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The Cliometric Society

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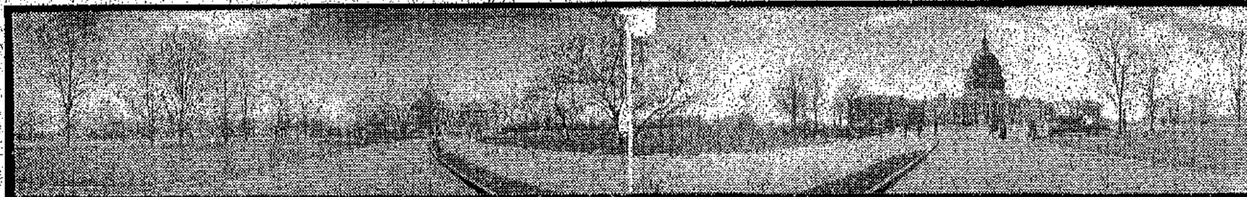

Capitol and Congressional Library c1904

The Cliometric Society and Economic History Association Sessions at ASSA 2003

By Jeffrey Owen, Indiana State, and Michael Haupert, UW–La Crosse

(Washington D.C.) Clioms joined their economist brethren in the nation's capitol for the annual gathering of economists at the Allied Social Science Association meetings January 3-5, 2003. The Cliometrics Society sponsored three sessions and the Economic History Association added two more – all packed into one busy Saturday.

The conference got off to an early start on Saturday morning with an uplifting session on three of everyone's favorite topics: death, taxes, and public spending. **Joerg Baten** (Tuebingen) presented the work he and **Andrea Wagner** (Munich) have done on autarchy, market disintegration, and health during the German Nazi regime from 1933-37. They analyze the development of the biological standard of living in Germany before the outbreak of WWII. Despite increasing real GDP per capita during this era, the welfare of the general population was not improving, since much of the expenditure went for public projects, including rearmaments. They determine that, contrary to popular view, the Nazi economic policy was not successful in raising the welfare of the majority of the German population during the early 1930s. John Brown (Clark) praised the work but wanted the authors to nail down the mortality evidence by comparing it to other eras. He wondered if it looked as bad relative to the immediate postwar years of the 1950s. He also asked about non-price control restrictions on consumption, such as import restrictions on beef. Gillian Hamilton (Toronto) noted that Gestapo reports are available in archives and should be examined. These are reports of general conditions in German cities during the prewar years and could yield valuable insights. Bruce Davie (Georgetown, Treasury) wanted to know if there was a public campaign to encourage women to breast-feed in an effort to conserve scarce milk supplies, and Pamela Nickless (NC-Asheville) was curious about the existence of mothers' allowances for children.

Werner Troesken (Pittsburgh) presented his work with **Joe Ferrie** (Northwestern) entitled "Death in the City: Mortality and Access to Public Water and Sewer in Chicago in 1880." Troesken and Ferrie link

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What's Inside

Executive Director's Notes

Greetings Gentle Members:

Many of you may not know this, but for many years your Executive Director has sailed between a personal Scylla and Charybdis on the issue of path dependency in general and the QWERTY keyboard in particular. The problem is that a distinguished member of the Cliometric Society, a certain Professor Paul David, is one of the more prominent persons associated with research on path dependency, and he published a famous article on the inefficiency of the QWERTY keyboard. Since Professor David is a Clio member who actually pays his dues regularly, as a trustee and the financial agent of the Society, I have hesitated to alienate him on the QWERTY front. Furthermore, when I was a graduate student and he was already a "grand old man" in the profession, he generously offered a detailed written evaluation of my research, even though I was neither his student nor even enrolled at his university. So, I've long had a soft spot in my heart for Paul David.

On the other hand, among the leading scholars on the other side of the path dependency/QWERTY debate, we find Stan Liebowitz and Steve Margolis. Professor Liebowitz is a former colleague, and Professor Margolis is not only a friend and current colleague, but has been for many years the head of the Economics Department at North Carolina State University – my university and the HOME OFFICE of the Cliometric Society. As department head, he makes recommendations to the dean concerning my annual salary adjustments; therefore, my children's orthodontist is very interested in my maintaining a good relationship with Professor Margolis. In addition to my narrow financial concerns, you can also think of Professor Margolis as the Society's landlord – a landlord who, by the way, currently offers us a very generous lease.

So you see my predicament. Well, I am proud to tell you that academic integrity has finally won out, and I am now prepared to share my position on this long-standing, scholarly debate. Following, with apologies to David Letterman and thanks to Dave Margolis, are the: Top 10 Reasons for the Universal Adoption of the QWERTY Keyboard.

Lee A. Craig,
Executive Director

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TOP TEN REASONS FOR ADOPTION OF QWERTY KEYBOARD

10. Ability to type cusswords using the shift and top-row keys saves cartoonists countless man-hours.

9. Z and P went camping one time and had a couple too many shots of schnapps late one night, and one thing led to another, and, well, it's just better this way, let's leave it at that.

8. Dvorak sounds like one of them foreign names.

7. Early sales bolstered by "bundling" with free celluloid collar and stereopticon viewer.

6. Ideal placement of the Hitchcock and the Reverse Hitchcock keys.

5. Improved efficiency over the rival Brooklyn Typomatic Corporation's QWOITY keyboard.

4. Produces a pleasing tappitty-tappity-tap sound.

3. Popularized by hard-bitten newspapermen won over by the extra "ashtray" key located next to the semicolon.

2. It worked just fine for George Stigler.

1. Acceptance of the highly efficient H.I.T.L.E.R. keyboard waned in the 1940s.

Call for Session Proposals

XIV International Economic History Congress

Helsinki, Finland
August 21-25, 2006

The deadline for the first call for session proposals for the next World Congress of Economic History in Helsinki is April 30, 2003.

