the University of Pittsburgh from which I got my doctorate. He recommended me to Carter Goodrich, one of his own mentors at Columbia, who directed my dissertation. My debt to him is thus over three decades old.

John McCusker, Trinity University

Cliometrics Is Not The Name Of A Disease

As there is almost no such a thing as economic history in the French-speaking economics departments of Quebec, many were wondering when they heard about the 1993 Nobel Prize if cliometrics was not just a form of econometrics. Others found 'clio' much more intriguing. I have been asked once if cliometrics was the name of a disease and last week if it was not something a little *osé* ('cochon'). There was nothing to change my colleague's mind when I thrust in his hands a copy of my recent article to explain it all to Quebec historians entitled '*Cliométrie, économie, et histoire: un ménage à trois*.'

Ruth Dupré, École des Hautes Études
Commerciales de Montréal

Perhaps a personal recollection will best illustrate Bob's approach to dissertations.

When I began discussing a topic with him he gave me only one piece of advice. Remember that committing oneself to dissertation is a very serious decision. It's like getting married except the dissertation may last longer. For a time I worked on a project related to his own current interest in slavery. I soon 'divorced' that topic and moved on to one in a completely different

area. But Bob remained equally willing to devote his time and energy to helping me.

Hugh Rockoff, Rutgers University

The awarding of the Nobel Prize to Bob Fogel and Doug North has been received as great news among the small but compact band of Spanish cliometricians, who have been busy explaining to the public in general and to some colleagues how deserved the award is. I personally received the news from a newspaper call asking me to write a note about the new laureates, something I did with great pleasure.

It has not constituted a total surprise, at least to me, since rumors about the possibility of the Nobel Prize being award to one or two economic historians (Bob and Doug being among the most insistently mentioned) circulated in the profession.

The last time I saw Bob and Doug was in Santander at the Second World Congress of Cliometrics. They impressed me as being in great intellectual shape, and the papers they both presented were extremely interesting.

Two final recollections: one, as many people have thought, something ought to be done about this to improve the image of economic historians among economists and also among historians. Although cliometrics is stronger in the US than in any other country, the chasm between economic history and its sister (or mother) disciplines, economics and history, is too large. I was appalled the last time I stayed in the States by the total lack of communication with the historians (mostly through the historians' fault), who seem to be fascinated by anthropology and horrified at statistics.

Second, although this could be considered the first Nobel award to economic history, our discipline has had other brushes with the Nobel Prize: this was the case when Simon Kuznets received it, and also W. Arthur Lewis, John R. Hicks, and Milton Friedman. They all have been at least part-time economic historians.

Now let us start working to get another Nobel Prize for economic history.

Gabriel Tortella, Universidad de Alcalá

Congratulations, damn it.

In my view, our 'new economic history' has three components:

1) The construction of some elements of a dynamic socio-economic theory that takes, at least, some imper-

fect account of change over time in major elements in economic life and behavior (*i.e.*, institutions) in their interaction with one another and with the surrounding social and socio-psychological environment;

2) the collection of quantitative as well as literary evidence and its deployment in a statistical structure to permit testing of generalizations derived from the theory;

3) The fusing of insights suggested by the arrayed evidence, by the theory, and by the historian's informed intuition, into the artistic construction of narratives and interpretive essays, showing the economic behavior of societies and social sub-groups, moving and modified by movement, over time

The Swedish Academy has not inappropriately awarded the Nobel Prize in Economics to recognize two bodies of fertile and powerfully displayed work which incorporate the first two of these components. Perhaps the younger generation now on deck – the students of our first, crude generation of fore-fathers – will strive, better than we seem to have done, to achieve the artistic synthesis that can make full-bodied economic histories of the West, or some other world area, accessible alike to social scientists, humanists, and an educated public at large. The third component, I have always felt, has an importance equal to, or greater than, that of the other two.

Bill Parker, Yale University

The awarding of the Nobel Prize in Economics to Bob Fogel and Doug North celebrates the accomplishments of two eminent economists and economic historians. In both cases the award is well deserved. Self-interestedly the Cliometrics Society and scholarly community of economic historians also view the award as recognition of the importance and contributions of economic history and in particular the application of the tools of economic analysis and quantitative methods to issues in economics and history.

The award, however, goes beyond the recognition of these factors. The scholarship of both Bob and Doug embodies two additional features: the importance of institutions and the usefulness of inductive as well as deductive modes of analysis. Indeed each of these scholarships has done pioneering work in assessing the influence of institutional arrangements on economic behavior, explaining of the origins of institutions, and analyzing the evolution of institutions. The work of Bob, Doug, and others has helped to bring the recognition of the importance of institutional analysis to the larger profession of economics. As a result of their work it has

become more widely understood that there are four basic components to any economic situation: resource endowments, preferences, technology, and institutions.

The work of each scholar also embodies the importance of inductive approaches in suggesting relationships among the factors that shape economic performance and change. Inductive generalizations arising out of historical inquiry have provided important insights for and challenges to economic theory.

In sum the Nobel Prize award recognizes two of the finest members of the tribe of the economic historians and the field to which they have both contributed.

David Feeny, McMaster University

A colleague met me in the hall as I came to the office and asked, "Do you know these guys Fogel and North?" I asked, "Why?" "They won the Nobel Prize," he said. I then told him some essentials about each. I have known Bob longer and had more contact with him but Doug's work has had much greater direct influence on my own research, especially his earlier work on regional trade. Both have written papers that, at the very least, altered the conventional wisdom of economic historians and economists alike. They have also generated external effects for other fields as well. Both men are first class scholars and superb spokesmen for their own views and economic history in general.

William Hutchinson, Miami University

Cliometrics Sessions at the 1994 ASSA Meetings

The Cliometric Society will sponsor four sessions at the 1994 annual meetings of the Allied Social Science Association in Boston, January 3 through 5. A Clio Cocktail Party to celebrate the Nobel Prize award to two of our members will be held Tuesday evening. Join us in congratulating Doug North; unfortunately, due to a prior commitment, Bob Fogel is unable to attend. The Monday session is a joint offering with the American Real Estate and Urban Economics Association, and the Tuesday morning session is co-sponsored with the American Economic Association. Program Chairs John C. Brown and Martha L. Olney selected and grouped papers sure to hold your interest. All sessions will be conducted in traditional Clio style: Authors and discussants will begin with a 15 minute presentation, with the remainder of the session devoted to open discussion of the papers. Summaries of the papers are attached so you can prepare to participate in the discussion. Complete papers, references, and bibliographies are available on request from the authors of correspondence with The Cliometric Society. Authors' addresses appear on Page 2.

Urban Economic History (Joint session with AREUEA)

Monday, January 3, 2:30 p.m., Sheraton Hotel, Liberty E
Presiding: Louis D. Cain, Loyola University-Chicago and Northwestern University

Curtis J. Simon and Clark Nardinelli (Clemson University): *Information as Infrastructure: The Growth of English Cities, 1861 to 1961*

Elyce Rotella (Indiana University) and Louis D. Cain (Northwestern University): *Death and Spending: Did Urban Mortality Shocks Lead to Municipal Expenditure Increases?*

John C. Brown (Clark University): *Trams, Income, and Urban Structure: Munich before World War I*

Edward McDevitt (California State University-Northridge): *The Evolution of Irrigation Institutions in California: The Rise of the Irrigation District, 1910-1930*

Discussants: Thomas A. Downes, Northwestern University

Jon C. Sonstelie, University of California-Santa Barbara

Growth and Technological Change: Lessons from History (Joint Session with AEA)

Tuesday, January 4, 10:15 a.m., Sheraton Hotel, Liberty A
Presiding: Gavin Wright, Stanford University

Yasukichi Yasuba (Osaka University): *Natural Resources in Japanese Economic History: 1800-1940*

Louis D. Johnston (Gustavus Adolphus and Bowdoin Colleges) and Karin E. Stawarky (Bowdoin College): *Cost Disease and Deindustrialization in Historical Perspective: The American Case, 1840-1989*

Alan Dye (Universidad Carlos III de Madrid): *Outgrowing the Plantation: A Regional Comparison of the Asset Specificity Problem and Technical Change in the Cuban Sugar Industry, 1899-1929*

Discussants: Gary Saxonhouse, University of Michigan-Ann Arbor

Carol E. Heim, University of Massachusetts-Amherst

Ross Thomson, University of Vermont

Topics in American Labor History

Tuesday, January 4, 2:30 p.m., Sheraton Hotel, Liberty A
Presiding: Michael R. Haines, Colgate University

Robert A. Margo (Vanderbilt University): *Explaining Black-White Wage Convergence, 1940-1950: The Role of the Great Compression*

Boris Simkovich (Harvard University): *Long-Term Trends in American Intergenerational Occupational Mobility*

Dora L. Costa (Massachusetts Institute of Technology): *Pensions and Unemployment*

Discussants: Jon R. Moen, University of Mississippi

Michael R. Haines, Colgate University

Samuel H. Williamson, Miami University

Cliometric Society Cocktail Party

Tuesday, January 4, 8:30 - 10:30 p.m., Sheraton Hotel, Hampton A & B

Understanding Collective Action and Outcomes

Wednesday, January 5, 8:00 a.m., Sheraton Hotel, Liberty G
Presiding: Martha L. Olney, University of Massachusetts-Amherst

Michael Huberman (Trent University) and Denise Young (University of Alberta): *What Did Unions Do? An Analysis of Canadian Strike Data, 1901-1914*

Eugene P. Sigel (University of Massachusetts-Amherst): *Populist Kansas: A Human Capital Interpretation*

Kyle D. Kauffman (Wellesley College): *The US Army as a Rational Economic Agent: The Choice of Draft Animals in the 19th Century*

Discussants: Gerald Friedman, University of Massachusetts-Amherst

Richard H. Steckel, Ohio State University

Robert McGuire, University of Akron

See Page 42 for information about other sessions of interest to members.

Cliometrics Sessions at the 1994 ASSA Meetings in Boston

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Session I: Urban Economic History

Joint Session with AREUEA

Presiding: Louis D. Cain, Loyola University-Chicago and Northwestern University

Monday, January 3, 2:30 p.m.

Information as Infrastructure: The Growth of English Cities, 1861 to 1961

Curtis J. Simon and
Clark Nardinelli
Clemson University

I. Introduction

We investigate the relationship between the population growth of cities and their initial level of human capital. We argue that if human capital generates external economies, cities whose industries use information more intensively should grow faster, other things the same. In our first (1992) paper, we found that American cities that employed higher proportions of highly skilled professionals grew faster over periods of 50 and 100 years over the period 1880-1980. This result suggested that the availability of information services drove the typical city's long-run population growth. If the connection between information and city growth is not merely a curious attribute of the American data, it should hold for other nations. In this paper, we examine the growth of 79 English (and Welsh) cities between 1861 and 1961. We find that English cities grew faster, the higher the employment share of business professionals in 1861, holding constant the city's size in 1861.

That we are able to corroborate our findings for America using data from England is valuable not only because replication is important, but because England's urban system was already mature in 1861, the starting year of our study and a reasonable date for the end of the first industrial revolution. In contrast to America's urban system, which had barely reached adolescence, the relative growth of English and Welsh cities mostly reflects the competition between existing cities for rural and between-city migrants.

Our findings provide support for theories that identify specialized human capital as the engine of economic growth. In Lucks (1988), for example, externalities arise from higher levels of human capital due to spillovers of knowledge. In Roamer (1987), increasing returns in the production of intermediate capital goods give rise to a wider variety of capital services. We think that it is plausible to model intermediate information services in a similar fashion. It is plausible that external economies are strongest within a city because the costs of acquiring and exchanging information, especially the costs of the frequent exchange of small bits of information are lower within a city. Indeed, Backer and Murphy (1992) argued that specialization and the division of labor are more often limited by the costs of coordinating specialized workers than by the size of the market. The city can be thought of as an organization that reduces the costs of coordination by reducing the costs of collecting and disseminating information.

II. Theory

We were led to consider the importance of human capital by recent developments in growth theory, especially Lucks (1988), who argued that cities were "mainly and convincingly concerned with the external effects of human capital" (p. 37). Lucas formulated a model in which the total level of human capital in a society increased the productivity of each person, given their individual level of human capital. The acquisition of knowledge of one person, then, generates external economies for everyone else in the city.

We adopt the notion of city-specific human capital, introduced by Glaeser *et al.* (1992) to help explain the relative growth of cities. The ability to contact people at low cost is important because much economic activity requires face-to-face contact. It may well be that the communications revolution may someday eliminate the need for face-to-face contact, but it certainly has not done so yet. In our theory, we associate the stock of city-specific human capital with an information service sector, that is, a sector in which people's main task is the gathering or dissemination of information. It is reasonable to suppose that much of the information gathered and provided by people within a city becomes a public good linked associated with those people and to that city.

The ties between firms and the patterns of commerce that become routine are a manifestation of what might be thought of as a city's information infrastructure. The acquisition of information capital is costly and irreversible; once acquired, much information can be exploited at virtually zero marginal cost within the city. To exploit such information between cities is more costly. Empirically, we associate the city's information sector with people who have high levels of human capital. The link between information and human capital is straightforward; the external economies that arise from activities that do not routinely involve the acquisition of or transmission of knowledge – examples include barbering, retailing, and tailoring – appear to be small.

III. The Model

In our work on relative city growth in America, we adapted Rivera-Batiz's (1988) model of equilibrium city size, which applies Roamer (1987) model of intermediate product differentiation to the city. We augmented the model to include the concept of city-specific knowledge, introduced by Glaeser *et al.* (1992). We assume that each city produces a manufacturing good (according to a Cobb-Douglas production function) using labor, land, and knowledge:

$$(1) \quad M_t = f(L_{Mt}, T, I_t),$$

where L_{Mt} is the quantity of labor, T is the quantity of land (which we assume is fixed), and I_t is the quantity of information services, all at time t . There are n_t types of information services, where n_t is determined endogenously. The output of each of the n_t components of the service sector is S_{it} , and each S_{it} is produced subject to increasing returns to scale using only labor. In Rivera-Batiz (1988), I_t is a function of $S_t = \sum_i S_{it}$ and the variety of services,

n_t . In our version of the model, we make I_t a function also of the current stock of city-specific knowledge, K_t . We therefore have:

$$(2) \quad I_t = g(K_t, S_t, n_t).$$

The relationship between I_t and the S_{it} depends on the degree of substitutability between the S_{it} . The less the degree of substitutability, the greater the impact of variety on I_t .

The first order conditions for employment in manufactures equates marginal revenue product to the wage at each time t . We can solve L_{mt} , S_t , and n_t in terms of total city population, $L_t = L_{mt} + L_{st}$, where L_{st} is the quantity of labor employed in the information service sector. It is then possible to show that the movement of wages over time is given by:

$$(3) \quad \ln(W_{t+1} / W_t) = \alpha \ln(L_{t+1} / L_t) + \beta \ln(K_{t+1} / K_t),$$

where α and β depend on the parameters in the production functions (1) and (2). Whether α is positive or negative depends on the degree of diminishing marginal returns to labor, the share of information services in output, and the degree of substitutability between information services. The term β is positive, indicating that other things the same, a higher rate of growth of city-specific knowledge leads to higher wage growth in the city.

The key question in our theory is what determines the rate of growth of city-specific knowledge over time. Jovanovic and Rob (1989) modeled the growth and diffusion of knowledge, emphasizing the importance of being able to exchange ideas at low cost. In particular, they argued that it is important for many people with different ideas to mingle, for people could then synthesize new, better ideas. We build this notion into our model by assuming that the rate of growth of city-specific knowledge is positively related to the number of people employed in the information sector, L_{st} , and the variety of information services, n_t :

$$(4) \quad (K_{t+1} / K_t) = h(L_{st}, n_t).$$

As the city grows (and wages rise) over time, residents bid up the price of land, causing utility to fall, other things the same. Rivera-Batiz (1988) assumed that city residents maximize a Cobb-Douglas utility function. In our version of his model, there are two goods: an internationally traded consumption good and housing. We assume that there is a fixed amount of residential land in the city. The rate of growth of utility of a resident in the city is then given by:

$$(5) \quad \ln(U_{t+1} / U_t) = \theta_1 \ln(W_{t+1} / W_t) - \theta_2 \ln(L_{t+1} / L_t),$$

where θ_1 and θ_2 are the (positive) exponents on goods and housing in a Cobb-Douglas utility function. Long run equilibrium requires that utility be equal across cities at each time t , implying that utility grows at the same rate across cities. Replacing equation (3) into equation (5), the growth of utility over time is equal to:

$$(6) \quad \ln(U_{t+1} / U_t) = \theta_1 \beta \ln(K_{t+1} / K_t) + (\theta_1 \alpha - \theta_2) \ln(L_{t+1} / L_t).$$

If cities were identical, they would grow at the same rate. One way to generate differences in growth rates is to assume that initial conditions differ. We assume that initially, some cities had a higher concentration of human capital, which in our model translates into a larger number of people in the information sector (L_{st}) and a wider variety of services (n_t). According to our hypothesis in equation (4), this will cause a higher rate of growth in the level of city specific knowledge, and, by equation (6), such cities will grow faster, other things the same.

IV. Growth Regressions

Our theory states that cities with greater initial proportion of highly skilled providers of information will grow faster, other things the same. We collected data on population and on certain occupations for the 79 leading cities in England and Wales for the year 1861, our base year. The census divided eighteen general occupational categories into numerous sub-categories. We experimented with several different measures of professional employment, but settled on a rather narrow one: business professionals, which includes bankers, brokers, agents, accountants, bookkeepers, insurance agents, merchants (not retailers), among others.

We entered professional employment as a share of total employment. Our city growth regressions are of the form:

$$(7) \quad \ln(\text{Final City Size} / 1861 \text{ City Size}) = \text{Constant} + \beta_1 1861 \text{ City Size} + \beta_2 1861 \text{ Professional Share} + \text{error}.$$

Final City Size is city population in either 1911 or 1961.

The empirical results are contained in column 1 in Parts A (1911 final year) and B (1961 final year) of table 1. The estimated coefficient on professional employment share is positive and significant, indicating that cities with a higher share of professional employment indeed grew faster over both the 50 and 100-year periods following 1861.

We tried adding lawyers and engineering and scientific persons to business professionals, but doing so only weakened the relationship between growth and professional share. On the one hand, this surprised us because the opposite was true of the results we obtained for US cities. On the other hand, the base years in our growth regressions for US cities were 1880 and 1900; perhaps the later time period is the reason for the different results. We also experimented with a very broad category of professionals, which included doctors, druggists, dentists, actors, artists, teachers, musicians, and writers, but this category did not help us to predict growth. In sum, although we find higher growth in cities with greater employment shares of professionals, the composition of professionals that help us predict growth is different for English and Welsh cities than for American cities.

A. Transport Cost and Manufacturing Base Theories of Growth

There are, of course, other theories of city growth. Some theories emphasize the role of the city as a break in transport, that is, as a crossroads, terminus, and cross-shipment point. We therefore tried augmenting equation (7) with various definitions of transport employment share. Other theories emphasize the importance of manufacturing. During and after the industrial revolution the leading English cities exported a high proportion of their output. Cotton had much to do with Manchester's growth, shipping with Liverpool's, and steel with Sheffield's. North (1955) used an export base model of regional growth to explain differences in population growth across regions. Some versions of the manufacturing base model imply that cities that start with proportionately larger manufacturing sectors should grow faster, other things the same.

We ran regressions similar to those in equation (1) above, but with transportation and manufacturing employment shares entered along with professional employment share as explanatory variables. We found that employment on river and sea were the only good predictors of city growth in the transport sector. Our measure of manufacturing employment

is restricted to employment in the factory industries: textiles, mining, iron and steel, metal-working, glass and pottery, industries that get chapters to themselves in histories of the industrial revolution and the ones that would head most people's Top Ten list in "manufacturing base" theories.

These regressions are contained in Column 2 of Table 1. As can be seen, manufacturing and water employment shares help to predict city growth over the 50-year period, but not over the 100-year period following 1861. This did not surprise us. The initial advantages of access to water must have been less important in the age of road, rail, and air. As for manufacturing, city growth due to a successful export need not be self-sustaining; when the particular industry's growth levels off, the city's growth may also level off. The coefficient on professional share, however, remained positive and significant.

We tried one last measure of information services: the employment share in telegraphs. These regressions are reported in column 3. The coefficients on professional share fall and the coefficients on telegraph employment share are positive, but insignificant. The coefficient on professionals for the final year 1961 is insignificant, but this is because of a single outlier. Coventry's growth rate (adjusted for variables other than professional share) was the highest of the 79 cities in our sample, a result that we attribute as probably due to the buildup of industry there during the Second World War.

V. Conclusion

We used information from the occupational census of leading cities in 1861 to predict city size 50 and 100 years into the future. The providers of information services were not a large proportion of city population, yet their presence may have made the difference between fast growth, slow growth, and stagnation. An early, and probably inadvertent, investment in information services and the professionals that provide them gave a city a continuing advantage over its rivals.

The results were remarkably similar to those we obtained in our study of American cities, which is all the more surprising in that America was still becoming an urban nation at the beginning of the period studied. As in America, transportation explained some part of city growth over the late 19th century period. In America's case, however, it was rail, and not sea, and we found that the effects of rail lasted longer in America than the effects of water in England. Cities with higher manufacturing employment shares grew faster in the 19th century in both England and in America; the effects in America, however, lasted considerably longer than we found in England.

The results of our exercise corroborate those theories of cities that focus on the exchange of information. The city is the place where knowledge is absorbed and acted on most quickly. As Hayek put it, "the economic problem of society is mainly one of rapid adaptation to changes in the particular circumstances of time and place." Although Hayek goes on to argue that only the "man on the spot" can know the particular circumstances of time and place, he cannot decide on the basis of his limited knowledge. "There still remains the problem of communicating to him such further information as he needs to fit his decisions into the whole pattern of changes in the economic system." Cities are the institutional innovation that still, even as the communications evolution proceeds, allows the dissemination of information at lowest cost.

Table 1. City Growth, 1861-1911 and 1861-1961

Dependent Variable: Log(City Size in Final Year / City Size in 1861):

A. 1911 Final Year

	(1)	(2)	(3)
1861 City Size	-1.9×10^{-7} (1.3)	-1.5×10^{-7} (1.0)	-1.0×10^{-7} (0.7)
1861 Proportion Professionals	39.790 (2.8)	34.019 (2.3)	28.797 (1.9)
1861 Proportion Manufacturing	...	0.667 (1.5)	0.768 (1.8)
1861 Proportion Water Transport	...	3.898 (2.0)	3.937 (2.1)
1861 Proportion Telegraph	363.968 (1.4)
Constant	0.368	0.276	0.218
R-Square	0.0941	0.1543	0.1766

B. 1961 Final Year

	(1)	(2)	(3)
1861 City Size	-1.3×10^{-7} (0.7)	-9.6×10^{-8} (0.5)	-2.0×10^{-8} (0.1)
1861 Proportion Professionals	40.608 (2.3)	32.532 (1.7)	25.308 (1.3)
1861 Proportion Manufacturing	...	-0.331 (0.6)	-0.192 (0.3)
1861 Proportion Water Transport	...	2.326 (0.9)	2.381 (1.0)
1861 Proportion Telegraph	503.522 (1.5)
Constant	0.603	0.651	0.570
R-Square	0.0645	0.4757	0.1123

Death And Spending: Did Urban Mortality Shocks Lead To Municipal Expenditure Increases?

