The Newsletter of

The Cliometric Society

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Report on the 39th Annual Cliometrics Conference

by John Lyons, Miami University

(*Oxford, Ohio*) This year's Clio Conference was held May 14th-16th at the Marcum Conference Center at Miami University. Following the wishes of the local arrangements committee, the weather was glorious, allowing for a pleasant walk to 'uptown' Oxford for dinner on Friday as well as a sun-drenched luncheon in the courtyard of the Marcum Center and a sylvan stroll to the evening's festivities on Saturday. The conference was supported financially by the National Science Foundation, with supplements from the Richard T. Farmer School of Business and the Departments of Economics and History at Miami. The 50 participants were welcomed to Oxford by O. Homer Erekson, Associate Dean of the Business School, an economist trained at the University of North Carolina, who not only had studied some economic history with the late Bob Gallman, but also had enjoyed a series of mint-julep afternoons at Bob's home on Kentucky Derby Saturdays. Sam Williamson (Miami) then reminded everyone of the ground-rules for discussion and we set to work.

The opening paper, on share liquidity in New England in the later 19th century, by Peter Rousseau (Vanderbilt), stresses the importance of improvements in stock market liquidity and the development of more specialized banking for industrial growth in the 'emerging market' of the region. Price Fishback (Arizona) asked what is meant by an 'emerging market'. Rousseau responded that the developing financial markets in 19th-century New England were not only important as sources of capital for early industrialization, but also that the Boston stock market was the first market for industrial equities. Tom Weiss (Kansas) thought that one Richard Sylla had shown that the Boston market was well-integrated with those of New York and Philadelphia; Sylla (NYU) added that he had found that the same securities (*i.e.*, bonds) were traded in the different markets, with similar price variations, but that industrial shares were not widely traded. Attention then turned to Rousseau's measure of liquidity (the average par value of traded industrial shares) which he found to have declined steadily over the years of the study. Josh Rosenbloom (Kansas) wondered whether this was simply reflective of a declining income threshold for those buying shares; Rousseau responded that lower par values would allow shares to be more widely held, and that this is what had occurred. Replying to a question from Mark Toma (Kentucky), he attributed the par-value decline to new issues at lower par, stock splits, and floating new companies with less expensive shares. Perhaps, suggested Jeremy Atack (Vanderbilt), since shares could be issued with a call option (*i.e.*, they were not initially fully paid), the declining par value was simply associated with a decline in call options, so that market liquidity is mismeasured. However, according to Rousseau, shares were fully paid by the 1860s. Ann Carlos (Colorado) and Sonali Garg (Ohio State)

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What's Inside

Executive Director's Notes

Trustees Meeting Agenda

I have told the Trustees that I want to step down as *Newsletter* Editor and Executive Director by the end of the year. During the past few months Dick Sylla, Chair of the Board of Trustees, has been collecting feedback from Society members about the future of The Cliometric Society. [See the February issue for his letter to members.] The Trustees will meet during the EHA meetings in October to discuss this feedback and make a decision. Any member who wishes to provide suggestions should contact Dick or any of the other Trustees by the end of September.

ASSA

Kyle Kauffman is serving as Cliometric Society Sessions Coordinator this year. Session information and the list of papers are on the Clio web site (**www.eh.net/Clio**). Summaries of the papers will be published on the web site in September, and the abstracts and schedule of sessions will be published in the October *Newsletter*.

World Congress Update

The September 15 deadline is fast approaching for proposals for the Fourth World Congress of Cliometrics. [See page 35.] Planning for this Congress has been under way since the Munich Congress of 1997, and I am pleased that again we have several co-sponsoring organizations. I am confident that it will be equal in quality to our previous Congresses, an event you won't want to miss. Although at least one author for each paper must be a member of a sponsoring organization, I want to point out that attendance at the Congress is open to anyone. We hope to get some financial support from the US National Science Foundation and from organizations in Montreal; funds will be used to subsidize graduate student travel and to cover some administrative costs. As always, Congress papers will be circulated in advance and sessions will be devoted to discussion.

George Grantham has been working hard on local arrangements. We have reserved blocks of rooms at the Holiday Inn Montreal-Midtown and at a McGill University dormitory. Montreal is a very interesting city – we plan to have a banquet in the Old City and possibly a walking tour and a cruise on the St. Lawrence. Jazz fans should be pleased that the Congress will take place during the annual international jazz festival, located around the corner from our hotel.

Call for Syllabi

The Economic History Syllabus Collection was initiated by Robert Whaples for the Economic History Association; it has since been supported by The Cliometric Society and is now accessible from the EH.Net home page. EH.Net officers are pleased to continue support for this project, and I encourage Society members to respond to Robert's "Call for Syllabi" on page 30.

On a Personal Note

I was glad that Jerry Flueckiger, my colleague at Miami and long-term Cliometric Society member, was able to attend the session on technology at the Clio Conference here in May. He enjoyed both the session and the opportunity to see old friends. Jerry was a graduate student at Purdue while I was there, and he presented papers at two Clio Conferences on how to measure technological change. Jerry passed away June 20 after a long illness.

While largely theoretical, his work was historically informed, and appeared in *EEH*, *Economic Inquiry* and *Mathematical Social Sciences*. His last publication was a book, *Control, Information, and Technological Change* (Kluwer, 1995).

THE CLIOMETRIC SOCIETY
Miami University
Oxford, Ohio 45056 USA
(513) 529-2850
Fax: (513) 529-3308
E-mail: CSociety@eh.net

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Samuel H. Williamson, Editor
Debra Morner, Managing Editor
Louis Cain, Associate Editor
John Lyons, Associate Editor

AN INTERVIEW WITH PAUL DAVID

Editors' Note: Paul A. David is Professor of Economics and (by courtesy) of History at Stanford University. He is Senior Research Fellow at All Souls College (since 1994), and titular Professor of Economics and Economic History in the University of Oxford (since 1997). David was William Robertson Coe Professor of American Economic History at Stanford (1977-94) and also has been Visiting Professor at Harvard (1972-73), Pitt Professor of American History and Institutions in the University of Cambridge (1977-78), Visiting Professor of Economics in the Hebrew University of Jerusalem (1978), Extraordinary Research Professor of the Economics of Science and Technology at Rijksuniversiteit Maastricht (1994-97), and Visiting Professor in the Economics of Innovation at the University of Paris Dauphine (1995-97, 1999-). David has served on the editorial boards of ***The Journal of Economic History, Explorations in Economic History*** *and a variety of other journals and monograph series in economics and economic history and is founding editor of* ***Economics of Innovation and New Technology.*** *He served as Vice President (1978-79) and as President (1988-89) of the Economic History Association and is currently a member of the Council of the Royal Economic Society.*

Our interview was conducted over two days in December 1996 by Susan B. Carter (University of California-Riverside). The text was transcribed and edited by Debra Morner. With editorial assistance from Louis Cain and John Lyons, the transcript has since been revised and updated by Paul David.

Thank you for doing this interview for the *Newsletter*. As you know, this is one of a series of interviews attempting to preserve the history of cliometrics. We might begin with a bit of biography. What brought you into the field?

Lack of preparation for something else would be the most historically accurate answer. Let me explain. I went off to college in 1952 intending to do chemistry, a subject I enjoyed greatly in high school. My Harvard freshman advisor joined me in this fantasy. After scanning my folder, he told me not to take the introductory course, but to start with stoichiometry – chemical arithmetic based on the determination of atomic weights. It was taught by a very popular chemistry professor, name of Nash. This would have been great advice for someone else. Nash's lectures and demonstrations were memorably brilliant; the labs were fun, albeit very time-consuming. But, virtually from day one I had that sensation of being in well over my head. Soon I was drowning in 'moles,' balance equations and 'rates of reaction' problems. With lots of help from my classmates I managed to emerge with a shocking C+. I also emerged convinced that I had quite the wrong idea about chemistry, that I needed to take some math courses, and that I needed to find a course to replace introductory organic chemistry – which had been pencilled in on my Spring schedule. Econ 10, Introductory Economics, happened to be offered at a convenient hour. So, you could say that I came to economics more as a refugee than a pilgrim.

What was it about economics that intrigued you?

I should say that I was not wholly innocent of economics. From a young age I was intrigued by history, and by the time I reached high school I had been exposed to a good many economic and social issues in US and European history. But that wasn't economic analysis, which came as something of a surprise. Happily, unlike stoichiometry, this was a surprise that I could manage, and so I stayed with it long enough to become thoroughly seduced.

The very idea of a unified theoretical framework for studying economic activity was a powerful one. Remember, at this time Samuelson (and Hicks) were already having a big impact on the way undergraduate economics was taught at places like Harvard – even though *The Foundations of Economic Analysis* [1948] and *Value and Capital* [1939] were not assigned until you got to the most advanced theory course.

Was there anything that was especially memorable about your introduction to theory?

I recall John Chipman's lectures as having had a big and sustained intellectual impact on me. His classroom style was the opposite of flamboyant, but the structure of the course and the classroom presentations were lucid and elegant. He took us from the formal theory of the household and the firm through to Walrasian general equilibrium analysis and its applications to real trade theory. Then he developed an interpretation of the Keynesian system as a special case of general equilibrium where some markets were characterized by price inflexibility – sticky prices, wage rate rigidities, and bond market expectations which created the liquidity trap phenomenon. This was very different from the mechanical presentation of Keynesian economics we received from Alvin Hansen's macro course. It was a revelation. I found the coherence of the whole thing exciting and wonderfully satisfying. That feeling remained even when, much later, I came to understand the serious problems that one glossed over in treating money as just another commodity whose price was determined along with those of all the other goods.

When Moe Abramovitz was interviewed for the *Newsletter*, he talked about his first economics course. The way he describes it, he stumbled into it and then was just swept away by the brilliance, the coherent vision of the changes and organization of society. Was it like that for you?

Well, yes, in the analytical sense I have just described. But the idea of the economy's relationship to the organization of society wasn't a new one for me. I'd already been exposed to it, although not to its representation in a formal system that could be analyzed rigorously. You see, I had some precocious acquaintance with economic history as a field of study, more or less by accident of birth. My father, Henry David, began his academic career as a labor historian. He published *The History of the Haymarket Affair* in 1936, the year after I was born. While I was in high school, he was editing volumes in the Rinehart series on American Economic History. So, Nettles, Taylor, Kirkland, Mitchell, and Gates were 'household names' to me, long before I actually read their books – also Larry Harper who, alas, wasn't able to complete the promised volume on the colonial period.

So, for you, it was the formal theory that was the new, attractive thing about economics?

Absolutely. I suppose that although it wasn't a conscious consideration for me at the time, it's not entirely coincidental that economic theory was the one aspect of the subject that seemed farthest removed from my father's areas of expertise and active interest. There was, however, another aspect of my interest in economic theory that developed very early – the intellectual history of the discipline. Why had economic theory developed in the way it did? Was it just a matter of logical progress towards 'getting it right'? Or were changing external influences, including economic conditions, what had led economic thinkers to change their minds? These questions were raised by reading Heilbroner's *The Worldly Philosophers* [1953] in my introductory Econ course, but I felt that Heilbroner hadn't really answered them – that he had not even posed them.

Can I suggest that's an unusual viewpoint for a beginning student?

Perhaps, although the idea of studying the history of economic analysis was something that crystallized in my thinking only much later on – sometime towards the end of my junior year. By then I had had an opportunity to read some of Schumpeter's monumental tome on the subject. Robert Kuenne, then the resident economics tutor at Adams House, was reviewing and indexing the manuscript at the request of Schumpeter's widow, and he let me see it. What intrigued me most was Schumpeter's notion of 'the vision' – the dominating conceptualization of the nature of the economy. He presented this as having shaped the way economists perceived the world around them and the directions in which they sought to extend economic analysis. Schumpeter contrasted visions of the economy as 'hitchless' (Smith, Mill, Bastiat) or 'hitchbound' (Malthus, Ricardo, Keynes). But it still wasn't clear where these visions came from, or why the dominant visions changed from one generation to the next. This seemed to me a good problem to pursue.

Did you pursue it?

Well, I tried. In my senior year I took Overton Hume Taylor's course in the history of economic thought, as it was the only offering in that subject at Harvard. Unfortunately, his approach to doing intellectual history was not particularly oriented to the questions that were intriguing me; but I learned something of the literature and the craft, and that didn't discourage me from writing my honors thesis in the area. The topic I picked even now seems a peculiarly esoteric choice: neoclassical international trade theory, the Edgeworth-Loria-Bastable controversy, and the emerging critique of the doctrine of Free Trade in Britain, *c.* 1880-1906. My faculty advisor was Jim Duesenberry, and he seemed to view this proposal with somewhat perplexed bemusement. But he let me go ahead. More than that, he was of real help in straightening out some analytical tangles that I got into. Despite, or perhaps because of, the esoteric nature of its subject, my honors thesis won high marks – and I wound up knowing more than anyone I encountered at Harvard about a topic that only I seemed to find interesting, rather than a curiosity.

Was that why you didn't go on with the history of economic thought?

That would have been a good, rational reason – certainly a sufficient reason. Yet, I don't recollect having made a deliberate decision to abandon the field. What I can recall is feeling, especially while struggling to finish the wretched thesis, that this form of intellectual history really was too difficult; that it called for too many varied kinds of knowledge, none of which I really had a firm grasp of – the previous theory, the individual economists' biographies and their mental states, the times through which they were living – much less the literary skill to weave all of that into a story! I think that's why I allowed myself be deflected from the history of thought.

In what way were you 'deflected'?

I came to focus more and more on the economic changes taking place in late 19th-century Britain. The argument of my thesis was that those changes had pushed some English economists into questioning the policy of Free Trade, and, more generally, underlay the increased the appeal in Britain of the ideas associated with the German Historical School. Of course, some of the 'deflecting force' was external. In the fall of my senior year I talked my way into Alexander Gershenkron's year-long graduate course in economic history. My pitch was that I needed to study economic history for my honors thesis, and Gerschenkron's was the only economic history offered at Harvard. That proved to be a potent experience for me. Gerschenkron was a man of great erudition, as probably everyone knows. His lectures ranged from the description of a Carolingian manor to subtleties of the index number problem, with lots of references to what Max Weber had said in between. He was vigorous then, and enthusiastic about infusing economic history with economic theory and statistics, and, to boot, he was personally very engaging with new students. We had to write a 20-page paper each semester and make an appointment to have him approve the topic. At my first such meeting with him, when I sketched what I thought my honors thesis was going to argue, he handed me a copy of Walt Rostow's *The British Economy of the Nineteenth Century* [1949] and said: 'Well...why not tell me what you think about this?' So, I wrote my paper on Rostow's use of economic models to study the past, particularly the Great Depression of 1873-96. Although its explanation of the Great Depression did not leave me convinced (I had found several critical reviews), I liked the methodologically pioneering side of that book and in my paper I tried to suggest ways of taking it further. From that point onwards, I was firmly 'hooked' on what I took to be a new and more useful approach to writing economic history.

Because of its theoretical perspective?

Sure. That was a major part of its appeal for me. The idea of looking at the 19th-century British economy through the lens of modern economic theory was the dual of the task for my thesis – using a better understanding of the changes taking place in the economy in order to understand the evolution of contemporary economic thought. Putting the two together, I thought modern theory could be used to help understand economic thought, but in a historically contextual way. This seemed to me to be better than the conventional 'internalist' approach of the scholarly literature, which was to examine each successive theory and critique it from the standpoint of how closely it had approached 'the truth' – as that was manifested in modern theory.

Pretty complicated. Let me try to summarize: You were more intrigued by the historical forces that led to theoretical structures than with the elegance of a particular theoretical structure that happened to be in place?

That's a good characterization, and short! I wasn't into theory for theory's sake. My initiation into advanced economic analysis occurred before 'the neoclassical system' – a self-contained axiomatized intellectual structure – was *the* form in which theoretical analysis was presented to students. When I came to try applying theory to understand some particular problem, I started from the premise that any bit of textbook analysis, or 'off the shelf' theory taken from a journal article would – more likely than not – have some implicit empirical suppositions buried in it; and those would constitute a limitation on its range of useful application, possibly a fatal limitation. One might have to shop around for something more suitable, or develop something better suited to the historical context. I still think that's so. I never felt moved by the missionary zeal that later came to characterize the proponents of studying history as a way of extending the disciplinary domain of economics, let alone the domain of neoclassical economics.

You don't consider yourself to be a neoclassical economist?

No, certainly not today. And not ever, if by that you mean believing that everything is everywhere convex, that tastes are exogenous, that agents always are maximizing well-defined objective functions, and that it's always best to start by assuming what we observe has been generated by a world of perfectly competitive markets. But, who does? To me there is an important difference between eclectically selecting some items that are in the neoclassical toolkit and buying the whole store.

So then you went off to Cambridge, England.

Okay, let's go back to 1956: that was when, after graduating from Harvard, I was very fortunate to be accepted as a Fulbright Scholar at Pembroke College, Cambridge. The people I met then, the friendships I made (indeed, a first marriage), formed the web of associations that would draw me back repeatedly to visit and live in Cambridge, and then in Oxford and elsewhere in Britain, throughout the decades that followed. They created a critical part on the path that eventually led me back to a Senior Research Fellowship at All Souls.

We'll come back to path dependence in a bit, but first, I wonder whether your primary academic interest at Cambridge was economics or economic history?

Cambridge in 1956-58 was a lively and active place for a would-be economist. D. H. Robertson was still giving wryly humorous lectures on price theory, and I went religiously to Maurice Dobb's excellent lectures on welfare analysis. But it was Kahn, Kaldor and Robinson, the once-young turks, who had come to dominate the scene. When I arrived, everybody was trying to figure out what Joan Robinson was saying in her recently published book, *The Accumulation of Capital* [1956]. Joan herself was not much help. She was formidable: in one seminar after another she simply stopped younger colleagues and graduate students who were brave enough to attempt expositions restating and interpreting her argument. When they would begin their talk by putting up some notation on the board, she would cut them off, saying something like, 'Look. I've written it all out in my notation, so what's the point of re-writing it in some other way?' All that was amazing and entertaining. And I couldn't help but pay attention to it, because, at the end of the academic year, I would have to 'sit' for the examination in five (of the eight) papers that then formed Part Two of the Economics Tripos. In addition to going to lectures and seminars, my economics tutor in Pembroke College was setting me weekly essays to write in preparation for the micro- and macroeconomics examinations.

Although theory was much on my mind during that year, taking my two years at Cambridge in all, it was economic history that occupied the major part of my attention. One of the fields I could choose to be examined in on the Economics Tripos was a 'special subject', and that year – fortunately for me – R. C. O. Matthews was offering special subject lectures on 'British Trade Cycle History, 1825 to 1850.' That was my chance to do serious economic history 'for credit' in the context of the Economics Diploma program in which I had enrolled. But, in addition, David Joslin, a history tutor in my college who had taken an interest in me, arranged for me to have some supervision in modern British economic history with Peter Mathias, then teaching at Queen's College. I think it was through Joslin and Mathias that I was invited to attend Postan's seminars in economic history, after I got through the Tripos and was accepted to do a second year as a research student. My next piece of good fortune came when Robin Matthews agreed to supervise my research, which I decided to do on British economic fluctuations during the 'disturbed' period from 1857 to 1869. It was an apprenticeship project, in which I tried to follow closely the model of Matthews's masterly book on the 1830s, *A Study in Trade Cycle History*. To have worked with all those outstanding people, and through them to have been introduced to Ashton, Habakkuk, Tawney and still others, scholars who for me previously had existed only as authors on Gerschenkron's (overly ample) course bibliography, certainly was the best, and most enduringly valuable, part of my British training to become an economic historian.