If you have not yet submitted a session proposal, please check out the guidelines on http://www.neha.nl/ieha, and submit your proposal, PREFERABLY BY EMAIL, to:

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An Interview with Charles Feinstein

Charles Feinstein was born in Johannesburg in 1932. He graduated from Witwatersrand University with a B. Com. degree in 1950 at the age of 18. Charles qualified as an accountant in 1954 before leaving South Africa for Cambridge, where he completed his doctorate in 1958. He remained in Cambridge until 1978, first as a research officer in the Department of Applied Economics (1958-63), then as a University Lecturer in Economics and Fellow of Clare College (1963-1978). He was Senior Tutor at Clare from 1969 until his departure for the Chair in Economic and Social History at the University of York in 1978. In 1987, he moved south to Oxford, first as Reader in Recent Economic and Social History and Fellow of Nuffield College and then, from 1989, as Chichele Professor of Economic History and Fellow of All Souls College. Charles retired from the Chair in 1999. Among other administrative charges, he served as managing editor of the ***Economic Journal*** *(1980-86), chairman of the Economic Affairs Committee of the Social Science Research Council (1985-86), and Vice-President of the British Academy (1991-93). He is perhaps best known for his work on the British national accounts between 1750 and 1939, although he has also written on Russian economic history, technology transfer in China, and, most recently, on the development of the South African economy. This spring will see the publication of a volume in his honor,* ***The Economic Future in Historical Perspective****, edited by Paul A. David and Mark Thomas, to be published by Oxford University Press for the British Academy. This interview was conducted by Mark Thomas (University of Virginia) in Professor Feinstein's rooms at All Souls College, Oxford in August 2002.*

I'd like to begin by asking you about your background. You were born in South Africa but have spent almost the entirety of your academic career in Britain. How did you become interested in economic history?

Well, I suppose the beginning would have been in school, where the subject I found most stimulating was history. But when it was time to think about university courses, history didn't seem to have any career associated with it. The real choice was between taking a B.A., which would lead to a law qualification, or a B. Com. I chose the B. Com. degree, and that was how I was introduced to economics. At the University of Witwatersrand, the degree involved courses in economics, economic history, and accounting. I do remember that part way through I decided that really this was all a mistake, and I would have been better doing history and a B.A. I tried to change, but the University wasn't interested, so I persisted with economics and economic history.

I was very fortunate in having two very stimulating lecturers. The first, in South African economic history, was Helen Suzman, who subsequently became very famous as a liberal member of parliament. More significantly, perhaps, in my second year, the University appointed Lionel Lachmann to the Chair in Economics. He came to South Africa as a refugee in two senses – he had been a refugee from Austria in the 1930s and had come to England. He was very much an Austrian economist, and I think he found the very strong Keynesian climate in England in the early postwar period uncongenial and probably (although I don't know this) was unable to get a job in England. So, he came to South Africa, and I still remember the excitement of this new professor coming from Europe and lecturing to us in his very heavy accent. It completely transformed what I understood by economics and the quality of intellectual thought that went into it. By the time I finished my degree, I was anxious to study economics as a way to contribute to solving the economic problems of South Africa and the world. My parents were very doubtful; they had never met a professional economist. They said that if I would first get a qualification which would enable me to earn a living, they would then support me to go overseas and study economics (there were no grants available in those days). So, I did three years of accountancy and became a qualified chartered accountant. I didn't enjoy that at all, but I think it had a very substantial effect on me, and it was something I was good at. It clearly influenced, although not consciously, what I did subsequently. And, at the end of that period, I came to Cambridge to study economics.

While I was doing accountancy, Lachmann started an Honors Degree in Economics, which three of us elected to do on a part-time basis. The teaching for the course was entirely conducted by Lachmann in a very idiosyncratic way. He chose a book, and we met to discuss it. It was a very detailed exegesis, page by page and line by line. The two books that occupied most of our time were Joan Robinson's *The Rate of Interest and Other Essays* and Bent Hansen on the *Theory of Inflation*. It wasn't really the importance of these books that mattered as much as the training and the method of taking each sentence and each word and thinking about what it meant. At the end of the course, we had a variety of exams and submitted a dissertation. I did very well in the exams, but my dissertation was a disaster. I chose to write something which reflected my political and Marxist interests at the time and was designed to show that it was quite wrong to reject the labor theory of value as orthodox economics had done. I didn't really understand what I was doing; it was heavily derivative, relying on Maurice Dobb and, more particularly, Ronald Meek. The external examiner was W. H. Hutt of the University of Cape Town, and he found any suggestion that there was merit in the labor theory of value totally unacceptable. As I later came to appreciate with some relief, instead of just giving it a third class mark (which might have destroyed my career), he failed it outright. Although I got first class marks on the exam, I was held to have failed the degree. I had already been accepted to go to Cambridge and wrote in great dismay to Piero Sraffa, who was in charge of graduate admissions. For whatever reason, he said don't worry, just come, and we will evaluate you here. So I came, and it was forgotten.

You went from studying with Lachmann, an arch anti-Keynesian, to Cambridge, the heart of Keynes' territory.

That didn't really concern me. I certainly wasn't an "Austrian," but I didn't think of myself as a Keynesian either. I don't think I would have said I was a Marxist either, since I had doubts about several aspects, but I was very much on that side of the divide. My reason for going to Cambridge was overwhelmingly to study under Maurice Dobb, but the faculty assigned me to be supervised by someone else, Malcolm Fisher. However, I saw Dobb on a regular basis, got to know him well, and edited a *Festschrift* for him.

Your parents had committed to helping finance this Cambridge trip. Was the expectation that you would go for two years and then return to South Africa?

It was fairly open-ended. The initial commitment was for one year, because I was admitted to do the one-year Diploma in Economics. But my parents were very keen that I should not return quickly to South Africa, as I had been heavily involved in what they saw as dangerous political activity, and when I did sufficiently well in the Diploma to be able to stay on to do a doctorate, they were willing to continue to support me.

Before I left for Cambridge, I was asked to review a book by Palme Dutt, the leading economist of the British Communist Party, for the South African Communist Party newspaper. It was titled *The Crisis of Britain and the British Empire*, in which he argued that the viability of the British economy was entirely dependent on looting the Empire, which would come to an end as the Empire gained its independence. Capitalism in Britain would therefore be undermined, and since this would apply equally to other capitalist countries, the world revolution would come, and we would all be happy.