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In an earlier paper we demonstrated that municipal expenditures on water delivery and treatment, sewers, and refuse collection dramatically reduced mortality in US cities from typhoid, dysentery, and diarrhea in the period 1899-1929. We estimated that a 1% increase in sanitation expenditures was associated with an annual saving of 27 lives in the average American city. Having established that expenditure increases caused mortality decline, we investigate here the reverse causation, *i.e.*, whether changes in municipal expenditures on sanitation were motivated by mortality shocks. Such a supposition is consistent with anecdotal evidence.

In the summer of 1879 heavy rains caused the Chicago River to discharge into Lake Michigan for thirty consecutive days, polluting the city's water supply. Under normal conditions, the sewage-laden river would have discharged into the Illinois and Michigan Canal away from the city's Lake Michigan water supply. The Citizens' Association of Chicago sponsored a committee "to devise a plan to dispose of the sewage of the city without contaminating the city water supply." Nothing came of this as a series of dry seasons removed the impetus for further action. Then, in August 1885, the city was deluged by an unprecedented storm. "And a Flood Came" read a *Chicago Tribune* headline after more than 5 1/2 inches of the rain had fallen on the city in 19 hours. Not only did the sewers prove totally inadequate, but the Des Plaines River overflowed into the canal, the canal into the Chicago River, and vast amounts of filth were carried into Lake Michigan. The *Chicago Daily News* reported, "The rainfall ... is... carrying out filth unspeakable and polluting the water far beyond the crib. This is what the majority of the people of Chicago will have to drink for days to come." This episode caused a substantial increase in sickness and mortality leading to calls for government action.

Among those calling for action was the Citizens' Association committee which released a report proclaiming that the South Branch was "in an abominable condition of filth beyond the power of the pen to describe." This report "amplified and urged" the committee's earlier recommendation (a new, larger canal with dimensions comparable to those of the Chicago River), but added a new twist by proposing a study by "a commission of experts" to put "a stop for all time to the unsanitary condition which then existed." This report, as well as pressure from Chicago's leading citizens, forced Chicago's City Council to create a Drainage and Water Supply Commission in the spring of 1886. Their preliminary report, issued in January 1887, essentially concurred with the new canal proposal. The Sanitary District Enabling Act of May 29, 1889, was a direct result of the Drainage and Water Supply Commission's recommendations. This story, and others that could be told from other cities, have contributed to the argument that, as the demand for urban infrastructure increased dramatically in late 19th century, municipalities responded posi-

tively only when faced with a crisis.

The most commonly held theory of municipal budgeting is incrementalism which argues that the best predictor of this year's allocation for a department is last year's allocation. The incrementalist approach implies that municipal expenditure patterns do not in general respond to specific conditions or to public pressures. As MacDonald and Ward argue in the introduction to their collection *The Politics of Urban Fiscal Policy*, "the local public sector was routinely both insulated from the crises generated by socioeconomic development and autonomous from the demands of specific groups in the urban environment." This relationship was broken only when there was a dramatic crisis, but not by every such crisis. During an acknowledged epidemic in Albany, city fathers rationalized a lack of response by noting that their new filtration works had halved the death rate in comparison to the pre-filtration days and that the epidemic was widespread, affecting more cities than Albany.

In this paper we make use of mortality and expenditure data for 50 large cities to test whether shocks do matter for municipal expenditures. Annual data on mortality and municipal expenditures were collected for the period 1899-1929. The sample was defined to include all cities having a municipal water supply and populations over 100,000 in the 1920 Census and for which we could obtain nearly complete data on mortality experience and sanitation expenditures.

The mortality data used in this study were collected from the Mortality Statistics of Cities which annually published death-by-cause statistics. We have constructed a waterborne death rate (WDR) series that includes deaths attributable to typhoid fever, diarrhea, and dysentery. In this paper we follow the convention of referring to this group of diseases as "waterborne," even though water is not the exclusive means of transmission. Typhoid, dysentery, and diarrheal diseases were spread by impure water and food, and by contact with feces and other filth. We expect, as did contemporaries, that these diseases were controlled by programs to deliver clean water and to remove and treat waste water. Only a small minority of cities with 1920 populations over 100,000 did not have municipal waterworks and had to be excluded from our sample. While historical evidence on death-by-cause is notoriously problematic because of changing definitions of diseases and changes in diagnoses, those studied in this paper were well identified in this period.

The data on municipal sanitation expenditures used here were published in various bulletins to 1903 and in *Financial Statistics of Cities* beginning in 1905. We use data on annual operating costs and capital acquisition costs of water and sewage works. Not every series was reported every year. Few direct figures are available for 1904, and *Financial Statistics of Cities* was not published in 1913, 1914, or 1920. For the regressions discussed below, missing observations were filled in by interpolation.

We ran regressions which attempted to explain current expenditures as a function of lagged expenditures and various measures of mortality shocks in the current period and

the recent past. Separate analyses were done for water and sewer expenditures, for operating and capital expenditures, and for all sanitation expenditures together. Regressions were run for each of 30 cities and for all cities together. This produced a total of 210 city-level regressions and 63 multiple city regressions.

Did cities respond to shocks from an increase in relevant death rates by increasing their expenditures? Unfortunately, the regression results give us little insight into the answer to this question. The mortality shock variable was statistically significant in only 20 of the city-level regressions and in none of the multiple city regressions.

We do not, on the basis of these disappointing regression results, reject the hypothesis that cities responded to mortality shocks by changing their expenditures. Many examples of such responses appear in the historical literature, however it appears that nonresponse was also common. What we conclude from the statistical analysis is that cities did not regularly and systematically exhibit expenditure responses to mortality shocks.

At this point, the more interesting question is: what determines whether cities will respond to a mortality shock or not? We begin the search for an answer by taking a straightforward counting approach in which we count the number of mortality shocks that were closely followed by a notable expenditure increase. We defined a mortality shock year as one in which the actual waterborne death rate is more than one standard error above its trend for 1899-1929. Using that definition, there are 187 mortality shocks with heavy concentrations in the years 1906-10, quite a few in the war years, and almost none in the 1920s. We defined a "response" to be a situation in which a shock in the expenditure series (*i.e.*, an expenditure more than one standard error above trend) occurred within three years of a shock in the mortality series. Once a response was identified, we turned to the *Engineering News* to see if we could identify the event and determine the actual nature of the expenditure.

The waterborne death rate shocks were grouped into 104 episodes (some lasting more than one year). In 32 of these episodes, there was no response from the affected city. Otherwise, an above trend expenditure occurred within three years. We therefore count responses in 72 (69.2%) of the 104 mortality episodes. Of these, 13 are cases in which the only response recorded is in the same year as the shock.

Many of these episodes could be found in the *Engineering News*. Without going into detail, the following headlines are all tied to episodes identified by the counting process. Each is illustrative of a response:

"The Typhoid Fever Epidemic at Columbus, O." (2-11-04)

"Typhoid Fever History and the Water Supply of Pittsburg, Allegheny and Vicinity" (2-25-04)

"A Filtration Plant for Pittsburg, PA." (3-3-04)

"The Sewage Testing Station at Columbus, O." (10-20-04)

"Progress on the Sewage Works and Water Softening and Purification Works of Columbus, Ohio" (9-21-05)

Through the *Engineering News* we are also able to identify failures to respond and, in some cases, the reasons why there was no response. An example is the failure of Albany to respond to an increase in the typhoid death rate in 1901. A story about Albany's new water filtration plant, then the largest in the country, appeared in the January 14, 1900 issue. In the August 9, 1900 issue an article appeared giving the typhoid death rates before and after filtration. The rate averaged 85 *per annum* for the period 1890-98. The data for the first eight months of 1899 are similar, but the total number of deaths for the last four months dropped from 24 to seven. This suggests the rate would be about 25 *per annum* after the opening of the filtration plant. A third article appeared in the June 27, 1901 issue, a WDR shock year in our data, reporting that Albany experienced 39 typhoid-related deaths through the first 11 months of the plant's operation, a rate that was unexpectedly high. The article noted that no response was forthcoming for two reasons: first, the State Board of Health reported that typhoid was unusually prevalent in the state that autumn, and, second, physicians reported 14 of the 39 cases were contracted elsewhere.

We know that cities did respond to many mortality crises by increasing their sanitation expenditures even though the results of our regression analyses tells us that such responses were far from universal. We are currently examining the history of specific mortality crises in an attempt to discover the factors which governed whether and how cities responded.

Trams, Income, and Urban Structure: Munich before World War I

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A key development in the structure of the cities of industrializing Europe and the United States after the mid-19th century was the transformation of the "walking city", which was typically only a few miles in diameter, into far-flung urban agglomerations such as New York or Berlin. Many historians argue that the suburbanization that accelerated after World War II reflected primarily a continuation of this development. The most outstanding feature of the transformed cities in the United States and Europe was the emergence of spatial separation by income group. In the United States and Great Britain, this segregation resulted as high and middle income suburban areas came to surround a core of lower-income neighborhoods. Elsewhere, the center was more likely to be held by upper income residents with lower income residents living at the periphery in neighborhoods that later became centers of working class agitation (Hohenberg and Lees [1985: 293-307] and Wischermann [1987]). This paper examines two alternative explanations for the spatial structure of Munich in the early 1900s. The findings suggest that explanations for the changes in structure must focus on changes in urban labor markets as well transport innovation.

Urban economists and many historians argue that the pattern of urban structure can be best explained by the standard urban model that posits a central business district containing all of the employment in the city (Mieszkowski and Mills [1993: 136-141]). If marginal costs of commuting reflect primarily the opportunity cost of time and speed of available transportation and housing is a normal good, higher-income residents will be willing to give up the savings in leisure received by living close in exchange for the increased consumption of (space-intensive) housing available most cheaply at the periphery.

Leroy and Sonstelie [1983] formalize the arguments found in studies of "street car suburbs" and the 19th-century journey to work (Hershberg, *et. al.* [1981]) within the context of this model. They introduce transport innovation into the model and focus on the relationship between modal choice and residential location. The horse-drawn streetcar, for example, implied a higher fixed cost per day than walking in exchange for speeds twice as high. Beyond a particular distance, the wealthy (with the higher valuation of time) would choose the faster over the slower mode since the fixed cost would be covered. Since this break-even distance would lie closer to the center for the wealthy than for the poor, higher income workers would outbid lower income workers in more distant locations, and lower income workers would outbid higher income workers at the center. Gin and Sonstelie [1992] offers the most sophisticated test of this hypothesis data from 1880 in Philadelphia, when the price of using the street car system is said to have restricted its use to middle- and upper-income residents. They find that higher income groups would generally have outbid low-income groups for access to the center if both had access only to walking, but lower income groups would outbid upper income groups if the wealthy could afford the streetcar and the poor could not.

Labor Markets and Urban Structure

This paper argues that heavy reliance on the standard urban model to explain the transformation of urban structure leads to an under-recognition of two important developments in labor markets in late-19th century cities: the relocation of and dispersion of employment and the life-cycle pattern of employment relationships. After 1880, periodic occupational and business censuses revealed that two processes were shaping urban structure in European cities (Wischermann [1987] and Hohenberg and Lees [1985: 206-209]). Reductions in transportation costs as well as localized agglomeration economies increased the attractiveness of city-center locations in larger cities relative to the periphery and smaller towns. The process of "city-building" resulted in the simultaneous depopulation of city centers and the expansion of firms with a decided advantage in the center, including finance, publishing, wholesaling, and retail sales. At the same time, improvements in inter-urban transportation as well as technological change increased the average size of manufacturing firms and their demand for land. Unable to compete with the firms of the "City", these firms moved towards the periphery. Data from Munich bear out this characterization of these developments. The center city's share of employment fell from thirty to twenty percent between 1882 and 1907, even as the total number employed in the city quadrupled. The chief sources of center-city employment by 1907 were in clothing and apparel, wholesale and retail sales, banking, insurance, publishing, and hotels and restaurants. The city's major industrial employers in machine-making, metalworking, and food processing had for the most part left the city center.

Observers point to another important feature of pre-war labor markets that exerted an important influence on the housing market: the length of job tenure. The short job tenures of younger workers recently arrived in cities tended to compel them to seek out employment with a wide variety of firms in the city. A center-city location – typically a room or bed in another household – minimized the average commuting distance for these workers as well as the search time for employment. With increasing age, job tenures tended to lengthen and the relative attractiveness of a central location to the worker declined.

Testing for the Determinants of Urban Residential Structure

The model used by this study to explain what influenced urban structure in the pre-automobile era retains the notion common to the standard urban model that any one location in the city will be taken by the highest bidder. It departs from the standard urban model by incorporating a richer specification of the household head's employment status and age. The analysis draws upon a unique data set from the 1904-1907 Munich housing census: a sample of 1600 apartments with detailed information on rent, housing characteristics, and location; part of the data set could be matched with detailed data from other sources on household composition, origins, and in many cases, income. Archival sources provided additional information on tram service. Munich is well-suited for such a study, since its employment pattern was relatively concentrated and the cost of travel on the tram – the alternative to walking – was relatively high. Munich had an unusually high share of employment in trade,

hotels, and restaurants; all these are industries for which the city center was increasingly attractive by 1910. In addition, while tram use increased steadily up to World War I, it still remained a relatively expensive means of transportation. Roundtrip fares in Munich equaled 5-7.5% of the daily wage paid unskilled workers and 3-5% of the wage for skilled workers in 1905, shares that are not too much less the 6-9% reported for Philadelphia as far back as 1880.

The test of alternative hypotheses uses the two-step hedonic approach to estimate the influences on marginal bids of households for locations in the city. This approach offers a key advantage: it views housing as a bundle of characteristics (z). In this application, the focus is on two locational characteristics of the apartment – its distance from the center of the city and its distance from the nearest tram stop – as well as its size. In equilibrium, if P is the market rent paid, z^* is the utility-maximizing choice of characteristics, and the household's bid is B , then $P = B(z^*)$ and the slope of P , p_i , is also the slope of the household's bid function with respect to z_i . The higher the implicit market price for the characteristic, p_i , the higher the household's marginal willingness to pay for z_i . The first step of hedonic analysis identifies the values of p_i for each household by regressing rent on housing characteristics. The second step of the analysis identifies the influence of household characteristics on p_i . This study tests four hypotheses suggested by the transport cost and labor market approaches to explaining household locational decisions. The transport cost approach suggests that so long as tram usage was affordable for upper income residents, higher income households would be willing to pay less at the margin for access to the center of the city than lower-income households. In addition, higher income households would place a higher marginal valuation on access to the tram than would lower income households. The labor market view of demand for location suggests that the shorter the typical job tenure, the higher the household head's marginal valuation of access to the center. In addition, the older the household head, the more likely he is in secure employment and the lower the marginal willingness to pay for access.

Preliminary estimation of the relationship $f(\text{Rent})=g(z)$ used Box-Cox transformations of the rent and 20 characteristics, which had been corrected for heteroskedasticity. The estimation results suggest that access to the tram system and the city center played highly significant roles in influencing rents. Rents declined at a rate of about 20% per mile from the city center, well above rates of discount reported for third-world cities today. Apartments located one-half mile from a tram stop rented at a two to three percent discount relative to those within a short distance.

Estimation of the marginal willingness to pay functions for access to the tram line, access to the center, and space of the apartment offered an opportunity to test for the importance of income (non-housing expenditures) and characteristics of the tenant. The transport cost view of urban structure argues that income would be an important determinant of the marginal willingness to pay functions for access to the center and access to tram service. The higher the income, the lower the marginal willingness to pay for access to the center and the higher the marginal willingness to pay for access to tram services. A focus

on centralized access to employment would suggest that those employed in lower-skilled jobs with shorter tenures would tend to have a higher marginal willingness to pay for access to both the center and tram service. Data from the Munich local insurance fund suggest that the average completed job tenure of day laborers was shortest, about one-half year, while the tenure of semi-skilled shop help was 1.3 years and the tenure of messengers was 1.6 years. All other workers averaged 2.5 years. Dummy variables control for each of these groups. The specification is rounded out with the age of the household head and a dummy for home workers, who tended to rely heavily on centrally-located merchants for raw materials and marketing. Estimation of the marginal willingness to pay functions for access and space employed a systems approach to increase efficiency. As Epple[1987] notes, estimation of this kind of system must recognize that the quantities purchased are endogenous, which requires the use of instrumental variables. The coefficients reported in Table 1 resulted from three stage least squares estimation. The dependent variable in each case is the natural log of the marginal willingness to pay.

The most notable result in Table 1 is the weak negative influence of non-housing expenditures on bids for access and the strong influence of other household characteristics. The dummies for household heads who were employed as day laborers or as home workers significantly raised on bids for access, particularly for the tram. The age of the household head exerted a significant negative influence on demands for access. The marginal bid of a forty year-old worker would be ten percent lower than the bid of a twenty year-old worker. Of particular interest as well, space and access tended to be strong complements, a result in keeping with the relative attractiveness of the Munich Center. Higher non-housing expenditures increased the marginal willingness to pay for space as did servants. The complementarity of space and access provided another route by which income could influence marginal bids for access; Table 2 offers estimates of conditional marginal bids that incorporate this influence. Household demands for space rose steadily with income, while the marginal willingness to pay for access followed a U-shape. Youth, uncertain employment, and wealth all raised the marginal valuation of access to the tram system. Homeworkers, the young and the very rich showed stronger marginal bids for access to the center.

Overall, the results suggest that a richer characterization of what influenced the attractiveness of center-city locations is needed before we can fully understand the forces that resulted in the transformation of urban structure in the late 19th century. They also point to the need to look more closely at the potential importance of employment decentralization and the stabilization of employment relationships in the emergence of the modern urban structure.

Table 1
Estimated Marginal Willingness to Pay for Access and Space

Independent Variable	Mean	Tram	Center	Space
Access to Tram (Km. to Stop)	.383	-2.47 (11.5)	-1.21 (6.12)	-.258 (5.01)
Access to Center (Km. to Marieaplatz)	2.31	-.178 (5.12)	-.376 (11.8)	-.059 (7.24)
Space (Cubic meters)	164	.004 (5.11)	.004 (5.56)	-.009 (2.21)
Day Laborer	.07	.264 (1.98)	.152 (1.24)	-.027 (0.76)
Shop helper	.16	-.056 (0.66)	-.086 (1.09)	-.069 (3.05)
Messenger	.15	.039 (0.43)	-.027 (0.32)	-.072 (2.82)
Home worker	.04	.665 (3.51)	.577 (3.30)	.017 (0.38)
Non-Housing Expenditures (in 1,000 Marks)	1.533	-.029 (0.66)	-.025 (0.60)	.024 (1.41)
Age	42.6	-.004 (1.63)	-.005 (2.36)	-.004 (1.03)
Age ²				.0003 (0.74)
Number in household over 14	2.45	.007 (0.40)	-.001 (0.06)	-.007 (0.13)
Room Renters or Subletters	.30			.017 (1.05)
Servants	.13			.088 (2.84)
Constant		-4.05 (22.9)	2.00 (12.33)	-1.22 (15.3)
F		86.0	74.1	28.4
Adjusted R ²		.81	.79	.62

Source: Results of three stage least squares estimation. The dependent variable is the natural log of the respective marginal price or demand. Asymptotic *t*-statistics are in parentheses. N=218.

Table 2
Conditional Marginal Willingness to Pay for Access

Occupation and income group	Marginal Willingness to Pay for		Demand for Space
	Tram	Center	
Day-Laborer (Y=800)	8.22	2.86	76
Home-Worker (Y=800)	13.66	5.06	100
Y=2000	7.94	3.10	135
Y=4000	9.36	3.68	184

Source: Results of estimation in Table 1. The marginal bids are calculated conditional upon the household living one-third mile from a tram stop and two miles from the center. The demand for space is calculated from separate regressions of the inverted marginal willingness to pay.

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The Evolution of Irrigation Institutions in California: The Rise of the Irrigation District, 1910-1930

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Abstract. This paper addresses the phenomenon of the sudden decline of private irrigation enterprises and the corresponding rise of public irrigation enterprises in early twentieth-century California. The existing literature contains explanations of this phenomenon that views public irrigation enterprises either as solutions to market failure or as non-efficiency enhancing, redistribution mechanisms. An alternative explanation is proposed. This explanation does not rely on the usual market failure arguments nor on rent-seeking theories, but rather it explains the rise of the irrigation district as a result of a complex combination of agricultural, regulatory, and legal changes during the early years of this century. Using newly collected primary data from irrigation districts and contemporary newspapers, as well as data from state and federal sources, these alternative hypotheses will be tested.

The first significant movement towards public ownership of water in California occurred with the passage of the Wright Act in 1887. This act created the irrigation district, a local governmental unit with the authority to issue bonds, levy taxes, and condemn property. The first wave of district formation (1887-1893) resulted in the creation of 49 irrigation districts. Faced with strong opposition from large landowners, the district movement quickly collapsed. In contrast, the second wave of district formation (1909-1927) was to meet with long-term success. During this second wave, 112 irrigation districts were formed, several of which remain active up to the present. (Table 1.) This period witnessed the decline of the private irrigation company as the irrigation works of many of the largest and most important private companies were acquired by irrigation districts.

Several questions present themselves: What factors explain the sudden rise of the irrigation district and the corresponding decline of the private irrigation company? Why was the first wave of district formation a failure, while the second wave of formation resulted in success? How do we explain the timing of the second wave of formation? The answers to these questions should provide some insight on the broader issue of institutional change.

The origin and development of the irrigation district has traditionally been explained in terms of market failure: private irrigation companies were not capable of raising large amounts of capital to finance large-scale irrigation projects, there were problems of monopoly, or of post-contractual opportunistic behavior. It will be one of the contentions of this paper that these traditional arguments fail to provide convincing explanations of the rise of the irrigation district.

Recently, this traditional view has also been challenged by Rodney Smith (1983). Smith contends that the irrigation district was a mechanism for facilitating intra-district income transfers. Smith develops a median-voter model to demonstrate how a majority of farmers in an irrigation district can manipulate the water toll-tax structure to grant themselves a subsidy at the expense of a minority of farmers. Any district costs not covered by water toll revenue must be covered by property tax revenue. Therefore, any given subsidy (the amount by which the water price is below the marginal cost of the water supply) implies a unique property tax rate. If the water-use intensity (water used per acre) of the "median voter" exceeds the mean water-use intensity for the district, then the median voter will support a pricing policy which sets price below marginal cost. The lower price will benefit the median voter in proportion to his water-use intensity, yet the property tax increase will be diffused over all district landowners.