Let's return to Cambridge, Massachusetts.

Well, that's what I did, as an economics graduate student, back at Harvard in the fall of 1958.

Was that a difficult decision?

No, it was an easy decision. By that time I was married, and save for the willingness of my bride to continue as a infant-school teacher in Boston, I was without visible means of support – except a fellowship offer from Harvard. So, I returned to the normal 'boot camp' greeting that awaits incoming graduate students: 'Never mind your undergraduate major and your two years at Cambridge; you really don't know anything; we are starting over from scratch to teach you economics.' By then, however, I did have a pretty good idea of what micro- and macroeconomics were about. Yet, what I had not encountered during my time at the other Cambridge, and what is both challenging and exciting for me, was econometrics, which was just beginning to be taught at Harvard. I took a year of quantitative methods from Houthhakker (who at the time was visiting from Stanford). Apart from what I learned, there were two interesting sequels that derived from my taking that course. The teaching assistant was a second year graduate student named Albert Fishlow, who had done well the year before in the econometrics course offered by another visitor, Johnston; that was how Al and I met and became friends, but only after he had marked my final exam and mentioned that I had done surprisingly well. The other thing was that two years later, Houthhakker was the only person who interviewed me for the job I was offered at Stanford – although by then he actually had switched to Harvard, and was asked to look me over as a favor to his former colleagues. I suppose one could say, with this tale in mind, that at least some of the roots of the Stanford-Berkeley Economic History Colloquium (which Fishlow and I organized after we got settled in California) trace right back to that econometrics course at Harvard.

You had already done Gerschenkron's course, and you had been studying economic history in Cambridge. Could you do further work in economic history back at Harvard?

Of course, although not in terms of course work. During the first part of the year I was given an assignment as a condition of my fellowship: I was to be a 'research assistant' to Gerschenkron. He had had a heart attack the preceding spring, and Seymour Harris, the department chair, thought that Gerschenkron should have somebody to help him fetch stuff from the library, carry piles of books and so forth. As I was someone whom Gerschenkron already knew, and as I had hoped to work with him, it seemed logical to assign this role to me.

So you would meet with Gerschenkron?

Well, I attended his lectures again, which was good, because he was on to some new material, and I thought it would be a way to keep in touch with him on a regular basis. But he hated the idea of having a 'helper' assigned to him. I think it suggested an 'incapacity', and he really had no use for the services of a real research assistant. He would say: 'You know, somewhere in Vico's work on vortices, there is a statement like this...Can you find that?' So, off I would go to Widener Library. He would have given me the citation in Italian, and my first task was to find an English translation. Then I would plow through the 435 pages of Vico trying to find something that resembled 'the passage.' Of course, as was not infrequently the case, it simply would not be there. In the 'quotation from Vico' episode, what Gerschenkron had remembered, almost perfectly, was a half-sentence from something like page 7 and the rest from something like page 430, and he had run them together. I thought maybe he read the beginnings and ends of books first, but, when I tried out that theory on later search occasions, it didn't work.

So, like a good retriever holding a bird in my mouth, I'd return after two days and plop it down on the desk of his office in Littauer. He would look up and say, 'Oh, very good! Very good! Yes! Yes! And the original Italian is, ... where? Oh. So, when you are going back to Widener to get that, so I can check this translation ... you know, it doesn't look quite right ... would you see, somewhere in the *Collected Works* of Freud, if you can find the essay on Michelangelo, or was it Davinci, where he remarks . . .' It went on like that. Paper chases.

Did you *learn* anything from this?

I learned nothing from the experience, although it did broaden my education. I took it as a job that came with the fellowship; challenging, but in a way that was rather a disappointment.

But you're not angry? You're not resentful?

I think it takes a lot to make me resentful. At the time it just seemed bizarre. I felt that that this wasn't serious activity; that it was a poor use of my time, given the amount of reading in economics that I had to do for my courses. I felt relieved when it came to an end. When, towards the end of that first semester, Gerschenkron said he felt he didn't need a research assistant, I agreed instantly and reported back to Seymour Harris. And for the Spring Semester I was assigned a really good job – being TA for an undergraduate course taught by Alfred Conrad. This turned out to be the first course in American economic history to be offered by the Harvard economics department.

At that time, Alf Conrad had just finished a paper with John Meyer about which he was quite excited: the economics of slavery. So, I was witnessing the beginnings of that strand of the New Economic History movement in the US, although at the time there were no portents of the future that I was conscious of. What I was delighted to learn was that Alf Conrad was a fine economist, and a wonderfully considerate person to work with. He was enthusiastic about what he was doing in applying economic methods to the study of history, and he let me give some of the lectures on topics that interested me. Bray Hammond's interpretation of Jackson, Biddle and the struggle over the Second Bank of the US was one that I remember spending a lot of time preparing.

Conrad gave some lectures based on a new paper he was writing, dealing with structural changes in the American economy and their impact upon economic growth and stability. This was very interesting to me, as it related to model-building work that Duesenberry had recently done, and so had a connection to the research I had done in Cambridge on trade cycle history, under Matthews's supervision. I mention this because nobody looks at that paper of Conrad's today, although it's accessible in his book with Meyer. I found it stimulating for what it said about the way that the movement of the frontier, and transport innovations, were affecting investment demand; and more generally about the disequilibrium dynamics of the growth process in the 19th century. Anyway, it was an encouraging impetus for me to continue along my previous line of research on growth and cycles, by shifting into the US context.

What was the impetus for Conrad and Meyer? Why were they studying the economic history of slavery – was it fashionable?

It certainly wasn't fashionable in economics at the time. I think the paper on slavery came out of conversations between Meyer and Conrad on the idea of applying capital theory to historical questions, but I really can't say that with certainty. It's also possible that John Meyer had started on the subject for a term paper in Gerschenkron's course. He subsequently did publish another economic history article that began life as a term paper for Gerschenkron, and those papers came in pairs. That was Meyer's paper applying input-output analysis to assess the effects on the British economy of the retarded growth of its staple exports in the 1880-1913 period. It's easy to imagine that the idea of applying capital theory to understanding slavery was prompted by the contemporary publication of Kenneth Stampp's *A Peculiar Institution* [1956], which attracted a good bit of attention at the time – but, again, that's just another surmise...

So, it was not an entirely imperialist impulse on the part of economists. It was a conversation with historians.

Well, 'imperialist' is what non-economists call the enthusiasm of economists for their way of thinking. But, really, I cannot recall either talk of disciplinary expansion or of efforts to actually engage historians in discussing economic history. The 'colonizing impulse' came later and from a different quarter. At the beginning, it was more a matter of economists having conversations about history among themselves. I'm pretty certain that neither Conrad nor Meyer nor anyone else in the Harvard economics department at that time ever had any 'trans-disciplinary conversations' with members of the history department, people such as Oscar Handlin and Fredrick Merk (a student of Frederick Jackson Turner), although they were teaching and writing on subjects that had a good bit of economic, as well as social and political, history content.

Nor did anyone in the Harvard economics faculty seem aware that Bernard Bailyn (also in the history faculty) had recently published a pioneering piece of computer-aided quantitative economic history – on Massachusetts shipping and shipowners during the late 17th and 18th centuries. I was, but only because I sometimes had lunch with Bud Bailyn at Adams House. His work was another 'straw in the wind' for quantitative economic history, but a straw that wasn't adequately noticed then, or since. Perhaps because Bailyn soon left colonial economic history to score a big hit with *The Ideological Origins of the American Revolution* [1967], his

Massachusetts shipping book has been forgotten by the annalists of 'the cliometric revolution.' But I've always thought that it was both substantively and methodologically more interesting, really far more interesting, than that much fussed-over Purdue paper on those 'first 1,942 British steamships' – or however many there were.

Can you characterize the conversation among practitioners in social science disciplines at that time? Were they in closer conversation, reading one another's work more carefully and more systematically than we see today?

Although the people having those quantitative-historical conversations didn't have so clear a self-image of themselves as a discipline with a distinctive rhetoric, I would say that among the community concerned with economic and social history there was then a greater sense of unity. The impetus for the interest that economic history held for people trained in economics derived from the problems of what then were called the 'less developed countries.'

Economists had the sense that the tools they innately brought to discuss economic development were not adequate. Keynesian macroeconomics supposed that the problem of poverty arose from effective demand deficiencies. When that was found to be wrong, attention shifted to revive supply-side approaches and models of capital accumulation. But they were not entirely adequate either; the resulting growth models were not taking into account some key dynamic processes of development (such as induced innovation and technology transfers), or certain aspects of the politics and culture of the developing world. Those missing elements were acknowledged as being 'historical', which created an opening for economic historians. That's how I eventually got into a highly theoretically-oriented economics department, as Stanford was in 1961. The graduate students all wanted to do economic development, and the faculty were persuaded – by colleagues like Moe Abramovitz and Paul Baran – that if you were going to have development as a field, you should have an economic historian to help teach it.

Are you suggesting that path dependence may have had an appeal in the 1950s in part because the economists' models left out huge areas like culture and expectations?

Not only that, they left out demography; they left out technical change. The core theory was much closer to neoclassical economics where 'the givens' (*e.g.*, tastes, endowment, and technology, the institutionalized aspects of markets, and regulatory structures) are formed through essentially historical processes – as most economists today would acknowledge. Of course, economic theory would later extend itself into those areas, but in ways that preserved the ahistorical structure of the core: the general equilibrium analysis of competitive markets.

If particular countries started with different givens, then their development paths would differ even though they faced the same current conditions?

That's right! It is relevant to understand the intellectual context in which my early thinking about 'historical economics' was formed: in the late 1950s and 1960s the idea that 'history mattered' had come to the fore in discussions of the developing economies. One aspect of such thought was to be seen in Paul Baran's book *The Political Economy of Growth* [1957]. If you strip away the Marxist rhetoric, the argument was that the condition of people in less developed countries was not something that could be understood in isolation from the persisting effects of their past interactions with the now-developed world. The legacies of colonial dependency (and 'exploitation', the word more often used) needed to be addressed if their future was to be different from their past; otherwise, as the argument went, the structures of dependency would go on reproducing themselves. This line of analysis had developed along with the perception that what was working in the advanced market economies of the West might not necessarily be workable in the LDCs. For one reason or another, their problems of market failure and coordination failure were more severe. The social infrastructure was different, and less geared to supporting capitalist paths of growth; other, compensatory measures, institutions, and government strategies might therefore be called for.

These were the ideas with which Gerschenkron's famous 1956 article on 'Economic Backwardness in Historical Perspective' had found resonance. His theme was that the 'follower countries' in the spread of industrialization had not been able to actually 'follow' in the footsteps of Britain. Their history had been different; they had to 'substitute' new modes of organization, institutions and government action in order to overcome shortages of entrepreneurial expertise, trust and other sources of coordination failure that had permitted the channeling of investments into 'industrial development blocks' characterized by mutually reinforcing positive externalities. In the absence of such concerted actions, it was suggested, those economies too might have remained trapped in a low level, pre-industrialized state.

For me, and for others who came into economic history at that time, this was the real stuff of 'historical economics' and, *mutatis mutandis*, it has remained so. The favorable reception and the attention stirred up in the profession at large by Conrad and Meyer's paper on the economics of slavery certainly was welcome. But it seemed to me to be orthogonal to the main reasons why economic historians should be and were, at the time, being hired by economics departments. Perhaps my view was incorrect about economists' reasons for accepting the New Economic History; I always seem to be underestimating the power of disciplinary narcissism in academic life.

Okay. You're saying that when you started your career, the idea that history was important for understanding contemporary economic development issues was mixed in with the concept of market failures and government intervention? How does that relate to the current literature on path dependence?

I think that those were two separable strands of thought at the time. One strand, with a direct connection with modern views about history mattering, is that there may be multiple equilibria – as in 'high-level' and 'low-level equilibrium traps', the terminology then popularized by Harvey Liebenstein. Under such conditions, it was well understood (at least for the case of deterministic systems) that where you started was likely to determine where you ended up – unless some exogenous action shocked the system or altered its structure. But this hadn't been formulated as a rigorous set of propositions about the nature of dynamic stochastic processes that were 'non-ergodic' – processes that would not converge to some 'fixed point' defined as a limiting probability distribution. So, it could be said that it was the economic historian's task to explore and expose for economists the nature of the self-perpetuating mechanisms that would prevent economies from behaving in a convergent way, ultimately shaking free from the influence of their initial conditions.

Today we talk about such processes as involving 'positive feedbacks' and as being 'self-reinforcing' and 'autocatalytic' – terms borrowed from the physical sciences. But the essential concepts and insights as to their implications certainly were quite familiar to economists and economic historians who wrote about 'big push' theories of industrialization. What they added to the diagnosis was that, without intervention, the self-reinforcing mechanisms would perpetuate an unsatisfactory equilibrium; state-planned investment was proposed as *the* way to escape from this. The latter prescription too often was not based on anything in the analysis, but came from somewhere else – from the philosophical traditions that shaped the style of welfare analysis, in which one was free to imagine the existence of an omniscient and benevolent public agent.

Let me bring you back to the origin of the slavery debate. Having been around Conrad and Meyer at Harvard, did you become involved in debates about the economics of slavery at this very early stage?

Not really. I was an interested spectator. As Alf Conrad lectured on the material, I felt I should study it closely enough to be able to answer questions and grade exam answers. There were some bright undergraduates in that class, who could and did give their TA a run for his money. I remember Marty Feldstein was one of them – bygone days! Of course there was the intrinsic interest in the material, and it was exciting to be associated with doing something new and slightly daring, like talking dispassionately about slavery. But that was the limit of my involvement at that stage – and for quite a while thereafter.

When did you first attend the meetings of The Cliometric Society?

We have been talking just now about 1958-60, when there was no Cliometric Society as such, but, starting in 1960, there were the conferences held at Purdue that later came to be known as 'Clio', and out of which grew The Cliometric Society. Those Purdue meetings, as almost everybody knows, played a formative role in the New Economic History movement in the States, and eventually internationally. I want to say something about their importance for my personal development as an economic historian. I attended my first meeting in 1961. It was a source, a vital source, of encouragement, of reinforcement, because there were so very few of 'us' at the time. We were thin on the ground and scattered across geographically separated economics departments. The formation of a network of people who one knew and with whom one could correspond casually was more crucial than you might imagine. For someone just starting out, as I was, the contacts, particularly those with the older, established people in the field were really the vital aspect

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Report on the Canadian Conference in Economic History

by Byron Lew, Trent University, with Ian Keay, McGill University, Chris Minns, University of Essex, and Sean Rogers, Mount Allison University

(Kananaskis, Alberta) The Canadian Conference in Economic History held its twenty-first meeting on April 23-25 at the Lodge at Kananaskis, the alpine skiing venue of the 1988 Winter Olympics. The program, adhering loosely to the theme "Canadian Economic History at the Millennium: What do we know? Where should we go from here?", was arranged by Herb Emery (Calgary), with help from the program committee: Morris Altman (Saskatchewan), Gillian Hamilton (Toronto), Ken Norrie (Alberta) and Rick Szostak (Alberta). There were 10 sessions of three papers each over two and one-half days.

Ken Norrie chaired Friday morning's session entitled "Patterns of Productivity and Profitability." Kris Inwood (Guelph) opened the conference with "Industry in a Rural Society: Canada during the Late 19th Century", about the structure and productivity of Canadian industry in the late 19th century. Using the 1871 *Census of Manufacturers*, he finds that about two-thirds of industrial proprietors lived in households that were also involved in agricultural production. Inwood unveils evidence of substantial multi-product manufacturing firms, but there appear to have been few multi-plant firms. On productivity, 15 of the 23 industries in his sample exhibited increasing returns to scale, which appear to be correlated with the use of mechanical power, but these establishments did not necessarily have higher total factor productivity. Finally, Inwood notes the presence of a "Quebec effect" through lower estimates of total factor productivity for Quebec manufacturing. Discussion focused on this effect. Marilyn Gerriets (St. Francis Xavier) commented that she would expect individual firms to "get it right" in all regions of Canada, and that differences in regional outcomes might be due to differences in the structure and types of industries. Altman noted that Quebec productivity estimates could suffer from a scale effect owing to the extreme concentration of Quebec's urban population in Montreal. Alan Green (Queen's) thought that the dispersion of the figures for wood products firms suggests that this industry might be worth more study.

Ian Keay's paper, "Canadian Manufacturers' Performance: The Failure Hypothesis Re-examined", is an effort to determine whether Canadian manufacturing was the weak link that caused *per capita* income growth to be slower than in the United States. Keay compares total and partial factor productivities between Canadian and US manufacturing industries. Finding that total factor productivity estimates for Canadian industries were roughly equivalent to their US counterparts, he then estimates input demand systems to determine whether Canadian firms had responded improperly to input price signals, finding that Canadian manufacturing firms did appear to use cheaper inputs more liberally. Discussion focused on the extent to which his productivity estimates capture the full story of Canadian manufacturing efficiency. Wayne Lewchuk (McMaster) observed that the firms in Keay's sample are at least 20 years old and that a comparison of successful Canadian and American firms might bias results relating to entrepreneurial performance. Keay responded that his results were consistent with findings from work based on cross-sections and that it wasn't clear in which direction his estimates would be biased. Sean Rogers asked whether Keay had considered comparing allocative efficiency between Canadian and American firms. Norrie noted that Keay's startling results run counter to research by the Canadian government that established in part the basis for the NAFTA agreement between Mexico, Canada, and the United States.

The final paper of the session was Byron Lew's "The Diffusion of Tractors on the Canadian Prairies: The Threshold Model and the Problem of Uncertainty." Lew shows that the threshold model predicts that all Canadian prairie farmers should have replaced horse teams with tractors by 1927, but evidence from Saskatchewan farms shows that tractors were not universally adopted through the 1920s. Lew argues that the threshold model fails because farmers evaluate the issue of whether or not to adopt tractor technology as a multi-period investment problem under uncertainty. Comments on the paper centered on the modeling of the farmers' choice between tractors and horsepower. Knick Harley (Western Ontario) noted that family production units might not maximize profits in the strict sense. Ann Carlos (Colorado) observed that prairie farmers did not cost capital as an economist would. Lou Cain (Loyola and Northwestern) wondered if Lew should consider using a quality-adjusted price of tractors in his model since the relationship between the quality and price of tractors may have changed over the 1920s. Keay asked whether changes in the cost of borrowing could be driving the pattern of tractor adoption, and Inwood wondered whether there really had been no resale market for tractors.