Given the atmosphere in South Africa at that time and also what was happening in the world economy – the success of the Chinese revolution and the strength of the Communist Party in France and Italy – it wasn't as absurd as it now seems to imagine that capitalism was in serious trouble. I was very taken with this book and saw imperialism as the subject on which I would work at Cambridge. I had realized from my efforts to write a theoretical dissertation that I was never going to do anything worthwhile on theory or the history of thought, but I thought that exploring the Empire historically and in a more scholarly way than Palme Dutt had done would be something I could do well. I still remember very vividly the idea being shot down by a single sentence from Joan Robinson. I met Joan at a meeting of the Cambridge University Socialist Club, and she asked me what I was going to be doing for my doctorate. I told her and she said, in a very brusque and dismissive way, "That's absurd." And when I asked why, she said, "How can you explain the prosperity of the Scandinavian economies if it is all due to Empire?" I went away and wrestled with that for some time and decided that she was right. It fit in with a whole number of other things happening to me at that stage. Once I got out of the hothouse atmosphere of South Africa and could reflect in the cooler climate of Britain, I remained left wing in my attitudes but came to realize that a lot of what I had believed, particularly about the Soviet Union and about imperialism, was untenable. I didn't abandon the subject, however. Cairncross's *Home and Foreign Investment* had just been published, and I thought there was still room to do a scholarly analysis, although it wouldn't be designed to lead to the conclusion that it was all going to collapse. So that was how I came to my thesis topic.

Can you tell us about the themes of your thesis, *Home and Foreign Investment, 1870-1913*?

I suppose there are two parts to the story. One part involved Prest's national income figures. I wanted to use his profits series as a way of looking at the profitability of the British economy in the period of high imperialism. When I came to do that, I immediately struck a problem: Prest hadn't disaggregated profits from the other components of non-wage income. I started out trying to do that, since it seemed to me to be essential to have that information. But as I got into it, I found some problems in what he had done, which led progressively to reconstructing the national income estimates. That became perhaps the most important chapter in the thesis. It obviously had an enormous influence on what I did afterwards.

At the time, I didn't see it as the heart of the thesis. That was meant to be the analysis. A lot of the thesis was devoted to trying to elucidate the interaction between home and foreign investment. One of my arguments, which was novel at the time, was that the critical interaction wasn't between domestic manufacturing investment and overseas investment, but between housing and overseas investment. I remember giving one of the chapters to Postan's seminar, and he was very enthusiastic. He was editor of the *Economic History Review* and said, yes, you must write it up and we'll publish it. But I never had sufficient confidence that what I had done was adequate. It always seemed possible to go on improving the analysis. And so it never appeared. I regret that in some ways. The only part of the thesis that was published was a paper on the national income estimates that was accepted by the *Economic Journal*.

There is a certain path dependency in all things, not least in academic careers. You went on to further work on capital formation and that pivotal period between 1870 and 1914, and the thesis helped shape your future commitment to statistical reconstruction of the British economic historical record. How did things unfold?

The Department of Applied Economics (DAE) at Cambridge advertised two vacancies at about the time I was finishing my thesis, and the two people who were appointed were Brian Mitchell and myself. His appointment was to work with Phyllis Deane and Max Cole on the *Abstract of Historical Statistics;* mine was to work with Jack Revell on the National Wealth. I had made one very minor contribution to that, doing some research on shareholding, when Brian Reddaway summoned me. I can still remember very vividly coming into the DAE one morning, when Brian, in characteristic fashion, accosted me from the top of the staircase and said, "Charles, come up here, there is something I want you to do." My task was to complete Maywald's work on domestic capital formation in Britain. Maywald was supposed to finish the book but was unable to do so to Reddaway's satisfaction. There was absolutely no meeting of minds and eventually relationships broke down and I was called in. The original brief was simply to bring the book to a state where it could be published. However, once I started working on it, I found more and more things where I thought revision was essential. After that, I began work on a larger project of reconstruction. Richard Stone had initially launched the project on the retrospective national accounts. The first volume on consumers' expenditure was his own work, and there had been the Prest volume, the Chapman volume, and now the Feinstein-Maywald volume on capital formation. But by the 1960s, he had lost interest in the project and moved on to other things. It seemed to me extremely unfortunate that all this work had been done on the components of the national income but nobody was going to pull it all together and provide the key series for GDP. So I went to Stone and Reddaway and said I would like to do this.

Who were your primary influences while at Cambridge?

Without question, by far the most influential person in shaping the work I did was Brian Reddaway, who had succeeded Stone as Director of the DAE. I learned an enormous amount from him. I obviously couldn't emulate his own skills, his incredibly penetrating critical faculty, but I could learn from his approach, his remarkably clear sense of what was important, and his ability to focus on that without being diverted into less important matters. This was crucial in relation to understanding what one should be doing and in writing it up so that the important points were clearly conveyed to the reader.

You were in a Department of Applied Economics, not an Economic History Department, and were then appointed to a lectureship in the Faculty of Economics. Did you think of yourself as an applied economist rather than as an economic historian?

I certainly did and, I suppose more relevantly, many others did, but I don't any longer. When I was in the DAE, I did a lot of applied economics, for example, writing articles on the current state of the economy. Soon after joining the Department, I was appointed statistician of the London and Cambridge Economic Service, which horrified me, because I didn't think of myself as a statistician. In those days, the London and Cambridge Economic Service produced a lot of statistical tables for which there was no official counterpart. The Service was a pioneer – it produced the first seasonally adjusted series, the first quarterly series of the national income, and so on. And the work of the statistician was to produce the numbers, so I had a crash course in British economic statistics.

Did you have much contact with the economic historians in Cambridge?

The economic historians who were at Cambridge when I was there were mostly in the history faculty with interests that didn't overlap with mine at all, and they very often worked in earlier periods. The initial Professor of Economic History was Postan, from whom I learned a lot, but that had nothing to do with the work I was doing; it was more just the stimulus of his intellect and range and his dynamic personality. His successor for a very brief time was David Joslin, who once again didn't have much overlap with me. And then there was Donald Coleman, with whom I worked closely together on faculty matters, but our approaches to economic history were so very different. More important were the applied economists who had done some economic history, especially Robin Matthews, my thesis supervisor, who invited me to collaborate with him on the study initiated by Simon Kuznets and Moses Abramovitz that became *British Economic Growth, 1856-1973*.