This hypothesis, while useful in explaining much about the first wave of district formation, does not succeed in explaining the second wave of formation. The Smith argument implies that we should at least expect to observe some patterns of opposition during this second period similar to the patterns of opposition observed in the earlier period of district formation. However, there is little in district histories that would indicate any substantial litigation between landowners within districts, nor do we find the other forms of opposition so common in the first wave.

If this rent-seeking hypothesis were correct, we should expect to observe district rules which would have made it relatively costly for district landowners to exclude their land from the district. Yet the evidence points in the opposite direction. The Bridgeford Act of 1897 made it easier to have one's land excluded from the district.

The wide voting margins in favor of district organization also cast doubt on the Smith hypothesis. In the 46 districts for which voting data was available, the average percentage "yes" vote was 92.2 percent. (Tables 2a and 2b) Although not conclusive, this finding does not indicate any strong potential for intra-district income distribution; rather, it suggests a nearly unanimous expectation of some net gain to those within the district. A more rigorous test would compare the actual vote with the vote that would be predicted by the Smith model. Using assessment data gathered from the Consolidated Irrigation District, such a test is performed in the paper. Again, the results do not support Smith's redistribution theory.

A further implication of this redistribution argument is that districts will set water prices below marginal cost. Using cost data from 1929, an estimate of the district cost function has been made to test this prediction. The evidence indicates that price was not significantly different from marginal cost.

This paper introduces a new hypothesis: a combination of agricultural, regulatory, and legal changes converged to increase the institutional advantages of the irrigation district relative to private irrigation companies. As large farm holdings were increasingly subdivided and settled during the late 19th and early 20th centuries, and with the imposition of water rate regulation in 1913, it became increasingly difficult for private irrigation companies (which were commonly joint land-water development enterprises that relied on land sales to turn a profit) to capture a sufficient amount of the benefits of new irrigation

projects to make them profitable. (Table 3 records the drop in average farm size over this period.) With much of the land now settled, most of the benefits of new irrigation projects would have accrued to the new landowners. Hold-out problems would likely have prevented irrigation companies from buying out these landowners, completing the project, and then selling the land at higher values. An irrigation district, on the other hand, would have been able to cover its costs by taxing the beneficiaries.

It might be argued that private irrigation companies would have been able to capture the gains of new irrigation works by engaging in some form of price discrimination. However, even ignoring the problem of the high costs of determining the proper set of prices, price discrimination was not a possibility since it was disallowed by the State Railroad Commission (the predecessor of the Public Utilities Commission). The Railroad Commission was established in 1911 and two years later it was granted the authority to set water rates for commercial irrigation companies. Irrigation districts were not subject to this regulation.

The combination of these factors – the intensification of agriculture and water rate regulation – imply that private irrigation companies would find it increasingly difficult to capture the benefits of new irrigation projects. With increasing water scarcity over this period (from about 1900 to the mid 1920s), we would expect to observe projects with positive net benefits which private irrigation companies were unwilling to undertake. To test this claim, a comparison of the costs and benefits of specific irrigation projects are examined and presented in this paper. The preliminary evidence indicates the existence of several socially-beneficial irrigation projects which were developed only after the creation of an irrigation district. In general, the evidence suggests that irrigation projects were becoming increasingly profitable from about 1900 to the mid 1920s.

The irrigation district movement can be seen as an attempt to capture these untapped social gains. However, irrigation districts had their own difficulties: the massive failure of districts in the 1890s raised considerable uncertainty about the soundness of district bonds. In 1911 and 1913, however, important legislation was passed which was to greatly enhance the standing of district bonds. In particular, the creation of the Bond Certification Commission (BCC) in 1911 was to have important consequences for the ability of districts to sell bonds.

The BCC was composed of the state engineer, the state attorney general, and the state superintendent of banks. Before the directors of the irrigation district could call a bond election, they had to submit to the commission the district plans, along with cost estimates of any construction work of purchases and the amount of bonds to be issued. If the commission did not approve the bond issue, this would not legally prevent a district from calling a bond election or selling bonds, although an adverse report would effectively prevent the sale of bonds.

The BCC was required to carry out a detailed investigation into the legal, financial, and engineering aspects of the project to be funded by the bond issue. Authorized bonds could not exceed 60% of the aggregate market value of lands in the district. Once the bond issue was approved by the commission, the district was not allowed to spend the proceeds on plans other than the plans originally submitted to the commission (unless commission approval was received).

How are we to interpret the role of the BCC? The well-known principal-agent problem associated with debt finance gives us a clue about a possible interpretation. Highly-leveraged firms have incentives to engage in a greater-than-optimal level of risky activities. Anticipating this, bondholders have incentives to impose restrictions on the use of the borrowed funds ("bond covenants") and to monitor the behavior of the firm. With high monitoring costs, bondholders would require a large discount to induce them to purchase the bond. In the case of the irrigation district, it is likely that the BCC could monitor and control district activities at a lower cost than the bondholders themselves (if we consider monitoring activity a public good, then bondholders had incentives to free ride on monitoring activities of other bondholders). If it is, in fact, true that the BCC had lower monitoring and enforcement costs, then a district could lower its capital costs by seeking certification from the BCC.

Abundant evidence is presented in the paper which demonstrates the significant impact that bond certification had on the marketability of district bonds. A comparison between district bond rates and several "benchmark" rates indicates a substantial drop in district rates as a result of certification by the BCC. There was a substantial increase in district bond sales in the years following the establishment of the BCC. Finally, evidence from Moody's bond ratings provides yet another indication of the improved marketability of district bonds as a result of certification.

TABLE 1.

IRRIGATION DISTRICTS FORMED IN CALIFORNIA TO DECEMBER 31, 1930

Year	Number Formed		
1887	4	1910	0
1888	7	1911	2
1889	6	1912	0
1890	11	1913	2
1891	13	1914	1
1892	3	1915	5
1893	4	1916	8
1894	0	1917	7
1895	0	1918	8
1896	0	1919	11
1897	0	1920	18
1898	0	1921	14
1899	0	1922	9
1900	0	1923	8
1901	0	1924	7
1902	0	1925	9
1903	0	1926	3
1904	0	1927	5
1905	0	1928	0
1906	0	1929	1
1907	0	1930	1
1908	0		
1909	2	Total	169

Source: Wells A. Hutchins, "Irrigation Districts, Their Organization, Operation and Financing, U.S. Department of Agriculture, Technical Bulletin No. 254, p. 5.

TABLE 2A.

IRRIGATION DISTRICT VOTING RESULTS, BY DISTRICT

Irrigation District	Date of Formation	Gross Acres	"Yes" Vote	"No" Vote	Percent "Yes"
Alpaugh	1915	8175	71	14	83.5
Anderson-Cottonwood	1914	32113	482	17	96.5
Banta Carbona	1921	14349	73	22	76.8
Beaumont	1919	4141	307	114	72.9
Byron-Bethany	1919	17200	173	14	92.5
Carmichael	1916	3121	45	3	93.7
Citrus Heights	1920	3076	65	0	100.0
Compton-Delevan	1920	12652	10	0	100.0
Consolidated	1921	149047	1121	62	94.7
Corcoran	1919	51606	111	2	98.2
Cordua	1919	5461	7	0	100.0
El Camino	1921	7548	7	2	77.7
El Dorado	1925	30703	679	141	82.8
Fairoaks	1917	3900	184	16	92.0
Fallbrook	1925	10216	186	0	100.0
Foothill	1920	50687	356	3	99.1
Fresno	1920	241300	1438	184	88.6
Glenn-Colusa	1920	121592	287	12	95.9
Grenada	1921	4948	74	1	98.6
Hot Springs	1919	9497	21	0	100.0
Imperial	1911	605000	1304	360	78.3
Island No. 3	1921	4620	88	13	87.1
Jacinto	1917	11554	103	28	78.6
James	1920	26265	8	0	100.0
Laguana	1920	34858	251	4	98.4
La Mesa	1913	18000	397	3	99.2
Lindsay-Strathmore	1915	15250	150	20	88.2
Lucerne	1925	33407	138	26	84.1
Madera	1920	352000	1642	47	97.2
Maxwell	1918	8819	9	0	100.0
Merced	1919	189682	1967	922	68.0
Montague	1925	26117	198	2	99.0
Naglee Burk	1920	2871	13	0	100.0
Nevada	1921	268500	636	168	79.1
Oakdale	1909	74240	349	27	92.8
Potter Valley	1924	5042	110	3	97.3
Provident	1918	22805	13	0	100.0
South San Joaquin	1909	71112	376	87	81.2
Table Mountain	1922	1955	7	0	100.0
Thermalito	1922	3110	135	6	95.7
Tranquility	1918	10750	68	0	100.0
Tule-Baxter	1917	24351	6	0	100.0
Waterford	1913	14100	70	1	98.5
West Stanislaus	1920	21400	115	4	96.6
Woodbridge	1924	13430	23	6	79.3

TABLE 2B.

	Number of Districts	Average "Yes" Vote
San Joaquin Valley		
All Irrigation Districts	20	90.41%
IDs with Gross Acres > 50,000	8	89.98%
IDs with Gross Acres < 50,000	12	90.70%
Sacramento Valley		
All Irrigation Districts	16	93.16%
IDs with Gross Acres > 50,000	2	87.40%
IDs with Gross Acres < 50,000	14	93.97%

Source: Frank Adams, "Irrigation Districts in California", California Department of Public Works, Division of Engineering and Irrigation, *Bulletin No. 21*, (Sacramento, 1929).

TABLE 3.

AVERAGE IMPROVED ACRES PER FARM

COUNTY	1889	1899	1909	1919	1929
SACRAMENTO VALLEY					
BUTTE	304	256	165	114	114
COLUSA	751	616	506	371	347
GLENN	--	673	467	255	244
PLACER	137	113	93	107	72
SACRAMENTO	266	235	172	134	90
SOLANO	302	299	272	220	213
SUTTER	352	284	229	162	145
TEHAMA	507	256	186	165	169
YOLO	357	289	253	186	212
YUBA	362	319	217	203	192
SAN JOAQUIN VALLEY					
FRESNO	405	239	95	75	62
KERN	191	295	270	194	100
KINGS	--	281	107	120	119
MADERA	--	531	683	188	214
MERCED	785	614	327	178	110
SAN JOAQUIN	406	332	186	133	104
STANISLAUS	917	655	191	105	84
TULARE	268	247	126	86	78
SOUTHERN CALIFORNIA					
IMPERIAL	--	--	133	109	110
LOS ANGELES	114	79	53	39	25
ORANGE	137	99	60	48	36
RIVERSIDE	--	92	104	88	74
SAN BERNARDINO	66	41	46	44	23
SAN DIEGO	97	85	102	82	53
VENTURA	180	137	165	123	115

Sources: U.S. Agriculture Census for 1890, 1900, 1910, 1920 and 1930.

Session II: Growth and Technological Change: Lessons from History

Joint Session with AEA

Presiding: Gavin Wright, Stanford University

Tuesday, January 4, 10:15 a.m.

Natural Resources in Japanese Economic History, 1800-1940

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1. 1800 - 59

Many students seem to have an impression that Japan suffered from the shortage of natural resources throughout its modern history, but such an impression cannot be supported by facts. It is true that the latter half of the Tokugawa Period was the one which was characterized by a stagnant population, but, judged from the pattern of limited trade, namely the export of copper and silver in exchange for manufactures, Japan in that period was a natural-resource-rich country. The major cause of the relatively low *per capita* income was a low level of technology and the scarcity of capital. Agricultural productivity was constrained mainly by the shortage of fertilizers (working capital) and not by the shortage of land.

2. 1859 - 99

This is a period in which trade expanded rapidly under a regime virtually prohibiting intervention by the government. The patterns of trade clearly revealed that Japan's comparative advantage was in natural-resource-intensive commodities and its weakness was in technology- and capital-intensive commodities.

As shown in Table 1, as late as 1880, Japan's major exports were raw silk, tea, waste silk, sea weed, potteries, copper, and coal. All but potteries were natural-resource-intensive commodities. In contrast, most of Japan's imports, namely cotton yarn, woolen cloths, cotton cloths, wrought iron, rice, cotton and rails were manufactured commodities, except for relatively unimportant rice and cotton. It can safely be concluded that the trade pattern of Japan in 1880 was that of a typical LDC, except that there were very little capital goods in imports unlike in many of the LDCs in the period after World War II. Rosovsky's thesis that in "the early years of her industrialization, Japan had to depend heavily on imports of foreign machinery" (*Capital Formation in Japan*, p.50) is untenable. The ratio of the value of imported producers' durables to total value of producers' durables installed (both adjusted for transportation costs and margins) was only 18.9% in 1876-80 and 17.8% in 1886-90.

Prebisch's contention that the terms of trade tends to move against an LDC is shown not to have applied to Japan in this period. It improved 395% between 1857 and 1875 and another 38% between 1875 and 1895. Since average trade dependency ratio was 2.5% in the former period, gains from trade was 9.8% (395×0.025) of GNP in this period. Similarly, it can be shown that the gains from trade was 9.5% of GNP in 1875-95. The gains were substantial, but not enormous as Huber insisted.

Table 1

Major Exports and Imports of Japan, 1880

(1) Exports		(2) Imports	
(a) Items	(b) Value (1,000yen)	(c) Items	(d) Value (1,000yen)
(1) Raw silk	8,607	(1) Cotton Yarn	7,700
(2) Tea	7,498	(2) Woolen cloths	5,792
(3) Waste silk	1,291	(3) Cotton cloths	5,523
(4) Tangles ^{a)}	697	(4) Kerosene	1,400
(5) Sardine	648	(5) Wrought iron ^{c)}	1,079
(6) Potteries	475	(6) Rice	434
(7) Copper	474	(7) Cotton	171
(8) Coal	460 ^{b)}	(8) Rails	163
	(1,086)		
Total exports	28,396	Total imports	36,626

Notes: a) Sea weed, b) Includes sales to ships, c) T's, angles, etc.

Source: *Nihon Boeki Seiran* (1935), *Passim*.

The economy in this period was close to a *laissez-faire* one despite the contention of many historians to the contrary. The slogan of *fukoku kyohei* (the rich nation and the strong military) is at fault. It certainly suggests that the government must have played an important role, but actually the government was very small by any standard.

The proportion of the expenditure of the central government in GNP was only 7.9% in 1888-92, somewhat larger than in Great Britain (6.0) but much smaller than in Italy (13.2%) or Sweden (12.5%) for the same period. Even the size of the military expenditure (the strong military) was small at least until the Sino-Japanese War. The military expenditure as a proportion of GNP, 2.3% in 1888-92, was smaller than the figure of 2.7% in 1954-56 under the "Peace Constitution". Since many of the naval vessels were imported in this period, the strain on natural resources coming from the military expenditure must have been minimal.

3. 1899 - 1940

Population growth, the emergence of protectionism and the rise of imperialism all tended to increase the strain on natural resources, with the rise of imperialism proving to be the most important.

The rate of growth of population rose gradually, surpassing 1% around 1900 and reaching 1.8% in 1925. There is no indication, however, that population pressure became an important deterrent of economic development in this period. On the contrary, an increase in arable land and, more importantly, the absorption of labor force in rapidly expanding secondary and tertiary sectors relieved population pressure on land so much that once-redundant labor on land disappeared around 1900 despite Ryoshin Minami's contention to the contrary.

Emigration to the United States and elsewhere was negligible at this stage. Even the internal migration to Hokkaido, Japan's last frontier, was only 30,000 to 50,000 annually in the first decades of the 20th century. There is no evidence whatsoever for the contention that the American ban on Japanese immigration, which was only about 10,000 per year, meant a severe blow to the Japanese economy, even though the psychological impact cannot be neglected.

The revision of the unequal treaties paved the way to the rise of protectionism. The average tariff rate on imports rose from 3.7% in 1898 to 1.5% in 1910 and 21.0% in 1933. The contention of Japanese economic historians that the revision of the treaties helped the economic development of Japan is dubious. High tariff rates on heavy industrial products and luxuries were particularly lamented.

A 100% duty on sugar and whiskey certainly retarded economic development and the rise in the standard of living. A 37.5% duty on ships cannot be defended except on a military ground. Even a 15.2% duty on pig iron (effective in 1932) can be blasted for inviting a disaster. There is no reason for a capital-short and resource-poor economy like Japan in the 1930s to welcome a rapid expansion of such a capital-intensive and resource-dependent industry as iron. Besides the expansion of the iron industry enabled imperialistic expansion which proved to be disastrous.

As shown in Table 2, by 1930 trade structure had been changed dramatically reflecting the change in industrial structure between 1880 and that year. Most of the exports were by now manufactures such as raw silk (which should probably be classified as a manufactured product by then), cotton cloths, silk cloths, rayon cloths, sugar, wheat flour and potteries. All but potteries were light manufactures. It should be noted that Japan had achieved a fairly high standard of living without exporting almost any heavy industrial goods.

Major imports including cotton, sugar, rice, soybeans, lumber, oil and coal were nature-resource-intensive commodities, either food, raw materials or fuel. This structure of imports resembled that for the 1970s, even though raw materials in 1930 were mostly for light industries, unlike in the 1970s.

It is said that the limitation in the supply of raw materials and fuel, population

(1) Exports		(2) Imports	
(a) Items	(b) Value (1,000yen)	(c) Items	(d) Value (1,000yen)
(1) Raw silk	416,647	(1) Cotton	369,261
(2) Cotton cloths	316,993	(2) Sugar	169,873
(3) Silk cloths	79,343	(3) Rice	167,785
(4) Rayon cloths	34,934	(4) Petroleum	82,629
(5) Potteries	27,171	(5) Soybean cake ^{b)}	58,960
(6) Sugar	26,735	(6) Soybeans	54,153
(7) Coal	26,200	(7) Lumber	53,058
(8) Flour	22,704	(8) Wheat	41,509
(9) Cotton yarn	15,032	(9) Coal	36,890
Total exports	1,871,173	Total imports	2,005,399

Notes: a) Trade between Japan and its colonies is included wherever it is identifiable; b) Crude and heavy oil, kerosene and gasoline.

Sources: *Nihon Teikoku Tokai Nenkan*, Showa 6-nen (1932) pp.171 - 80, and *Nihon Boshi Seiran* (1935), *passim*.

pressure, and the depression were the economic causes of imperialistic aggression in the 1930s and 1940s. It is true that the military tried to justify the expansion based on the logic of "have nots". Such an expansion, however, was not only immoral but also disastrous economically. Japan could have raised the already fairly high standard of living by the expansion of light industrial exports and the exports of light machinery such as bicycles and appropriate-technology automatic looms.

As it was, the policy of military expansion and of the development of heavy industry was adopted. The military expenditure which was so small in the period before the Sino-Japanese War expanded later, but, partly thanks to disarmament in the 1920s, the ratio of military expenditure to GNP was still as low as 3.6% in 1926-30. This ratio rose rapidly in the 1930s with the rise of imperialism, to reach 19.0% by 1940.

The expansion of military expenditure increased demand for natural resources, eventually creating a severe shortage of natural resources. As a result, the terms of trade deteriorated by 41.6% between 1929 and 1940. Economic historians tend to say that the deterioration in the terms of trade in this period was caused by the decline in the relative price of raw silk and other textiles. This is only a part, a smaller part of the entire picture. The shortage of natural resources, or the rise of the relative price of imports (a 54.0% rise from 1932 to 1940) was more important (See Table 3).

Thus, the shortage of natural resources was created, and a quest for territories had come to look more reasonable. Clearly, a vicious circle was put into motion. As a result, a huge number of people (including the military) had to move out of Japan proper to Manchuria, Korea, China and South America. The number of the Japanese who were stranded abroad (including territories) at the end of World War II was as many as 7 million.

In the 1930s (1929-40), real GNP grew at an "unprecedented" rate of 4.7% *per annum*. Ohkawa-Rosovsky's "trend acceleration", however, was highly misleading. For one thing, a large part of the growth was accounted for by the increase in military expenditure, leaving only 3.1% for the growth of civilian real GNP. Secondly, even though the growth of population slowed down due to the exodus referred to above, the average rate of population increase for the period was still 1.2%, leaving 1.8% for the growth of per capita civilian real GNP. Finally, the effect of the deterioration in the terms of trade (4.8% x average trade dependency ratio of 24.2% = 1.2 points) will have to be deducted. All that remains is a 0.6% average annual growth for 1929-40, lower than in any upswing period before.

In fact, even this much rise in the standard of living did not materialize. Due to the shift of the center of gravity to capital-intensive industries, real wages declined by 8.1% between 1929 and 1940. Real *per capita* consumption declined by 9% with the consumption of alcohol, tobacco, and clothing decreasing by 20% or more. It is clear that even before the Pacific War started, imperialistic expansion was causing a disaster in people's standard of living.

4. Conclusion

If Japan had not launched the imperialistic aggression starting with the Manchurian Incident, the United States and other Allied nations would never have taken action against Japan. The depression in the 1930s would have been cured by the Takahashi fiscal policy which would have expanded expenditure on public works and agrarian relief more extensively without wasting money for military purposes. The standard of living would have risen sharply rather than declining as it actually did. This is all conjectural, and more work will have to be done to show the point really convincingly. All that this paper did is to reveal beyond any reasonable doubt that the military expansion proved itself an economic flop even before the outbreak of the Pacific War.

Table 3

a)
Relative Export and Import Prices and the Terms of Trade, 1929 - 40.

(1929 = 100.0)			
(1) Year	(2) Relative export price	(3) Relative import price	(4) Terms of trade (2)/(3)
1929	100.0	100.0	100.0
30	83.3	93.3	89.7
31	75.4	81.0	93.1
32	74.8	92.1	81.2
33	84.5	107.6	78.5
34	79.0	114.7	68.8
35	74.5	119.3	62.5
36	76.5	119.7	64.0
37	84.6	123.3	68.7
38	74.2	109.3	67.9
b) 39	88.2	121.9	72.4
b) 40	82.8	141.8	58.4

Notes: a) Relative to GNE deflator. b) Prices for 1939 and 1940 were estimated by the author.

Sources: 1929 - 39. Ippel Yamazawa (1975) "Yushutsunyu Kakaku Shisu"

[Indices of Export and Import Prices], in Kazushi Ohkawa and Ryoshin Minami (eds.) *Kindai Nihon no Keizai Hatten*, Tokyo: Toyo Keizai Shinpo-sha, p.539;

1939 - 40, *Showa Kokusei Soran*, Tokyo: Toyo Keizai Shinpo-sha, 1980, pp.616 - 37.