George Emery (Western Ontario) chaired the second session examining the state of Canadian economic history. The first paper, by Ruth Dupré (HEC) and Michael Huberman (Montréal), searches for a conversation between cliometrics and Canadian history. The authors examined some 650 articles published in the *Revue d'histoire de l'Amerique francaise* and the *Canadian Historical Review* (*CHR*) between 1970 and today and classified them into one of three categories: traditional economic history, cliometrics, or the use of economic theory and statistical sources. They conclude that, while certain historians are listening to economists and are more likely to be Francophone than Anglophone, a true conversation appears absent since economists do not seem to be listening to historians. Much of the discussion centered around the future. The chair said he was optimistic given the shift from economic to social models of behavior; Jose Igartua (Université du Québec à Montréal) was not so optimistic. The discussion switched to the power of the editor. Douglas McCalla (Trent), a former editor of the *CHR*, did not dispute the power of editors to shape discourse, but mentioned that editorial decisions have been overturned by reviewers. He also noted, "Editors are open to publishing articles where they don't speak the language." Commenting that there are days when he is optimistic, McCalla noted there is a focus towards narrowing a journal's coverage, given today's professional requirements, *versus* the more broad-based nature of journals in the past. Altman suggested including more journals in the project.

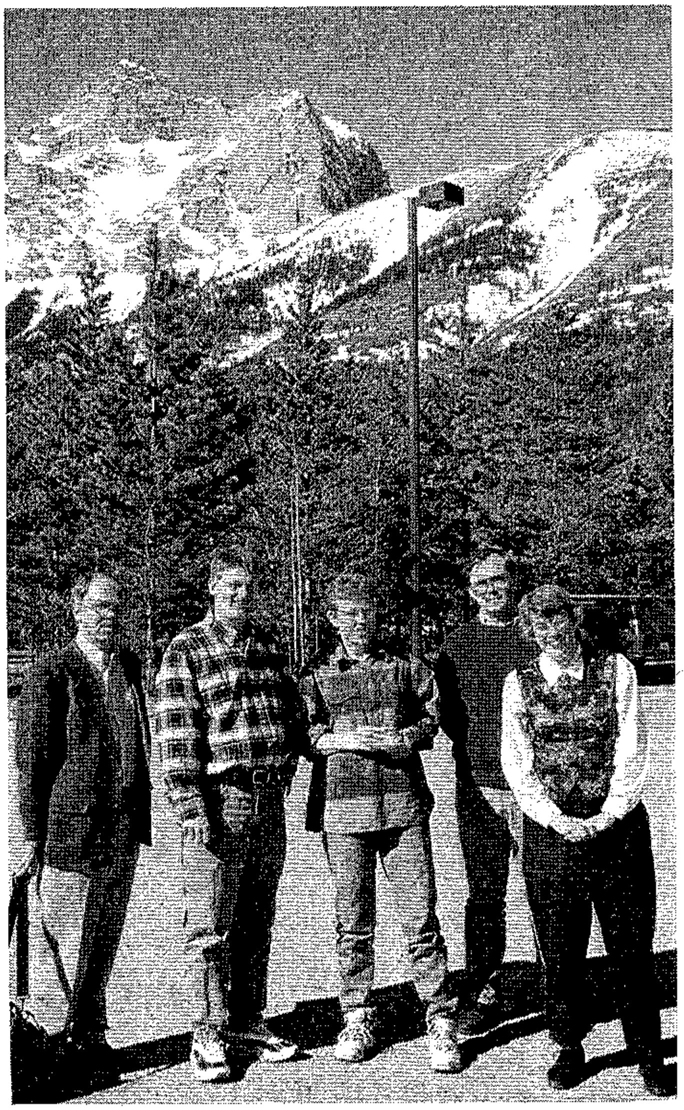
The second paper, by Jose Igartua, surveys the state of historical writing on 20th-century Quebec. Despite the presence of social scientists, Igartua asks, "Where have all the historians gone?" Adopting an economic approach, Igartua finds on the supply side that there are almost no *Québecois* economic historians currently training graduate students in Quebec history departments. This, in part, reflects the heavy requirements of economic history research, problems with archival research, and a lack of role models, both public and private, within the province. Igartua argues that demand side factors play an even greater role as other historical fields, such as labor, cultural, political and women's studies attract students away from the study of 20th-century Quebec economic history. Leonard Dudley (Montréal) suggested that students are motivated to work on topics where there is a sense of injustice, topics that lend themselves to social or political history. Dupré said this might reflect self-selection on the part of students who were not equipped to deal with economic history, although Igartua countered that students may feel there is less they can do about it. Don Paterson (UBC) expressed pessimism for the future because of a deep-seated resistance among history students to learning proper quantitative methods.

Rick Szostak, sporting a tie with the red Canadian Maple Leaf, showcased the soon-to-be-released Canadian history CD-ROM; one tie is available free with orders of 10 or more. Paterson provided an overview of the database he and Bob Allen (UBC) are developing, whereby students can deposit and gain access to quantitative information gathered for their class projects.

The afternoon of the first day opened with the session on "Technology, Institutions, Resources and Growth", chaired by Richard Pomfret (Adelaide). In his paper "Technology, Institutions and Destiny: North-South Divergence in Early Modern Europe and Its Implications for the Americas," Leonard Dudley attempts to link Harold Innis's theory of information networks to Weber's emphasis on the role of religion in Northern Europe's success. Dudley shows that convergence did not occur between North and South, and he proposes that the difference in growth rates lies not just in literacy (human capital) *per se*, but in how writing was disseminated through networks. Growth in Northern Europe was the culmination of the interaction of literate individuals sharing a common code. Altman wondered whether Dudley's use of population growth, rather the growth in *per capita* incomes, affected his conclusion. Dupré was troubled by Dudley's simple classification of North and South as Protestant and Catholic. Frank Lewis (Queen's) observed that most of the difference in growth between North and South appears in the constant term of his model, suggesting misspecification. Carlos wondered whether the use of Barcelona rather than Madrid as a southern node might influence the results, since Barcelona was integrated into the Atlantic economy but Madrid was not.

Morris Altman indirectly addresses the work of Innis in his paper "Staple Theory and Export-Led Growth: Constructing Differential Growth." He defends the use of the staples thesis, while criticizing general equilibrium models which, by design, assume away linkage effects and therefore cannot attribute growth to staples. As support, he compares growth rates among several late 19th- and early 20th-century New World staple-exporting economies to show the lack of convergence among the group. Further, he focuses on regional growth in Canada during the Wheat Boom of 1896-1914 to show that the most rapid growth occurred in regions that were not staples producing. Kieran Furlong (Toronto) wondered how work by Marvin McInnis, showing that the Wheat Boom was largely unrelated to growth before 1907, might modify the staples view. Green reiterated his recently published conclusion that population growth was of greater importance to income growth after 1907 than was export growth. Livio Di Matteo (Lakehead) suggested that, in light of this work, the staples model might better be viewed as a supply-side story of linkages and innovation rather than a demand-side story of Old World demand shocks inducing New World growth.

In the last paper of the session, "The Origin of a Hinterland: Agricultural Resources and Manufacturing Development at Confederation", Marilyn Gerriets adopts and adapts the argument that export success may in fact be detrimental to long-run growth. Her research focuses on differences in growth between the Maritimes and Central Canada. In her view, industrialization proceeded most extensively in regions with the greatest population densities, which were determined by the pace of settlement, which in turn was determined by the availability of good agricultural land. Thus, the uneven distribution of agricultural land explains regional patterns of industrialization. She argues, contrary to the staples approach, that the quality of agricultural land is determined not simply by its capacity to generate marketable surpluses for foreign markets, but also by a diverse supply of locally-produced goods that reduces settlers' dependence on imports. Much of the discussion focused on the determinants of the pattern of settlement. Keay emphasized the importance of previous settlement, Cain focused on expectations of future settlement, and Harley rebutted both comments by doubting the importance of immigrant networks. On an alternate tack, Dudley discussed the link between good agricultural land, innovation, and staples in general, while Pomfret pointed out that good land might have allowed for the freeing up of labor necessary for industrialization.



A Rocky, with conferees Doug McCalla, Herb Emery, Angela Redish, Kris Inwood and Mary MacKinnon

The second session of the afternoon, chaired by Livio Di Matteo, was "Institutions, Communities and the Primary Sector." Knick Harley presented the first paper, "The North Atlantic Meat Trade and its Institutional Consequences, 1870-1913." The Atlantic trade in meat products was as important in value as the grain trade but is less studied. While liners continued to carry the traditional preserved meats, by the late 19th century they began carrying both live animals and chilled meat. Technological changes allowing the transport of perishable products resulted in organizational adaptations: a greater degree of integration among packing, shipping, and distribution, and larger firms. The industry on both sides of the Atlantic came to be dominated by three American firms, Swift, Armour, and Morris. Questions centered on the organization of the shipping itself. Pomfret asked about cargo ships, and Harley answered that chilled meat was shipped on passenger liners, as were live cattle, making better use of shipping capacity.

The next paper, "Institutional Change in the Newfoundland Fishery," by Ken Norrie and Rick Szostak, asks three related questions. Why did the admiral system, by which property rights over scarce shoreline were assigned by order of arrival in the harbor each spring, survive for centuries? Why did three quite distinct organizational forms coexist in the inshore fishery through the 18th century, with the resident fishery suddenly rising to dominance at the end of the century? How was the admiral system transformed into private property rights? The authors argue that the savings in negotiating costs, and the assurance that the best shoreline would be used each year, outweighed the rent dissipation and environmental disruption inherent in the admiral system. The transformation to private property could not occur directly, because migratory fishers opposed it. However, they introduced an intermediate institutional structure by leaving men to winter in Newfoundland, so that the first man rather than first ship was entitled to the best shoreline. Once established, this practice paved the way for private property. Angela Redish (UBC) asked about the availability of documentary evidence. Inwood wondered about the impact of the Little Ice Age of the 17th century and asked for more detail on demographic processes. Carlos asked whether migratory fishers had been able to cooperate to limit environmental damage. She and Sean Cadigan (Dalhousie) noted that some recent research suggests it was the Beothuk natives rather than the fishers who were responsible for deforestation. Cadigan also pointed out that it wasn't the departing Europeans who destroyed their own drying racks; it was the Beothuk who burned the racks for the iron nails.

Steven Mavers (Guelph) presented the final paper of the day from work on his doctoral thesis, "Economics and the Formation of Community Identity – The Ideational Impact of Ontario's 19th-Century Salt Industry." Mavers's entertaining presentation showed the extent that communities celebrated and defined themselves according to their primary industrial activity. He focuses on the impact of the salt industry's development on a group of largely agricultural communities in 19th-century Ontario. The discovery of salt at Goderich in 1866 generated a great deal of local "boosterism." As the salt industry emerged, something of a local discourse developed. Further, the historical development of Ontario's salt industry does much to challenge the traditional staples thesis. More "manufactured" than "mined", salt provided valuable linkages to local foundries, cooperages and farms, yet the development of the local economy also resulted in greater linkages to the outside world. As Ontario salt was exported to North American markets and shown at international exhibitions, local identity evolved to reflect a new conception of self. Questions from the floor centered on the extent to which the public displays and boosterism of the towns really reflected a collective "salt" consciousness, or whether they were to advance the marketing of the salt output and the interests of landowners.

The second day opened with the session "Canada and Australia", chaired by Patrick Coe (Calgary). In the first paper, "Canada and Australia in the World Economy," Richard Pomfret offers explanations for these countries' differing tariff policies over the postwar period. Despite their obvious similarities – small open economies relying heavily on agriculture and resources – tariffs remained high in Australia until the early 1980s, while Canada reduced tariffs steadily. Pomfret points to several important differences. Canada had a free trade lobby very early on, while Australia never did. The settlement of western Canada gave a significant fillip to the free trade lobby since wheat producers were vociferously in favor of free trade. By contrast, Australia's largest export industry, wool, was capital intensive with few operators, and wool producers tended to ally themselves with the protectionist position of agriculture in general. Canada's tariff structure was dismantled in step with the expansion of Canada-US trade but Australia's wasn't dismantled until the Pacific Rim replaced the UK as its principal trading region. Ron Shearer (UBC) wondered how the proximity of the US to Canada might have informed Canadian consumers of the high cost of protection. In a similar vein, Lew pointed out that western grain farmers in Canada were well aware of lower prices for equipment in nearby US markets. Mary MacKinnon (McGill) wanted information on the wealth of wool producers in Australia and its impact on their lobbying activity.

The next paper, by Tim Rooth (Portsmouth), "Australia, Canada and the International Economy in the Era of Post-War Reconstruction", was complementary to Pomfret's. Rooth first shows that trade policy was domestically driven in Australia, whereas it tended to be a strategic response in Canada. He argues that Australia was overwhelmingly committed to full employment and made use of bilateralism (*i.e.*, food exports to Britain) as a tool to that end. Canadian multilateralism arose as a foil to its increasing reliance on the US as well as a response to the

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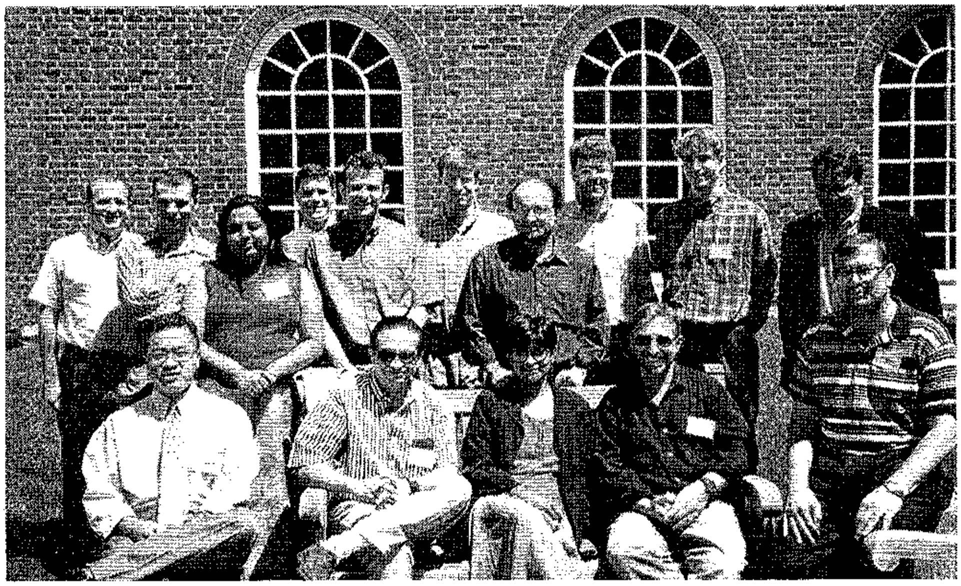
Clio Report *(continued from page 1)*

asked whether this was truly a market in shares and about its geographical compass. It was a market, but a local one; a good deal of monitoring was required by shareholders. Bill Kennedy (LSE) had a set of company-specific questions, but learned that Rousseau had looked only at the aggregate measure of par values. Undaunted, he wondered whether there was a relationship between par share values and company performance, about the dividend component of total returns, and whether banks had been trying to sell particular stocks in the market – that is, selling good ones to enhance their reputations or specializing in 'selling dogs'. Rousseau replied that he should investigate the last point. His paper shows positive temporal links between market liquidity or capitalization and the earnings of industrial mill operatives using VAR techniques, causality tests, and impulse-response functions. Rosenbloom asked about the *development* impact of rising market liquidity, since the plotted function falls to zero fairly quickly, but Rousseau stressed that the plot illustrates the impact of just one shock, while during the period as a whole there was a series of liquidity shocks with a cumulative impact on development. Jeff Williamson (Harvard) suggested further discussion of the economics of the finance-labor nexus, since labor markets were not yet well integrated. Finally, Knick Harley (Western Ontario), concerned that the argument might be generalizing from one industry, asked how dominant were the shares of the big textile firms in the market. Rousseau replied that total shares were heavily skewed towards textiles, but that by the end of the period there were many other firms' shares being traded, such as those of machine shops.

The second paper of the afternoon, by Bishnupriya Gupta (Essex), examines the failure of collusive agreements in the Indian jute manufacturing industry during the interwar period. Eugene White (Rutgers) was intrigued by Gupta's comparison of successful collusion in tea with its failure in jute and was concerned about the dearth of quantitative data. Gupta responded that the tea/jute contrast was interesting because both tea plantations and jute mills were managed for their owners by agents (often the same ones). Unfortunately, although the Indian Jute Mills Association (IJMA) had supervised the collusive work-time restrictions, they kept no records of firm or industry output. However, after Ian Keay (McGill) asked whether there were any data on firm performance to assess the pattern of collusive rents, Gupta said that there are some data on profits. There followed a series of comments and suggestions about placing the Indian jute industry in a broader theoretical and empirical context. Fishback suggested that a 96-firm industry was too large for a collusive agreement and wondered about the profitability of the member firms. Gupta stressed the concentration of control by managing agents and said that profit levels were 'reasonable' for the older, inside firms, as well as for some of the newer outsiders. After asking for additional descriptive material on the international jute industry, Jeff Williamson wondered what sorts of industries make good candidates for collusion, and Carolyn Moehling (Ohio State) asked why one would expect a successful cartel in jute as well as in tea. Gupta replied that India produced a major share of raw jute output and was also a major contributor to manufactured output. John Lyons asked why, in the depressed circumstances, there had been any entry by Indian firms, and was told of the high jute goods prices in the post-war boom of the early 1920s. Despite the abandonment of the IJMA working-time restrictions



Attending a Cliometrics Conference for the first time (l. to r.):
Front: Henry Siu, Juan-Manuel Renero, Bishnupriya Gupta, Les Oxley, Bill White
Back: Peter Rousseau, Radek Szulga, Sonali Garg, Jim Sullivan, Christian Stögbauer, Ben Chabot, Harry Kitsikopoulos, Liam Brunt, Ian Keay, Fred Smith

in 1937, Rebecca Menes (UCLA) asked why the ultimate outcome should not be seen as a success; after all, there was an effective one-shot attempt, followed by working-time restrictions imposed by government in 1938. Gupta said the jute agreement was certainly a failure relative to what had occurred in tea. The IJMA had attempted to draw in the Indian-owned 'fringe' firms, but, once they refused to join the agreement, the restrictions were abandoned in a 'punishment' phase of the cartel game; finally, the government stepped in out of concern for jute growers as well as the manufacturers.