What was it like writing about capital formation in Cambridge at the height of the capital controversies? Did you perceive any tensions with these new theoretical approaches, which seemed to suggest that trying to measure capital was impossible?

Yes, I did. One couldn't be in Cambridge and not be conscious of the intensity and the fervor with which that debate was conducted. And I obviously had to think it through. The resolution I arrived at quite quickly was that although we were both using the word capital, we were really doing different things. They were saying that there was an inherent circularity which couldn't be overcome: you needed a measure of capital in order to estimate future profits, and you needed to estimate future profits in order to have a measure of capital, and there was no way around that. Whereas I was not trying to estimate the future value of the capital stock in that sense; I was trying to estimate how much money had actually been spent in the past on creating the stock of capital.

And was this a point the critics accepted?

I don't think they ever looked beyond the theory. Nothing else mattered.

Your first exposure to American cliometrics and American economic history came in the late 1960s.

Yes. When I was appointed lecturer in the Economics faculty, the expectation was that I would follow the person I was replacing, Frank Thistlethwaite, and teach American economic history. I said that I would rather lecture on Russian economic history. So I was sent to Moscow for two months to learn the language and study Russian economic history. A more exciting consequence was that the choice enabled me to go to America for a year in 1967/1968, when I had my first sabbatical leave. Postan had introduced me to Gerschenkron, who was visiting Cambridge, and when I said that I would like to visit the Russian Research Center at Harvard, he offered to arrange that. I spent a lot of time that year reading microfilms in the Harvard Center on prewar Russian economic history. I did quite a lot of research, but I wasn't very satisfied with the papers I wrote, and nothing really came of it. During that same year, I was also finishing the national income book, so I was moving between the two projects: working with Bergson on the Russian side and also occasionally seeing Kuznets.

You attended the Gerschenkron seminar while you were at Harvard. What was your reaction?

It was tremendously stimulating. There were some remarkable people there, most obviously Gerschenkron himself, who was very warm as a host. I still have very fond memories of our meetings. The seminar used to be held after dinner, and afterwards he would invite me to his room for a brandy. He always began by wanting to discuss baseball, but with a certain amount of effort, I could bring him around to more interesting topics. I also got to know Peter Temin and saw a lot of him that year and some of the others who were in the Gerschenkron seminar, such as Dick Sylla, Peter McClelland, and Bobby Solow. It was perhaps the first time that I felt that I was part of a community of economic historians, because in Cambridge I didn't have a strong sense of an academic community with shared problems and shared interests. In Harvard I did.

And you also went to that hotbed of cliometrics, Purdue, for their annual meeting.

Yes, I didn't know much about it beforehand. It was Peter Temin who suggested I accompany him, so we flew out together. It was my first encounter with people like Bob Gallman and Doug North and others and was a very exciting occasion. I particularly remember a paper by Bob Gallman criticizing some of the assumptions that underlay Doug North's work on interregional links in the American economy, and I found that really very interesting.

What was the tenor of the sessions? Were they more combative than in Cambridge?

No. I don't know if you have heard the famous quip by Bob Solow – that a Cambridge seminar consisted of Joan Robinson talking for 75% of the time and Nicky Kaldor talking for the other 75%. That was very much Cambridge (it was a very sharp place in those days), so I found America in some ways more restrained.

Were there glimmerings of a similar revolution in Britain?

I've always thought that the Americans needed the cliometric revolution, because their work had lacked quantitative analysis entirely; whereas in Britain, we'd had a very long tradition of it. This was not cliometric in the shiny sense that it developed in America, with neoclassical economics and econometrics at its core, but it was deeply quantitative in terms of measuring what had happened and making the numbers the basis for any analysis. For me, there were three exemplars of that style of work in Britain: Alec Cairncross, Robin Matthews, and W. Arthur Lewis while he was at Manchester. Brinley Thomas was also very influential. So I didn't find what was being claimed in America something that we'd never thought of in Britain. It seemed to me a very well-established and significant tradition in Britain, and I saw myself as part of that. The American revolution was also influenced by the personalities of some of the leading figures and their strong desire to proselytize, which was absent in Britain. Apart from personalities, I think there was a sense in America that this way of doing history had to fight against other ways, and that struggle was reflected, for example, in the tensions over slavery. There was no counterpart to this in Britain. After my visit to America and after McCloskey organized the conferences on the British economy (first in America and then here), we were all drawn into the movement. Some of the work that was done then – for example, Sandberg's work on the cotton industry – was in my view a very important contribution to British economic history. Some of the other work perhaps less so.

Do you consider yourself a cliometrician?

You would have to define cliometrician.

How about if one defined cliometrics as testing hypotheses with a combination of economic theory and formal quantitative methods?

I would have no difficulty with that. I would simply have to say that it is obvious that my own contribution has been primarily in providing the data, not in testing hypotheses.

Have you regretted not having followed that econometric path, if only because others have?

No. I would regret it in the sense that I regret that I am not able to open the batting for England. It would be fine if one could do it, but I've no doubt that I wouldn't do it terribly well.

So, comparative advantage is the correct way to think of it?

It is partly comparative advantage, and it is partly temperament, though you might say that is part of comparative advantage.

The quantitative work that you have been involved with has contributed to two primary historical debates: the first is the debate over the so-called climacteric at the end of the 19th century; the second is the debate over industrialization and its consequences. Let's talk about the first. Not everyone believes in the climacteric. Do you?

Yes. But you have to look behind that. I certainly believe that the evidence from the real wage side, showing a pronounced slowdown in the Edwardian period, is very strong, and attempts to wash it away with sophisticated statistical techniques don't persuade me in the least. I believe, as I have for many years, that the root of the problem in the British economy had to do with labor relations and a combination of attitudes on the part of the workforce that were detrimental to productivity, reinforced by employers' refusal to recognize what would have been necessary to overcome those attitudes. I think this class-based attitude of employers towards the working class and workers' response to that was also an extremely powerful factor in Britain's early post-1945 problems, though comparative performance in that period was dominated by the catching-up process in countries like Germany, France, and Japan. I think that it took a long time before Britain saw its way through that legacy.

The early literature that built on the concept of the climacteric emphasized economic decline, while the fashion now is to deny any decline in Britain. Do you subscribe to that?