Cost Disease and Deindustrialization in Historical Perspective: The American Case, 1840 - 1989¹

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The January 27, 1993 issue of *The New York Times* contained a front-page article entitled "Clinton Job Plan in Manufacturing Meets Skepticism." The article began:

Over and over, President-elect Bill Clinton has said that creating more manufacturing jobs was at the top of his agenda. But even staunch supporters in the economics profession say he is doomed to frustration on that score.

For example, Mr. Clinton told a small group of economists last August that he was concerned about creating more factory jobs, and he asked, "How can we do that?" Paul Krugman, an expert on international competitiveness at M.I.T., replied 'Basically, nothing.'

And 'that's still the correct answer,' Professor Krugman said recently.

This same view was put forth by Alan S. Blinder in a recent article in *Business Week*. Professors Krugman and Blinder refer to the same study to support their belief: *Productivity and American Leadership: The Long View*, by William J. Baumol, Sue Anne Batey Blackman, and Edward N. Wolff (hereafter referred to as BBW).² BBW argue that the US is not "deindustrializing" (i.e., becoming a service economy). Rather, the US is suffering from "cost-disease illusions." Specifically, the illusion is that "the United States is rapidly evolving into a service economy, if it has not become one already (BBW 1991: 115)." According to BBW, the reality is that manufacturing employment is shrinking because productivity in that sector is improving at a faster rate than productivity in the service sector. The share of manufacturing in real output has actually remained constant and thus more and more workers must move to the service sector in order to keep its share of real output from falling. Consequently, as Professor Krugman put it, there is "basically nothing" we can do to stop the expansion of service sector employment, short of slowing down productivity growth in the manufacturing sector.

BBW support their hypothesis by presenting data on US sectoral productivity growth from 1947 to 1976; this is hardly the "long view" promised in the title of their study. In our work, we return to the long view by extending BBW's analysis in two directions. First, we employ newly available data on GNP by industry to extend their analysis through 1989. We believe that these years are very important, since the late 1970s and early 1980s are often described as periods of accelerated deindustrialization.³ Second, we examine the period 1840-1900 to see whether or not the cost-disease model is applicable to a period of industrialization, or "de-agriculturalization." In both cases, our findings demonstrate that

BBW's examination of the postwar data does not withstand additional scrutiny and tests, and that the cost disease model is not a sufficient explanation of American deindustrialization or industrialization.

I. Deindustrialization, Cost Disease, and the Rise of the Service Sector: A Brief Summary of BBW

Employment in the service sector expanded from less than 30% of the workforce in 1900 to nearly 70% in 1985 (Hughes 1990: 538). This has lead many analysts to argue that the US is "deindustrializing."⁴ Barry Eichengreen (1989) points out that there are both domestic and international explanations of this phenomenon. He writes: "The problem that plagues this search for culprits should be familiar to fans of the board game *Clue*. As in *Clue*, the problem is one of too many suspects, and some method is required to eliminate the candidates (1989: 280)."

BBW propose a method to eliminate all of the suspects, and to turn the "murder" of the manufacturing sector into a suicide. They contend that deindustrialization is a myth: "There has been virtually no change in the share of real national output constituted by the services (Baumol, Blackman, and Wolff 1991: 116, emphasis in the original)." The alternative explanation they propose is based on the cost-disease model developed by Baumol (1967). The basic results of the model are easily summarized.⁵ Suppose that there exist two sectors, one "progressive" and one "stagnant", where the growth rate of labor productivity is higher in the progressive sector than in the stagnant sector.⁶ Assume also that wages are equal in the two sectors, and that the shares of real output produced by the two sectors remain constant. Then, as productivity growth in the progressive sector outpaces that in the stagnant sector, workers will be "pulled" into the latter in order to keep that sector's share of output from falling. At the same time, the nominal value of the stagnant sector's output will increase because of the additional labor being employed to produce the same relative amount of real output.⁷ Thus, rather than witnessing deindustrialization, we are actually observing the impact on employment of relatively high rates of productivity growth in manufacturing combined with a constant share of the stagnant sectors' output in real national product.

BBW's empirical analysis is straightforward. They examine data on output by sector from 1947 to 1976. Using the data to separate the economy into progressive and stagnant sectors, they find that those labelled as progressive are primarily manufacturing sectors; stagnant sectors are primarily found among the services. They proceed to show that (1) the share of real output accounted for by the stagnant sector has remained constant; (2) the share of nominal output accounted for by the stagnant sector rose; (3) employment in the stagnant sector rose. Therefore, given the data employed by BBW, the cost disease hypothesis appears to be supported.

II. Cost-disease and deindustrialization, 1947-1976

The first step in our analysis was to replicate BBW's calculations for the period 1947-1976 using data from *The National Income and Product Accounts of the United*

States, 1929-82. These data are more refined than those used by BBW, and we wanted to check to see if these adjustments altered BBW's conclusions.

BBW's empirical analysis consists of five steps:

1. Calculate the annual growth rate of labor productivity for each industry from 1947 to 1976.
2. Divide the economy into stagnant and progressive sectors using the growth rates of labor productivity calculated in step 1.
3. Calculate the share of the stagnant sector's output in real output and nominal output.
4. Calculate the stagnant sector's share of total employment.
5. Examine whether or not the stagnant sector's share of real output is constant. If it is, then examine what has happened to the stagnant sector's share of nominal output and total employment.

After following these steps, we found notable differences with BBW's results. Specifically, the distribution of stagnant industries shifted, due to changes in the productivity growth rates for specific industries, and stagnant industries were not just in the service sector (construction is an example.) And, more importantly, we found that the shares of the stagnant sectors in real GNP were not constant from 1947 to 1976. This conclusion held when we employed the stagnant sector configuration adopted by BBW and the one that we arrived at according to the dispersion of productivity growth rates in our own data. Thus, one of the fundamental tenets of the cost-disease model is not true for the period 1947-1976.

III. Cost-disease and deindustrialization, 1947-1989 and 1977-1989

After discovering the tenuous nature of BBW's conclusions, we then used the same framework to study the extended period up until and including 1989. For this extension, we used data for 1977 to 1989 provided in the *Survey of Current Business*. We also chose to separate the analysis into two separate time frames: 1976-1989, and the entire period 1947-1989. This allowed us to judge the accuracy of the cost-disease hypothesis from two additional perspectives.

We found that for 1947 to 1989, BBW's hypothesis comes under fire once more. We again encountered difficulties in determining sectoral allocations, as there arose two different potential divisions between stagnant and progressive industries. We used alternate definitions in our empirical work in order to test the hypothesis to the fullest extent possible. The series of tests reaffirm the conformance of stagnant shares of nominal output and employment to the BBW standard in each of the scenarios. Regardless of the stagnant industry distribution selected, however, the performance of real output shares does not reflect that predicted in the BBW model; in each case, the share declines. Since, once again, the fundamental premise of the cost disease hypothesis does not hold true, namely that the progressive and stagnant sector shares of real output are constant, we cannot simply attribute deindustrialization to cost-disease.

The calculations from the data for 1977 to 1989 provide the only glimmer of hope

for the cost disease model. A single pronounced break in the distribution of productivity growth rates arose and was employed to designate stagnant industries. Counter to our earlier results, shares of nominal output, employment, and real output all perform in accordance with the predictions of the cost disease model.

We do not believe that this conclusion is robust. Throughout the course of our analysis, one troubling fact emerges: the composition of industries within the stagnant sector is highly variable and time-sensitive. We addressed this problem by applying each time-specific definition of stagnant sectors to the other two periods we tested. For instance, the stagnant industry distribution we identified for 1947-1989 was also applied to the periods 1947-1976 and 1977-1989, respectively. In addition, we subjected BBW's original sectoral allocation to the same scrutiny. Given the observations made thus far, the outcome of this test was not surprising: the stagnant sectors' share of real output was not constant, as is assumed in the cost-disease model, no matter how the industries are selected. Even as with the earlier case using the 1977-1989 allocation, we find that the results are not consistently repeated for more than one specific time interval.

IV. Industrialization and Cost-disease, 1840-1900

The second direction in which we extended BBW's analysis was to study American industrialization. Recall that the cost-disease model only requires a progressive sector and a stagnant sector, not "manufacturing" and "services." Thus, we analyzed data on output per worker in seven sectors from 1840 to 1900 and checked to see whether or not the cost-disease model applied to this period of "de-agriculturalization," i.e. the movement of workers from agriculture to industry.

The analysis for 1840 to 1900 was similar to that for the post-World War II period. We utilized data on output by sector from Gallman (1960) and Gallman and Weiss (1969) and data on employment by sector from Lebergott (1966) and Weiss (1986). The seven sectors which we examined were: agriculture, construction, education, manufacturing, mining, trade (retail and wholesale), and transportation and public utilities. We calculated productivity growth rates for each sector for 12 different time intervals; we then divided industries into stagnant and progressive sectors, and calculated their shares of nominal output, employment, and real output.

The general conclusion we reach is simple: stagnant shares of nominal output, real output, and employment were falling over the period 1840 to 1900. (We will present tables containing the specific industries and sectoral shares at the session.) There are three one-decade intervals for which the cost-disease hypothesis does hold: 1840-50, 1860-70, and 1890-1900. When the time periods are extended to intermediate lengths (20-30 years), shares consistently fell, as they do for the entire period 1840-1900. The industries within the stagnant sectors also tended to remain the same. Manufacturing, mining, and education were consistently in the progressive sector, while agriculture, construction, and transportation were usually in the stagnant sector. This contrasts with our findings for 1947-1989, when the composition of the sectors changed over the period.

These data lead to the conclusion that demand conditions and changes in relative prices were important factors in the movement from an agricultural economy to an

industrial economy. We cannot employ a model which assumes that the shares of the progressive and stagnant sectors in real output remain constant over time, and then trace out the effects of productivity changes in the various industries. This reinforces our conclusion from the post-war data that the cost-disease model is not sufficient for studying large sectoral shifts in employment.

V. Conclusions

Our analysis indicates that the cost disease model fails both for the periods 1947-1989 and 1840-1900. The reason is the same in both cases: the share of the stagnant sectors' output in real GNP has not remained constant, contrary to the basic assumption of the model. We believe that these findings lead to three conclusions. First, additional empirical work needs to be done within the framework of the basic cost-disease model. In particular, the model should be applied to other industrialized countries to see if the US case is exceptional; we suspect that it is not. Second, these findings throw doubt upon the idea that there is "basically nothing" that can be done to promote increased employment in the manufacturing sector. Policies which affect the composition of aggregate demand may also affect sectoral employment levels. Thus, for example, "strategic trade policy", as discussed by Krugman (1987), could affect the sectoral distribution of American jobs both through its effects on productivity growth and its effects on domestic and foreign demand. Third, we believe that the theoretical model needs to be changed in order to accommodate changes in sectoral shares and in relative prices. We are currently applying the insights of Corden and Neary (1982) and Matsuyama (1992) to develop a model which applies to both industrialization and deindustrialization.

Endnotes

¹Michael Starr provided valuable research assistance.

²Baumol, Blackman, and Wolff (1991).

³See, for instance, Bluestone and Harrison (1982).

⁴The classic works are those of Bluestone and Harrison (1982, 1988).

⁵The complete propositions and proofs for the model are contained in Baumol (1967) and Baumol, Blackman, and Wolff (1991: 313-319).

⁶BBW also include services which they call "asymptotically stagnant." These are services which exhibit "outstandingly rapid productivity growth followed by a gradual onset of the cost disease" (1991: 131). We ignore this sector in our analysis, since it is not germane to the basic idea of "cost-disease illusions."

⁷This hypothesis is in sharp contrast with the various deindustrialization hypotheses. These all argue that labor is "pushed" out of manufacturing because productivity is falling due to foreign competition, bad management, high labor costs, or injurious government policies. See Eichengreen (1989) for a summary.

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Outgrowing the Plantation: A Regional Comparison of the Asset Specificity Problem and Technical Change in the Cuban Sugar Industry, 1899-1929

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People have become accustomed to thinking of Cuba as isolated from western capitalism, but in the 19th century that was far from the case. Then, Cuba had one of the more innovative and advanced sugar industries in the world – a leader in the adoption of both steam power and vacuum-heating technologies in sugar manufacture. After the Cuban War of Independence (1895-98), producers wanted to regain that status, this time with closer political ties to their major market, the United States. For the first time, North American direct investment was attracted in a big way to the Cuban sugar industry, but surprisingly, it did not seem to reinforce the once highly competitive establishment that had been located in the western provinces before the separatist insurrections had begun. Rather, foreign investment went more frequently to the east where in 1899 most existing sugar producers were small and backwards. Cuban prominence in the sugar industry was regained, but the technologically dynamic center of the industry suddenly shifted to the east. The new continuous-process technologies of the second industrial revolution were adopted more quickly and more thoroughly in the east.

The change was dramatic, and it has drawn the attention of contemporaries and historians, who more than anything have emphasized the social discontinuities that resulted. Explanations of foreign investors' attraction to the east have been indirect or implicit, but they have tended to attribute it to easier North American domination of the backward and vulnerable eastern provinces. While domination may have been a consequence, the motive of investors participating in international capital markets was not domination but, rather, maximization of the returns to their investments, and it is not obvious why they chose the backward region over the developed. What was the economic incentive for investing in the east rather than in the west when the west had been the former bastion of Cuban sugar's competitiveness?

This paper argues that the institutional setting in the west, combined with the

influence of pre-existing infrastructure, created a disadvantage for western sugar producers in attracting capital to invest in the latest continuous-process technologies. The argument relies on Oliver Williamson's transaction-cost theory of the effects of asset specificity on contractual relations (*Economic Institutions of Capitalism*, 1985). Using that approach, I show that the western part of the industry suffered a relative disadvantage in the adoption of new technologies because mills in that region were more likely to suffer holdup problems due to the transaction-specificity of canefield assets. Accordingly, institutional persistence in the west seems to have caused its relative decline¹; indeed, it may be said that it was the west's very success with the previous century's technology which was at the root of its difficulties in the early 20th century.

Continuous process innovations were stimulated in the 1880s which revolutionized sugar manufacturing. These innovations were similar to those Chandler emphasizes in industrial countries, except that they were scattered in tropical (or semitropical) countries outside the industrial centers. Over the next few decades, further development and adoption of these new techniques in cane sugar manufacturing was widespread. Although the response in Cuba was interrupted and delayed by political conflict and war in the 1880s and 1890s; after 1901, Cuban sugar producers came to be among the most successful at employing the new technologies. In the next three decades, Cuba came to produce over 30% of the cane sugar supplied on the world market, and construction of capital-intensive, high-throughput mills in Cuba lasted until the late 1920s.

How these innovations were assimilated into sugar production in Cuba is interesting to observe from the point of view of its industrial organization. Two consequences are noteworthy. (1) There was an enormous change in the optimal scale of production, as occurred in other industries when continuous-process technologies were adopted. This resulted, interestingly, in the vertical disintegration of cane cultivation and milling into what was known as the central factory system of production. Where the self-contained plantations once reigned, central mills now contracted with nearby cane farmers, known as colonos, to procure the vast quantities of cane they had come to require. (2) Although mill capacities grew significantly in all parts of the island, those in the western part of the island grew less rapidly than those in the eastern part of the island. Cubans who lived through these events commonly noticed the difference in average mill capacities that developed between the sugar industries of the east and the west, and they emphasized the coincidence in the east of the largest mills, the greater presence of North American capital, and the greater concentration of land (*latifundismo*) in the hands of mills. However, behind these differences must have been economic incentives that made the east more attractive to investors than the west. These incentives need to be explained if we wish to understand why this regional investment pattern formed. In answering the question, we also learn something about the influence of rational economic responses filtered through institutions on regional differentiation.

To understand the economic incentives, it must be recognized that cane fields were site-specific assets. First, the working of the cane transaction was dominated by technical attributes that tied the cane cultivation site spatially to the mill. This is because cane is very

sensitive biologically and deteriorates rapidly once it is cut. Deliveries from the harvest to the mill thus had to be made promptly. Consequently, the cane railroad became an essential component of sugar production. Investment in railroads had to be made by the mills in order to access the cane supplies, and that investment fixed the site for the long term. Given the specificity of the investments made by mills, theoretically one would expect to find threats of "opportunistic behavior" on the parts of *colonos* attempting to capture the quasi-rents created by the investments, unless contractual mechanisms were in place to preclude them. In the paper, an argument and empirical support is offered for the proposition that such holdup threats were effective when contract renewals were negotiated.

To summarize the argument, because of the site specificity of cane field investments made by the mill, dependence on short term cane supply arrangements was dangerous because of the potential for opportunistic behavior. Once the central mill (or *central*) and the cane supplier had agreed on a cane price and the *central* had made the commitment to the site-specific assets, the supplier could afterwards threaten to withdraw the cane supplies, holding out for a higher price than the one agreed upon. The salvage value of the site-specific investments was less than its productive value; therefore, the supplier's threat to withhold the cane supplies could effectively force the *central* to accept short-term losses of a magnitude up to the difference between the productive and salvage values of the assets. In this way, the supplier could appropriate some of the quasi-rents generated by the investment in railroads. Further, this appropriation was not limited to a redistribution of the profits between participants; it was feasible for the supplier appropriate as well some of the rents intended for the replacement or updating of equipment.²

Explicit contractual stipulations safeguarded against interruptions in cane deliveries during the term of the contract; therefore, the threat of holdup tended to take place during the negotiation for contract renewal. Such holdup threats were most effective if the *colono* owned the land he cultivated, so land ownership increased the relative bargaining position of the *colono* with respect to the mill. Consequently, the mills' cane costs tended to depend on the initial distribution of property in land, which differed between the two regions. Table 1 briefly summarizes some of the more salient regional differences. Mills capacities come to be considerably larger in the east where the percentage of cane delivered by landowning *colonos* had been and remained small relative to the west. (More detailed data as well as explanation about the technical and organizational changes that occurred in sugar production are given in the paper.)

While the introduction of the new technology and the increase in the optimal scale created incentives to invest in larger scales of production, the effective threats of holdup by landowning *colonos*, who were more prominent in the west, caused existing mills in that region to confront higher cane costs. The figures show that the percentage of cane coming from landowning *colonos* was two to three times higher in the west than in the east. The reason for this has to do with the histories of the two regions. The west was the center of the older 19th-century plantation industry. Whenever the larger scale technology was adopted, in the west this took place by consolidation of pre-existing plantation properties so that many of the *colono* contracts that were signed were signed with former plantation owners who ceased to mill their own cane. In contrast, the east consisted principally of new

lands into which the sugar industry was being introduced. In these lands there was a greater tendency for the mills to own their cane lands and to contract them out to *colonos* who occupied them only as long as they were under contract.

This initial institutional difference implied a greater disadvantage, from the western mill owners' perspective, in attracting capital for investment. In contrast, in the eastern part of the island the institutional and infrastructural landscape was pliable so that contractual relations could be established differently based on the logic of the new technological system. This gave the eastern region an relative advantage in attracting scarce capital for investment in new technology and larger mill capacities. Consequently, the east came to be associated with larger, more technically advanced mills and larger concentrations of land holdings. That the east also came to be associated with the destination of inflowing North American capital was endogenous to this incentive structure.

Two kinds of empirical support are presented to support the proposition that this regional institutional difference had an effect on the returns to investment in sugar mills. First, evidence in negotiations of contract renewals between *centrales* and landowning *colonos* is presented in which this kind of holdup behavior was apparent at the point of contract renegotiation. This evidence is in the form of correspondence between mill managers describing the holdups in ongoing renegotiations. Second, evidence of higher rates of payment for cane to *colonos* in the west, where landowning *colonos* were prominent, is presented. (See Table 2.)

The pre-existing dense railroad network in the west contributed as well to this disadvantage. Railroad regulatory policies from the turn of the century tended favor the use of private railroads (offering service exclusively to particular mills) over public railroads (offering services to the public). Public railroads were heavily regulated by the Cuban Railroad Commission while few restrictions were placed on the construction of private, company railroads. This tended to encourage the construction and use of private railroads for hauling cane, and a number of mills invested in rail systems that allowed their cane to be hauled exclusively on private company railroads. However, a 1902 US War Department restriction on the construction of private railroads created a disadvantage for the western mills. Any private railroad could connect to, but could not cross, a public railroad – if a company wished to build a railroad that crossed a public railroad, it had to operate (to offer services) as a public railroad. Therefore, wherever the pre-existing system of public railways was dense, the construction of private rail networks was restricted. In the 19th century, a relatively dense public railroad network had been built to service the western sugar plantations so that under the new regulations, the exclusive use of private railroads to haul cane was restricted, and the cost of cane transport was likely to be higher. In contrast, in the east few public railroads had been built, and the mills were freer to construct private railway networks without encountering this regulatory restriction.

In conclusion, the combination of greater numbers of landowning *colonos* and the denser network of public railways in the west caused cane costs to be higher in the west. The higher rates of payment for cane that had to be offered in the west inhibited investment in the mills in the west because preferable alternatives for investment in sugar mills could be found in the east. If one observes the paths of growth of the mills in the island, it is apparent

that many mills were constrained from growing to the enormous size of the largest mills in Cuba. The scarcity of capital was certainly an important influence in these constraints. Much of the capital on the island was destroyed during the War of Independence. The reconstruction of the Cuban sugar industry after the war depended greatly on North American capital, which could not be attracted with equal abundance to all mills. The pattern of flow of investment funds affected the abilities of mills to invest in the newest technologies, given the large increases in scale which the new technologies implied. But it was the competitiveness of each mill that determined its ability to attract scarce capital. It was difficult for single proprietors anywhere to accumulate the capital necessary for the modern industrial enterprise. Sugar had outgrown the plantation. The technology and the scarcity of capital in Cuba dictated that sugar producers needed North American capital markets. Their attraction to the land abundant east has been associated with *latifundismo*. But from the point of view of the *central*, it was driven by their concern to maintain control over the site-specific investments necessary to remain competitive as the world sugar industry adopted continuous processing.

Endnotes

¹Persistence or path dependence arises because the formation of institutions in the past had a longlasting effect on current economic constraints. This aspect of the analysis is reminiscent of a study by Paul David, "The Landscape and the Machine," in *Innovation, Technical Choice and Economic Growth* (Cambridge, 1975), in which the incompatibility of pre-existing infrastructure hindered the adoption in Britain of innovations that had been successfully implemented elsewhere. S.a. Marvin Frankel, "Obsolescence and Technological Change, *American Economic Review* 45 (1955), pp. 215-49. However, in this case the pre-existing institutional framework, rather than the infrastructural layout, is seen to have affected current contractual relations.

²See Oliver E. Williamson, "Credible Commitments: Using Hostages to Support Exchange," *American Economic Review* 73 (1983), pp. 519-40, and *The Economic Institutions of Capitalism*; and Benjamin Klein, Robert Crawford and Armen Alchian, "Vertical Integration, Appropriable Rents, and the Competitive Contracting Process," *Journal of Law and Economics* 21 (1978), pp. 297-326.