The paper by George Grantham and Franque Grimard (both of McGill) was discussed third rather than first, owing to Grantham's extended visit to the Detroit airport after his scheduled flight had failed to show. In his opening remarks on the labor force participation (LFP) of French women in 1851, he related good news (lots of data), bad news (the data supply function was related to ease of railway travel from Paris to the archives), and stressed his belief in the reliability of the occupational designations. Despite Grantham's attempt at preempting his critics, Joel Mokyr (Northwestern) opened the discussion with two points: the probit analysis of women's LFP was really a 'kitchen-sinky' heuristic regression that mixes demand and supply factors, and – even if that were all right – asking whether women were 'in the labor force' at that time is an anachronism. It is silly to ask such a question, since in 1851 *all* farm women worked. Kennedy agreed with Mokyr's second point, stressing that a crucial issue was the number of children present. Continuing the onslaught, Ruth Dupré (École des Hautes Études Commerciales) remarked that determining the LFP of farm wives even 20 years ago is difficult, and Lee Craig (North Carolina State) said that defining economic activities is a major problem in US agricultural history. The LFP rate is a red herring anyway, since what is most important to know is the allocation of time. Grantham responded in sequence to these points, beginning with a weak defense of the probit analysis, that he had 'duked it out' with his co-author and they chose this technique because 'that's the way it's done in development economics.' On the other issues, he responded that this Census in particular was designed to elicit information about sources of income and thus the estimates were likely to have some meaning. There were appropriate LFP variations (*e.g.*, for wives of day laborers with and without land and for women whose husbands had agricultural *versus* non-agricultural sources of income). Reported sub-group LFP rates ranged from as low as 17% to more than 95%, and he did not believe the variation was pure artifact. Simone Wegge (Lake Forest) then asked whether people who worked only during the harvest had been counted, Rick Steckel (Ohio State) suggested that data about multiple occupations be presented, and Phil Coelho (Ball State) asked about family size distributions and family incomes. Grantham replied that, although he was trying to merge data from the cantonal agricultural census returns with the population census, the best he could do is construct estimates of rents, capital, and wages at the cantonal level, since there are no household income data. Fishback was concerned with the lack of comparability (and perhaps reliability) of the 1851 data with those of other censuses, but Grantham expressed his faith in the greater reliability of the 1851 data than later French censuses, which were concerned with tariff-related issues rather than everyone's means of support. Harley asked whether the authors were looking for differences in regional labor market behavior *vis à vis* varying market opportunities, *à la* deVries's 'Industrious Revolution.' The short answer was 'Yes' with concern for what had happened to productivity in late 19th-century France. Jeff Williamson concluded by asserting that the entire approach of the paper was a 'big mistake', that the authors should pay more attention to the economic development literature and should really focus on the question of what, if anything, is new or different about France.

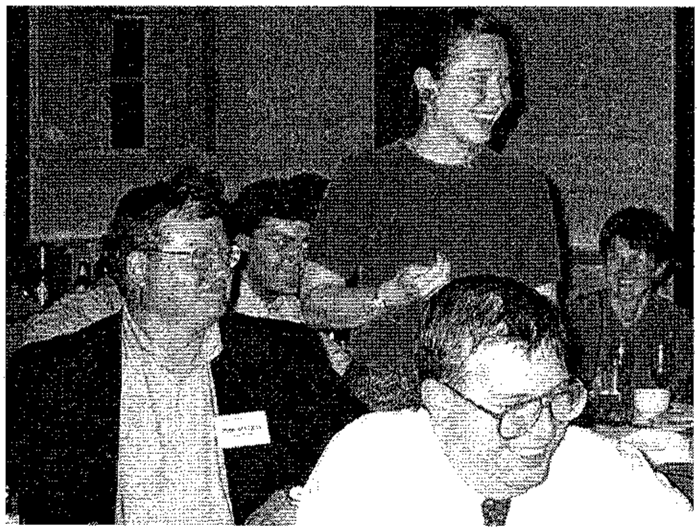
Proceedings early Saturday morning opened with discussion of the paper by Joe Ferrie (Northwestern) on rural to urban migration in 19th-century America. Self-confessed macroeconomic thinker Jeff Williamson did not like the absence of connections to macro issues. We know about massive European immigration to the USA, which competed with rural-to-urban movements of the native-born. For example, small cities may have been at a disadvantage compared to larger ones, and the paper would benefit from including data on wage gaps. Ferrie agreed but said the foreign migrant stream is less likely to have affected domestic migrants in the mid-19th century as compared to the later period. Mokyr, after expressing some confusion about the meaning of 'negative selection', noted the constant migration to cities in Europe and wondered whether the effects of high urban death rates and the attractions of 'city lights' could be accounted for in the US earnings estimates. Ferrie responded that he had tried to limit the amount he was trying to explain. Lou Cain (Loyola and Northwestern) asked about elderly people moving to towns. Atack, noting the variety of types of towns, was skeptical that all urban places were really 'urban', while Kennedy asked about results which could be related to specific dates of migration, such as responsiveness to business cycle conditions and how much time was required to realize a return to migration (either positive or negative). Ferrie said he plans to do more detailed work on occupational change. He can follow family groups and will be able to look at 'elderly' ex-farmers; he can compare migration to 'new' towns *versus* older, more established ones, and can compare occupations of long- and short-distance migrants. There is a problem of interpreting just where and when losses or gains to migration took place, but using birthplace data and making recourse to a few state censuses might help. Given Ferrie's use of wealth data, Sam Williamson asked about the locations of real estate holdings and Fishback wanted to know the relative values of farms and urban real estate. They were told that the Census does not provide such information, even though it supplies data on personal as well as real property holdings. Coelho suggested looking at migration-investment relationships, for example, differences between movers and stayers in age and farm value, and asked about the reported wealth aggregates by county. Ferrie said that wealth, both by county and individual, was gross, not net, whereupon Keay asked about levels of mortgage debt, and learned that it was unusual in the period for anyone to have a mortgage greater than one-third of the property's value. Grantham argued that there were still many opportunities in agriculture in the 1850s and '60s, so rural-to-urban migrants must have been without such opportunities, and suggested a comparison with, say, 1900-1910, and the migration patterns of professionals and others who were well-educated. It is possible to make comparisons by occupation for the turn of the century, responded Ferrie, but wealth data were not collected after 1880. Wegge wondered about the 'friends and neighbors effect', that there might have been joint migration rather than negative selection. Ferrie said that with the complete set of links, 1850-60-70, he could try to examine specific locational patterns. Harley thought that contrasts between the 'urban' characteristics of such places as Paris (Maine) and Schenectady should receive some attention. Ferrie then stressed that about 80% of migrants had moved to places with less than 10,000 population, many of which were then just on the cusp of urban status. Sylla rejoined that even some very small towns were in fact quite 'urban'. Finally, Kennedy suggested it would be important to re-estimate a couple of relationships, so that 'normal' migration might be distinguished from 'distress' or 'opportunity' migration.

Next up was a paper by Christian Stögbauer (Munich) on spatial patterns of relationships between unemployment and the Nazi vote at the end of the Weimar Republic. 'Clio folk' should do more of this type of work in political economy, asserted Mokyr, but perhaps, since the 'time series' data are for only two elections (1928 and 1933), Stögbauer should employ a more articulated model of the voting decision for both occasions. Stögbauer stressed that, *contra* the time series, there were in fact negative relationships between unemployment rates and the Nazi share of the vote in 1933, showing up in small administrative units (*Kreise*) but obscured in work that has investigated larger regions. Kennedy thought the paper's lack of discussion of anti-Communist sentiments should be rectified. Jeff Williamson suggested two improvements to analysis and exposition: first, pool the data for 1928 and 1933 with information from other elections and with other macroeconomic data; second, avoid an elaborate rational-choice model, but specify what had distinguished the parties, such as a menu of relevant issues and party positions. A literature on the basic features of German elections through the 1960s-70s does exist, said Stögbauer, showing the importance of religious confession and of the disappearance of laborers' parties in the Weimar period. Unfortunately, at the *Kreis* level, the only available economic indicator is the unemployment rate (although the level of farm indebtedness is apparently related). Eugene White pleaded for a more complete description of the voting system generating the data: did people vote for a party list or individuals? Dupré asked which party offered jobs more effectively, and whether there had been a 'contagion' effect in adjacent *Kreise*. On the former, Stögbauer couldn't say, since the Socialists wanted to raise wages while the Nazis wanted to redistribute land. Toma then asked about the public choice underpinnings of the analysis: given the macroeconomic problems world-wide, could anyone identify who was truly to blame for German economic problems? Voters were generally myopic, Stögbauer thought, and, although he does not analyze the government's vote directly, the deflation under Brüning's Chancellorship was certainly not necessary from a world point of view. Les Oxley (Waikato) argued that the spatial modeling is good, but he was doubtful of some of the statistical inferences presented. What would Stögbauer do if the tests were to fail (since the various test statistics reported are not necessarily independent)? Sam Williamson asked, in closing, whether local issues had been raised by the local candidates. Stögbauer replied that the Nazis had a strong party line – a national message transmitted by the party *Gauleiter* to the 35 regions; however, local candidates had been able to add some elements of local appeal.

(continued on page 20)

CABARET

The Mullah was looking forward to the annual spring rites of the Cliometricians with greater than usual anticipation. After all, the clan was gathering back in Oxford after a long absence. He was so eager to return that he arrived a day early on the big silver bird to refresh his memory about the locale. With the aid of a newly-minted cliom and the bow-tied scholar from the wolfpack tribe who had been raised in the vicinity, he was able to locate a fabulous restaurant that served the typical fare of the region – *escargot*, soft shell crab. He was also able to locate a hotel room that enabled him to appreciate the work ethic of the Midwest. He was surprised and a bit overwhelmed when this work ethic was displayed outside his hotel room at 3:00 AM. Apparently not even dark of night can stay the appointed rounds of repairs on an interstate highway exit.



The King of Clio heard a chorus of praise from plioms junior...

His arrival in Oxford was thus a bit muted as a result of his research analyzing hours at work, not to mention the tour of some nearby, and relatively unknown, canal. He was also dismayed a bit because the Great Orator (or is that Orateuse?) of the Southwest would not be present. On the bright side, he who has studied the potato at great depth would be in attendance, so there was an excellent chance that wisdom would be forthcoming from his and others' orifices.

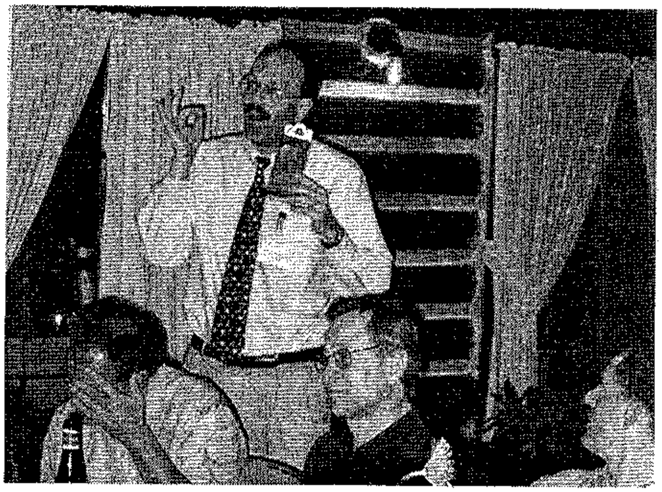
Although the sessions in which wisdom is spouted from all quarters and for all time got off to a slow start, or at least it seemed so to him, the pace quickened. At times he and his scribes could barely keep up with the flow of words – and not just the words, the melodies too. It seems clear that this year's representatives drew heavily, albeit perhaps unconsciously, on the Broadway stage for inspiration. The subtle ways in which they wove lyrics into their presentations may have escaped many of the clioms, but luckily not all. He who has studied sewers checked on their sources simultaneously *via* the internet.

The musical extravaganza began with the lighthearted ditty "Tea for Jute", by she whose first name spelled backwards is no harder to pronounce. He of the wildcat tribe who rows the big boat refreshed our memories of the City of Lights with "April in Schenectady" and the wonderful lyrics "Why, oh why, do I love Schenectady; because it rhymes with Paris." The show continued when the latest member of the Komlossian tribe presented his analysis of the Nazi vote. He employed the brilliant, underused artistic concept of the negative space to evoke the lyrics, "Willkommen, bienvenue, welcome; zu kabaret, au cabaret, to cabaret." Ah, if he could only have seen the election through my eyes. The curtain then was brought down when he with the great burden from over there struck up the chords of "Plant a Radish", one of the remarkable tunes from *The Fantasticks*, whose lyrics for those who have forgotten are: "Plant a turnip, get a turnip, maybe you'll get two. That's why I love vegetables; you know that they'll come through."

And indeed did the non-musical clioms come through this year as the Mullah had hoped. They came through to such an extent that he was reminded of a marvelous saying written in the recently rediscovered Great Book of Cliom Wisdom, "It is difficult to count all the manure", once uttered by he who does not think highly of farmers in some parts of the world. The value of this phrase was attested to by the related additions to knowledge that were forthcoming this year: "Excremental farming makes it difficult to distinguish between clover and turnips" (he who has studied the potato at great depth) and "Buying manure can affect all equations" (he with the great burden). No wonder it is difficult to count it all, and all the more amazing that the initial aphorism was uttered so long ago.

A number of clioms spewed wisdom that reminded the Mullah of yet another of the great sayings, "French data are too beautiful to be true" (he whose name is not misspelled twice). This year we learned the "best French samples reflect train routes" and "The poorest departments make the best cheese", both of which were pronounced by he whose tribe lives *au derriere du Mont Royal*. Beyond that, he who has studied the potato has apparently noticed that "There are no three-armed women in France." No wonder the fashion industry is so prominent in Paris. And what of Schenectady?

Following on that international sort of knowledge we were handed some localized wisdom as well. He who is named after a great, classic motorcycle paraphrased Lloyd Bentsen when he uttered, "I know Paris (albeit Maine), and it is no Schenectady." And we also now know "there are things in the Detroit airport that should not be missed." The Mullah will remember should the silver bird ever choose to land there.



...and clioms [more] senior

As interesting as this wisdom was, none was among the contenders for aphorism of the year. As everyone knows, the prize-winning adage must be uttered in the heat of battle, must contain wisdom for all time and place, and above all must be pithy enough to be easily remembered and fit in a reasonably small space in the great book of sayings. With the Y2K problem in sight, this may be hard to test, but the clioms' search goes on. While each year they search for a maxim to rival "Never open a can of worms larger than the universe", it remains the clioms' favorite bit of wisdom.

This year we were offered the usual methodological tidbits: "Every time you use bigger aggregates, the results change" (the latest Komlossian), "Those in their 30s are relatively old" (an apprentice from the reclaimed land of the great lake), "If you add another lag, you can just forget about it" (he who is either a great philosopher or a hockey player). She who is leaving la-la land set things straight by pointing out that "If one owns all the stock, one has complete control" and by discovering that "The dumbest things look really smart." Someone, perhaps he who will be in charge next year, noted that "Eight year old mothers are a statistical error." But, of course, one wonders whether this is significant in the McCloskey sense.

The clioms tend to look for wisdom in the market economy, much like the drunk who looks for his lost car keys under the street light because it is easier to see there. Thus, the clioms have some difficulty with things like externalities – all that manure notwithstanding. He who has studied the potato clarified for us what cities are all about – "There is more in town than a job; there are externalities." While he who looks at the Federal Reserve differently informed us that "Crops don't have externalities." Bumper stickers are already being prepared for those who would like to replace their aging stickers proclaiming "Guns don't kill people."

But enough prelude! The aphoristic finalists were three. The bow-tied scholar from the wolfpack tribe, the same he who grew up in the vicinity, made a bold attempt at the last second with "The unallocated are unsprinkled." If we only knew what it meant, it could very well be wisdom for all ages. If only he knew what it meant, we could all be more convinced of its value. But how do we know who is unallocated? And what does it mean to be unsprinkled? Unsprinkled with what?

He whose tribe lives *au derriere du Mont Royal* continued his assault on the prize with two contenders. "Timely stabbing prevents cattle from blowing up." This is insight into a problem that few clioms even knew existed. It thus has special intrigue for them; it could launch a research agenda to rival the classic hog-weight episode. On the spot, it provoked much discussion and could have been part of the musical portion of the program, for he who came baack after a long absence provided his rendition of the classic "I shot the sheriff." In his version, "I've stabbed my share of [bloated cows]." As valuable as it may be to know that cow bloat can be relieved, unlike some other subtle emanations, its universality is not quite that of the winner. Indeed, the winning bit of wisdom contains such clarity and indisputability that it was invoked the following morning. So, it shall be inscribed in the Cliom Book of Wisdom for all to know that "There is at least one fact that may not be true."

The night's proceedings began with a tribute to the King of Clio. Many clioms related stories of days long past before the King grew his mane, of the year he may – or may not – have been bar mitzvahed. Tribute was also paid to one of the Mullah's particular favorites who he hopes to see in Montreal next year. After all, what good is sitting alone in Miami; come hear the clioms play.

As has happened so often in the past, the final event was the passing of the Can. The boater from the Big Apple presented the Can to he who is named after a great, classic motorcycle. And he did so with a minimum of singing and dancing (read that as none). Luckily for the clioms, he is eloquent enough to get away with that, but the Mullah longs for musical renditions and hopes that next year there may be an Ed Sullivan-type presentation with perhaps the entire Royal Canadian Mounted Police on stage.

Unlike most other years, several bits of wisdom were saved until Sunday morning. When this happens, the Mullah wonders whether to reconsider the award, but each year he concludes – "Nah!" The line has to be drawn, and so the general rule is the early word gets the bird. Nevertheless, for the record, he who got canned was in fine form with "The problem with looking at the details is that they just serve to confuse." And there was much discussion about the proper thing to do with temperate fruit. A final methodological point was made as well by he who is named after a classic motorcycle, which had some comment about how techniques from CGE analysis might be close to something that is not!

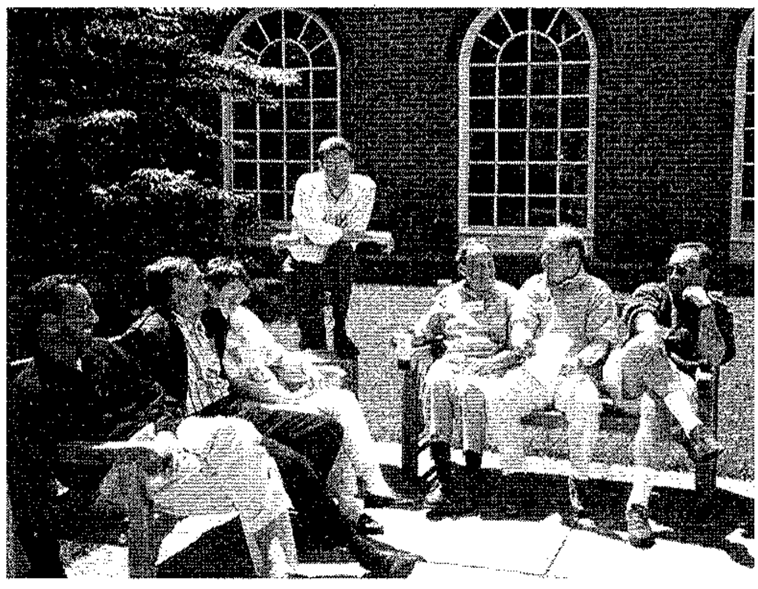
So, auf Wiedersehen und à bientôt 'til Montreal!

Submitted humbly by the
faithful and obsequious servant of the Mullah.