To a large extent, yes. I joined the catching-up school very early and published a paper in which I argued that most of what happened after 1948 could be explained simply by where the different economies started. Similar arguments would have applied earlier. But because of the way I presented the results, it didn't make the impact it might have done if it had been presented in econometric terms.

If we had a holistic view of national income accounting, which would incorporate these elements, then Britain would actually still be growing.

It is not so much an issue of growth rates but that 19th-century Britain wouldn't be seen as a failure. I think that is the right interpretation, and it applies more strongly to the late Victorian or Edwardian period than to the period after WWII. In the early post-1945 period, there were more pronounced problems of arrogance and incompetence and a failure to recognize how the world was changing, which you couldn't simply justify away in wider welfare terms, but in the 19th century, yes.

Have you therefore been tempted to try to move towards broader measures of welfare, which would incorporate these factors?

No, I doubt whether they could be quantified. What I would like to have done (and have started but left unfinished) is to write a history of the postwar period in which this would be one of the themes.

You have also been drawn in recent years to the period of industrialization.

Yes, this goes back to the completion of the work on capital formation and my second visit to Harvard in 1987-88, where I thought I would go back to my early ideas on imperialism. But then shortly after I arrived, I saw a copy of *Explorations* with an article by David Greasley criticizing my work on wages, and that led me to think about wages. I had found capital formation in the end rather arid. I was dealing with things that had no human interest, whereas once I got started on issues of wages, that opened up questions such as the standard of living. And it also linked up with work which went all the way back to my dissertation on the climacteric. Having started on that project, it seemed desirable and interesting to extend it back to the late 18th century. That then overlapped with the debate about the pace of economic growth and the nature of the Industrial Revolution. It seemed to me that all the debate was being focused on the Hoffman index and output data and was ignoring the evidence available on the income side, so I thought I could make a contribution by working on the income estimates. The original project was to start on wages and then extend through into the other elements, and in some ways, I'd still like to do that. But, having completed the work on earnings and the cost of living, which led to contributions to the pessimism debate, I got involved with the issue of the relative size of value added in different sectors of the economy, which in turn led to my current project. It has occupied me for the last three or four years and involves constructing a very detailed social accounting matrix for 1851.

Let me ask you this about the pessimism argument. The combination of slow growth and stagnant real wages has implications for the distribution of income

during industrialization. This takes you back to an earlier controversy, perhaps the one most familiar to cliometricians, the Kuznets curve debate: the argument that industrialization is accompanied by a rise in inequality, which gradually diminishes over time. You were sharply critical of that argument in your famous review of *Did British Capitalism Breed Inequality?* Would you like to talk a little bit about the origins of the contribution and any new thoughts you've had on that in the last 15 years?

I don't think I've particularly had any second thoughts. I think the thing to be said is that I didn't approach that project, or the one on pessimism or any other controversy I've been involved in, with a strong *a priori* view. This may seem rather surprising for someone who started off with dogmatic Marxist views, but perhaps abandoning those led me to become generally more agnostic. My attitude has typically been to do the research needed to find out what the data can tell us and to report that as faithfully as I can. In the particular case of the inequality debate, I hadn't previously given much thought to the Kuznets curve or to the underlying theory that Jeff Williamson had developed. I was asked by the *Journal of Economic History* to review his book, and I took it with me to the States, thinking it was something I would do during my sabbatical year. When I had agreed to write the review, I thought it was a rather daunting thing to undertake, because I was aware that there was a lot of general equilibrium modeling in the book with which I wasn't familiar. I began by thinking about one of the measures that Jeff had used to produce the results about inequality – the tax data – and became aware that it was flawed. I then got caught up in work on wages and set the review aside. Nine months later I moved to Stanford for the summer and took up the review as something it was now urgent to get done. I started by looking at the next measure of inequality and found that there were problems with that, and my review went through progressively. As I took up each measure and looked at the procedure that Jeff had adopted or the sources he had used, I found that there was something wrong. I think there were seven measures that Jeff used. He said that some of these may be deficient because the data are uncertain, but they all point in the same direction. I had worked through five of them, thought that was enough for the purposes of the review, and sent a preliminary version to Jeff and also to Peter Lindert. One of the responses I got back was well, you haven't said anything critical about the other measures. So I then looked at those and particularly the estimates Williamson had derived from the work of Colquhoun and the other social arithmeticians. I found those too were flawed. Having gone that far, I asked myself this question: If all the estimates that underpin this are seriously flawed, how does the general equilibrium model produce the results that it did? That became the final part of the critique. I should say that I have always greatly admired Jeff's ingenuity and his innovative approaches to economic history and still do, so there was no personal animus in it. I had certainly not set out to do anything destructive. If it ended up being highly critical, that was simply because intellectually that was where the numbers led.

But doesn't the combination of slow but positive growth and stagnant real wages over that crucial period from 1780-1840 point to some movement in the distribution of income? Does this suggest a reconsideration of the legitimacy of the Kuznets curve?

It might, but what it leads me to again is to think that it would be extremely useful if one could get a better grasp of the quantitative record of what happened to the non-wage components of income. If I were going to make any sort of contribution, I could do it more effectively that way rather than by speculating about what might or might not have happened to the components that I hadn't been able to measure. So, what I had in mind when I finished the pessimism paper was that I would try to cover the other components of income. I have collected a lot of evidence towards that, but there is still a long way to go. And some parts, particularly in relation to trading profits, may not be amenable to quantification. I have also been thinking more about the output side, which is how I got diverted into wanting to know more accurately about the composition of value added, sector by sector, and that led to the construction of the 1851 matrix.

So, as part of your philosophical approach, leaving something as a residual is not really good enough.

That's right. And also (perhaps as a result of my work on capital formation), I am very conscious of the length of life of assets. I think that the assets I construct are more likely to prove durable if I do one type of work rather than another. It might be more exciting and more intellectually demanding to try and do more speculative and theoretical research, but I doubt that it would make a lasting or worthwhile contribution.

Do you see yourself as a sort of archeologist of numbers?