TABLE 1

REGIONAL COMPARISON OF MILL CAPACITIES AND CANE SUPPLY SOURCE, 1904-29

(Mill capacities are expressed in thousands of bags (of 325 lbs. each).)

grinding season:	1904		1917		1929	
	west	east	west	east	west	east
average capacity	40.9	45.2	129.0	188.7	207.2	417.4
capacity range	3-201	9-241	25-450	15-600	50-600	30-1300
share of cane supplied by landowning colonos	43.2	15.3	--	--	22.8	10.3
no. active mills	143	31	138	59	96	67

Notes: The west is defined to include the provinces of Pinar del Río, Havana, Matanzas, and Santa Clara. The east is defined to include the provinces of Camagüey and Oriente. (Figure 4 indicates the geographical locations of the provinces.) All active mills are reported in 1904 and 1929; 99% are reported in 1917. For 1917 and 1929, the mill capacities reported are engineering estimates. In 1904 actual production is used to proxy mill capacity.

Sources: Computed using Cuba, Secretaría de Agricultura, Comercio, y Trabajo, *Memoria de la zafra* (Havana, 1916/17, 1930); Cuba, Secretaría de Hacienda, *Industria azucarera* (Havana, 1903/04, 1904/05).

TABLE 2

LIQUIDATION RATES AND SUCROSE CONTENTS FROM A SAMPLE OF WESTERN AND EASTERN MILLS

	west	east
number of mills	13	4
liquidation rate:		
mean	6.94	5.00
standard deviation	0.28	0
sucrose content of cane:		
mean	13.00	13.29
standard deviation	0.40	0.36

Sources: Computed using Braga Brothers Collection, series 127; Cuba, Secretaría de Agricultura, Comercio, y Trabajo, *Memoria de la zafra*.

Session III: Topics in American Labor History

Presiding: Michael R. Haines, Colgate University

Tuesday, January 4, 2:30 p.m.

Explaining Black-White Wage Convergence, 1940-1950: The Role of the Great Compression

**Robert A. Margo
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Between 1940 and 1980 the ratio of weekly wages of black males relative to white males increased by 29 percentage points. Recent work by Donohue and Heckman indicates that black-white wage convergence did not occur at a continuous pace between 1940 and 1980, but rather was concentrated in two episodes: the 1940s, and 1964 to 1975. Post-1964 wage convergence has been attributed to racial convergence in the quantity and quality of schooling, and to the impact of federal anti-discrimination legislation enacted as a result of the Civil Rights Movement, while studies of wage convergence in the 1940s have emphasized black migration from the South, where wages were far below the national average. Wage convergence in the 1940s, however, may also have been aided by a set of forces unique to the decade that produced a marked erosion of wage differentials between skilled and unskilled labor -- the so-called "Great Compression". Because black workers were, on average, less skilled than white workers in 1940, wage compression would have benefitted blacks disproportionately. Although previous studies have noted this possibility, there has been no systematic attempt to quantitatively assess the effects of decade-specific shifts in the structure of wages on black-white wage convergence in the 1940s.

Using a decomposition technique invented by Chinhui Juhn, Kevin Murphy, and Brooks Pierce, I examine the sources of black-white wage convergence in the 1940s, based on samples drawn from the public use tapes of the 1940 and 1950 censuses. Wage compression contributed to racial convergence throughout the wage distribution, with a slightly greater impact observed in the upper tail among blacks. Computed at the sample means, between half and two-thirds of black-white wage convergence can be attributed to the general changes in wage structure induced by the Great Compression.

The Great Compression of the 1940s produced a substantial narrowing in wage differentials in the United States. I present a brief discussion of the quantitative dimensions of this economic phenomenon and the factors causing it. Readers desiring additional evidence and a fuller treatment should consult the paper by Goldin and Margo (*QJE*, February 1992). Goldin and Margo demonstrate that wage compression took place at both tails of the wage distribution during the 1940s. The gap between the median wage and the 90th percentile fell by -0.15 in logs, and the gap between the 10th percentile and the median decreased by -0.12 in logs. Consistent with these patterns, the average earnings of less-skilled and less-educated workers increased relative to the average earnings of skilled and educated workers, as indicated by the decline in the ratio of average weekly wages of college and high school graduates and in the earnings of white-collar workers relative to the non-

farm average. The Great Compression, however, was not solely a narrowing of mean wage differentials *between* education or occupation groups; wage compression also occurred within groups.

Although their relative significance is a matter of debate, the factors behind the Great Compression are not difficult to identify. Some portion of the compression occurred early in the decade in response to wartime shifts in labor demand and of government regulation of the wartime economy. As the war progressed, however, firms could not simply bid up the relative wages of workers in short supply, because of wage and price controls. The National War Labor Board (NWLB), established in 1942, was responsible for approving all wage increases. Analysis of case studies performed by the Bureau of Labor Statistics before, during, and after World War Two reveals that, with a few exceptions, the NWLB did indeed compress the wage structure at its left tail. The data also demonstrate, however, that compression took place in the right tail of the wage distribution during the war and, most importantly, continued immediately after the war. Postwar compression was aided by a rising federal minimum wage, strong unions, and an unexpectedly large increase in the relative supply of educated workers fueled by the GI Bill of Rights, which subsidized college attendance by veterans. Despite these forces, a shift in labor demand towards better-educated workers had become evident in the early 1950s, and by 1960 the effects of Great Compression were partly reversed.

My empirical analysis derives from samples of black and white adult males from the public use micro-data samples (PUMS) of the 1940 and 1950 censuses. Both public use samples are large random samples of the population in their respective census years. To be included in the analysis, an individual had to be between the ages of 25 and 64, and a wage or salary earner who worked at least 40 weeks in the year prior to the census and who earned at least one-half of the federal minimum wage on a weekly basis. Following Goldin and Margo, the earnings of individuals whose reported wage and salary income exceeded the highest census earnings level (so-called "top-coded" individuals) were estimated by multiplying their reported earnings by 1.4. The limitations of the above sampling criteria should be noted. In particular, the restriction to wage and salary workers eliminates the vast majority of persons in agriculture, relatively more of whom were black than white in 1940. Unfortunately, the restriction to wage and salary workers cannot be relaxed, because the 1940 census did not report sources of income other than wages and salaries.

I first compute the extent of racial wage convergence in the samples at various locations in the wage distribution. At the 10th percentile the racial gap in log wages actually widened over the decade. At all other percentiles, however, the gap fell, with the greatest convergence observed in the upper tail of the black wage distribution (50th-90th percentiles).

The earlier discussion that the Great Compression contributed to racial wage convergence between 1940 and 1950. To investigate this possibility, I first present race-

specific calculations of interdecile ranges. The effects of the Great Compression are clearly evident among whites, particularly in the lower tail of the wage distribution; the gap in wages between the 10th and 50th percentiles fell by 0.236 in log terms between 1940 and 1950 while the gap in log wages between the 50th and 90th percentiles fell somewhat less, by 0.097. Wage compression also occurred among blacks, but only in the upper tail of the black wage distribution. The gap in black wages between the 10th percentile and the median was larger in 1950 and 1940, so much so that the spread between the 10th and 90th percentiles was only slightly smaller (0.029 in logs) in 1950 than in 1940.

Based on these findings, a *prima facie* argument that wage compression was a factor behind racial wage convergence could be made if the upper tail (50th-90th percentiles) of the black wage distribution overlapped with the lower tail (10th-50th percentiles) of the white wage distribution. Accordingly, I locate the race-specific percentiles, measured in terms of the number of standard deviations (σ) from the median white wage (σ refers to the year-specific standard deviation of white wages). In 1940 the median black wage fell 1.282 standard deviations below the median white wage – almost exactly the distance in standard deviations between the 10th and 50th quintiles of the white wage distribution – while the black wage at the 90th percentile fell -0.193 standard deviations behind the white median. In 1950 the upper tail of the black wage distribution continued to overlap the lower tail of the white wage distribution, except at the 90th percentile where the black wage exceeded the median white wage by a small amount (0.037 standard deviations). Although the evidence is consistent with the hypothesis that wage compression served to reduce the wage gap between white and black workers, it does not measure the quantitative impact *per se* of shifts in the structure of wages. To measure this impact, I employ the decomposition technique developed for this purpose by Juhn, Murphy and Pierce.

The first step in the Juhn-Murphy-Pierce procedure is to estimate a standard log wage regression for white workers:

$$y_t = X_t \beta_t + \sigma_t \epsilon_t \quad [1]$$

where y_t = log of weekly wages for person i in year t (the subscript i is suppressed), X_t = characteristics of person i in year t included in the regression, β_t = "prices" (*i.e.* regression coefficients) of the characteristics X_t , ϵ_t = "standardized" residual and σ_t = standard deviation of the (unstandardized) residuals. Also, let a "d" in front of a variable – for example, dy_t – indicate the difference between blacks and whites, computed at some location in the wage distribution or at the sample means. Suppose that equation [1] has been estimated for both 1940 and 1950. I compute the following predicted wage for each individual, black and white, in 1950, y_1 :

$$y_1 = X_{50} \beta_{40} + \sigma_{40} \epsilon_{50} \quad [2]$$

The difference between y_1 and y_{40} is made up of two terms

$$y_1 - y_{40} = (X_{50} - X_{40}) \beta_{40} + \sigma_{40} (\epsilon_{50} - \epsilon_{40}) \quad [3]$$

The first term on the right hand side measures the effect of changes in "observable" quantities (the X 's). The second term measures the effect of movement up ($\epsilon_{50} - \epsilon_{40} > 0$) or down ($\epsilon_{50} - \epsilon_{40} < 0$) the residual distribution, valued (in log wage terms) in the base year (σ_{40}). Therefore, the difference between blacks and whites

$$dy_1 - dy_{40} = (dX_{50} - dX_{40}) \beta_{40} + \sigma_{40} (d\epsilon_{50} - d\epsilon_{40}) \quad [4]$$

measures the impact of racial convergence (or divergence) in observable quantities and relative (compared with whites) movement by blacks in the residual wage distribution. Following Juhn, Murphy, and Pierce, I label the first term on the right hand side of [4], " X 's" and the second term, "Gap".

Next, I compute a second predicted wage, y_2

$$y_2 = X_{50} \beta_{50} + \sigma_{40} \epsilon_{50} \quad [5]$$

The difference between y_2 and y_1 ($= X_{50} (\beta_{50} - \beta_{40})$) measures the impact of changes in observable prices (the regression coefficients) on wages. Hence $dy_2 - dy_1$ measures the impact of changes in observable prices on racial wage differentials. I label this term, "Prices", in the decomposition. The difference between y_{50} and y_2

$$y_{50} - y_2 = \epsilon_{50} (\sigma_{50} - \sigma_{40}) \quad [6]$$

measures the impact of compression ($\sigma_{50} - \sigma_{40} < 0$) or widening ($\sigma_{50} - \sigma_{40} > 0$) in the residual distribution. Thus, the final term in the decomposition, which is labeled "Residuals", is $dy_{50} - dy_2$; it measures the effect on the wage gap of a change in the residual wage distribution among whites, holding fixed the relative quintile positions of black workers. By convention, the Prices and Residual terms are what is meant by shifts in wage structure.

The effects of changes in wage structure – the prices and residual terms – are evident throughout the wage distribution; indeed, had only the wage structure changes occurred, the black-white wage gap at the 10th percentile would have fallen, rather than increased. Although the relative impact varied somewhat through the distribution, compression in observed prices (the regression coefficients) was three or four times as important as compression in the residuals. Overall, at the sample means, wage compression accounted for 52 to 62 % of black-white wage convergence between 1940 and 1950, depending on the base year.

Because the numerical effect of wage structure changes was similar throughout the wage distribution, variation in wage convergence across percentiles was due primarily to observable quantities (X 's) and by movement by blacks up the residual white wage distribution (the Gap term). At the 10th percentiles the impact of observable quantities served to widen the racial wage gap; at the median and upper quartile, blacks succeeded in narrowing the gap with whites in observable quantities, thereby causing substantial wage convergence. At the sample means, however, the impact of changes in observable quantities was more modest, accounting for 22 to 30 % of the narrowing in the racial wage gap.

Except at the extreme left tail, the Gap terms are positive: blacks moved up the percentiles of the distribution of residual wages among whites. Regardless of the base year, the magnitude of such movement was greater above, than below, the median. At the sample means, the Gap term accounts for approximately 17 % of wage convergence, regardless of the base year. Studies of racial wage convergence since 1960 have emphasized the importance of racial differences in the school characteristics, such as the length of the school year, teacher-pupil ratios, or per pupil expenditures. It seems unlikely that changing racial differences in school characteristics could have played a similar quantitative role prior to 1950 because the pre- 1950 pattern of change was not one of monotonic convergence. In particular, racial differences in school characteristics actually widened for blacks born in the South between 1886 and 1910. Racial differences in school characteristics did decline

somewhat among cohorts born in the South after 1910, but a substantial narrowing was not apparent (among labor market participants) until after 1960. Still, it is possible that changes in school characteristics could account for a portion of the Gap term even in the 1940s, if for no other reason that the numerical representation of the 1886-1910 southern cohorts in the black sample fell from 64 % in 1940 to 44 % in 1950.

A defensible estimate of the effects of school characteristics can be made by redefining black educational attainment downward relative to white educational attainment, and re-computing the decomposition. In particular, I shall assume that, for blacks born in the South before 1910 completing more than five years of schooling, the appropriate reduction is four years; for five years or less, the appropriate reduction is to one year of schooling. Thus, for example, a black man with eight years of schooling is assumed to have received an education functionally equivalent to that of a southern white with four years of schooling. For blacks born in the South after 1910, I assume that the corresponding reduction is three years. As a result of this adjustment, racial convergence in school quality will occur as older black cohorts are replaced by younger cohorts. No adjustment for school quality is made for northern-born blacks or for whites, regardless of region. Measured at the sample means, the adjustment for school quality can explain about three-quarters of the Gap term; the explanatory power is somewhat less at the median or 75th percentile, but still substantial. Adjusting for school quality also increases the quantitative significance of the wage structure terms (the sum the prices and residual terms) but the magnitude of the increases is small. Based on these results, I conclude that adjusting for school quality can account for the majority of the Gap term, except perhaps at the median. Put another way, virtually all of the narrowing in the racial wage gap at the sample means can be explained by a narrowing of racial differences in observable quantities (including school quality), prices, and within-group residual inequality (the residual terms). The remainder of the gap term is presumably due to changes in (unobserved) quantities other than school quality and possibly to a decline in racial wage discrimination over the decade.

I have demonstrated that the Great Compression increased the relative wages of adult black males in the short run (*i.e.* the 1940s). The compression may also have helped in the long run. Recent research indicates that the educational achievement of black children during the first half of the 20th century was a positive function of their parents' economic status. By (temporarily) boosting the economic status of black parents, the Great Compression may have increased the schooling levels of black teenagers beyond what would have occurred otherwise in the 1940s. If so, these generations would have been better equipped to take advantage of government anti-discrimination efforts that enhanced the demand for educated black labor in the 1960s.

To investigate the relationship between schooling levels of black teens and parental earnings, I estimate equation [7], using data from the 1940 PUMS:

$$\ln(S/S^*) = X\delta + \epsilon \quad [7]$$

where S = schooling level (highest grade completed); S^* = "expected" schooling level for a person of age t ; X = determinants of schooling, including weekly wages of fathers; δ = regression coefficients, and ϵ = random error. The expected schooling level is the modern reference standard: individuals enter the first grade at age six and complete each grade in

a year's time. Negative values of $\ln(S/S^*)$, therefore, indicate (in log terms) age-in-grade retardation (for those still in school) or dropouts.

The regression results confirm a positive and statistically significant association between child schooling and adult earnings: the elasticity between $\ln(S/S^*)$ and the father's weekly wages is about 0.1. I use this coefficient to predict how much larger the average schooling level would have been in 1940, had the increase in adult earnings associated with the Great Compression. For this purpose, I use the gain in earnings unadjusted for school quality (0.114). The predicted increase in $\ln(S/S^*)$ is 0.011 [= 0.114 x 0.098]. Compared with the mean schooling level among black teens in 1940 (-0.331), the impact of the Great Compression was very small. Compared with the increase in schooling levels over the decade, however, the impact would have been larger. Unfortunately, because of a peculiarity of the 1950 PUMS it is impossible to create a matched sample of black teens (whose fathers were full-time wage and salary workers and whose mothers were present) in both years. For all black teens ages 14 to 19, the value of $\ln(S/S^*)$ rose by 0.11 between 1940 and 1950. Using this figure as a base, the Great Compression can explain 10 % [= 0.011/0.10] of the growth in black schooling levels between 1940 and 1950.

Like the experience of the post-1975 period, the 1940s illustrates the sensitivity of black-white earnings differences to shifts in the overall structure of wages. In the 1940s, however, shifts in wage structure served to narrow racial differences in earnings. By narrowing wage differences between skilled and unskilled workers, and by compressing wages within occupations and other labor market groups, the Great Compression led to greater racial wage convergence between 1940 and 1950 among adults than would have occurred otherwise. By indirectly raising the schooling levels of black teens, the Great Compression also contributed to racial wage convergence in subsequent decades, although its effects here were more modest.

The results of this paper could be extended. In particular, census data cannot determine the exact timing (within the 1940s) of the changes documented in the paper, nor can census data reveal the precise institutional mechanisms at work (for example, the NWLB vs. the minimum wage vs. unions). Further analysis of wage studies performed by the Bureau of Labor Statistics, specifically of industries that employed large numbers of blacks, may shed light on both issues.

Long-Term Trends in American Intergenerational Occupational Mobility

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1 Introduction

An interest in social and economic mobility is a common thread in the social sciences. In fields such as sociology, study of the topic is central to the discipline. In other fields, such as history, investigation of mobility occupies a prominent position, but is a comparatively new addition to the standard research agenda. Finally, in fields such as economics, mobility is not always an explicit subject of concern, but one that is nonetheless implicit in many important analyses. In economics, for example, almost every discussion of inequality or poverty implicitly involves issues related to the economic mobility of individuals or groups of individuals.

While a concern for mobility is shared by social scientists of almost all persuasions, research on the subject has not been entirely catholic. This is particularly evident if one partitions mobility research into two sub-fields: (a) the study of intragenerational mobility, or changes in the status of individuals over the course of their adult lives (where "status" is measured in terms of social class, occupation, or income level), and (b) the study of intergenerational mobility, or changes in the status of adults relative to the status of their parents. Whereas researchers from a variety of the social sciences — including economics — have carried out investigations of intragenerational mobility, the analysis of intergenerational mobility has been primarily the domain of historians and sociologists.

Recently, however, a number of economists have produced important contributions to the literature on intergenerational mobility. Works such as Solon (1992) and Zimmerman (1992) add markedly to our understanding of economic mobility in the United States. The Solon and Zimmerman papers, for example, provide some of the first estimates of intergenerational income correlations based on data from nationwide samples of individuals, and their studies also employ new techniques to eliminate potential biases in the measurement of parent-child income correlations.

Nevertheless, by calling attention to the extent of intergenerational economic mobility in contemporary America, these and other related studies raise two obvious questions. First, how does the pattern of intergenerational mobility today compare to that of the past? Second, to the extent that changes in mobility have occurred, how can one account for those changes? The answers to these questions are the focus of this paper.

Documenting trends in American intergenerational mobility is the task of the first half of this paper. This analysis concentrates on data from four nationally representative samples of white men aged 25-34 at different points of time during the past 100 years. While the data in these samples refer to a somewhat narrow segment of the overall population (young white males), the results they supply are nonetheless useful in gauging the mobility experience of a sizable fraction of all Americans. Similarly, although data constraints dictate that occupation, not income, be used to measure economic status when tracking long-term trends in intergenerational mobility, the use of this metric has advantages as well

as disadvantages. On the one hand, the categorical nature of occupational information greatly complicates attempts to determine trends in the actual *correlation* of parents' and children's economic status. A discussion of such trends thus is not included in this paper. On the other hand, observed patterns of intergenerational occupational mobility are direct evidence of the extent and direction of intergenerational flows between specific segments of the economic hierarchy. Such patterns therefore disclose a great deal about the importance — or unimportance — of parental economic status in the determination of an individual's own economic status.

It is for this reason that the results summarized in the first half of this paper are so important. Those results reveal that, during the past century, there has been both significant change as well as surprising continuity in the patterns of American intergenerational occupational mobility. Much of the change reflects the dramatic shifts which have occurred in the composition of the American labor force. Thus, for men of all family origins, there has been a sharp decline in the proportion entering farm occupations, and the growth in the share of the labor force in white collar occupations has been mirrored in improved opportunities for the advancement of blue collar and farm sons into such occupations. Not all changes in mobility patterns can be attributed, however, to long-term shifts in the composition of the labor force, nor have all changes taken place over the entire course of the last century. In recent decades, for example, there has been for men of all family origins a perceptible decline in the proportion achieving the economic status associated with a high white collar occupation.

In addition, while change has characterized a number of important trends in the overall pattern of American mobility, there has also been marked continuity in many aspects of that pattern. Intergenerational flows between a number of occupational categories have remained startlingly similar over the past 100 years. In 1900, for example, the share of sons from middle status origins ending up in low blue collar jobs was 22.5 percent. In 1987, it was 23.3 percent. Likewise, relative flows between a number of occupational categories have remained essentially constant. Thus, in both 1900 and 1987, the share of farm sons entering low blue collar occupations was about twice the share entering professional, technical, and managerial positions.

The observed continuity in a number of mobility patterns should not overshadow, however, the many important changes which have occurred in such patterns. One of those changes — the long-term rise in the accessibility of white collar occupations — is arguably the single most important aspect of the evolution of American intergenerational mobility. The second half of this paper therefore considers the forces which may have been responsible for that development.

The discussion in the second half of the paper begins with a description of two competing hypotheses concerning the evolution of intergenerational mobility in the United States. The first hypothesis argues that the increased accessibility of white collar occupations has been the result of the tremendous expansion during the past century in the provision of public education. In contrast, the second hypothesis contends that increased spending on education has been merely a response to a more fundamental economic stimulus: technological change.

To determine which of these competing hypotheses is best supported by available

evidence, the paper introduces a general equilibrium model of intergenerational occupational mobility. The parameterization of that model is not yet sufficiently advanced, however, to admit any definitive conclusions concerning the merits of the two hypotheses.

The rest of the abstract is organized as follows: Section 2 briefly summarizes existing literature on trends in American intergenerational occupational mobility. Section 3 details new evidence on such trends. Section 4 presents alternative explanations for observed trends and describes a model for evaluation of those explanations. Section 5 concludes.

2 The existing literature

Sociologists and historians have been responsible for most of the existing evidence on long-term trends in American intergenerational occupational mobility. That evidence is not as substantial, however, as one might hope. On the one hand, the research conducted by sociologists has carefully documented mobility in the United States during the 1960s and 1970s, and it also has motivated subsequent – albeit less detailed – research on mobility in the 1980s. In addition, sociologists have drawn attention to studies in other fields which provide insight into the mobility regime of the 1950s.