Clio report *(continued from page 17)*

The next session considered the paper on technological determinants of the skill premium at Ford Motor Company, by Henry Siu and Jim Sullivan (both of Northwestern), that examines in particular the impact of automatic welding (1928) on the wages of skilled die setters and makers relative to semi- and unskilled workers. Grantham argued that relative wages would be driven by both supply and demand factors, and the paper had no supply side. He thought there had been an excess supply of workers at Ford and he wondered whether Ford's innovation had affected the workers of other auto manufacturers. The authors replied that they were concerned with the efficiency wage issue and stressed that Ford paid above-average wages, especially for the unskilled. Rosenbloom mentioned the importance of quantity as well as wage data and asked how Ford had recruited skilled workers, how it adjusted at other margins (*e.g.*, by training), and whether die-setting was a *new* skill. Siu said that some setters had been promoted from inside the firm, while many others had been recruited. Brian A'Hearn (Glasgow) appreciated the useful technological detail in the paper, but he couldn't tell whether labor was substitutable by skill and asked how skill formation had taken place. Sullivan was unsure whether the data would allow them to detect skill substitutability but said that the 'Henry Ford Institute' had been founded to teach auto manufacturing skills. Cain was concerned about the implicit *ceteris paribus* assumptions in the paper: the relatively high proportion of African-American unskilled workers at Ford, the impact of the shift from the Model T to the Model A, the role of union contracts, and the effects of the Blue Eagle regulations on hours and wage rates. Eugene White said the discussion needed more on the supply side issues, especially on how well-integrated was the labor market. Coelho asked whether they were discussing general or firm-specific skills and capital. The authors conceded they should look at the broader labor market and said that skills were not entirely firm-specific, since many former outsiders show up in the data. Rousseau was concerned that the sharp relative wage spike (skilled to unskilled) of 1932 was specified as the spike explaining itself. Menes wondered about the elasticity of labor supply and asked whether there had been wage spikes for other skilled workers; Siu said there were data that would permit an answer. Lyons thought there were several possible stories about skill (*e.g.*, did automatic welding require relatively more workers of a given high skill level, or did each skilled worker require more skill?). Sullivan believed the former since there is no evidence of an increase in training costs. In the closing minutes, Rosenbloom observed that skill-biased technical change should show up as a permanent change in relative quantities of workers, and Jeff Williamson said he was still unsure whether wage rates were endogenous or exogenous. Finally, Bill White (Ohio State and Northwestern) wondered why the authors had discussed three technological shocks when two of them had clearly failed to have an impact.

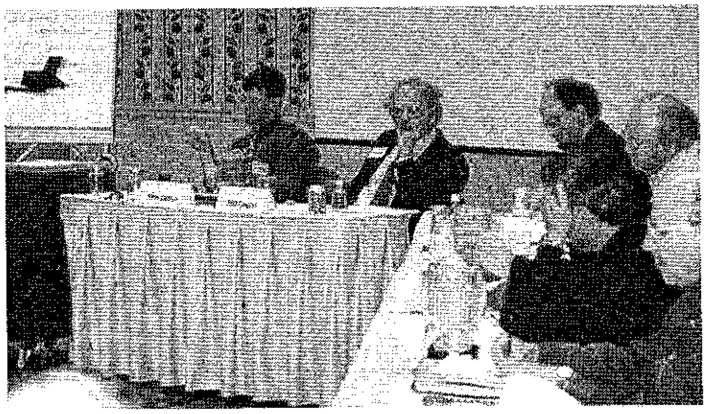
Appropriately, after lunch participants discussed 18th-century English animal (and human) fodder. Liam Brunt (Oxford) develops an arbitrage model of crop rotation using individual farm data compiled by the unavoidable Arthur Young. Grantham, after asserting we already knew about the negative effect of clover cultivation on wheat yields, asked about constraints on the crop mix (*e.g.*, a rising marginal cost of capital or inelasticity of labor supply). He pointed out that using fresh clover as cattle fodder incurs the risk of bloat. Harry Kitsikopoulos (NYU) noted the nitrogen-fixing effect of clover cultivation, but Mokyr wondered whether nitrogen was the important constraint. Crop rotations not only restore soil fertility; they also break insect pest and weed cycles. Harley asked about the price data, suggesting that clover prices were probably poorly measured, and mentioned the regional variation in crop rotations. Brunt said he would look at the clover price data again. He asserted that the degree of regional specialization in the 1760s was relatively low and was simply overstated in the literature. Ben Baack (Ohio State) asked where were the beginning and end of the story (this paper is about the middle). That is, where does this issue fit into the 'Agricultural Revolution'? He also asked about how to price fallow land and about the types of property rights in effect on the farms Young had visited. To the first question, Brunt replied that his story can be placed into the larger picture of changing crop rotation systems in agricultural progress, making one of several appeals to 'another paper.' Jeff Williamson observed that Brunt could employ more choice variables, such as number of livestock per farm, and Oxley asked for a more detailed discussion of the demand factors involved. Menes said the entire argument hinges on equating direct plus indirect values of actual output across crops at the margin, so that the practices of 'dumb' farmers nevertheless look good by assumption. Kennedy, thinking about relative volatility of output values by crop, asked whether farmers were 'dumb' or just cautious.



A break in the action: (l. to r.) Tarik Yousef, Joel Mokyr, Ann Carlos, Knick Harley, Jeff Williamson, Lou Cain, Joe Ferrie

In opening the session on his paper concerning peasants' living standards and capital formation in pre-plague England, Harry Kitsikopoulos said that the role of the peasantry is obscure in the standard stories of the collapse of feudalism. Despite evidence of expanding markets in southern England, it appears that the peasantry of the more sparsely settled north and west invested more and were more innovative than those in the south. Brunt said he was worried about the elasticity of output supply of the peasant sector and wondered whether manors had increased output. Kitsikopoulos stressed that in the commercially oriented region there had been a revival of labor services, with a negative effect on the peasantry. Harley thought it striking, given the greater productivity of convertible husbandry than traditional methods, that there had been a sub-optimal outcome in the regions near London. Was it that the property-rights regime had not allowed convertible husbandry or that the cost of capital was high? There were huge differences in farming practice between the 16th and the 13th centuries, Kitsikopoulos responded, convertible husbandry was not as large a 'payer' as one might think, and the costs of transition were quite high. Mokyr thought that lower inequality of land rights might well have raised output, but – aside from the Low Countries – the English economy was already the most advanced in Europe, so why the strongly pessimistic view of markets and peasant response in the paper? The situation *was* bad, insisted Kitsikopoulos, citing the high level of seigneurial exactions, and noting that at the end of the 15th century 40% of peasants on the Winchester estates had not even one cow to pay the heriot. Fishback said he had been told that the peasantry had received something of value for the dues they paid, but Kitsikopoulos reminded him of the trend to smaller holdings over time as well as the higher rental rates for smaller holdings. Melissa Thomasson (Miami) said it appeared to her that agricultural innovations had not diffused in the low rent areas, even if they had been blocked elsewhere. Kitsikopoulos mentioned the regional distinctions made in the paper, stressing that the northern and western regions were not backward; indeed, they had higher yields than in the south and east. Grantham was not convinced of the picture of immiseration in idealized or generic models and wondered why people had not actually starved. He suggested that smaller farms had been viable and that rural by-employments had been important; that is, specialization had been well-developed. Kitsikopoulos agreed, but he said that many peasants had been very close to the margin of subsistence, as indicated by events in the famine of 1315-22. Steckel asked what exactly was meant in the paper by 'standard of living' and put in a plug for his current joint project using data on skeletal remains to assess biological living standards. Sylla, in closing, asked Kitsikopoulos to clarify how he gets to his pessimistic bottom line.

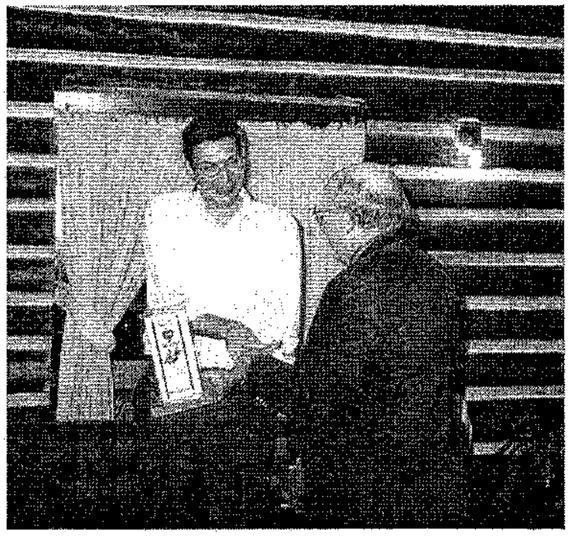
The final paper of the afternoon, by Richard DePolt (Wake Forest), was an interindustry analysis for the United States in 1859. In his opening comments, he noted that input-output analysis is an under-utilized technique in economic history, despite the fact that there is good information to be drawn from rich data sources for the late 19th century. Virtually the entire discussion dealt with measurement issues. Craig began with a question about how activities not yet allocated to sectors would be handled. Atack added that the Bateman-Weiss sample had coded as many as six types of inputs and outputs (reported in the Census) into only four categories. Gupta remarked on the surprising number of zeroes for consumption of machinery output. In reply, DePolt said that he has yet to allocate packaging materials and maintenance and repair activities where spare parts for machinery would fit. On I-O details, he said that major inputs appear to be well recorded, while secondary inputs are quite minor. Weiss then raised the problem of firms producing less than $500 per year (since they were not counted by the Census takers and we don't know their contribution to output). He asked what DePolt expected the I-O method would ultimately reveal – perhaps the shift from home to commercial manufacturing at a detailed level. Fishback asked about disaggregating services and how the project would deal with such activities as banking; Sylla strongly urged DePolt to try to subdivide the three big sectors (trade, services and transportation) which made up 40% of output. Sam Williamson suggested clarifying the meaning of 'service' and asked why the trade sector had only one input. So far, replied DePolt, that is the only estimate he has made. Craig inquired whether the unallocated values had been 'sprinkled' to appropriate sectors; some, but not all, was the answer. Mokyr asked how the construction sector would be treated. DePolt replied that this sector raised a problem with the static I-O approach, since construction could be treated either as a final investment good or as an intermediate input.



Presenter Rich DePolt and Chair Phil Coelho

Following the afternoon sessions, the participants walked to the banquet site through the scenic campus of Western College, since 1973 part of Miami University but founded in 1853 to educate women as the western offshoot of Mount Holyoke College. At the rustic Western Lodge, we were treated to a fine buffet dinner and entertained by light-hearted reminiscences and the usual Awards Ceremony.

Having been awarded the Clio Can the previous evening, early Sunday morning Knick Harley placed it between himself and his audience before introducing his paper, written with Nick Crafts (LSE), on British foreign trade and productivity growth during the Industrial Revolution. Essentially, they are defending themselves against recent work by Peter Temin, who argues from the growth in exports of a broad range of goods that technical change in British manufacturing was more widespread than in the Crafts-Harley view. 'We thought that had to be wrong', Harley said, and this paper is the result. Jeff Williamson said Harley and Crafts were missing the point. The paper ignores the major changes in trade regime from 1770 to the 1840s, from virtually a closed economy during the French Wars to a wide-open economy bolstered by improvements in transport. Furthermore, he was unsure the CGE model employed was stable and could be applied to the pre-Industrial Revolution regime. Harley was not inclined to disagree with these points, but he said Williamson's agenda was quite different from theirs. Discussing the impact of the wars is fine, but the 'classical Industrial Revolution', with emphasis on textiles and metals, is the focus here. They are concerned with British growth and its long-period acceleration relative to northwest Europe; Williamson wants to consider a good deal of spectacular change imposed on a longer-run process. Mokyr then asked why the Temin paper is important. Answering himself, he stressed that the Industrial Revolution story is one of spectacular technical change, although most of the 'oomph' may have been in a few sectors. Temin's argument is about potential changes accruing from *many* small industries, which implies examining micro events and classifying many products carefully, even though that is a difficult task. The question is 'Did the UK *maintain* comparative advantage, and, if so, how?' Harley responded that the crucial issue is the limitations on British agriculture despite its record of rapid productivity growth, since rising imports of foodstuffs and raw materials required more exports to finance them. Revenue expansion from textiles was inadequate given low price elasticities of demand. Likewise, the evidence about industrial output, such as it is, shows rather small output *per capita* in the non-revolutionized industrial sectors. He was unhappy that the impact of the 'other' sectors seemed so small because he believes there was an underlying growth acceleration in the 'not-so-famous' industries. He and Crafts think the spectacular industries should be de-mystified. Fishback asked if there were some appropriate standard of accuracy that could be applied to the numerical results generated by the CGE exercise. Before Harley could reply, outside there was an enormous boom like thunder, but from a cloudless sky, conjured, some thought, by the Clio Can to protect its holder. Afterwards, and perhaps not entirely in jest, Harley said that the best one could do is wave one's hands and hope for a credulous audience. Both Kennedy and Coelho were concerned about the paper's assumption of the non-tradability of services, given actual British net exports of shipping and commercial services and, at some point, a net inflow of property income. Weiss asked about the costs of maintaining the Empire, which do not show up in the paper at all.



To thunderous applause, Knick Harley receives the Clio Can from Dick Sylla

Ian Keay opened the next session by summarizing his conclusion that 20th-century Canadian manufacturing business people should be exonerated of the charge that they were and are entrepreneurial failures. Although there have been obvious differences between the US and Canada in living standards, Cain noted that there were also differences across the specific industries compared in Keay's paper. He then wondered what would have occurred had there actually been free trade after the US Senate failed to ratify the US-Canada reciprocity treaty in 1911. Fishback lauded Keay for his data and analysis, but asked if Keay were willing to say who was really at fault for Canada's relative backwardness. Keay replied that his micro view and a macro view address essentially the same question: Why were input prices as they were? He suggested that Canadian immigration patterns and policy may have led to lower wages than in the US, and that the Canadian tariff has raised the purchase prices of capital equipment. However, he confessed to avoiding Fishback's question because he is not prepared to pin the blame on any particular factor. Garg remarked that the premise attacked in Keay's paper is that Canadian business is assumed inefficient unless proven otherwise, while earlier in the conference the assumption for British farmers was precisely the opposite. Dupré observed that much of Canadian industry actually isn't; about 60% of Canadian manufacturing is by US-based companies, and the Canadian branches may have behaved to benefit the US parent rather than the Canadian economy. Brunt added that both market and tax structures differed between the two countries. Kennedy was concerned that the paper reported only periodic averages of the economic variables, while variation is important. Likewise, he thought the historical dimension of the problem was slighted, since the data in Keay's sample include many commodity and 'sunset' industries, whereas the entrepreneurial problem is best addressed for new industries and their venture capital arrangements. Keay responded that not only does he have detailed periodic estimates, he also has firm-by-firm data that space limitations forced him to omit. His selection criterion for both countries was to provide a broad cross section of industry. In addition, addressing a follow-up question by Jeff Williamson, he said that the data are at least qualitatively 'representative'; for example, he includes the major manufacturing sectors and regions, the classic basket cases (textiles) and the winners. Oxley said that the estimates of factor substitutability seemed to be high, as in steel and textiles. His gut reaction was that maybe the econometrics 'tells you wrong'. An alternative way of addressing the question is to try to identify the efficient production frontier. Menes and Rosenbloom were skeptical of Keay's assumption of a homogeneous US economy, given wide variations of input prices and strong patterns of regional specialization. Steckel then asked what we would be discussing if Keay had written a different paper, say, a Canada-Mexico comparison. That is, economic institutions differ internationally, but they are apparently assumed to be identical in the estimation of the model. Further inspiration might come, he suggested, by considering why the US South was so poor for so long. Keay said there is a similar question for Canada, 'Why the difference between southwest Quebec and Ontario?', for which he would need detailed and regionally specific price data. Cain then asserted that there is a missing story of scale. Because of the tariff, all 10 goods in a product group might be produced in Canada in a single plant, while in the US there is ample opportunity to exploit internal scale economies. In his final response, Keay expressed regret that we don't have data sets good enough to address such questions.

The closing paper of the Conference was Mark Toma's industrial organization interpretation of the Federal Reserve Banks' open-market operations during the 1920s and early 1930s. Eugene White applauded Toma for his bold and innovative approach, but he feared that Toma had found only a second-order effect. He was unsure the interest rates used for the construction of the 'competitive index' were appropriately measured and was doubtful that the banks' open-market trading behavior was large enough to swamp the effects of Board policy, international gold flows, and the like. Fishback was worried about the paper's competition *versus* cartel framework since there was no direct competition in discount operations. Toma replied that there is a policy story concerning discounts at the center, but one loophole was the ability of branch banks to conduct open-market operations independently. Sylla admitted the story was partially plausible for the 1920s, but he was not convinced that the mechanisms described would normally restrict the availability of reserves. Mokyr suggested that Toma estimate the counterfactual of monopoly/cartel behavior in the 1920s. He also wondered about the profit-maximization assumption for reserve bank behavior, asked whether a branch bank could have become bankrupt, and said his impression was that the New York Fed always played the pivotal role in the system. Toma noted that the Federal Reserve Act contained no language specifying the number of branches, so the governor of a small reserve bank might have been worried about failing. Likewise, although the New York Fed was a price-leader and always had a large share of open-market trading, Toma stressed that *all* the branches responded to the competitive index. Kennedy was curious about how the open-market safety valve worked, noting that in the 1930s market interest rate spreads rose sharply and that there was a shortage of eligible securities for the branch banks to buy. Toma pointed to a very high level of his competitive index in that period, which in the competitive model would have led to substantial open-market operations, but by then there had been a regime shift to centralized operations which did not respond to market signals. Oxley remarked that he had begun to feel like a train spotter in an anorak: he thought all the models in the paper must be misspecified, at least from his reading of the reported diagnostics. Tarik Yousef (Georgetown) was curious that the Treasury plays no role in Toma's story of Fed profit maximization, to which Toma responded that, while the Treasury was upset during the 1930s because they received limited seigneurage from monetary expansion, during the 1920s there was no revenue requirement imposed on the Fed. It simply paid for some services supplied to member banks (*e.g.*, check-clearing). Noting the sporadic attempts at branch bank collusion during the 1920s, Sam Williamson asked why some branches had not simply merged. Sylla argued that the paper should be re-written to focus on the underlying primary issue, whether the Fed was an agent of the System's member banks or an agent of the government. Eugene White concluded by stressing that, politically, mergers were impossible and reminding the audience that the System's mandate was also to follow policies that would smooth seasonal and regional variations in the money stock.

Having concluded this year's deliberations precisely on schedule, the participants grabbed their box lunches, departed the air conditioning, and dispersed to various highways and runways. Many will reconvene at the Fourth World Congress of Cliometrics in Montreal next July. You come, too.

David Interview *(continued from page 10)*

of 'Clio' at that stage. That had a lot to do with the very good dynamics among the group that regularly attended – they set the style. There was a sense of commitment, excitement, a wonderful openness in sharing data and helping the younger members of the group focus their research. My first conversations with Bob Gallman, Dorothy Brady, Bill Parker and Dick Easterlin on that occasion are still vivid in my memory, and it was only later that I made some connections with 'the Purdue gang' proper.