I wouldn't have put it like that, but there is clearly an element of truth in that description. It is partly a matter of knowing where to look and of uncovering "lost" information, but much more of it is seeing that information in perspective, assessing its strengths and weaknesses, knowing how it relates to other evidence and to the historical context, and deciding how it can be used and how it should be interpreted. A paleontologist who reconstructs an entire species from a single skull or a leg bone is perhaps another apt analogy. I think it comes out most clearly in the social accounting matrix I have compiled for 1851, because it is in many ways the most elaborate project I've ever done. And, because it all relates to only one year, one can spend far more time on each sector than one could afford to when doing a long-run series. Plus, I get a certain satisfaction in finding evidence from disparate sources and establishing that they are in fact consistent and that it is possible to reconstruct how the economy actually functioned, even for a time when there were no censuses of output.

In essence, your comparative advantage is not just patience but also the skill and judgement of being able to discern what is the right number and what is the wrong number.

I would be pleased if my work was evaluated on that basis.

One change associated with the 1990s is your return to South Africa after a gap of over 40 years. You left to go for one year to Cambridge and then for a second year, and as the political situation in South Africa deteriorated, you chose to absent yourself. Now, however, you've resuscitated your connections. What was it like returning?

It was very, very exciting. I was extremely pleased with the success of the transition and am still moderately optimistic about its prospects. There are a lot of things that are enormously worrying, most obviously in relation to the government's response to the HIV/AIDS crisis. But much is happening that is highly encouraging, and I find it an extremely vibrant and interesting society to live in. I get up in the morning and switch on the radio with far more enthusiasm to hear what's happened the previous day than I did in Britain, where politics seem to be incredibly repetitive and usually trivial (though war with Iraq may change that). Most of the time, you go away for three months and you come back and say, "What's happened?" and the answer is, "Nothing." It's not like that in South Africa. I get a lot of pleasure, interest, and stimulus from observing the democratic transition. The fact that I've been able to go back more or less every year now for the last decade and to teach at the University of Capetown has been very fulfilling.

You've not only returned to South Africa to teach, but you are now taking up the challenge of returning to South African economic history.

I am thinking about that. Until recently, I've had so many unfinished projects on Britain that it seemed more sensible to finish them before moving on to something new. But I am near a turning point now where I can see space to do something on South Africa, and I find that a very attractive challenge.

What are your immediate academic plans?

My immediate priorities are to write up the work on the social accounting matrix for 1851. It began as a short paper and has grown inexorably into a large book, and I have a couple of chapters still to write. Then I have to complete the Ellen MacArthur lectures that I have been invited to give in Cambridge in the autumn of 2003. The economic history of South Africa will be the subject for these. Where I go after that depends partly on how the lectures turn out. If it seems fruitful, I may do more work on South Africa. Almost certainly, I will also undertake projects based on the results of the social accounting matrix. Using the estimates of value added, for example, may allow us to resolve some of these issues we've talked about. We can look backwards from 1851 to analyze growth patterns and issues of income distribution over the Industrial Revolution and look forward over the period of the climacteric.

What about the future of economic history in Britain?

It is clearly in a contraction phase at the moment, but even in this period of depression, the students who have come in have been very good, and some outstanding recruits to the profession have emerged. My fundamental conviction is that the intrinsic interest in economic history, the importance of the problems it addresses, and its ability to draw on the strengths of both history and economics to create something which, in its sphere, is stronger than either of them alone ensures that it will remain viable and that it will always attract good students.

Where will its home be? Those who have a more pessimistic view argue that economists are becoming increasingly intolerant of applied economics and that they are becoming much more interested in pure theory or econometric theory, while among historians, the cultural climate has turned empiricism into a dirty word.

I think that both of those are yesterday's attitudes. In economics, the evidence is very strong that people are already moving away from that approach. Obviously, there is still a profound interest in theory, but there is also a strong and growing interest in more relevant practical applications. There's a lot of evidence that economists, either voluntarily or under duress, are being forced to take notice of the real economy. And similarly, I think the cultural turn has largely run its course. It never was of much consequence in Oxford, but even in places where it was more important, its standing is no longer what it was. I am not suggesting that economic history will go back to the glory days of the 1960s, but I don't doubt that it will continue to thrive.

Any final thoughts on your own career and the contributions you have made?

I think I've made a contribution in three areas. The first is in research, which we have discussed at some length. The second is as an administrator, where I think I was moderately influential on a number of occasions. For instance, while I was Senior Tutor at Clare College, Cambridge, the College was in the vanguard in admitting women. I also played quite a large part in the transformation of the College, introducing a whole web of changes in the rather archaic regulations that I inherited. That was a long, slow, diplomatic task. More recently, when I was appointed to the Oxford chair, the history faculty was overwhelmingly dominated by undergraduate teaching. There was no serious commitment to graduate teaching. I found that very unsatisfactory and initiated one-year and two-year taught courses in Economic History. These were quickly influential in improving graduate studies in our subject and ultimately in persuading other areas in the history faculty that this was the right way to go. My third contribution was in lecturing, something I've always found rewarding. I put a lot of effort into preparing lectures, and although I would never say that I was an inspiring teacher, a very large number of students who completed the annual course evaluation forms reported that I was the best lecturer that they had encountered in their time at Oxford. A few years ago there was a 50th anniversary conference at the DAE in Cambridge, and one of the people who attended asked if I realized that I was single-handedly responsible for having introduced Russian economic history to the majority of people who are currently the specialists in Britain on the Russian economy. That was a pleasing tribute to the success of my first course of lectures.

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Conference on the Political Economy of Globalization

By Kevin O'Rourke, Trinity College

(Dublin) A conference entitled "The Political Economy of Globalization: Can the Past Inform the Present?" was held August 29-31, 2002 at the Institute for International Integration Studies, Trinity College Dublin. Kevin O'Rourke (Trinity College Dublin), Karl Gunnar Persson (University of Copenhagen), and Jeffrey Williamson (Harvard) organized the conference according to four sub-themes: Globalization and Governance; Trade: Protection, Politics and Performance; Global Labor Markets: Migration, Labor Standards and Inequality; and Global Finance: Stabilization, Repression, and Policy Tradeoffs.