For perspectives on mobility in the US prior to World War II, however, one must turn to the results of historical research. This research often documents mobility patterns for periods as early as the first half of the 19th century. Nevertheless, much of the historical research suffers from two disadvantages: it focuses only on specific cities or towns in the US, and it generally tracks mobility in those areas over relatively limited periods of time.

As a result, while there is a large body of evidence concerning intergenerational occupational mobility in America during particular decades of the post-war era, knowledge concerning mobility patterns in other time periods is considerably more limited. This has seriously hampered attempts to draw accurate conclusions concerning the long-term course of intergenerational mobility in the United States.

3 Intergenerational occupational mobility during the past 100 years

The data

While investigators traditionally have faced serious obstacles to documenting long-term trends in American intergenerational mobility, such obstacles are beginning to crumble in the face of fresh research. Two developments in this regard are particularly noteworthy. First, a team of sociologists has recently constructed a dataset with extensive evidence on intergenerational mobility in the entire United States during the period 1880–1900. In addition, data collected by the Census Bureau in 1987 provide important new insights into mobility in contemporary America. As a result, sufficient evidence now exists to sketch the major trends in the evolution of American intergenerational occupational mobility over the past century.

In the analysis that follows, I make use of the two datasets just described as well two other datasets created during the past 35 years. Together, these datasets document mobility patterns for nationally representative samples of individuals at four different points in time: 1900, 1962, 1973, and 1987. The first dataset is the National Panel Study (NPS), which was compiled at the University of Washington during the late 1980s. It is based on a sample of matched records from the 1880 and 1900 population census

manuscripts. The second dataset is a subset of the well-known Occupational Changes in a Generation (OCG) study by Blau and Duncan in 1962. The third dataset is a subset of the 1973 follow-up to the 1962 OCG. Finally, the fourth dataset is a subset of the results in the Census Bureau's 1987 Survey of Income and Program Participation (SIPP), Wave 2.

All of the data analyzed in this paper pertain to the mobility experience of white, native-born males aged 25–34 in the relevant years. This group was the focus of the compilers of the National Panel Study in their efforts to match a sample of 1880 census records with records from 1900. As a result, subsamples of individuals with the same characteristics were selected from the more comprehensive datasets available for the postwar era. The numbers of observations in each of the samples used in this study are as follows: NPS, 1,814; OCG 1962, 3,816; OCG 1973, 7,324; SIPP 87/2, 1,401.

All of the samples described above provide information on the occupation of the individuals in the sample. In addition, information on each individual's parents' occupations is also available. The parental information relates to occupations held when the sample members were children. In this paper, discussion centers on comparisons between sample members' occupations and the occupations of their fathers. If an individual's father was not present when that person was a child, the occupation of the head of the childhood household – whether the head was male or female – is used as the sample member's "father's" occupation.

Documenting the trends: methods

To facilitate the description of mobility trends, the analysis in this paper employs a four-category occupational classification system. That system categorizes jobs as either high white collar, middle status, low blue collar, or farm. The first category consists of professional, technical, managerial, and official occupations. The second includes sales, clerical, and crafts occupations. The third covers operative, laborer, and service occupations, and the fourth consists of farm owners, farm tenants, and farm laborers. The detailed occupational information available in the four samples has been used to classify each sample member's own and parental occupations into one of these four categories.

The classification of all occupations into four categories enables the creation of a compact *mobility table* for each sample. A mobility table is simply a cross-classification of individuals' occupations and the occupations of their parents. Table 1 presents the mobility table for the 1900 NPS sample.

Category of Father's Occupation	Category of Sample Member's Occupation				Totals
	High White Collar	Middle Status	Low Blue Collar	Farm	
High White Collar	42	68	26	14	150
Middle Status	37	138	63	42	280
Low Blue Collar	49	130	160	80	419
Farm	79	140	168	578	965
Totals	207	476	417	714	1814

Source: Guest et al. (1989).

Mobility tables can be used for a number of different purposes. In this paper, they are used primarily to facilitate the calculation of a convenient measure of mobility, the *outflow percentage*. One outflow percentage exists for each cell in a mobility table. The percentage is simply a given cell's share of all observations in its row. It thus describes the fraction of all men from a particular family origin who are employed in a specific category of occupations. Table 1 indicates, for example, that in 1900 14.5% of all men whose fathers were farmers became workers with middle status jobs. This, then, is the outflow percentage for farm-to-middle status intergenerational occupational flows.

Documenting the trends: results

Figures 1 and 2 plot time series of outflow percentages for sons from farm and low blue collar families. In each figure, the labels "farm," "low," "middle," and "high" mark the lines plotting flows into farm, low blue collar, middle status, and high white collar occupations, respectively.

The most dramatic change is evident in Figure 1, which focuses on the experience of men from farm origins. There, the decline in the share of the American labor force in

farming is reflected in a concurrent decline in the farm-to-farm outflow percentage. In 1900, 60% of the young sons of white American farmers were farmers themselves; by 1973, the share had dropped to only 13.5 percent. Since 1973, however, the share of farm sons entering farm occupations has rebounded, reaching half what it had been in 1900.

Perhaps the most remarkable aspect of the flows depicted in Figure 1 is not the variation in the share of young men entering farm occupations, but rather the way in which the flows to the other three occupational categories have changed. In particular, those three flows have maintained essentially constant proportions since 1900. For example, at the beginning of the century, the share of farm sons entering low blue collar occupations was 2.1 times the share of farm sons entering high white collar occupations. In 1987, the equivalent ratio was also 2.1. The lock-step fashion in which farm-to-non-farm flows have changed over the past century raises interesting questions about the forces affecting such mobility.

While the trends in Figure 2 are not nearly as dramatic as those in Figure 1, they are intriguing nonetheless. The flows in Figure 2 document the occupational attainment of young men from low blue collar family origins. Once again, the historical decline in farm employment is mirrored in declining intergenerational flows into the farm sector. The declining flows into farm employment have been offset, however, by rising opportunities for advancement into middle status and high white collar occupations. This, in turn, has meant surprising continuity in a flow which has been of particular interest to a number of previous researchers: the share of men from low blue collar origins who have remained low blue collar workers themselves. Throughout the past century, that flow has remained essentially constant at the relatively high level of 40 percent.

Although the results in Figures 1 and 2 depict only the experience of men from farm and low blue collar family origins, it is possible to carry out similar analyses for men from middle status and high white collar families. Unfortunately, space limitations preclude a presentation of such results in this abstract. It would be inappropriate, however, to end the foregoing discussion without a description of an important development involving occupations in these higher status categories. This development is the long-term rise in the share of men from all family origins who have been able to gain employment in white collar jobs. Table 2 documents this development.

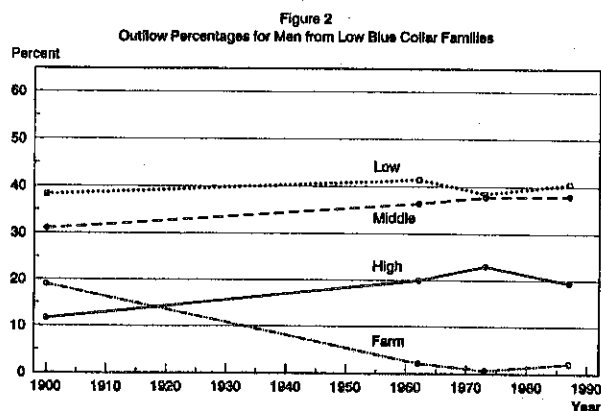
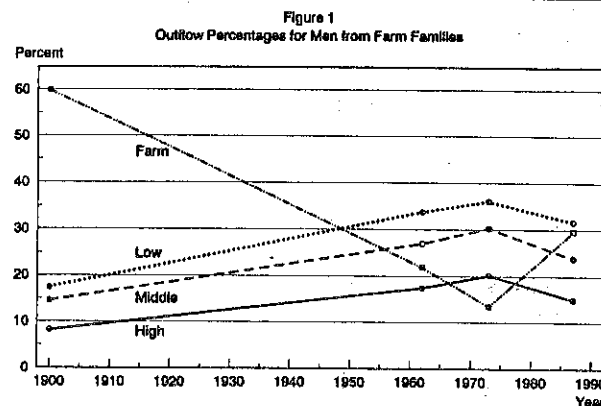


Table 2
Shares of Young White Men Entering White Collar Occupations
(percent)

Year	Father's Occupation (Four Categories)				Father's Occupation (Two Categories)	
	High White Collar	Middle Status	Low Blue Collar	Farm	Non-Manual	Manual
1900	56.0	32.1	23.9	13.7	58.0	18.1
1962	75.1	48.6	32.5	26.8	72.8	33.0
1973	68.2	48.1	34.3	30.0	66.3	36.1
1987	67.2	48.7	28.6	37.5	65.0	34.0

Source: Author's calculations.

The results in Table 2 indicate that, between 1900 and 1962, fathers in all occupational categories saw significantly greater shares of their sons entering white collar occupations. Since 1962, trends in the share of men entering such occupations have differed somewhat from family origin to family origin. Nevertheless, it is still the case that, regardless of family background, a young, white male today has a significantly greater chance of entering a white collar occupation than a young, white male at the turn of the century.

4 Explaining the trends

Some hypotheses

The increased accessibility of white collar occupations has been one of the most important developments in American intergenerational occupational mobility. It has meant significantly improved economic opportunity for individuals from all family origins. Yet this important development raises an equally important question: why? What forces have been responsible for this critical change in the pattern of American intergenerational mobility?

While there are any number of possible answers to such a question, two explanations appear to offer the greatest promise. Both explanations stress the link between education and mobility.

The first explanation stresses supply-side forces. It begins by noting the tremendous expansion in public education which has occurred during the past century. It then argues that it has been precisely this expansion which has been the source of the increased accessibility of white collar occupations.

The second explanation focuses on demand-side forces. It contends that increased spending on education has been merely a response to a much deeper force in the economy: technological change. Such change, it is argued, has motivated parents to demand more education for their children, and the resulting rise in educational expenditures has thereby enabled increased mobility into white collar occupations.

There are actually two variants of the second explanation. The first argues that the most important aspect of technological change has been its effect on the level of income. The second contends that the relevance of technological change lies in the bias it has exhibited. To summarize what would otherwise be a rather lengthy discussion, the first variant relies on assumptions concerning the income elasticity of demand for children's education, while the second focuses on altered incentives for such education.

Weighing the hypotheses

None of the explanations just outlined are necessarily mutually exclusive. It is conceivable that elements of each have played a role in the observed change in the American intergenerational mobility regime. To determine, however, which of the explanations best accounts for that change, one needs a framework for weighing the relative significance of the different forces which have influenced mobility in the United States.

Until recently, such a framework has been unavailable. Most theoretical discussions of the forces affecting long-term trends in intergenerational mobility have been either relatively informal or have focused on intergenerational *income* mobility. Fortunately, research concurrent to the work summarized in this paper has led to a framework well-suited

to an analysis of the hypotheses discussed above. That framework consists of a general equilibrium, overlapping generations model of intergenerational occupational mobility. The essential components of the model are as follows:

- [1] Individuals in the economy live for two periods. During their first period of life, individuals are "children" who are incapable of any productive activity. In their second period of life, individuals become "adults." At the beginning of this second period, every individual gives birth to a child. Throughout the second period, adults inelastically supply their labor services in a competitive labor market.
- [2] There are two types of labor services, manual and non-manual. All individuals are innately capable of supplying manual labor services when they are adults. Individuals are only capable of non-manual work, however, if they receive an "education" as children.
- [3] A child receives an education if her parent contracts for the "teaching" services of non-manual workers. A specific amount of non-manual labor is required to educate every child. This amount varies, however, from child to child depending on the child's "learning ability," which is determined stochastically at birth but is correlated with the learning ability of the parent. Every parent observes her child's learning ability as soon as the child is born.
- [4] A parent desires an education for her child because the lifetime utility of the child enters the parent's utility function. At the same time, the parent's own consumption is also an argument in her utility function. Preferences for consumption and the utility of children do not vary between parents.
- [5] There are no capital markets for the funding of education. There is, however, a government subsidy financed by an income tax. Whether or not a parent invests in an education for her child depends, therefore, on the parent's after-tax income, the cost of the education net of the subsidy, and the expected benefit to the child of education. All of these depend, in turn, on the level of wages in the economy.
- [6] Wages are determined as the marginal product of each type of labor in the aggregate production function for the economy's single consumption good. All manual labor is employed in the goods-producing sector, but non-manual labor is supplied to both the goods-producing and educational sectors. Non-manual workers employed in the education sector are assumed to receive the same wage as non-manual workers in the goods-producing sector.
- [7] Manual and non-manual labor are the only inputs in the aggregate production function. There is no capital accumulation; all income is spent on either consumption or educational services. Technological change is exogenous to the model, and may be biased or factor neutral.

To characterize equilibrium in a model such as the one just outlined, one needs to specify and parameterize a number of different functions. That parameterization must be done for two time periods: the late 19th century and the late 20th century. At the present time, complete parameterization of the model is still underway, and it is therefore not possible to draw any firm conclusions concerning the hypotheses discussed earlier.

5 Concluding comments

While the modeling necessary to account for the evolution of American intergenerational mobility is still underway, the results in this paper nevertheless add markedly to our understanding of that evolution. The results indicate that both continuity and change have characterized the various patterns of intergenerational occupational mobility in the United States during the past century.

That there has been continuity in the midst of such dramatic shifts in the composition of the American labor force is perhaps the most surprising of the results summarized above. Yet it is the long-term rise in the accessibility of white collar occupations that has been especially relevant for so many young American men. An explanation of that trend – as well as further details concerning the evolution of American intergenerational mobility – are the promise of the full version of this paper.

Pensions and Unemployment

Dora L. Costa

Massachusetts Institute of Technology

For the old, unemployment is frequently a prelude to retirement. The 19th century was no exception. Margo (1993) finds that the probability of long-term unemployment rose with age and that the older a worker was when he became unemployed, the more likely he was to subsequently retire. This paper investigates the impact of the first large government transfer, the Union Army pension program, on unemployment rates circa 1900. In a simple search model (Lippman and McCall 1976a, 1976b), pensions should induce higher entry into and exit from unemployment. In fact, recent studies have found that unemployment insurance decreases the probability of leaving unemployment (Meyer 1990; Katz and Meyer 1988) and increases the probability of entry into unemployment since the availability of benefits will lead firms to reduce their labor force through layoff (Feldstein 1978; Topel 1983, 1985). If Union Army pensions increased the incidence of long-term unemployment, then, because older workers who were unemployed were more likely to retire, they were a causal factor behind early retirement.

The relationship between age and unemployment has changed since the beginning of the century. Census data shows that in 1900 the incidence of unemployment rose with age among both agricultural and non-agricultural workers. But, by 1980, unemployment no longer dramatically rose with age. Several factors may account for these differences in unemployment status by age. First, as retirement incomes have risen, withdrawal from the labor force has become an alternative to the continuation of a job search. Secondly, labor markets have changed in ways that benefit older workers who are already employed.

The difficulties faced by older workers at the end of the 19th century were widely recognized by contemporaries. Compared to younger men, older workers were more likely to experience some unemployment and, when they were unemployed, they were unemployed for longer periods of time. Men in seasonal industries were especially likely to suffer unemployment.

Unemployment had become a great social concern by the end of the 19th century. Although some men were fully compensated for layoff risk, there were wide disparities in the degree of compensation. For many men wage premia covered only half of income lost because of layoffs (Fishback and Kantor 1992; Hatton and Williamson 1991). Contemporaries cited unemployment as the single most common cause of poverty (Dubofsky 1975: 22; Lauck and Sydenstricker 1917: 76). Unemployed men faced the danger of gradually drifting into the casual laboring class as their health and skill deteriorated. Unemployment of the head of the household frequently resulted in sending children to work (Lauck and Sydenstricker 1917: 170-171; Goldin 1979, 1981).

This paper uses Union Army pension records to study the impact of the first large-scale government transfer – the pension program covering Union Army veterans, on unemployment rates among older workers in the 19th century. Union Army veterans were eligible for a pension that was on average equal to 55% of *per capita* income. Receipt of a pension did not depend upon current income or labor force status, but did depend upon health, as judged by a panel of three examining surgeons appointed by the Pension Bureau.

Men who could trace their disabilities to their war-time service received a greater pension for the same disabilities than men who could not. Therefore, the effect of poor health and of pensions on unemployment rates can be distinguished.

The sample used in this research is restricted to men who were in the labor force and who reported months of unemployment in the year June 1, 1899 - May 31, 1900. The total number of observations is 379. Because the sample is restricted to men who were in the labor force, men who were omitted from the sample were on average collecting larger pensions and were in worse health. The restriction to men currently in the labor force implies that there are only two possible states, employment and unemployment. Retirement is not an option. Therefore, the findings are conditional on current labor force participation.

Unemployment in the veteran sample is compared with a random sample of white, non-institutionalized men drawn from the Public Use Sample of the 1900 Census. The random sample contains both veterans and non-veterans. Veterans cannot be identified in the random sample, but approximately 16.7% of all white men aged 50-64 were on the pension rolls and about 11.0% of all men aged 65-81. Compared to the general population, veterans were more likely to be unemployed and when they did experience unemployment, they experienced more of it. Differences between veterans and the general population persisted even when 1) the random sample was restricted to men either born in a Union state or, if foreign-born, who immigrated prior to the Civil War and when 2) the restricted random sample was adjusted to have the same age distribution as the sample of veterans. Greater periods of unemployment among veterans do not result from veterans' being in more seasonal jobs than the general population. I divided non-farmers into 24 occupational categories and looked at the mean months of unemployment for white males aged 25-49 in the Public Use Sample of the 1900 Census (Preston and Higgs). Within each occupational category, veterans experienced more months of unemployment.

Men who were receiving higher pensions and men who were rated by the examining surgeons as being in good health experienced less unemployment. When the sample was grouped by health status, men who were receiving larger pensions experienced more unemployment. However, these cross-tabulations are inconclusive. Men who could trace their disabilities to the war and hence were eligible for larger pension amounts were not chosen at random. Men with war-related disabilities were employed in different occupations and differed in terms of marital and head of household status.

In order to test whether the relation between months of unemployment and pension amount persisted when I controlled for other characteristics, I divided months of unemployment into three classes: 0 months, 1-5 months, and 6-12 months. I separated the unemployed into the long-term unemployed (6-12 months) and into the short-term unemployed (1-5 months), since the chance of short-term unemployment is more likely to depend upon seasonal factors and thus may be unaffected by pension income. I then used a multinomial logit regression. The use of a multinomial logit model allows me to distinguish between short-term and long-term unemployment without imposing too many assumptions on the data. I later estimate entry and exit hazards, but I can do so only under very strong assumptions. I use an unordered logit model rather than an ordered logit model because if long-term unemployment does indeed differ from short-term unemployment, then the slope parameters will not be equal. In fact, I rejected the hypothesis that the slope

parameters were equal.

Both farmers and non-farmers are included in the sample. When the sample was restricted to non-farmers, the magnitude and the signs of the coefficients remained unchanged, but most of the variables were no longer significant.

I control for seasonality in the regression by using the mean months of unemployment within job classifications calculated from the public use sample of the 1900 Census (Preston and Higgs 1980) as one of my independent variables. A better proxy for seasonality would be the coefficient of variation around the mean of monthly employment by industry. Although monthly employment information is available in the 1900 census of manufacturing, I have many occupations, including that of farmer, for which I do not have information on monthly employment. Therefore, I prefer to use mean months of unemployment within a job classification.

The regression results indicate that 1) mean months of unemployment within an occupation significantly increases the probability of either short-term or long-term unemployment instead of no unemployment, but not of unemployment of 1-5 months instead of 6-12 months; 2) compared to farmers, laborers and artisans were more likely to be unemployed 1-5 months rather than 0 months, but not 6 months instead of 0 months or instead of 1-5 months; 3) compared to professionals and proprietors, artisans and laborers were more likely to be unemployed 6-12 months and 1-5 months instead of 0 months, but not 6-12 months instead of 1-5 months; 4) the larger the pension the more likely the veteran was to be unemployed 6-12 months instead of 0 or 1-5 months; and 5) pension size did not affect the probability of unemployment of 1-5 months rather than 0 months.

The significant impact of mean months of unemployment within an occupation on the probability of some unemployment instead of no unemployment, but not on the probability of unemployment of 1-5 months instead of 6-12, suggests that while seasonal factors are an important component of unemployment, seasonal factors alone cannot explain long-term unemployment. Health and pensions may help explain some of the long-term unemployment among Union Army veterans. A 25% to 183% increase in pensions increased the probability that the veteran would be unemployed 6-12 months instead of 0 months by 0.10 and increased the probability that the veteran would be unemployed 6-12 months rather than 1-5 months by 0.24. A 125% to 400% increase in pension amount increased the probability that a veteran would be unemployed 6-12 months rather than 0 months and 6-12 months rather than 1-5 months by 0.18 and 0.46, respectively. When pensions are reduced, it is mainly long-term unemployment that falls. Although the percentage of men experiencing no unemployment rises slightly, there is an increase in the percentage of men unemployed 1-5 months.

While pension size affects the probability of long-term unemployment, pension size does not significantly affect the chance of unemployment of 1-5 months rather than 0 months. One possible explanation is that while pensions decreased the probability of exiting unemployment, pensions did not increase the probability of entry into unemployment. This possibility is investigated in the next section.

Since information on the duration of an unemployment spell, or whether the veteran is currently unemployed, is unknown, the probabilities of entering and exiting unemployment can be estimated only under very strong assumptions. These assumptions

are spelled out below. The discussion closely follows Margo (1990b; 1993).

The census enumerators reported the number of months of unemployment experienced in the year June 1, 1899 to May 31, 1900. Let $u(0)$ be the unemployment rate on June 1, 1899, p the probability that a worker employed at the beginning of the year would experience some unemployment, and f the fraction of men who had experienced some unemployment in the year. Then, $f = u(0) + (1 - u(0))p$.

Several assumptions are necessary in order to express $u(0)$ and p in terms of probabilities of entering and leaving unemployment. These are 1) that the size of the labor force did not change during the year, 2) that employment and unemployment were the only possible labor force states, 3) that the labor market was in steady-state equilibrium, and 4) that the probabilities of moving from employment to unemployment and vice-versa did not depend upon time. The steady-state unemployment rate, u , and p can then be written as

$$p = e^{-\beta}$$

$$u = u(0) = \frac{\beta}{\beta + \delta}$$

where β is the probability of entering unemployment, or the cumulative entry hazard, and δ is the probability of leaving unemployment, or the cumulative exit hazard. The steady state unemployment rate can be estimated as the average months of unemployment divided by average labor force months. Following Margo (1990; 1993), I assume a full-time work year of 50 weeks or 11.5 months. The average monthly entry and exit hazards are $\beta/12$ and $\delta/12$.