You've reminded me of a comment one of my colleagues made who was attending an All-UC conference for the first time. You know, these conferences are modeled on the Clio format. He said, 'This was the strangest conference I ever attended. It wasn't *about* anything, but everyone's quite involved.'

It was clear that the sub-text of the Purdue meetings in the early 1960s was the emerging program for New Economic History. This was to bring more sophisticated theoretical and statistical approaches to bear on the problem of writing a quantitative history of the American economy. Other subjects were heard and discussed, but running in the background always was the creation and refinement of new estimates, and the application of analysis of new data sources to build up a picture of the development of industries and regions. There was a sense that a shared methodological approach was being forged, and there was a sense of a shared outlook. The substantive topics, of course, were distributed over quite a range and there was no lack of criticism and disagreement on specific issues – quite the opposite!

Let me try to pin you down. Within our profession people are pigeonholed as either 'empirical' or 'theoretical' economists. How would you characterize this 'shared outlook?' Is it theoretical? Is it empirical? Who's the audience?

First, almost all the people at those early meetings had been trained in economics, so they had a common theoretical orientation. Second, this was the beginning of the rise of econometrics, so there was a statistical orientation to much of the work, simple at first, but soon becoming more sophisticated as the recent products of graduate economics programs began to join the company. 'What could you do to extract more from the numbers?' That, too, was a question to which almost everybody responded. People didn't have a common view about modeling style; it was more eclectic at that stage than it subsequently became. There was both an empirical commitment to develop new sources of information, most of them statistical, and to assemble a record. That clearly was an undertaking which was still very strongly influenced by the tradition of Mitchell, Kuznets and Burns, despite the shift that had taken economists away from the inductive legacy of the National Bureau, and towards a structural modeling approach of the sort championed by Koopmans and the Cowles Commission.

Some people felt that that unified vision and, certainly, the collegiality and camaraderie of the early days of cliometrics ended with the debate over the publication of *Time on the Cross* [1974]. You were an important participant in that debate. I wonder if you can tell us why it was so emotional, and so divisive.

Well, it's a good question, but it's not an easy question. I won't be able to give you a satisfactory short answer. I think one has to approach this with three things in mind. First, by the early 1970s, the common unifying program of research that had characterized the early days of Clio had been left behind. The field had expanded, and there were people who were working on a wider variety of topics. Also, in the early period the sense of unity flowed from interest in the problems of economic development on the part of the economics profession at large, but, by the early 1970s, new topics related to social and economic developments in the contemporary US economy – racial discrimination, labor market discrimination, urban economics, income distribution, and still other issues – had come to the fore. These caught the interest of younger economic historians, who naturally sought to work on topics that related to the current interests of their economics department colleagues. Then, too, the early program of interrelated work on American economic growth, which provided a unifying, overarching framework, had culminated with the Chapel Hill conference and the eventual publication (in 1966) of volume 30 of the NBER *Studies in Income and Wealth*. Sure, there were follow-on studies that used the estimates for growth accounting analysis, yet that too had become an increasingly specialized pursuit, rather than a unifying focal point. This all meant that when *Time on the Cross* was approaching publication, we had already left behind the initial atmosphere of there being a coherent, unifying intellectual purpose in what we 'New Economic Historians' were about. Maybe it had never existed in reality, but by then even the outward semblance was hard to discern.

The second ingredient was that by that time the New Economic History had become more than just self-conscious; it had acquired a formal sense of itself as a transformative disciplinary movement that people were celebrating. It was not primarily about substantive achievement so much as having been successful in professional, academic terms. The triumph of the New Economic History was measured in terms of the NBER conference volumes, the growth of publications in the *Journal of Economic History*, sessions at AEA meetings, and articles that had made their way into the mainline economics journals. There was a sense that here was a movement that had triumphed, and we had more and more celebratory pieces about this success. So, in a sense, the organizational aspects of the sub-discipline's growth had come to replace the intellectual coherence of the early movement. Consequently, the unity of the field in terms of the degree of public consensus among the people identified with it had taken on a value in itself. Our views now were noticeable, and people had become concerned about the continued growth of funding from the NSF and other such issues. Back in the early 1960s, nobody particularly cared whether economic historians agreed or disagreed, because they were a rarity and were presenting themselves as new and developing, not as an arrived and established branch of economics. But a decade later, the people in the field who had a proselytizing impulse, a mission to convert new followers, were beginning to turn to fields beyond economics; we had filled up the readily available slots in the leading departments, and the prospects for continuing expansion and jobs for our new Ph.D.'s were looking less promising. It was time to press forward onto new terrain, the history departments. You could see this in the serious efforts that were being made at the time to have economic historians on the programs of the American Historical Society and the Organization of American Historians.

Are you suggesting that there was an externality, that people had an interest in having colleagues do a great job, and be widely acclaimed, because that would make it easier for them?

I think it would be too strong to say that there was a political feeling resembling a call for a 'united front', but a consciousness of the shared interest in 'professional identity' certainly had developed. There was the idea that this was a movement that deserved to command the enthusiasm and the loyalty of a growing number of people, and that this was relevant in the larger competition for resources within academic economics.

And the third factor you mentioned?

The third factor was the intense social interest that then pervaded all issues connected with race. This made the history of slavery and the history of race relations extremely loaded from the viewpoint of interpretations that people other than professional economic historians would place on the findings in this field. Hence, the subject was exciting: here was an avenue through which economic historians could reach a much larger and engaged audience. Was the current condition of Black Americans the legacy of slavery? Was it due to something that occurred after slavery – to racism in the North? What had been the role of state and federal government programs in reinforcing discrimination? These were serious and difficult issues, and the scholars who addressed them, however indirectly, through studying the historical record were sincere and not unaware of the volatile nature of public reactions to what they might say. Thus, when *Time on the Cross* appeared, it was seen to be a bold bid for attention from a wider audience, and it used that platform to make a claim on behalf of the New Economic History's power to reveal new and important truths about the history of slavery, the institution that many people saw as the root of the most pressing social issues in America. It attached to that message a still larger set of intellectual claims on behalf of cliometrics, claims that many early reviewers read as preaching a second crusade to establish this approach to doing history in history departments.

So there are the three aspects of the scene: the effort to resume the momentum of a unified 'New Economic History'; the appeal to colonize another discipline, which already had created confrontations with historians who were somewhat dubious about that proposal; and a firecracker tossed into the tinderbox of public discussion of the history of slavery and racism in America. With such a mixture, it seems to me that it's a 'tribute' to the way in which the debate about *Time on the Cross* was conducted *within the economic history profession* that it really didn't explode into, or degenerate into, personal animosities. Most of the serious disagreements that emerged about the book's substance were pursued at the level of 'What was the historical evidence? What was the nature of the theoretical structure within which it was being interpreted?'

Contrary to what may be the perceptions of some people who were not active at the time, this was not so divisive a development within the profession. A few intemperate denunciations were flung at the critics, for 'undermining the cliometric cause', and their personal motivations were questioned, but only on one or two occasions that I can recall. This was unworthy behavior, confined to a very few agitated souls, and it was far from the way in which the authors of *Time on the Cross* conducted their side of the controversy.

There were, it's true, quite a number of other academic historians (especially those outside economic history) who seemed to take delight in the fact that the folks who had only recently appeared massed on their borders in a unified invasion force were now publicly at odds with one another. But they hadn't been reading our journals beforehand. And, furthermore, what participants from inside repeatedly pointed out was that such glee on the part of anti-cliometricians reflected a serious misperception. The strength of the new methodology was that, by comparison to many historical debates that had occurred in the past, what both sides were doing was focused on identifying and defining the set of issues about which there was disagreement *within a common disciplinary framework.* That seems to me to be a very significant, enduring accomplishment of the New Economic History. It raised up the level of the conversation, as intense as it had become on this issue, to that of disputes about quantitative methods and the ways in which economic reasoning could be used to arrive at certain kinds of interpretive statements. It was not a controversy, as so many historical controversies have been, that was animated by politics and prejudice. The spectators sometimes took a different view of what was going on; they made out of it what they wanted for purposes of their own.

Let's talk about your work on path dependence, which has attracted a lot of attention, both from economic historians and also from theorists and policy-oriented people. There have been some very spirited discussions of the concept and its implications on the EH.Res list. As far as I know, however, you have not responded to the debate you've instigated, at least not in print, and I know a lot of our readers would be interested in hearing what you think about the comments your work has generated.

Well, it would be too big a task to respond here to everything that has been said on EH.Res, nor do I think I need to do that. I did post a long paper on 'Path Dependence and the Quest for Historical Economics' back in the fall of 1997. In it I tried to sort out a number of confusions that have crept into the discussion: what constitutes path dependence, the respects in which it is and is not associated with market failure, and the distinction I believe should be drawn between path dependence as a phenomenon and the class of models that properly belong to what I'd referred to as 'the economics of QWERTY.' That paper, which came out as *University of Oxford Discussion Paper in Economic and Social History,* Number 20 (November 1997), has been revised and abridged for publication, but the original still is available.* Possibly the most useful thing in it is the bibliography listing the places in which one can find the other papers that I've written since 1985 dealing with conceptual and methodological issues involving path dependence in economics. These haven't appeared in the *JEH*, *EEH* or the *AER*, so they aren't under everybody's nose. But, I am still surprised that people who express a keen interest in the subject, and argue about it endlessly on the internet, don't seem to have found their way to any of them.

Are you saying indirectly that the QWERTY example is not essential to this set of ideas?

I can say that more directly. I know the thing that some people seem to be hung up on is whether QWERTY is or is not the best keyboard available today, and, if it isn't, whether that entails a big economic inefficiency. Sure, there is a rhetorical force in this illustration, and I maintain the illustration is soundly grounded in the historical evidence, but to suppose that it is substantively crucial to any of the interesting issues is plain silly. Not something I have wanted to further encourage. To be focusing so much attention on this particular question in the history of typewriter technology, as if the relevance for economics of the whole subject of multiple equilibria in stochastic processes (and the mechanisms whereby 'selection' occurs among them) somehow turned upon the answer to it, seems to me a quadruple-headed mistake. Maybe I should take the time here to enumerate those heads?

I think people would like you to...

Okay. The first thing to notice is that you can have multiple equilibria that aren't uniquely Pareto-ranked. The issue of what is and is not 'inefficient' is separable from the study of path dependence.

Second, I cannot see any justification for accepting the

*as a pdf file on the Nuffield College web site (www.nuff.ox.ac.uk/economics/history/paper20/david3.pdf) and as a text file on The Cliometric Society web site (www.eh.net/Clio/Publications/pathdepend.html).

burden of proving empirically that the outcome of a competitive market process has been other than efficient, when you have situations in which the source of the positive feedback can be seen to be the presence of positive (network) externalities, or non-convexities such as learning effects and habituation in a dynamic process. The theoretical presumption that the market would select the most efficient option among the available alternatives no longer exists under those conditions. This isn't news; it's old hat. So, the burden of proof plainly falls on those who say that everything has turned out for the best; that QWERTY is better – in terms of social efficiency criteria – than anything that was and is available. They should try to substantiate that claim, and maybe explain whether that was just a stroke of good luck or whether something far deeper, something economic theory hasn't recognized about the workings of markets, was going on.

Third, it is not as though QWERTY were the only story of path dependence for which it has been suggested that some outcome, other than the one that people in the past lived (or with which we are still living), was not the best 'in the best of all possible worlds.' Why obsess on this single – manifestly minor – illustration? Why not look at the stories of lightwater nuclear reactors (a 'sub'-optimal technology if there ever was one!), or pesticide- and herbicide-intensive agriculture, or at the whole bevy of information technologies that managed to become industry standards by displacing alternatives whose adoption certainly would not have been worse, and arguably would have been more advantageous to society?

Fourth, empirical demonstrations in such cases, either way, aren't really so simple as has been suggested by those who focus on assessments of QWERTY today. Such assessments never will be easy to carry through properly when technologies and institutions have evolved along path-dependent trajectories. The notion of identifying the question of efficiency with the evaluation of just the currently observed state can't make much sense in such circumstances; you also have to consider, in the case of the QWERTY keyboard, to take a good illustration, the questions of the comparative ergonomic properties of the alternative keyboard layouts that were implemented on manual typewriters, and on machines of different vintages.

Or, if you let me shift to the case of the millennium bug (another wonderful heuristic that I have tried to get people to explore analytically on EH.Res), you might need to gauge inefficiencies in terms of the path-integral of the costs of what I've called 'path-constrained melioration.' That's a fancy term for the process through which modifications are made in a technology, or an institution, in order to mitigate the costs of its dysfunctional properties. If you accept those dysfunctional characteristics as part of the status quo, then you look at the costs of remediation as an investment which either is or is not worth making: it's often better to throw money at the problem than to start again from scratch. But why set up an accounting system that at each point accepts the status quo as having been unavoidable; shouldn't one gauge the costs of the problems we have been handed to fix as a consequence of the poor selections made in the past? If we don't engage in research of that kind, are we likely to figure out how to avoid or mitigate more costly burdens that might be created for future generations to cope with?

All this seemed pretty transparent to me when I first read the attacks that were being directed at the concept of path dependence, in the form of critique of the historical evidence regarding QWERTY. I accept now that allowing nonsense to go unanswered is likely to be a mistake. Even though people eventually will figure out that it is nonsense, a lot of time and effort can be wasted in the process.

When you wrote the original QWERTY article, you presented it as an interesting example of a process that would produce a sub-optimal outcome, but you ended by backing off and saying the number of QWERTY worlds is an empirical question yet to be answered. But, as I hear you talking now, it suggests that you're thinking there are many processes that may lead to these multiple equilibrium situations, and you see this as something quite general.

I would certainly agree with the latter statement. At the close of my 1985 *AER* article I wrote that I believed there were 'many QWERTY worlds out there.' I could have said that there were certainly even more cases of path dependence in the selection of equilibria in pure coordination games. Maybe I ought to have added that, but would it have had the same rhetorical force in the profession at large? It is the prospect of something being inefficient that automatically grabs economists' attention. So, I raised the stakes by going with 'QWERTY worlds.' What I did want to get across was the point that the whole world is not path dependent, and, *a fortiori*, that it is not like QWERTY. There are lots of dynamical systems that, for practical purposes, we can analyze as convergent. Sorting out the ergodic from the non-ergodic economic processes, and then, among the latter, identifying those that are subject to market failures and thus belong to the economics of QWERTY still seems to me to be a very worthwhile empirical program. It's a program that economic historians should be taking the lead in. We needn't start this 'cold', for it has long been a strong prior among economic historians that, when it came to discussing technology, institutions, legal systems, culture and taste formation with economists, they should resist the incursion of ahistorical theorizing and press for a more evolutionary approach instead.

One last question. You're now spending much of your time in Europe and talking with social scientists there. Tell us about the connection between that locational shift and the development of your ideas of path dependence.

What I've found is that European economists, and social scientists more generally, are more eclectic in their thinking than their American counterparts. In no way could one say that their eclecticism reflects a casual, low-tech approach to the subject, but there still remain the effects of an intellectual tradition that is less disposed to be dogmatic about these matters. I have found that attitude rather refreshing, in that it more readily accommodates exploring new ideas in which I have a keen interest – such as the practical policy implications of path dependence. I should mention another noticeable contrast between the two intellectual environments, as it also touches on my work. History, the idea of history, and a sense of the weight of history, are thoroughly embedded in European culture and discourse, whereas Americans are much more disposed to focus upon what's new, revolutionary and going to transform the future. This is something of a truism, but the statement is no less true for being commonplace. You might be surprised at how usual it is for high-level policy conferences in Europe – whether convened by the OECD or by EC directorates, by a business association or under national government auspices – to lead off with an invited 'historical benediction' on the economic topic under consideration. I suppose I may be forgiven for finding that a most congenial custom.

Yet, the most wonderful thing is that I have not been obliged to choose between extremes; All Souls College and Stanford form the best convex combination of academic environments that a historical economist could dream about. I wake up every day thankful for the reality of having been allowed to enjoy both places.

Thank you so much. I can understand your reasons for going away, but please come back and see us.

You can be sure of that.

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Canadian Conference *(continued from page 14)*

UK's lack of commitment to continuing the bilateralism of the prewar Empire. Financial concerns were also important. Australia tended towards protectionism to offset trade deficits, while Canada received substantial US direct investment. Harley felt more detail on Britain's role was needed. Inwood wondered how important Australia was to Britain. Igartua suggested that part of the Canadian move toward independence from Britain might have been due to a growing national identity.

The final paper of the session, by Stewart Wilson (Queen's), highlighting his dissertation, was "The Savings Rate Debate: Does the Dependency Rate Hypothesis Hold for Australia and Canada." He tests two competing theories of the increase in savings rates in the two countries in the 20th century. Savings rates rise because of growth in the pre-retirement population aged 45-64 or a fall in fertility rates. Wilson's time series analysis distinguishes between short-run and long run effects, showing that while income is the only significant factor explaining savings rates in Australia, in Canada there are some additional short-run influences attributable to growth of the 45 to 64 year-old cohorts. Redish asked for a better specification of the microeconomics of the life-cycle model underlying the choices he examines. Carlos felt tax policy changes might be important. Altman asked about other types of institutional changes, while MacKinnon suggested that separating the different types of savings might alter the results. Di Matteo wondered about the effect of regrouping the age cohorts from 45-64 to 35-50. Rogers thought that life expectancy changes might also be important.

Saturday morning's second session, "Consumption and Wealth: Microeconomic Approaches to Economic History", was chaired by Don Paterson. The three papers were of interest for their diversity in using micro data in quantitative economic history. Ann Carlos and Frank Lewis use records of the Hudson's Bay Company's trading post at York Factory to trace out changes in consumption patterns of Native groups participating in the fur trade. They argue that the falling share of producer goods and rising share of consumer goods in the Natives' consumption bundles reflects increasing effort directed towards trapping and decreasing effort directed towards hunting as pelt prices rose. Natives enjoyed greater access to non-Native goods in commercial trade. Green, Altman, and Igartua all pursued various aspects of the importance of alcohol and tobacco in the trade for furs at York Factory. Lewis and Carlos admitted to the importance of these commodities in the composition of the consumption goods within the Natives' bundles, and they argued that the giving of gifts prior to the negotiation of trades may have lowered the quantity of brandy consumed by Native trappers.

In "Village Stores and Rural Consumption in Upper Canada, 1808-1854", Doug McCalla uses account books from Upper Canadian country stores to show that the pre-industrial economy of 19th-century Ontario was vibrant, competitive and capitalist. Paterson wondered whether McCalla's store accounts reveal anything about the switch between home-produced and purchased commodities. MacKinnon, Inwood, and Altman asked about the nature of the market in which McCalla's country stores operated. In particular, there was interest in the role of peddlers, travel costs between stores, and the presence of price discrimination. McCalla allowed that these points are pertinent, but could not be addressed explicitly from data in the store accounts alone.