In the first session, **Michael Hiscox** (Harvard) presented "Will Trade be a Partisan Issue Again (and Why Isn't it Already)? Trade and the American Political Parties in the 1890s and 1990s." He observes that in the United States, as in most western economies, international trade was *a* (if not *the*) central issue for political parties in election campaigns and legislative battles during the 1890s. Yet the current era of globalization has produced nothing like the same level of partisanship. Hiscox argues that two long-term changes in the economy help explain the political differences between the 1890s and 1990s. First, a general decline in levels of inter-industry factor mobility, at least in the manufacturing sector, suggests that it is unlikely that class differences in the constituencies of the parties generated partisan conflict over trade. Second, a general decline in the geographic concentration of exporting and import-competing industries makes it unlikely that regional differences in party strength generated partisanship on the trade issue. Despite the current political backlash against globalization, it appears unlikely that divisions over trade policy will assume a central place in American electoral politics. In the discussion, Tim Hatton (University of Essex) pointed out that there is more regional concentration in voter opposition to immigration in the US, so the immigration issue may become highly partisan even if trade does not. Several others noted that trade policy has become less important as a counter-cyclical tool with the growth of welfare programs and reliance upon macroeconomic instruments and that this might explain the decline in partisanship on the issue. Hiscox stated that the reliance on trade policy for various purposes relative to other policy tools is endogenous to the preferences of economic actors and is therefore part of the puzzle and not an explanation for it.

Carles Boix (University of Chicago) followed with "Democracy, Inequality and Country-Specific Wealth." He develops and empirically tests a theory of political regimes that relates capital mobility, inequality, and democracy and contends that economic equality and capital mobility promote democracy. Boix states that as the distribution of capital becomes more balanced across individuals, the redistributive impact of democracy diminishes and the probability of a peaceful transition from an authoritarian regime to universal suffrage increases. Similarly, a decline in capital curbs the redistributive pressures from noncapital holders. As capital can be more easily moved away from its country of origin, tax rates decline in equilibrium, political conflict diminishes, and the likelihood of democracy rises. In contrast, at higher levels of both inequality and specificity, authoritarian regimes will prevail. When both variables are very high, the necessary conditions for civil wars and revolutions are in place. Thus right-wing dictatorships, under which little or no economic redistribution takes place, are likely to face revolutions and be replaced by left-wing dictatorships. Boix tests these predictions using a cross-country panel of transitions from and to democracy and civil wars, while looking at qualitative evidence from Switzerland and the United States. The discussion focused on the following three points: first, the concept of asset specificity and how endogenous it was to political regime and to conditions in the world political economy; second, the political and economic origins of both inequality and nonspecific assets; and third, ways to test the relationships. **Richard Higgott** (University of Warwick) ended with "From Colonialism to Global Governance: A Genealogy of Political Development Theory."

In the session on "Trade: Protection, Politics and Performance," **Michael Clemens** (Harvard) and **Jeff Williamson** asked "Who Protected and Why? Tariffs the World Round 1865-1940." They observe that tariff rates in Latin America were far higher than anywhere else in the century before the Great Depression. Also, tariffs in the European periphery and the English-speaking new world were far higher than they were in the European core. Moreover, tariff rates rose everywhere in the periphery up to 1900 and then planed off before WWI. Consequently, the great antiglobal leap during the 1930s in Latin American and the European periphery was not new policy territory, since these two regions had plenty of previous experience with very high tariffs. Clemens and Williamson reason that economic historians have ignored these facts in their focus on Europe and that modern endogenous tariff theory does not explain them. They use panel data on a sample of 35 countries to explore competing hypotheses on factors that drove policy in the century before WWII: revenue motivation, optimal tariffs, strategic tariffs, deindustrialization fears, and Stolper-Samuelson forces among others. They conclude that the world environment mattered for the evolution of policy, as did trading partners domestic geography factor endowments institutions and politics. During the discussion, suggestions were made that the authors restrict their estimation to 1865-1929 in order to reduce the problem caused by the proliferation of nontariff barriers later on. Others wanted Clemens and Williamson to introduce exchange rate regime variables as additional explanatory variables and consider how the use of domestic railways might have affected the level of tariffs.

James Foreman-Peck (Middlesex) continued with "Trade Wars and the Slump." He measures globalization as an increase in real world exports per capita. He seeks to estimate the impact of the major trade controls of the world's four largest trading partners during the Great Depression. Foreman-Peck uses simulations to model optimal policies for GDP, the current balance, and prices in a model of the interaction of the four economies. Simulations show objective reasons for the failure of the international cooperation necessary for globalization between the world wars. Britain was not worse off in a Nash tariff equilibrium, nor was the US when the alternative optimum cooperative equilibrium gave all nations equal weight (as in voting in supranational organizations). Failures of domestic policy optimization, particularly in the US, underlay this inability to cooperate internationally. Higgott pointed out the apparent contradiction between more policy instruments leading to greater efficiency in achieving policy targets and more issue areas leading to greater difficulty in reaching international agreement (1933 conference). Foreman-Peck felt that the first proposition concerned the (derived) demand for policies, whereas the second dealt with the supply of policies. Elhanan Helpman (Harvard) proposed that the author should compute all feasible, incentive compatible cooperative policy equilibria, not just the two types considered in the paper, while Giovanni Federico (EUI) asked about the implications of not modelling the rest of the world apart from the four countries. Foreman-Peck remarked that the rest of the world was in the model but not as strategic players purely because of the additional complexity. If the four largest economies could find a cooperative solution, then it was likely that the rest of the world would follow.

Asaf Zussman (Stanford) closed out the session with "The Rise of German Protectionism in the 1870s: A Macroeconomic Perspective." Zussman challenges the conventional view of the German 1879 "iron and rye" tariff as reflecting a backlash against globalization and offers a new explanation for its imposition. He traces the roots of the process that led to the adoption of the tariff to the indemnity imposed by Germany on France following the Franco-Prussian War of 1870-71. The indemnity translated into a massive monetary and fiscal stimulus and led to the creation of an economic boom. When the indemnity payments stopped, the boom turned into a depression that set in motion a protectionist campaign and increased the dependence of the German federal government on the states for tax revenues. This matching of forces is what ultimately led to the adoption of the 1879 tariff. Several participants had reservations about the choice of deflators (specifically the wholesale price deflator) that Zussman uses to show that the decline in the relative prices of grain in the 1870-1913 period was not large.