Average monthly entry and exit hazards are calculated for men aged 50-64 in the veteran sample who were collecting a monthly pension of less than or equal to \$8 and of more than \$8. The sample is also subdivided into farmers and non-farmers and men in poor health and good health. Age-adjusted hazards are calculated. The sample was restricted to men aged 50-64 because entry and exit hazards can be calculated only under the assumption that retirement is not a possible labor force state. The estimated hazards suggest that pensions exerted a much larger impact on the probability of entering unemployment instead of on the probability of exiting unemployment.

The main findings are that pensions increased the probability of long-term unemployment, but not of short-term unemployment. Pensions appear to have mainly decreased exit hazards, but they also slightly increased entry hazards. The findings are consistent with a job search model. Union Army pensions were a pure income transfer and, therefore, led men to search longer. They may also have led men to try searching for better employment instead of continuing to work in their current jobs. If so, pensions increased the already high quit rates characteristic of 19th-century labor markets.

The findings permit the quantification of the impact of a program that affected a large proportion of the population. A reduction in pensions would have decreased long-term unemployment and increased the incidence both of no-unemployment and of short-term unemployment. Because pensions increased the prevalence of long-term unemployment and because older workers who were unemployed were more likely to retire, the

results imply that pensions led to an increase in early retirement.

The findings do not pertain only to the Union Army pension program. They also shed light on the secular rise in decade unemployment averages. From 1800 to 1960 there has been a slight increase in decade unemployment averages from 1-3% to 5%, partly driven by a decrease in self-employment (Lebergott 1964: 189). In 1970-1979, average unemployment was 6% and in 1980-1990 the average was 7 percent. Shifts in industry type can explain most of the difference in the probability of entry into unemployment, but not the greater duration of unemployment (Margo 1990). Two possible explanations for the increased duration of unemployment are the prevalence of unemployment insurance and rising wealth, permitting longer search until the worker finds an attractive job. Lebergott (1964: 165) noted, "As real incomes have risen over the decades so has the ability of the unemployed to refuse the first job available (if it is below standard), to delay until he finds 'suitable' work. And as social institutions have changed (in particular, as unemployment compensation has been introduced, supplemental benefits pioneered, health service extended), this effect has been intensified." A pure income transfer, the Union Army pension, mainly decreased the probability of leaving unemployment. Thus, the findings suggest that the income effects arising from unemployment insurance and the secular rise in incomes are significant factors in explaining the rise in unemployment rates over the course of the century.

Session IV: Understanding Collective Action and Outcomes

Presiding: Martha L. Olney, University of Massachusetts-Amherst

Wednesday, January 5, 8:00 a.m.

What Did Unions Do? An Analysis of Canadian Strike Data, 1901-14

**Michael Huberman, Trent University, and
Denise Young, University of Alberta**

1. Motivation and Theory

Gavin Wright (1987) observed that economic history forms a bridge between labor economics and labor history. The gap between the disciplines is most evident in their different approaches in understanding the determinants of strikes and strike dimensions. Using a new data source on individual strikes in Canada, the aim of this paper is to bring together recent contributions by labor economists and historians in an analysis of the determinants of strikes durations between 1901 and 1914.

The new information economics maintains that strikes serve the important function of generating information. In its most popular variant workers go on strike to get information about the profitability of firms; firms are more willing to settle in periods of expansion, and as a result strike durations are countercyclical. Empirical tests of this model for Canada, Harrison and Stewart (1989) is the best example, have been restricted to the post 1945 period. But how robust are these models? It would be expected that in the early period of union consolidation, as collective bargaining became regularized, environmental variability would be reduced. More information would have been available to both parties on wage offers and demands, and there would have been alternatives to strikes. It would be expected, therefore, that as unionization grew strike frequencies would be reduced and they would be limited increasingly to periods of cyclical change when environmental variability was greatest. Similarly, durations would become increasingly countercyclical over time.

For more than a decade, a new generation of Canadian labor historians has been exploring many of these issues. Much of this work has been published in *Labour/Le Travail*, and summaries of this literature are found in Palmer (1992) and Morton (1988). Although there is some agonizing over finding trends or explaining Canadian 'exceptionalism,' this new research situates strikes in their historical context: their location, sector, and political and social background. Kealey (1989, p. 228) wrote: "Each national working class experience must be studied historically and understood in light of contrasting experience, not held up against a reified model, which never existed."

This literature has eschewed quantitative methods and questions. An exception is the work of Kealey himself (1989, Cruikshank and Kealey 1987) who, in a project for the Canadian Historical Atlas has revised Canadian strike data and, using the familiar framework of Shorter and Tilly (1974), has produced statistics on strike dimensions from 1891 to 1961. In his own description of the data until 1930, Kealey emphasized the strike waves of 1899-1903, 1912-1913, and 1917-20, as well as the political, regional and sectoral features of these mobilizations. Kealey also observed, however, the cyclical character of

strikes before 1930, and as for strike durations he found that they declined throughout the period, with the exception of the 1920s. Kealey relies exclusively on descriptive statistics. Multivariate analysis offers a more rigorous method of separating cyclical effects and strike waves and their determinants.

It is precisely here that the historical and economics literature intersect. The economics literature needs to incorporate the historical context in evaluating their models, while the historian needs theory, however reified, to assess the relative importance of mobilization and cyclical strikes.

2. Data

The data for this paper is based on the Strikes and Lockouts File of the Department of Labour in Canada. In response to the growing number of work stoppages in the late 19th and early 20th centuries, the Department began collecting detailed information on strikes: length of dispute, size of work force involved, cause of dispute, how the strike was settled, in whose favor, and so on. The reports did not include some important information, notably on the actual wage settled upon at the end of the dispute. From these accounts we have been able to assemble a data set of 1129 strikes. This does not represent the entire population of Canadian strikes in the period, but the time series of the strikes from the files corresponds closely with the aggregate time series reported by the Department of Labour. As for the geographic and sectoral distribution of strikes, it is probably the case that the reports to the Department may underestimate strikes in Quebec and in the eastern and western regions, but it is unclear how seriously this bias affects the analysis that follows.

The basic descriptive statistics of the sample are reported in the first column of Table 1. For indicator variables, like the location of the strike, the means are just the proportion of the sample with the given attribute. Some features stand out in comparison to the post 1945 period. Harrison and Stewart (1989) reported that for their sample of contact data between 1946 and 1983 the mean and median duration were 35.51 and 20 days; in the early period the corresponding figures were 22.7 and 7 days. Another difference is that after 1945 the median duration of wage-issue strikes was greater than non-wage strikes (20 and 9 days) but that this pattern was reversed in the earlier period. The median wage strike lasted 6 days, while the median duration of a strike about a union issue (e.g. recognition) was 12 days before 1914. In the longer version of the paper we also compare survival rates in the two periods.

3. Method and Results

A hazard model is used to model strike durations, taking into account firm and worker characteristics, policy initiatives, macroeconomic indicators, a strike wave indicator, and other relevant variables. In such a model the focus is on the likelihood that an ongoing strike will end in the next short interval. There are several possible functional

forms for hazard functions and we use the log-logistic form. The coefficients on the independent variables in the hazard model are then the partial derivatives of (the expected value of) log duration with respect to the corresponding independent variable. If duration is negatively related to an independent variable, the settlement rate is positively related to that variable.

Table 1 presents some of our results. (The longer version of the paper includes OLS results with log duration as the dependent variable.) The last column in the Table reports the change in the predicted median duration due to (1) the change of the respective dummy variable from 0 to 1; or (2) the change of the non-dummy variable from its mean value to mean +1. For example, the addition of 100 extra workers to a strike adds about two days to a strike. Our more important findings are:

- Bigger (*i.e.* more strikers) strikes, strikes involving more than one firm, violent strikes and strikes of skilled and machine workers lasted longer.
- The number of strikers (whether lagged or current) – an indicator of a strike wave – was positive and significant when the entire sample was used. In a smaller sample which distinguishes between the type of union involved (Canadian or international), the strike wave variable is insignificant.
- Disputes including women were not different from those involving men only.
- Union led strikes were longer, but there was no difference whether the strike was led by an international or Canadian union.
- Durations of strikes was not significantly different across the country. The labor history literature suggests otherwise. Moreover, labor historians have observed that sectors like textiles, where the intrusion of new managerial techniques were greatest, incurred lengthy disputes. The results do not confirm this. The literature also suggests that the introduction of legislation in 1907, which introduced mandatory conciliation in certain sectors, had the result of lengthening disputes that did occur. The dummy for 1907 onwards is insignificant.
- The real output (*per capita*) variables indicate that the response of durations to output (measured in this case as deviations from trend in levels) depended on the strike issue. Wage and work issue strikes were countercyclical. The relevant coefficients are the sum of the GNP and interaction variables. The absolute value of the t-statistics for both work and wage issue strikes are both greater than two. Union issue strikes, however, were insensitive to the business cycle. Other ways of measuring output changes were tried, including an indicator for good and bad years, but the results did not change greatly.

4. Remarks

The period 1901-14 were years of transition in Canadian industrial relations. Although there is some support for both the strike wave hypothesis and information model, perhaps it is best to see the two approaches as complementary. Union organizing drives were a feature of strike waves that did not necessarily correspond to the business cycle before 1914. (The t-test value for the combination of the GNP variable and the UnionxGNP is 0.77.) This result confirms similar findings using aggregate data on union membership. As unions consolidated they began to serve the important function of acquiring information for their members about the profitability of firms; the duration of union led disputes about wages and working conditions became increasingly longer and countercyclical. In light of

our introductory observations, models of strikes need to be considered in their historical settings.

TABLE 1
DETERMINANTS OF STRIKE DURATION¹

Variable	Mean	Coefficient	T-Ratio	Δ Duration
Constant		30204	2.15	
Strikers	183.81	0.0003	3.76	0.002
Firms involved	0.35	0.5768	5.27	4.786
Females	0.08 ²	-0.1576	-0.73	-1.147
Union involved	0.81	0.7046	4.55	4.534
Skill	0.63	0.2219	1.97	1.675
Violence	0.03	0.7058	3.45	7.691
Legal (1907)		-0.1208	-0.48	-0.958
Number of strikes in year		0.0042	1.96	-0.033
Year		-31.715	-2.15	-0.201
Yearxyear		0.0036	2.16	-0.202
Output effects:				
GNP		-0.0205	-0.64	-0.158
UnionxGNP		-0.0009	-0.05	-0.007
IunionxGNP		-0.0211	0.68	0.166
IworkxGNP		-0.0347	-1.00	-0.265
IswagexGNP		-0.0347	1.23	-0.264
IsmultxGNP		-0.0161	-0.57	-0.124
Location effects:				
West	0.10	-0.3028	-0.60	-2.332
East	0.22	1.1152	0.64	0.939
Central	0.67			
Strike issues:				
Multiple issue	0.25	0.7483	3.74	6.733
Wage issue	0.67	0.4511	2.28	3.740
Union issue	0.20	0.4590	2.14	4.209
Working Conditions	0.20	0.4485	1.68	4.301
Misc. issue	0.20			
Industry effects:				
Building	0.06	-0.7763	-2.53	-4.470
General	0.26	-0.7566	-2.70	-5.123
Apparel	0.11	-0.3053	-0.09	-0.234
Food&Tobacco	0.05	0.0882	0.25	0.713
Machine	0.02	0.7667	1.80	8.849
Metals	0.09	-0.3297	-0.11	-0.253
Mining	0.07	0.1011	0.34	0.819
Shoes	0.03	0.0327	0.09	0.257
Transport	0.13	-0.0871	-2.94	-5.005
Wood products	0.04	-0.5194	-1.15	-3.187
Services	0.04	-0.6508	-1.83	-3.805
Misc. mfg.	0.05	-0.2694	-0.76	-1.853
Other	0.13			

¹ Maximim likelihood estimation. Log of likelihood function = -2689.24, N = 698.

² Indicates that 8.00% of strikers were women.

Populist Kansas: A Human Capital Interpretation

Eugene P. Sigel

University of Massachusetts-Amherst

Introduction

Why did nearly 107,000 men turn out in the fall of 1890 to vote for a previously unknown political party? That is the *Populist Puzzle*. In this session I address that question through a new quantitative sample of farm protest behavior in the Populist era, the 1890 precinct voting returns from 1,263 minor polling stations in eastern and central Kansas. Using a multinomial logit regression model of party selection, I demonstrate that economic influences played an important role in motivating farm voters. Populism took hold amongst farm communities that had been shut out from the indirect benefits of railway construction. It took hold amongst less wealthy, unfenced farming townships in the early stages of economic development. And perhaps most importantly, it took hold amongst farm families that were experiencing a migration-induced devaluation of their human capital. I argue that Kansas Populism was triggered in 1890 because many native-born farmers found, when confronted by a multi-year infestation of agricultural pests and severe drought, that they lacked the farm management skills necessary for profitable exploitation of the subhumid agricultural lands. Populism, I argue, is fruitfully interpreted as a movement rooted in the frustrating regional adjustment difficulties faced by individual farm families, rather than as simply a democratic, collective movement in American politics.

The Populist Trigger

The sudden emergence of Kansas Populism in 1890 remains a puzzle because few analysts have accounted for the timing of the movement's growth. Goodwyn argued that it was because farmers had been mobilized through cooperative ventures prior to 1890; when established merchants blocked the growth of cooperatives a structure was in place to channel farmers' anger into politics. But in Kansas there were few cooperatives – estimated at just over one in a thousand businesses – by 1890. Mayhew suggested that farmers were reacting to the exigencies of commercialized farming, which made them more price sensitive. But Populism began in 1890, well after the sharp drop in field crop terms of trade from 1881-1884 and well before the national depression of 1893-1896. Nor were any of these price movements unique to Kansas. And although I note that an index of building materials prices did move sharply against farmers developing a treeless region, the index shows no sudden shift in Kansas in 1890. Higgs's suggestion that farmers were reacting to a flat trend in real freight rates may have been too pessimistic: the freight rate terms of trade for Kansas that I construct show the sharpest *growth* of any series over 1870-1900.

McGuire and Stock proposed that the Populist impulse was strongest in Kansas and the central West because greater regional uncertainty coincided with a heightened fear of foreclosure. But the Kansas farmer has often faced greater fear of foreclosure and always

greater economic uncertainty than farmers in Ohio, New York, and Pennsylvania; that gets us no closer to understanding why Kansas Populism emerged in 1890 and died out at the turn of the century. The shifting crop mix provides the missing link: agriculture in Kansas was more uncertain in 1889 and 1890, I argue, because farmers across the central wheat belt of the state – the backbone of Populist support – were rotating their fields away from wheat and towards Indian corn. Rarely again would so many Kansas farmers grow so much corn and so little wheat as they did in 1889 and 1890. And by McGuire's own measures corn was substantially more "uncertain" than wheat in both prices and yields.

Yet if Populism emerged in Kansas because a short term shift in the crop mix greatly enhanced the level of regional uncertainty, what explains the change in the crop-mix? For DeCanio and Halcoussis the answer was relative prices: farmers mistakenly shifted towards corn and away from wheat because they failed to anticipate the rising relative wheat prices of the last few years of the 1880s; the result was an 18.7% cut in counterfactual 1889 gross farm income, the largest estimated in any year. But this explanation of the crop-mix places a strong faith in relative price movements. In fact relative yield – rather than *price* – ratios were far more volatile in central Kansas.

And that is why farmers grew a more risky mix of corn and wheat on the eve of the Populist revolt: they had a fear of income losses from yield-reducing insect infestations. Small insects called chinch-bugs appeared in farmer's fields in 1886 and 1887 and forced farmers to choose either corn or wheat, *but not both*. These bugs sucked the life from the ripening winter wheat in the spring and then migrated to corn fields in early summer. The suggested pest-management technique was to eliminate either wheat or corn altogether for several years. The choice of which crop to abandon depended on which region a farmer felt he was part of: the corn belt, or "the golden belt." Native-born migrant farmers trained in the corn-belt commonly chose to eliminate wheat, with unfortunate consequences. In 1889 corn reached historically low prices; and in 1890 Kansas was hit with drought and hot winds that lowered corn output by 80% (but winter wheat output by only 19%) of the previous year's bountiful harvest.

The Populist impulse did arise in a region of greater economic uncertainty. But part of that uncertainty was rooted in individual choices that reflected a farmer's endowment of human capital. When confronted by bugs attacking both crops, farmers made decisions about how to react based on their accumulated endowment of management experience. The shift to corn was an especially risky strategy because most of Kansas is not well suited to Indian corn. Hot winds and the heightened likelihood of persistent drought in July and August make Indian corn more susceptible to environmental stress than winter wheats and sorghums. Ultimately Indian corn would come to play but a minor role in farm rotations across most of Kansas; yet it required the test of actual experience for farmers to make that determination. By contrast, the Russian and German farming communities, which specialized in wheats to a greater degree, failed to greet the Populists enthusiastically in 1890. Their human capital endowments, many formed in the subhumid regions of southern Russia, led them to specialize in winter grains rather than corn as the chinch bugs and drought appeared.

This theory of Populism explains the rise of Populism in 1890 across Kansas as a response to the frustrations of adapting crop selections and pest management routines to the subhumid environment. The solution to one problem – pests – became the source of

another – crop failures. This had a strong economic impact as well. Between 1889 and 1890 I estimate that *per capita* operating farm income fell on average by 45 percent. Farmers raised hell in the fall of 1890, in part, because they had not yet formed the farm management skills that could have prevented such a collapse of operating income.

A Protest Model

In order to test the hypothesis that Populism was triggered by crop failures, I have assembled a new sample of farm protest behavior: the official 1890 voting returns, as reported in local newspapers of record, from 1,263 polling stations in 72 counties of central and eastern Kansas. In total, 1,181 townships and 92% of all Populist voters in Kansas are included. These data provide what is most likely the best cross-sectional measure of the patterns of Populism. Other scholars have relied on Alliance membership lists or county voting returns to test the distinctiveness of Populist farmers. But presence or absence on the three surviving Kansas membership lists does not provide an adequate measure of how an individual voted on election day. And the county voting returns used by Williams – aggregated over an average 800 square mile area – cannot adequately disentangle the issue of "railroad monopoly power" from simply the presence or absence of any railway station.

The precinct voting data form the dependent variable in a multinomial Logit model of party selection, estimated by weighted least squares:

$$(1) \quad \text{Log}_e \left[\frac{P_{v,t}}{P_{\text{pop},t}} \right] = \beta_0 + \beta_1 \text{Distance} + \beta_2 \text{Stations} + \beta_3 \text{Competition} + \beta_4 \text{Town-Aid} + \\ \beta_5 \text{County-Aid} + \beta_6 \text{Wealth} + \beta_7 \text{Density} + \beta_8 \$ \text{Stock} + \beta_9 \text{Farm Income} + \\ \beta_{10} \text{Wheat/Corn} + \beta_{11} \% \Delta \text{Farm Income} + \beta_{12} \% \text{Fenced} + \beta_{13} \% \Delta \text{Land Value} + \\ \beta_{14} \% \text{German} + \beta_{15} \% \text{Other Ethnic} + \beta_{16} \% \text{Urban} + \beta_{17} \% \text{Black} + \beta_{18} \% \text{Abstain} + \varepsilon_{v,t}, \\ [t = 1, 2, \dots, \text{Townships}; v = \text{Democrat, Republican}; \text{pop} = \text{Populist}].$$

The 18 independent variables in the model are divided into three groups: Railroads, Agriculture, and Population. The railroad variables were constructed from contemporary railroad maps and State records. "Distance" is the beeline distance (in miles) measured from the geographic center of each township to the nearest railway station. "Stations" is the number of stations in each township standardized by the square miles of each township. "Competition" is a binary variable equal to "1" if A) a township's shortest beeline distance station was served by two or more independent lines or B) if the township had several stations that were served by two or more independent – but not necessarily intersecting – railway lines. "Town-Aid" and "County-Aid" are binary variables equal to "1," respectively, if a township either voted municipal aid to a railway construction

company in order to obtain a railway station, or if it was located in a county which had voted aid to obtain railway lines. Agricultural variables were derived from the manuscript township abstracts of the annual Kansas agricultural censuses of 1889 and 1891 (the 1890 manuscripts are incomplete). "Stock" is the \$ value of cattle, milch cows, sheep, and swine per farm acre. "Farm Income" is net *per capita* operating farm income for 1890 from marketed grain, livestock products, and garden, horticultural and timber products. "%Δ Farm Income" is the percentage change in "Farm Income" between 1889 and 1890 as a result of drought. "%Δ Land Values" is the change in the self-reported value of a farm acre between 1889 and 1891. "Wheat/Corn" is the ratio of planted acres in wheat to those in corn. "% Fenced" is the percentage of farm acres that were fenced. Population variables were taken from a linguistic atlas and State records. "Wealth" is the *per capita* assessed value of a township's wealth. "Density" is the number of people per square mile. "Urban" and "Black" are the percentages, respectively, of urban and black residents in each township. "German" and "Other Ethnic" are the percentages of people in each township, respectively, residing in German, or French-Canadian, Norwegian, and Swedish, families. "Abstain" is the percentage of each township's voters that abstained in the 1890 election.

The railroad variables were designed to test various theories about railroad resentment. If Populist voters were strangled by monopoly power, the party should have done significantly worse in those townships served by several competing lines. But if Populist voters were motivated by a frustrated hunt for capital gains on land through proximity to infrastructural improvements, Populist candidates should have prospered as they moved farthest away from a rail station. And if Populist voters were motivated simply by a resentment over the fact that *their* township did not have a railway station then Populist candidates should have done better in a township with no railway station. A township which had voted "Town-Aid" to the railroad lines had a binding service contract enforceable by the State's railway commission; it did not need to worry about losing its station as outgoing freight declined in drought years. Voters there should have been less interested in the Populist platform. The "County-Aid" effect is less certain, for all of a county's voters had to pay for a share of the bonds, but only certain townships actually received the rail line. But the Populists did threaten to outlaw this activity if elected, and this should have frightened those counties which had successfully employed it.

If farmers were the basis of Populist support, we should see a steep falling off in Populist support in townships with an urban presence. And if James Turner's "relative isolation" hypothesis is correct, after controlling for occupation – "Urban" is the occupational proxy for non-farmers – we should find the more densely settled townships rejecting the Populists in significantly greater numbers. The stage of development should also have been important to a voter. A township with a lower level of wealth and a greater degree of unfenced farming acreage may have been relatively more dependent on "Eastern capitalists" for farm development. They were certainly the townships which must have felt the pinch of rising building materials prices: with a partially fenced farm often went a sod hut.

Finally if the Populists thrived in Kansas because they were successful at mobilizing voters who might otherwise have stayed away on election day, the Populist candidate should have been substantially more successful in townships with the lowest rate of abstention.