The final paper in the session was Livio Di Matteo's study of changes in wealth in the Thunder Bay district of northwestern Ontario during the Wheat Boom. The use of data from probated estates and some non-parametric smoothing techniques indicate that a significant break point in wealth can be identified in 1907. According to Di Matteo, these results suggest that the effects of the Wheat Boom may not have been felt until well after the date that has been traditionally associated with its commencement (1896). Harley, Green, and Paterson were curious about the role real estate and capital gains on residential housing played in Di Matteo's wealth measures. Inwood worried about attributing changes in wealth to national, rather than strictly local, economic forces. Wilson asked whether the sample of inventories could be extended to study the duration of increases in wealth. Di Matteo responded that local influences, such as real estate and increases in housing prices, were important, but detailed analysis of these issues would require additional data sources. However, the ongoing collection of probate inventories will allow Di Matteo to address Wilson's concerns regarding the duration of the wealth boom in the Thunder Bay district.

Lou Cain was chair and commentator for Saturday's third session on Canadian monetary, fiscal, and trade policy. In response to a question about his competence to comment on a paper involving Canadian monetary history, Cain said he had told the organizers he "would be happy to chair the session." He didn't realize he would have to comment as well. As it turns out, he did not.

In the session's first paper, Eugene Beaulieu (Calgary) and Herb Emery examine the impact of the Canadian-US Reciprocity agreement on the 1911 Canadian general election. Using non-nested methods, they test three models of voting cleavages, by factor, industry and region, and find that Canadian voters aligned themselves along regional lines and according to the interests of the sector/industry in which the factors of production they owned were employed. The significance of vested interests in the Canadian vote meant that Canadian tariffs raised the income of particular industries and regions and that factors of production were not mobile across regions or industries. Consequently, Beaulieu and Emery characterize Canada during this period as a collection of regional economies united only by a common trade policy. In his comment, Cain said that, given the proxies used in the estimation of the factor and industry cleavage approaches, the authors may not really be testing two different models. He also asked about the potential role ideology and its changes across decades might have had on the outcome for that particular election. Green thought that literacy was too blunt a measure for discerning human capital differences among voters, since both a farmer and a physician could read and hence would meet the census requirements for literacy. Dupré asked "Why leave out the Quebec ridings from the analysis?" Emery replied that elsewhere in Canada the election was based on the single issue of reciprocity, while in Quebec other issues were relevant. Igartua suggested including the Quebec ridings where Anglophones represented a majority of voters. Dudley thought that tariffs might reflect the strength of the central state, so that nationalist sentiment may also have been a factor in the determination of voting behavior.

Trevor Dick (Lethbridge), adopting a game-theoretic model, examines the search by colonial and imperial elites in Canada 1840-1876, to find a consensus about self-enforcing, market-preserving limits on government. Governments can do one of two things: create a structure of property rights that allow markets to work or confiscate and annihilate them, with obvious consequences. Dick concludes that, owing to a commitment to cultural duality coupled with ongoing imperial control, the involved parties could not construct a coordinating mechanism (*i.e.*, a constitution) that would police the activities of the state to preserve markets through universally-binding, self-enforcing rules. Consequently, suboptimal solutions arose that benefited one group at the expense of another. Such an arrangement worked because it did not threaten the government's power. Cain admitted he wasn't really sure where the paper was headed and asked whether the markets to be preserved were on a macro or micro level. Dick replied, "Both." Much of the discussion that followed centered on confusion about what "market-preserving limited government" meant. Cain noted the apparent absence of a counterfactual. Harley asked what a Pareto-optimal situation might look like and voiced his opinion that market-preserving limited government had emerged largely by accident in the UK. McCalla noted that perhaps the state should be viewed as a tool for market-making rather than market-breaking and referred to the state's role with respect to Crown land.

The final paper of the session, by Michael Bordo (Rutgers), Angela Redish and Ron Shearer, is an interpretive essay on Canadian financial history with an eye to the future. It examines the interaction between Canadian monetary and fiscal systems and the exchange rate regime, dividing the discussion into three broad periods: the gold standard era, the interwar transition, and the postwar period. In response to external shocks, Canada had to adopt brief, temporary measures that violated the gold standard ideal, but the mentality continued. The concurrent rise of Keynesianism and the calamity of the Great Depression in the 1930s drove the gold standard and its ideology from the country. Canada's recent experience with a floating exchange rate regime serves only to underline the international context in which regime choices and targets are made in Canada. The authors conclude that, despite some minor deviations, Canada's overall experience largely followed that of other countries. Cain noted that the paper was a work of synthesis and that, for many years at these meetings, two of the authors had earnestly debated the issues addressed in the paper. He posed a counterfactual to Shearer and Redish, asking them how this paper would differ if either of them had been sole author. Shearer remarked that using the exchange rate as a device to insulate monetary policy moves was the wrong path for Canada. Harley commented that the gold standard was good but subject to crisis, which set off a dialectic. Shearer neatly summarized his role in such a dialogue by responding, "You will never find me defending the gold standard in any way." The absent Bordo and his representative Redish were speechless. Pomfret commented about the role of transaction costs under alternative regimes, and Dudley asked about the possible interaction between governments and exchange rate flexibility.

The day's final session, "Incorporating the Environment into Economic History", was an interesting discussion of fisheries and property rights. Rosemary Ommer (Memorial), who chaired, began with a project overview of "Sustainability of Fish and Fisheries in Canada", in which she outlined the work of the multidisciplinary ECO Research Project at Memorial University of Newfoundland that has studied the collapse of the Atlantic fishery.

The first paper, by Robert Hong (Memorial), "Pandora's Box and the Thin Edge of the Cultural Wedge: Technological Adaptation and Accommodation in the Newfoundland Codfishery, 1870-1920", placed the current predicament in historical perspective. In the past, fishers were able to adapt to disequilibria caused by new technologies. His focus is on the introduction of the cod trap; fishers responded to the consequent depletion with catch quotas and limited access to the fishery. Hong notes that the mesh used in cod traps in the Newfoundland fishery was particularly tight relative to the looser mesh used in European fisheries; the latter trapped fewer young. Hong cannot yet offer any explanation why Newfoundland adopted the tighter mesh, but he hopes to learn more. Norrie was interested in the cod trap and why it was overused in Newfoundland. Minns wanted clarification on the interaction between the cod trap and the institutional framework under which the fishery operated.

Miriam Wright (Memorial) gave the next paper, "Industrialization, Environmental Change and the Newfoundland Inshore Fishery, 1955-1970." She discusses the role of the state in promoting new technology, and then outlines the forms by which technical change diffused into the fishery and the detrimental consequences arising therefrom. Starting in the 1940s, the size of trawlers increased rapidly, with active government support, so fishers were able to use larger nets that increased landings. Simultaneously, the fish processing industry changed from salt preservation to freezing with a consequent shift from home to factory production. The industry also became more closely integrated into the New England market. Inwood asked about the direction of causality between technological adaptation and increased landings. Lewis wondered whether it was simply the efficiency of the new technology, noting that by the 1970s landings had increased at least tenfold. Rooth asked about the impact of entry by foreign vessels. Cain asked whether governments had attempted conservation measures like license fees or landing taxes.

In the final paper of the session, "Whose Economy? Nature, Polity and Morality in the History of Fishing and Forestry in Newfoundland", Sean Cadigan addresses the question of responsibility for resource depletion in the last half-century. His thesis is that governments continually ignored the demands of locals in favor of the interests of industry by abrogating the collective mechanisms proposed or utilized by the fishers for controlling access. He uses the example of the short-lived forestry industry that the Newfoundland government encouraged (and managed to depletion) as a lesson for fishery management. Much of the discussion was concerned with his assertion that a system of private property rights had failed. Shearer pointed out that a system by which government extends property rights to new land after previous land is fully depleted is not an example of private property. Norrie noted further that the problem in the forestry example appears to have been a lack of government commitment. Altman wondered what institutions could have been adopted that would have been more effective, while Gerriets asked how community restraint might have evolved in an open-access fishery.

The final day opened with the session on "Work and Human Capital", chaired by Morris Altman. In the first paper "Acquisition of Human Capital and the Transition from School to Work: Montreal and Toronto in 1901", Alan Green and Mary MacKinnon use the 1901 Census to ask why incomes in Quebec were equal to those in Ontario in 1901 and why, by 1921, they had begun to fall. They conclude that Quebec underinvested in education beginning around the turn of the century, supporting their thesis by comparing school expenditures, student attachment, and participation in Ontario and Quebec, as well as comparing English *versus* French within Quebec. Altman opened the discussion by questioning the importance of education, asking for information on occupational structure. Much of the following discussion concerned the expenditure data. Dupré argued that expenditures by the Church on French education in Quebec would not have been well recorded; in particular, salaries paid to nuns would not be comparable to those of lay teachers. Igartua asked whether private school expenditures were also recorded. Lewis suggested that perhaps a calculation of the opportunity cost of labor and capital used would be a better basis for comparison.

The next paper, by Wayne Lewchuk and Michael Huberman, "Glory Days? Work Hours, Labour Market Regulation and Convergence in 19th Century Europe", starts with the observation that underlying Angus Maddison's data showing convergence of *per capita* incomes in this period is a simple assumption regarding hours worked. They note that labor regulations in European countries were introduced at different times and differed across countries. Using the Carroll Wright data, which include cross-national comparisons of hours worked, they derive what they call a Worker Development Index (WDI) consisting of Jeffrey Williamson's real wage data, hours worked from the Wright data, and a series reflecting the rate of introduction of labor market regulation. Their WDI shows much less tendency toward convergence. Their use of these data was not without controversy. Inwood pointed out that the wages reported were for industrial workers only, not for the workforce as a whole. Cain argued that the data make no distinction between regular hours and overtime, leading to a discussion of the meaning of overtime in the 19th century, with Harley questioning the ability of workers themselves to choose their hours, particularly with the transition from piece-rate to time wages. A minor discussion arose concerning the WDI index. Redish thought that choice of weights would be difficult and, furthermore, that separate presentation of the three sub-indexes would be equally or more informative.

The final paper of the session, by Chris Minns, was entitled "Immigration and Assimilation in U.S. Labor Markets at the Turn of the Century." He elucidates immigrant earnings patterns by occupation and time to determine whether or not immigrants matched the earnings profiles of the native-born. Minns uses two cross-sections of IPUMS data for 1900 and 1910, thereby controlling for time-specific effects on immigrant earnings. The data, however, do not report earnings, only occupational information, so occupations must be ranked by prestige. He finds support for the notion that immigrants may actually have outperformed the native-born. Furthermore, he finds that the newer immigrants from southeast Europe performed as well as immigrants from northwestern Europe. Redish wanted to see details of the breakdown of blue-collar occupations and also a discussion of literacy. Altman wanted to see occupations broken down by ethnic composition and commented on the assumption that occupation and income could be equated. Inwood and Harley asked about the possible implications of return migration.

The closing session of the conference on "The Depression", chaired by Rick Szostak, opened with Sean Rogers's "Canadian Recovery from the Depression – the Role of Monetary and Fiscal Policy." He considers the Canadian government's major policy tool of the day – tariff policy – as its fiscal policy. During the Depression, he notes, the Canadian government was loath to run budget deficits several years in a row. He first uses a simple estimate of potential GNP to show that a strong recovery starting in the mid-1930s closed the output gap by 1941. Then, using Christina Romer's multipliers for the effects of fiscal and monetary policy, he shows that GNP grew faster in the late 1930s than one would expect. Finally, he examines Canadian tariff rate reductions to determine the impact of trade expansion with the United States and Great Britain in the second half of the 1930s. Shearer asked whether it would be better to look at the separate impact of changes in British and American tariffs on Canadian imports, and Dudley asked which country was a more important export destination. Rooth commented that Canada's current account surpluses with the US and Britain resulted from Canada having been a net borrower from these countries before the Depression.

Kieran Furlong closed the conference with "Technological Change and the Great Depression in Canada." A work in progress, his stimulating paper starts from the writings of economists of the day, particularly Mitchell, to return to technological explanations of the onset of the Depression, rather than the more conventional Keynesian explanation of deficient aggregate demand. In particular, he postulates the importance of price-cost margins and is interested in correlating changes in the cost of production with the severity of the Depression. Like others before, he notes the foreshadowing of the slowdown in the second half of the 1920s, arguing that the US boom of the 1920s was in fact over by mid-decade whereas in Canada the boom continued. There was some discussion by both Szostak and Dudley of the relevance of Schumpeter's work. Szostak suggested that price-cost margins might vary among industries, and Dudley asked whether Furlong's story was intended to be consistent with a Marxian overcapacity view or a Schumpeterian story of uneven innovation and growth of output.

Despite the unseasonably warm and sunny weather and the beautiful setting of the Lodge, the sessions were well-attended. While there was no formal banquet, participants enjoyed excellent hospitality at the Lodge. Many participants were spotted walking in the woods pondering the effects of 1907. There were no losses to bears. Most, perhaps all, were agreed that the stimulative effects of the beautiful surroundings had been a boon to the conference and future organizers should consider an equally attractive setting for subsequent meetings.

CALL FOR PAPERS

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July 6-9, 2000

Montreal, Canada

The Fourth World Congress of Cliometrics will be held July 6 - 9, 2000, in Montreal, Canada. George Grantham is serving as Chair of Local Arrangements. All members of sponsoring organizations are invited to attend. Registration will be open but will be conducted in advance so participants can receive the papers prior to the Congress. Sessions will be held in traditional Cliometrics Conference format: Authors will provide a five-minute introduction to their work, which will be followed by an extended period of discussion involving session participants.

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To guarantee consideration by the Program Committee, proposals must be submitted by **September 15**. Authors who are unable to meet this deadline should send a letter of intent including the title and a brief description of the proposed paper. Proposals should be two to five pages in length, and may be submitted by post, fax, or e-mail to the address below. Authors are encouraged to use the proposal submission form on The Cliometric Society web site:

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39th Annual Cliometrics Conference Abstracts
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An Arbitrage Model of Crop Rotation

Liam Brunt
Nuffield College
Oxford OX1 1NF United Kingdom
Telephone: 44-1865-2786500
lbrunt@econ.fas.harvard.edu

In this paper we formulate an arbitrage model of crop rotation which offers a new method of estimating the effect of crop externalities on wheat yields. Estimating the effect of crop rotation is important for both historical and developing economies, where the agricultural sector is large and crop rotation is the primary source of soil fertilisation. The arbitrage method has two important advantages compared to the traditional production function approach.

First, the arbitrage method relies on different data series to the production function (yields and prices rather than inputs and outputs) and these data series are much more commonly available. This means that the arbitrage method can be used as an independent check on production function estimates, and it can often be estimated for periods and places where there are insufficient data for a production function analysis. Moreover, regression estimates of agricultural production functions typically suffer from severe missing variable bias owing to an inability to control for natural soil fertility; this problem is entirely absent in the arbitrage model.

Second, the arbitrage model allows us to estimate the yield effect of each crop in the rotation on each individual farm. This is theoretically attractive because we can treat each farm as an individual population rather than an observation drawn from a single population (which is the assumption underlying regression estimates of production functions). This is important if we have reason to believe that farms in the data set have access to different technologies, for example. It is empirically attractive to isolate the externality effect on each farm because we can then analyse the variation in the externality much more precisely (with reference to environment and management, and so on).

We test the arbitrage model on a data set of English farms from 1770, for which it is also possible to estimate a production function. It reinforces the recent claim that earlier estimates of the effects of turnips and clover on wheat yields during the Industrial Revolution are inaccurate. Turnips had a positive effect on wheat yields – but clover had a negative effect. This unexpected result is supported by an analysis linking the variation of cropping patterns (clover *versus* turnips) and output prices (beef *versus* wheat). When beef prices were high relative to wheat, farmers sacrificed wheat yields to raise their beef output by switching from turnips to clover. We show that this was true both in 1770 and into the late 19th century.

Further examination of the externalities reveals that they varied dramatically in response to management practices. Farmers could increase their output of fodder by sacrificing the fertility effect of turnips and clover. In particular, there was a strong adverse yield effect from removing turnips and clover from the field in order to stall-feed animals. This new information improves our understanding of the agricultural techniques employed in the 18th century and will lead us to revise our understanding of productivity change during the Industrial Revolution.

Behind the Scenes: An Interindustry Analysis of the United States, 1859

Richard DePolt
Department of Economics
P.O. Box 7505 Carswell Hall
Wake Forest University
Winston-Salem, NC 27109 USA
Telephone: 1-336-758-5231 Fax: 1-336-758-6028
depoltrx@wfu.edu

During the second half of the 19th century, the economic landscape of the United States underwent a multidimensional transformation. One dimension was economic growth. Each decade, the amount of final goods and services available *per capita* increased. However, the composition of that output also changed, indicating a second dimension, structural change. While we know the economy grew and its structure changed between 1859 and 1899, we do not have a comprehensive analysis of how the two dimensions were related, and therefore, we have an incomplete understanding of the transformation.

To fill this gap, two things must be done. First, the levels of GNP for benchmark years need to be supplemented with details about what goods and services were produced and how they were produced, *i.e.*, details about the structure of the economy during each of those years. Second, measurements of growth based on these estimates need to be augmented with details of the changing arrangement of productive activity, *i.e.*, structural changes, which occur simultaneously with changes in the level of output. We need to look behind the scenes to discover the interactions and relationships among productive sectors necessary for generating the final output flows. These mutual relations determine the character of the national economy and by studying them, we will more fully understand how economic growth and structural change were related.

This paper takes a big step in this direction by performing the first task for one benchmark year. I constructed an Input-Output table of the United States economy for the census year 1859. The table has 32 endogenous sectors: 22 Manufacturing, 4 Mining, 3 Extractive, and 3 Service. The table complements Gallman's GNP estimate for 1859 and provides the inter-industry detail excluded from income accounting exercises to avoid double counting. This framework permits a systematic investigation of the inter-industry relations between the sectors of an economy and how these relations are a function of the composition of final output.

Using the table, I examine the structure of the economy in 1859 to improve our understanding of the relationship between the level and mix of final output in that year and the arrangement of economic activity necessary to make that output possible. I also study the impact of an important change in the composition of final output – the increasing share of output devoted to capital formation – on the structure of the economy. Future work will link this table with one constructed by William Whitney for the United States economy in 1899 and perform the second task, a study of growth and structural change during the last four decades of the 19th century.

The paper has three parts. In the first part, I present the Input-Output table. In Part II, I provide an overview of how I constructed the table. Finally, I use the table to analyze the structure of the economy in 1859 and address the issue of capital formation.

"How Ya Gonna Keep 'Em Down on the Farm [When They've Seen Schenectady]?": Rural-to-Urban Migration in 19th Century America, 1850-70

Joseph P. Ferrie
Department of Economics
Northwestern University
Evanston, IL 60208-2600 USA
Telephone: 1-847-491-8210 Fax: 1-847-491-7001
jferrie@nwu.edu

Migration from America's farms to its cities began in earnest in the decades preceding the Civil War. Using new data on 2,890 males linked from the 1850 census to the 1860 census and more than 800 males linked from the 1860 census to the 1870 census, this essay assesses the micro-level causes of these changes in location. It employs a "mover/stayer" framework to account for the endogeneity of individuals' migration decisions and estimates the direction of the selectivity in the rural-to-urban migration process. Both individual and community-level characteristics are examined.

The analysis reveals that in this era, those who made rural-to-urban moves tended to be younger in general and the younger sons within farm households. About 20% of individuals located in rural places made such moves over the 1850s, though perhaps only half as many did so over the 1860s. Although these moves were associated with little improvement in wealth holdings and some significant deterioration in occupational status on average, they were consistent with the expectation of better opportunities in urban places.

Such moves were made more often when the cost of migration was lower, either because of individual characteristics associated with better information about alternative locations or county characteristics associated with lower transportation costs. Migrants were of somewhat lower quality than those they left behind in rural places: they fared worse in urban places than would have those who remained in rural places would have fared had they moved to urban places. Cities drew migrants from areas roughly proportional to their size. Although these findings suggest the responsiveness of rural-to-urban migration to economic forces, it remains to be seen whether the migration produced by those forces was sufficient from the perspective of economy-wide efficiency.

Female Labour Force Participation in Nineteenth Century France and the 1851 Census of Population: A Quantitative Analysis

George Grantham* and Franque Grimard
***Department of Economics**
McGill University
855 Sherbrooke Street West
Montreal, Quebec H3A 2T7 Canada
Telephone: 1-514-398-4841 Fax: 1-514-398-4938
grantham@heps.lan.mcgill.ca

The 1851 French census of population is generally considered to have contained a grossly exaggerated estimate of the French labour force because it is so far out of line with the information contained in later censuses. This paper argues that the census is in fact the most accurate of all the nineteenth century estimates because it explicitly includes as working those persons who were employed on family farms and other businesses in which they were not explicitly remunerated. The paper analyzes in particular the reported occupations of women, based on a sample of 70,000 persons drawn from the nominative census lists of 130 communes in rural France. We find (1) that the labour force participation of women in households headed by farmers was extremely high – on the order of 75 to 80%, (2) that the apart from agriculture, the main determinant of participation was family income, insofar as it can be inferred from the occupation of the head of household. Analysis of the occupations of the wives of farm labourers with and without land indicates that landholding raised the participation rate. The economic plausibility of these findings suggests that the 1851 census estimates are a truthful representation of the French labour force at mid-century.

Collusion in the Indian Jute Industry in the 1930s: Why Did it Not Work?

Bishnupriya Gupta
Department of Economics
University of Essex
Colchester CO4 3SQ United Kingdom
Telephone: 44-1334-462423 Fax: 44-1334-462444
bgupta@essex.ac.uk

This paper examines the collusive agreements at output restriction in the Indian jute industry in the 1930s. These agreements were generally unsuccessful, in contrast to the relative success of collusion in the Indian tea industry. We interpret the reasons for this failure in the light of two different theories: first, repeated game theories of collusion (such as those of Friedman and Green and Porter), which focus on the enforcement problem, and second, theories of cartel stability which focus on the incentives of firms to participate in the cartel, but assume away the enforcement problem. Our overall findings indicate that cartel firms initially behave in line with the cartel stability theories, but over time learn to behave more in concordance with repeated game theory.

Collusion in the jute industry was under the aegis of the Indian Jute Mills Association (IJMA). The agreements pertained to restrictions on working time and were monitored by inspectors. Hence monitoring problems were not so important. However, newer and smaller firms stayed outside the association and did not participate in the output restriction. This is in line with the predictions of cartel stability theory. In line with this theory, the IJMA initially sought to maximize cartel profits by continuing output restriction and did not respond aggressively to punish the outsiders. However, over time the market share of the outsiders increased, and this promoted a debate on strategy within the IJMA, with some members arguing for a more aggressive strategy. Finally, the IJMA changed its response and abandoned the working time restrictions so that collusion broke down. Hence we also find that the cartel learnt to behave more in line with the repeated game theory, and finally ended output restriction as a way of punishing the outsiders, so as to induce them to join the output restriction. We also find some evidence that the attitudes and response of outside firms became more conciliatory in response to this breakdown of the cartel.

Productivity Growth during the First Industrial Revolution: Inferences from the Pattern of British External Trade

C. Knick Harley* and N. F. R. Crafts
***Department of Economics**
University of Western Ontario
London, Ontario N6A 5C2 Canada
Telephone: 1-519-679-2111 ext. 5393 Fax: 1-519-661-3666
charley@julian.uwo.edu

The Crafts-Harley view of the Industrial Revolution with moderate aggregate TFP growth concentrated in relatively few sectors has gained support but doubts persist. Critics [Williamson (1987) and Temin (1997)] point to an apparent inconsistency with Britain's external trade. Temin attempted to discriminate between two views using trade data but his test is valid only in special circumstances. Diminishing returns in agriculture, population growth and imperfect substitution between domestic and imported goods – issues that must be considered – destroy the simplicity of Temin's test. Complications destroy clear theoretical predictions but simulations from computable general equilibrium (CGE) models provide a way forward.

We present a CGE model with what we see as essential characteristics of the British economy. Simulations support our position and also provide valuable insights into the international context of the British economy during the Industrial Revolution. The model extends and revises Harley (1993). It includes diminishing returns in agriculture and demand structures that allow differentiated goods in industry. There are two trading countries – Britain and the rest of the world – and, as before, the model is benchmarked at 1841. Britain consists of seven production sectors: agriculture, industry disaggregated into cotton textiles, other textiles, metal industries, other traded manufactures, other (non-traded) industry (primarily food processing and construction) and services. All except the last two are traded internationally. The rest of the world has an additional tropical agricultural sector that produces both raw cotton and tropical foodstuffs that are imported by Britain.

Our analysis simulates the effect of inferior technology and lower factor supplies representing the British economy prior to the Industrial Revolution. The model replicates the historical results well. Exports of other traded manufactures grow even though the simulation allows zero TFP growth in the industry. Thus British trade fits our view of technological change. Technological leadership in cottons and iron was a major source of export growth, but trade evolved under other important influences as well. Higher population increased the demand for food and British agriculture experienced diminishing returns and rising costs, despite impressive technological change, so food imports increased. Cotton and iron exports increased because prices fell so foreign exchange revenue increased much more slowly than export volume. Old exports continued, despite the absence of technological improvement, because they helped finance expanded food imports. In addition some old exported goods continued to appeal to foreign buyers because they differed from similar foreign goods.

Rapid structural change culminating in a very low agricultural share rather than fast growth still strikes us as the exceptional feature of the British Industrial Revolution. Britain's structural transformation occurred in an open economy context that needs to be understood. The general equilibrium model underlines that both substantial TFP growth in part of the manufacturing sector and diminishing returns in agriculture contribute importantly.

Technology, Efficiency and Entrepreneurial Failure: Canadian and American Manufacturing Firms, 1907-1990

Ian Keay
Department of Economics
McGill University
855 Sherbrooke Street West
Montreal, Quebec H3A 2T7 Canada
Telephone: 1-514-398-4835 Fax: 1-514-398-4938
ikeay@leacock.lan.mcgill.ca

In this paper I use firm level data to quantify the performance of Canadian manufacturers throughout most of the 20th century. I measure Canadian relative to American manufacturers' TFP in nine industries using a Tornqvist index of relative partial factor productivity ratios. The results suggest that, on average, the Canadian industries represented in my sample have not been consistently and substantially less technically efficient than their American counterparts. However, it does appear that Canadian labour and intermediate input productivities have tended to be slightly lower than American. These lower Canadian partials are offset, on average, by higher Canadian capital productivity. This pattern in the relative partials suggests that input prices may have played an important role in determining the cross-country variation. On average Canadian labour and intermediate input prices have been lower than American, while Canadian capital costs have been higher. These differences between Canadian and American input prices could be responsible for the cross-country variation in the partials if the Canadian and American manufacturers were choosing factor combinations and technology which reflected the input market conditions they faced. Responding to input prices by altering technology or substituting amongst inputs is behaviour which is not consistent with claims of inflexible, myopic and conservative entrepreneurial activity.

To quantify these potential responses to input market conditions amongst the Canadian and American producers in my sample of firms I have estimated input demand systems derived from generalized Leontief cost functions for the nine Canadian and nine American industries represented. The dependent variables in these input demand systems are inverse labour, capital and intermediate input productivities. The cross-country differences in the predicted partial factor productivities have been attributed to cross-country differences in input prices, scale, neutral technology and differences due to domestically unique biases in technology.

This disaggregation exercise reveals that the input price differences between the Canadian and American industries in my sample resulted in the Canadian firms choosing different points on their isoquants and domestically unique technology which reflected the Canadian input market conditions. This evidence indicates that the Canadian manufacturing firms in my sample were responding to domestic input market conditions in at least two ways. The Canadian firms were choosing input combinations which were consistent with cost minimizing behaviour and they were adopting, developing and adapting their technology in a manner consistent with theories of induced innovation.

The total effect was such that the Canadian firms used the inputs which were relatively inexpensive liberally and the inputs which were relatively expensive conservatively. This behaviour is inconsistent with much of the anecdotal evidence quoted in the literature which criticizes the performance of Canadian entrepreneurs.

Peasants' Standards of Living and Capital Formation in Pre-plague England: Some Regional Contrasts

Harry Kitsikopoulos
Department of Economics
New York University
New York, NY 10003-6687 USA
Telephone: 1-718-204-5474
kitsikop@fasecon.econ.nyu.edu

This paper examines the issue of technological diffusion among peasant holdings during the century leading to the Black Death. The dynamics of the medieval economy have been interpreted in the past based on the behavior of manorial estates, due to the greater abundance of records, but little effort has been made in studying the technological infrastructure of peasant holdings.

The main thrust of the argument is that the ability of peasants to innovate was conditioned by a number of factors which varied greatly in regional terms. In pursuing this line of argument, the country is divided into two regions: that of mixed arable farming which encompassed mainly the southern and eastern counties, is contrasted to the western and northern counties which focused on pastoral husbandry. Differences in their ecological profiles, referring mainly to soil conditions, established two distinct "technological matrices" that ought to have been adopted in each case. This distinction is very important because often students of the period apply the same standards in evaluating the behavior of peasants and manorial estates.

The meager evidence that exists regarding the degree of capital formation among peasant holdings shows that northern and western peasants adopted more flexible rotational patterns and occasionally larger quantities of capital inputs, for instance in the form of livestock. This tentative conclusion may appear odd in light of the fact that the north and west of England lacked, in addition to favorable edaphic conditions, a high population density and urbanization rates. The latter would have raised prices thereby inducing peasants to intensify production and would have expanded employment opportunities during the idle periods of the agrarian cycle. Despite the absence of these elements, peasants in the north and west were able to retain a larger proportion of their annual output due to the lower level of seigneurial extractions.

In contrast, peasants in the south-east faced intensified pressures on the part of manorial officials as the growth of trade evolved *c.* 1300, pressures that aimed mainly in reactivating commuted labor services. So, despite the presence of more fertile soils and a denser network of markets, peasants in this region failed to become part of a Smithian scenario that views the market as a stimulus that feeds back to instances of technological innovation.

Finally, beyond its main thesis, the paper differs methodologically from the three traditional accounts of the medieval economy (*i.e.*, Neo-Malthusian, Boserupian, Marxist) in that it rejects monocausal explanations in favor of a wider and more complex set of factors in regard to the process of technological change in medieval England.

Share Liquidity and Industrial Growth in an Emerging Market: The Case of New England, 1854-1897

Peter Rousseau
Department of Economics
Box 6182 Station B
Vanderbilt University.
Nashville TN 37235 USA
Telephone: 1-615-662-9993 Fax: 1-615-343-8492
peter.l.rousseau@vanderbilt.edu

The rapid growth of equity markets in emerging economies over the past decade has prompted policymakers to raise important questions about their macroeconomic impact. Although the relative brevity of this expansion has made it challenging to perform such an evaluation, there remains a strong notion that liquidity promotes participation in equity markets and is thus central to their deepening. Interestingly, the first US market for industrial equities arose in Boston more than 150 years ago, when capital flows were considerably less volatile than those associated with today's emerging markets. This difference makes it possible to gain insights about the long-run effects of growing sophistication in equity markets by studying the full period of Boston's emergence.

One problem commented upon by dealers and brokers in the early days of the Boston stock market was that high par values of industrial equities (usually $1000) limited demand for these securities by placing some potentially interested and willing savers outside of their budget constraints at a time when *per capita* incomes ranged from $100 to $300 *per annum.* This study suggests that decreases in the average par values of traded industrial shares that occurred between 1854 and 1897 eased these participation constraints and increased the liquidity of an increasingly sophisticated market in banking and industrial securities, which in turn fueled the sustained growth of the industrial sector.

From primary sources hitherto unused for scholarly investigations, namely the running annual worksheets of securities price fluctuations which underlie Boston broker Joseph Martin's volumes on the history of the Boston stock market, the paper first formulates and presents broad-based indices of annual prices and returns for banking and industrial equities in the second half of the 19th century, as well as measures of overall market capitalization in these sectors. A set of vector autoregressive models then relates increases in liquidity, as measured by the falling par values of industrial shares, to rising prices and capitalizations of firms traded in the Boston market. Increases in liquidity and the real market value of equity capital in banks and industrials are also linked to higher annual earnings among the region's industrial workers. The results support the view that share liquidity was a key factor in the rise of the US as a classic case of finance-led industrialization.

Is the Skill Premium Technologically Driven? Evidence from the Ford Motor Company

Henry E. Siu and James X. Sullivan*
Department of Economics
Northwestern University
Evanston IL 60208-2600 USA
***Telephone: 1-847-492-1030 Fax: 1-847-492-7001**
jxsullivan@nwu.edu

Recent theory explains observed movements in the skill premium as due to skill-biased technological change. In this paper, we test this theory with historical evidence from the Ford Motor Company during the period 1918 to 1947. We find this to be a suitable environment in which to test the hypothesis of skill-biased technological change given the roles of skilled and unskilled labor, documented instances of radical technological innovation, and the availability of wage and occupation, industry-specific and macroeconomic data.

To test the hypothesis, we conduct vector autoregression (VAR) analysis using wage data for skilled, low-skilled and unskilled factory workers. After identifying instances of skill-biased technological change at Ford, we analyze the impact of these innovations on our system of VAR variables, and in particular, various measures of the skill premium. We compute impulse response functions in order to determine the direction, magnitude, and duration of the impact that our identified "shocks" had on the skill premium.

Our preliminary results show some support for persistent increases in the skill premium being attributable to skill-biased technological change. In response to at least one innovation – the development of automatic welding technology – our baseline impulse response function is large, positive and hump-shaped. We conduct various robustness tests to ensure that the qualitative features of the response function are not peculiar to the baseline case. For the automatic welding innovation, we provide additional evidence for the wages of low-skilled workers exposed to the new technology. These results provide little evidence of wage behavior indicating unique responses to technological change and, thus, do not refute the hypothesis that new technology affects only the relative wages of skilled labor.

Spatial Insights into the Relationship between Unemployment and the Nazi-Vote Twilight at the End of the Weimar Republic

Christian Stögbauer
Department of Economics
University of Munich
Ludwigstrasse 33/IV
Munich D-80539 Germany
Telephone: 49-89-21 80-53 77 Fax: 49-89-33 92 33
Christian.Stoegbauer@econhist.vwl.uni-muenchen.de

In the course of the Great Depression unemployment in Germany soared from 1.1 millions in May 1928 to 6 millions in March 1933. At the same time, the radical parties NSDAP and KPD increased their share of the vote from 13% to 56%, while the electoral support of all other parties considerably decreased. Voters in the final phase of the Weimar Republic voted obviously not only for an opposition within the system but an opposition to the system. In such an economic situation one would expect unemployment to have had a positive impact on the share of the vote of both radical parties. Although on the level of the administrative unit of the Kreise a strong positive impact of rising unemployment and the Communist vote could be detected, the NSDAP-vote was found to be negatively correlated with the unemployment rate.

By using techniques of spatial data analysis this paper aims at showing that in spite of the overall negative relationship between unemployment and the Nazi-vote for the entire Reich, there were a considerable number of spatially clustered Kreise where the relationship was positive. This indicates that there were regional contextual variations in the voting behaviour of the unemployed, meaning that the cleavages in the final phase of the Weimar Republic were not so clearly defined as previously supposed. Considering this contextual variation, a spatial model of the Nazi-vote for a subset of Kreise with a positive relationship between change in unemployment and change in the NSDAP-share is specified. This model is applied to make a crude counterfactual estimation: we explore the extent to which in spite of an overall negative relationship between unemployment and the NSDAP-share a decrease in unemployment in a subsample of Kreise with a positive relationship between unemployment and the Nazi-vote would have lowered the total NSDAP-share. It will be shown that at least for our simple counterfactual model such a policy would only have had a minor effect.

Monetary Policy and the Great Depression: The Role of a Monopoly Federal Reserve

Mark Toma
Department of Economics
University of Kentucky
Lexington KY 40506 USA
Telephone: 1-606-257-1940 Fax: 1-606-323-1920
mtoma@pop.uky.edu

The Federal Reserve System that emerged from the aftermath of World War I consisted of 12 reserve banks each having the power to conduct open market operations for its own account and each financing itself from the earnings so generated. To the extent that monetary economists have commented on this peculiar period in Fed history, they have tended to spin a story of fragmented decision-making and disjointed policy. Fortunately, according to the conventional wisdom, the System became more adept at coordinating monetary policy over the course of the decade. The lessons were soon forgotten, however, as fragmented Fed decision-making from 1930 to 1933 contributed to the severity of the Great Depression. The basic implication is that decentralized open market operations create problems of monetary contraction that can be overcome only through cooperation.

As an alternative to the conventional account of the Great Depression, this paper develops an industrial organization (IO) model of the Fed that views decentralized and centralized open market operations as competitive and collusive activities. While the Fed is viewed as a competitive system in the 1920s, a government sanctioned open market cartel committee was formed in the spring of 1930. For the 1920s, the primary prediction of the IO model is that a reserve bank will conduct open market operations when its payoff, as measured by a competitive index, is positive. A second issue is the impact of open market operations on total Fed money. Under competitive conditions, open market operations lead to a scissors effect at the industry level – an increase in government security holdings is offset by a decrease in discount loans, leaving total Fed money unchanged. Finally, the IO model predicts that with the shift in market structure in 1930 the competitive index will lose its predictive significance and open market operations need not result in a scissors effect.

Fortunately, the Federal Reserve *Bulletin* provides a readily available but largely untapped source of data on the balance sheets of each of the 12 reserve banks over the 1922-1929 period, which can be used as the basis for a series of regression tests of the IO model. Overall, the results strongly support the implications of the IO model. Open market operations in the competitive setting of the 1920s acted as a safety mechanism that worked more or less automatically. A corollary is that the safety value does not work if shut off. A restrictive monetary policy outcome would be expected to emerge and, as indicated by regime shift tests for 1921-1933, actually did emerge when the Fed's monopoly powers were broadened in the spring of 1930 to include open market operations. The results shed new light on the conventional view that open market policy was unduly restrictive during the Great Depression. Monetary contraction is a predictable outcome of a change in market structure from competitive to monopolistic.