If the sharp swing in farm income caused by the failure of the corn crop motivated voters, then a larger $\% \Delta$ drop in farm income should have been an important aspect of the Populist's success. Higher levels of net income, after controlling for the swing from 1889-1890, might have reduced Populist support. But higher income levels were most often recorded among the eastern corn belt townships. They had avoided the worst of the effects of the 1890 drought relative to their central neighbors, but they also had been most specialized in corn in 1889, when corn prices hit bottom; the effect is therefore ambiguous a priori. A larger $\% \Delta$ in land values meant farmers felt the drought also decreased the future expected earnings from farm land; they too should have been more Populist. And if a long run strategy of wheat specialization made farms more profitable over a series of years, increases in the wheat to corn acreage ratio should have spelled lost voters for the Populist candidate. Finally if over the long term a Teutonic predilection towards specialized wheat farming gave the German and Russian migrants an agricultural edge on the subhumid prairies of Kansas, they too should have been less Populist after controlling for short run income effects. This should not have carried over to the "other ethnics", however – they faced many of the same agricultural handicaps as the native-born, such as previous residence in the American corn belt – and the Swedish vote in particular may have been driven by a youthful rebellion against church regulation of political life.

The Results

The model was estimated over two samples: the "All Townships" sample of 1,181 townships and a wholly rural sample of 853 townships. The use of a rural subsample was necessitated by an inability to completely isolate the rural vote; not all counties separately reported the urban and rural returns within a township. Full results are available in the complete paper. In Table 1, I report the effect of a one standard deviation change in each continuous variable on the probability of voting Populist, calculated as the product of the estimated partial derivative (constructed at the sample mean) and a one standard deviation change in the indicated variable. All of the variables included in Equation (1) were significant at conventional levels – except for the "% Abstain" and the railway "Competition" effect – in at least one of the two party selection equations for each sample.

An additional set of railroad variables which were *not* included in the final specification was a group of binary variables that recorded the independent line(s) which served a particular station; the thought being that certain lines may have earned a greater resentment amongst the settlers they served. As Mrs. Lease stated in 1892, "Kansas suffers from two great robbers, the Santa Fe Railroad and the loan companies..." But it proved impossible to reject the hypothesis that Kansas voters felt an equal resentment against *all* of the State's railway lines.

It also proved impossible to reject the hypothesis that it did not matter *at all* to voters whether their township was served by one or several independent railway lines. This "Competition" effect was in all cases extremely small and not statistically significant in any regression. What was most important was 1) whether a township had a railroad station and 2) how far the average farmer had to travel to reach a railhead. Adding a station to a 36 square

mile township without one lowered the probability of voting Populist by .0543-.0558. And if it was a station secured by "Town-Aid" the probability of voting Populist fell another .0294-.0331; these townships had been guaranteed by the Board of Railroad Commissioners that they would not lose their stations as drought and depressions slashed the value and volume of outgoing freight.

From the urban effect, clearly the Populist voter was most often a rural farmer. But lower density also significantly enhanced the probability of a rural voter turning Populist. And so too did the "stage of development" variables, most important where the settlement density was lower. Moving one standard deviation down from the mean wealth meant a boost of .0174-.0243 in the probability of voting Populist, while a similarly sized drop in farm fencing added .0271-.0189 to the probability of voting Populist. Those farmers still in the process of developing a farm were a fertile market for the Populist product.

But perhaps most important was the counterfactual effect of "No 1890 Drought." Without the drought farm income and farmland values might have risen along trend, resulting in a drop in the probability of voting Populist of .061 in the Rural model, or 11 percent. Surprisingly the effect of higher income – statistically significant from zero in every estimation – was to increase the Populist voting base, but not by enough to offset the losses from the reduced negative swing in farm income. And this most likely *underestimates* the counterfactual effect, for the counterfactual conditions of "No Drought" are not well represented in the data sets. Only 38 of 1,181 townships saw *any* income growth between 1889 and 1890; the average decline between 1889 and 1890 was 45 percent. This swing in income could have been vitiated, if only farmers had raised more wheat and less corn in 1890.

Conclusion

The Populist movement rapidly spread across Kansas in a year of near complete failure in the fields of the chief feed grain – corn. It came on the heels of a multi-year infestation of agricultural pests. And it arrived suddenly at the end of a sustained – but uneven – railway construction boom. The farmer's response to crop failures, insect depredations, and the realization that *his* township might never get a railway station varied. Some farm families simply gave up and returned East; others joined protest movements, such as the Alliance or the Populists; but almost all of "those who stayed behind" in Kansas also turned inward, toward the farm enterprise itself. Populism has become a movement "in search of context" precisely because the story of how the farm families of the central West adapted to the subhumid agricultural conditions has always been subservient to the story of how a band of colorful political entrepreneurs achieved short-term success in the marketing of a new political product.

Lawrence Goodwyn has argued that in Populism one finds a clear example of how "an intimidated people could create the psychological space to dare to aspire grandly." But the regressions results indicate a simpler explanation. The Populist political entrepreneurs presented a new political product which appealed to farmers because it legitimized their collective failure, as farmers, in terms of outside influences: railroads, moneylenders, or the

loss of foreign markets. Certainly these factors *were* important to the success or failure of farmers as a class. But individual farmers also failed because their human capital endowments led them to make the wrong choices. The cultivator's choice – which crops to plant in order to obtain the greatest return on invested resources – forced farmers to draw upon their own individual decision-making capacity: management skills and techniques learned through the trials of past experiences and transferred to them by their parents, neighbors and kin, and the periodicals of the day. It was this human capital that drove a farmer to specialize in either wheat or corn – but not both – as the chinch-bugs appeared prior to 1890. I conclude, just as the Secretary of the Kansas State Board of Agriculture argued in 1889, "much of the failure in farming in the State is justly attributed to a want of knowledge in regard to proper methods, and the kinds of crops to be grown."

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Table 1 The Probability of Voting Populist

Variable/Counterfactual	Estimated Probability	
The Sample Mean ^a	.481558	
The Rural Mean ^b	.542792	
Wholly Urban ^c	.215217	
Wholly German ^d	.130143	
No Germans ^a	.512447	
All = 1,181 Townships Rural = 853 Townships	Δ in Estimated Probability	
Variable/Counterfactual	All	Rural
<i>One Standard Deviation Δ</i>		
-Railway Distance	.02174	.02007
-Railway Station	-.01586	-.01885
-Township Wealth	-.01738	-.02433
-Settlement Density	-.00583	-.01695
-\$ Livestock/Acre	-.01409	-.00208
-Farm Income	.01599	.02335
-Wheat/Corn Ratio	-.00463	.00030
-% Δ Farm Income	-.01792	-.03454
-% Δ Land Values	-.00789	-.00782
-% Fenced Acres	-.02713	-.01891
-% Germanic	-.05670	-.05456
-% Other Ethnic	.00765	.01468
-% Urban	-.08178	-----
-% Black	-.01988	-----
-% Abstain	-.00351	-.00142
<i>A New Railway Station in a 36-Square Mile Township</i>		
-Railway Station Effect	-.02203	-.02756
-Distance Effect	-.03226	-.02827
-Township Bond Effect	-.03314	-.02941
<i>No 1890 Drought</i>		
-Land Value Effect	-.00417	-.00445
-Income Variation Effect	-.05457	-.11025
-Increased Income Effect	.03372	.05358

^a = Calculated at the 1,181 township mean.

^b = Calculated at the 853 rural township mean.

^c = 1,181 townships, % Urban = 100.0.

^d = 1,181 townships, % German = 100.0.

The US Army as a Rational Economic Agent: The Choice of Draft Animals in the 19th Century

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I. Introduction

It has long been believed that waste and fraud was the rule rather than the exception in government agencies. These allegations have been leveled against those who buy government property, use it, and ultimately dispose of it. Few in government escape the charge of wasting taxpayers' money. To put the best light on it, those who, for example, control purchases for government bureaucracies are not spending their own money – thus the incentive to carefully agonize over the cost of each purchase is reduced relative to the scenario where the bureaucrat was buying something with her own money. This is not to say that those who hold government purse-strings may spend freely on any purchase they deem necessary; certain spending controls are in place. However, it has been widely documented in the popular press that these controls are not terribly rigid.

Recently, striking examples of such procurement practices have been uncovered regarding US Army purchases. These include such highly publicized items as the \$800 toilet seat, the \$500 coffee maker and the \$400 monkey-wrench. Just such procurement problems were so pervasive in government purchases that Senator William Proxmire developed the highly discovered "Golden Fleece Award." This award was contrived to expose wasteful spending by the government and seems to be a measure of desperation on the part of the Army's would-be monitors.

Given that government's share of spending in the economy is so high; it is important to know, is much of it wasteful? Even if entities such as the military are not perceived to be "rational agents" one must agree that even an institution such as the Army will endeavor to make the most of the budget it is assigned. Surely, it would be conceded that those who control the procurement of military goods want to be surrounded by as much high quality equipment as their budgets could afford.

This paper looks at a very specific type of army procurement, the Army's purchasing and issuance of draft animals. During the 19th century the Army purchased a great many of the draft animals available in the United States. In 1862, for example, the Union Army alone owned nearly 4% of all horses and mules enumerated in the entire US census of 1860.

The Army issued both draft horses and draft mules to its enlisted men to use to pull wagons and move equipment. Reports of the Quartermaster General show that between 50%-90% of draft animals purchased in the latter part of the 19th century were mules. In light of the fact that mules were always more expensive to buy than horses, the argument of overspending on Army purchases quickly comes to mind. Given that the Army had a clearly cheaper substitute in the horse, the high percentage of mules bought seems to suggest that extravagant purchases by the Army were endemic problems throughout its history. This paper aims to show that in fact draft animal procurement by the Army can be defended

on rational economic grounds. They can be viewed like the decisions made by landlords and mine owners where the owners of work stock often found it in their best interest to issue employees the more expensive, yet more resilient mule (Kauffman 1992, 1993a, 1993b). The Army wanted to preserve its capital from excess depreciation brought on by abuse and neglect from enlisted men who had little incentive to care for the public animals in their charge.

II. Draft Animals in the US Army

From its inception the army made use of a great number of animals. The type and number varied owing to the ever changing needs of the army, but all of the various breeds can be categorized into two basic types: riding animals and draft animals. Nearly all of the riding animals used by the army were horses, whereas the draft animals could be either horses, mules or, in a very few cases, oxen.

For purposes of inventorying animals, the Army used the following categories: cavalry horses, artillery horses, private horses, horses, mules and oxen. Cavalry horses were ridden by enlisted men and some low ranking officers. Private horses were ridden by officers and had the distinction of also being owned by them. All officers had to provide their own mounts, but these horses were boarded and attended to at no cost to the officer. Artillery horses pulled the artillery, but many were also simultaneously ridden by soldiers so as to guide them while pulling the artillery. The simple classification of "horse" was an all-purpose draft horse that was used to pull wagons, move logs, haul material on its back, etc. The classification "mule" was also a generic title in which the animal would also be used to pull wagons, move logs, and haul material on its back, as in the case with the "horse." Finally, a few oxen were used to pull wagons, but their use was very sparse.

The use of horses for cavalry purposes was not surprising for a number of reasons. First, the horse was used by nearly everyone in the United States as the riding animal of choice. And because the gait of the horse and the mule are somewhat different it would make sense to issue to soldiers a type of animal they were already accustomed to riding. In a sense, the army wanted to make use of the human capital built up by the soldiers in their private life. Second, because the soldier riding the animal did not have a direct vested interest in preserving the capital value of the horse it should be somewhat surprising that riding mules were not issued to them. As it was, if the horse died the army would supply him with a fresh remount. But, the assignment of blame if something happened to the horse was not terribly difficult. The same soldier rode and cared for the same horse all the time. So it would be easy to tell if someone was systematically abusing their mounts – they would have a higher injury or death rate as compared to the others in their brigade.

The Army chose to have the officers buy their own mounts to circumvent the principal-agent problem involving the use of public animals. In this case the rider had a much higher incentive to keep the animal healthy as he was the residual claimant. The question automatically arises: why did the army not ask the enlisted men to buy their own cavalry horses? This would seemingly eliminate the agency problem regarding riding animals completely. The differential treatment may have to do with ease in monitoring

enlisted men *versus* officers. It seems clear that in the military setting the enlisted men act as agents and the officers as the monitors, and to some degree principals. So, the officers can watch over the treatment and use of horses by the enlisted men, but who monitors the monitors? While each officer has a superior as his monitor, he may not be consistently surveying the use of the animals used by an officer, whereas the enlisted men would presumably be monitored more closely. This may explain the practice of having officers buy their own horses, such that monitoring monitors was not necessary.

The use of horses to pull artillery pieces could be explained with the same argument applied to using horses for cavalry duty. Many of the animals used to pull artillery pieces were simultaneously ridden. And since most were familiar with riding horses before joining the army, their use in the army would reduce the cost in training soldiers.

Nearly all forts and outposts used horses for cavalry duty, horses for officers to buy and horses to pull artillery. It thus seems that for riding purposes the horse had a comparative advantage over the mule. But, for draft work the story is different. Certain installations tended to use the horse as the primary draft animal, whereas others used the mule. Still, others would use nearly equal numbers of both. Why this peculiar division of draft animals when the use of horses for riding seems so clear?

The precise similarities and differences between horses and mules are important in the decisions of which type of animal to use. As I have previously argued (Kauffman 1993c), both horses and mules are pound for pound equally productive at pulling, the differences lay more in their ability or inability to resist injury and abuse. Because mules are a hybrid cross between a donkey and a horse, they exhibit qualities not possessed by either of the parent subjects. The trait of most interest here is their innate ability to resist injury and abuse that may cause them harm. Horses, however, do not possess such acute innate qualities of self-preservation.

So what was the deciding factor for each of these forts in their choice of which type of draft animal to use? With a scarce amount of resources with which to procure supplies the army would like to minimize its costs whenever it could. I argue the mules that were purchased were issued to forts and outposts with the most severe agency problems. The more severe the agency problem, all else equal, the more likely mules should have been used to abate the effects.

Before going about testing the behavior of the Army, it should be established that there actually were agency problems regarding the use of animals in the US Army. In discussing this problem M. C. Meigs, the Quartermaster General of the US Army reported that, "the consumption of horses has been very great. Mules bear the exposure and hardship of the campaign much better than horses, and they are used to a great extent in the trains" (Report of the Quartermaster General 1863, p.72). He further went on to say that "ignorance and carelessness of raw soldiers waste our horses" (p.72). The abuse of army animals was still a problem by the end of the Civil War. According to the Quartermaster General, "the waste in active service [horses] is still too great; but as the cavalry has improved in discipline and knowledge, it is believed that the horses last longer" (Report of the Quartermaster General, 1865, p.133). Therefore, it is fair to say that there was a problem with issuing horses to its soldiers. The horse's lack of resistance to abuse coupled with the fact that those

using the animals were not the residual claimants resulted in an agency problem that ultimately meant the loss of many US Army horses.

III. Principal-Agent Problems in the US Army

To measure the severity of the agency problem in the use of draft animals by the US Army, we must lay out clearly the relevant parties in the relationship. The agents were the enlisted men as they were the ones actually working the draft animals. The animals were not owned by them and if anything happened to these animals it would be difficult to assign blame. The principals were the owners or the American people; they wanted to preserve the capital value of these animals and keep the cost of military spending as low as possible. Those charged with seeing that the principal's wishes were carried out were the officers; so in this sense they were the monitors. Their role as monitors of the use of public owned animals was clearly laid out in an 1863 directive stating that, "it is the design of the War Department to correct such neglects [ill treatment of public animals], by dismissing from service officers whose inefficiency and inattention result in the deterioration or loss of the public animals under their charge" (General orders No. 236, 1863). Therefore, the role of the officer as the monitor of the use of horses and mules against abuse was clearly stated.

The severity of the agency problem varied across each fort, outpost, division or brigade owing to the relative number of enlisted men to officers. In certain cases the number of enlisted men to officers was equal, hence the agency problem was very low. In others, like the Army of the Potomac, the enlisted men outnumbered the officers 50 to 1. It was in cases such as this where the ratio of enlisted men to officers, or agents to monitors, was high that we would expect the Army to use the more expensive, yet abuse resistant mule. If the army worked as a rational economic agent, as in the cases of southern agriculture and mining, we would expect them to use the less expensive horse in cases where the agency problem was less severe and the mule in cases where the agency problem was more severe.

The test of this hypothesis uses data collected from the "Report of the Means of Transportation, Number of Officers, Men, Animals, &c." This monthly report, which was in existence from 1840-1870, was to be sent to the Quartermaster General in Washington by the local Quartermaster of each army corps or outpost. Unfortunately, only 486 of the actual reports survive and only for the years 1863-1866; the rest were destroyed. Yet, the remaining reports should represent an unbiased sample of the relative proportions of draft horses and draft mules within the US Army.

Because this report enumerated the numbers of officers and enlisted men as well as the number of animals at each site, the direct question of how and why particular types of work stock were issued can be addressed. The numbers of enlisted men and officers varied considerably with the mean number of enlisted men at 7026 and officers at 326. Thus the average ratio of enlisted men to officers was nearly 22 to 1. The absolute number of draft mules was quite high at 1831, while the average number of horses was 410. This

resulted in a mule to horse ratio of over 4 to 1. So while the relative level of enlisted men to officers was seemingly high, so too was the average ratio of mules to horses.

It will prove useful to separate the reports by the number of enlisted men at each installation as a way of measuring size. In doing this, it is clear that the smallest installations, those with under 100 enlisted men, had the highest ratio of officers to enlisted men. It is therefore not surprising that this same category had the highest ratio of horses to mules. In fact it is the only one in which this ratio was greater than one. The next two smallest categories also have the next highest ratios of officers to enlisted men and consequently had the next two highest horse-to-mule ratios.

As the number of enlisted men per installation becomes larger, over 1500, the ratio of officers to enlisted men does not vary much. Presumably, however, as the sheer number of enlisted men grows the difficulty in monitoring them would increase. This is evident from the fact that above the 3000 enlisted men per installation level, the ratio of horses to mules never gets above 27 horses for each 100 mules.

To test whether mules were issued in situations of more severe agency problems and horses in less severe cases, the severity of the agency problem in each case must be measured. To do this the number of enlisted men as a percentage of all soldiers was calculated for all 486 reports. The higher the number of enlisted men as a percentage of both enlisted men and officers, the greater the agency problem. In other words, the higher the percentage of enlisted men at an installation, necessarily meant there was a smaller percentage of officers to monitor them. Therefore, we would expect that the higher the percentage of enlisted men, the higher the percentage of mules should be used for draft work. The following Ordinary Least Squares (OLS) regression was estimated with the percentage of mules as a dependent variable and percentage of enlisted men as a regressor:

$$\text{PercentMule} = -0.045 + 0.834 \text{ Percent Enlisted} \quad (1)$$

with t-statistics of 3.54 for the constant and 53.39 for percent enlisted, $\bar{R}^2 = 0.58$.

The results of this regression strongly suggest the economic rationale for mule use was to abate agency problems. In situations where the percentage of enlisted men was high, so too was the percentage of mules used. For example an increase in the percentage of enlisted men at a particular installation of 10 percentage points resulted in an 8.34 percentage point increase in mule use. This single regressor is highly significant, both economically and statistically, and this simple model explains nearly 60% of the variation in mule use by the Army.

IV. Conclusion

It generally has been accepted that army procurement was, and still is, somehow irrational. The exorbitant prices paid for some equipment cause many to believe that this institution does not base its purchasing decisions on solid economic judgements.

Were the procurement and distribution practices of the US Army during this

period irrational? At least with respect to draft animals, it appears not. Data from the Report of the Means of Transportation suggest that at installations where there were relatively few officers watching over the enlisted men using these animals, the draft animal of choice was the more expensive yet more abuse-resistant mule. This was done to diminish the amount of capital depreciation on the overall draft animal stock of the US Army. Whereas at smaller installations where the proportion of officers to enlisted men was higher, hence easier monitoring, the army could issue the less expensive horse.

This one example of army procurement could shed additional light on other such seemingly irrational procurements by the Army. As this exercise suggests the Army could, in certain instances, be spending more on certain purchases which are more abuse-resistant – a desirable characteristic considering the endemic agency problems found in the army – than would individual consumers where such agency problems are not as pronounced. If Senator Proxmire had been doling out "Golden Fleece Awards" in the 19th century, one given for purchasing mules rather than just horses at army installations would have been naïve.

Society members are advised of additional economic history sessions of interest. Regrettably, times of some sessions conflict with Cliometric Society Sessions.

Economic Impact of the Great Irish Famine

Monday, January 3, 8:00 a.m.

Presiding: Barbara Solow, Harvard University

Timothy Guinnane (Yale): *The Famine and Population: The Long View*

Kevin O'Rourke (University College, Dublin): *Economic Impact of the Famine in the Short- and Long-Run*

Discussants: John R. Harris, Boston University

Ruth-Ann M. Harris, Northeastern University

T.N. Srinivasan, Yale University

A Long View of Women's Work and Status

Monday, January 3, 10:15 a.m.

Presiding: Ethel B. Jones, Auburn University

Linda Barrington and Cecilia A. Conrad (Barnard): *The Feminization of Poverty from 1939 to 1959*

Theresa J. Devine (Pennsylvania State): *Changes in the Returns to Skill and the Recent Rise in Female Self-Employment*

Elizabeth Field-Hendry (Queens): *Wage Discrimination in 19th-Century Manufacturing: Philadelphia, 1860-1880*

Discussants: Mary King, Portland State University

Donald Williams, Kent State University

Topics in Economic History

Tuesday, January 4, 10:15 a.m.

Presiding: Peter Temin, Massachusetts Institute of Technology

Brigitte Bechtold (Central Michigan): *Economic Causes of Infanticide in 19th-Century France*

Douglas Galbi (Cambridge): *Child Labor and the Division of Labor in the Early English Factories*

George Bittlingmayer (Chicago): *The 1920's Boom and the Great Crash*

Curtis J. Simon (Clemson): *Marginal Wages During the Great Depression: Evidence from Job-Wanted Ads*

Discussants: Judith Chevalier, Harvard University

Rebecca Menes, Harvard University

Wallace Mullin, Michigan State University

Voluntary Provision of Public Goods: Historical Studies

Wednesday, January 5, 2:30 p.m., Marriott Hotel, Simmons

Presiding: Randall Kroszner, University of Chicago

Lynne Kiesling (William and Mary): *Moral Sentiments and Conspicuous Donation: Social Norms and Strategic Behavior in Charity during the Lancashire Cotton Famine*

David Beito (Nevada-Las Vegas): *Social Insurance by Mutual Aid: The Fraternal Societies in America, 1900-1940*

Dan Klein and Chi Yin (UC-Irvine): *The Private Provision of Frontier Infrastructure: The Toll Roads of California, 1850-1890*

Discussants: Donald McCloskey, University of Iowa

Peter Boettke, New York University