After lunch, the participants returned to the conference room for a session on global labor markets. **Tim Hatton** presented "UK Overseas Emigration: 1870-1913 vs. 1950-2000," in which he measures the effects of immigration policies in the US, Canada, Australia, and New Zealand since the 1960s. Using a model of UK emigration estimated for 1870 to 1913, Hatton predicts that the high rates of emigration from Britain in the 1960s would have continued until the early 1990s in the absence of policy changes in the receiving countries. Questions were raised about the surprising decline in the incentive to emigrate in the 1990s due to the apparently sharp increase in the UK real wage. Hatton said that he had experimented with different data sources but had obtained similar results. Estimating the model for the period since 1966, including policy dummies, he illustrated the importance of policy shifts, particularly in Canada and Australia, and the waning importance of variables like the real wage ratio and unemployment rates at home and abroad. Discussants asked whether the policy dummies might be capturing other effects and about the absence in the model of controls for the costs of passage. Hatton replied that direct measures of the stance of immigration policies and of migration costs would be highly desirable, but they are not currently available.

Next, **Michael Huberman** (Montreal) spoke on "International Labour Standards and Market Integration Before 1913." He discusses the current debate about whether globalization ultimately will lead to a race to the bottom in labor standards in developed countries. Evidence from the 19th century on labor standards and practices suggests that this is not the case. If anything, national labor standards before 1914 actually increased with degrees of openness. Labor practices, such as work hours, did not exhibit any tendency to converge. Moreover, there was very little consensus on core labor standards. Drawing on a case study of the French-Italian labor agreement in 1904, Huberman asserts that protectionist countries raised labor standards to open markets. Williamson asked how the New World would rank in its labor legislation. Huberman believes that compared to most European countries they are close to the bottom, with the gap between the Old and New World actually widening over the period.

Concha Betran and **Maria Pons** (both Valencia) presented "Skilled and Unskilled Wage Differentials and Economic Integration 1870-1930." Betran and Pons analyze the differences between the wages of skilled and unskilled workers from 1870-1930 for five countries (US, France, UK, Italy, and Spain), with different levels of development and economic integration. They construct a ratio of skilled-unskilled wages (skill premium) in the industrial sector for these countries except the US. They study the impact of globalization, technological and structural change, and labor movements on the skill premium growth rate. Their main conclusion is that the globalization variables (migration and trade) explain only part of this growth. Technological and structural change and labor movements also affect the skill premium growth rate. Betran and Pons also analyze the migration policy of the countries and find that such policies did help to reduce inequality in the labor market. However, they believe that since migration is not the only origin of inequality growth, other educational and distributional policies have to be implemented. The main concern of the discussants related to the absence of schooling as an explanatory variable of growth in the premium on skill. Betran and Pons indicated that at this stage of the paper they had not yet included this variable but that they planned to include it in the next version. Another comment was made regarding the possibility of increasing the number of countries in the sample. The authors think their selection of countries is sufficiently representative to explain the origins of skill premium growth. Moreover, increasing the number of countries is not straightforward given the laborious nature of the construction of the skill premium ratio and considering the problems of finding sources.

In the final session, "Global Finance: Stabilization, Repression, and Policy Tradeoffs," **Stefano Battilossi** (Universidad Carlos III) presented "Financial Globalization and Financial Repression in Italy 1950s-1970s: A Public Finance Perspective." Battilossi studies the emergence of an extensive regime of financial repression (both domestic and external) in Italy, a consequence of a backlash against creeping financial globalization and enhanced capital mobility in the late 1960s. He says that the Italian case raises issues that are common to the historical experience of a wider number of countries in the European periphery. He aims to determine why financial repression lasted until the late 1980s, in spite of its widely recognized distortionary impact. Following the literature on the political economy of capital controls and the fiscal implication of financial repression, Battilossi suggests that, in the Italian case, comparatively high revenues from implicit taxation provide an explanation. In the presence of a relatively inefficient tax system, governments resorted to financial repression as an attempt to postpone the structural change in the underlying fiscal policy regime that capital liberalization and financial deregulation necessarily entailed. To support this view, he uses the economic relevance of revenues from financial repression (following a method proposed by

Giovannini and De Melo in 1993) as a proxy for the cost of financial liberalization, which prove to be of a magnitude comparable to revenues from seigniorage. High costs of liberalization and deregulation in terms of lost revenues from implicit taxation may account for the durability of financial repression.

The debate initially focused on the macroeconomic and political economy assumptions of the paper. Questions were raised about how capital controls keep domestic equilibrium interest rates below world market interest rates. It also was suggested that the author consider whether people were able to avoid implicit taxation by reducing demand for broad money or by raising their holdings of physical assets. Battilossi agreed that the housing market could be an important aspect of the story; however, the slow advance of financial innovations certainly constrained the ability to avoid implicit taxation. Finally, in a political economy perspective, it was suggested that a closer look at losers and winners from implicit taxation is appropriate, as capitalists may actually benefit from a regime of financial repression. Battilossi commented that this may well be the case in the Italian experience, where real interest rates were negative over a long period, and implicit revenues from financial repression were partly redistributed to public and private sector companies in the form of subsidized credit.

Next, **Jay Shambaugh** (Dartmouth) and **Alan Taylor** (UC-Davis) presented "The Trilemma in History: Policy Choices for Exchange Rates, Monetary Policies and Capital Mobility," written with **Maurice Obstfeld** (Berkeley). The authors use time series data covering 130 years to investigate the empirical basis of the open-economy monetary policy trilemma: the incompatibility of maintaining fixed exchange rates, capital mobility, and monetary autonomy simultaneously. Though this rule of thumb has guided policymakers in recent years, some studies question the empirical foundations of the trilemma and suggest that there is limited autonomy even for floats. Other studies suggest that between the "gold points" of the classical gold standard there was room for monetary autonomy. Shambaugh, Taylor, and Obstfeld define monetary autonomy as the ability to set local interest rates in a manner independent from a base country, and they code exchange rate regimes based on the actual behavior of the exchange rate. They then measure the extent to which the interest rates of individual countries follow the world rate in three epochs: the gold standard (1870-1914), the heyday of the Bretton Woods system (1959-1970), and the years since its collapse (1973 to the present). Based on comparative perspectives both within and between different eras, the authors believe that the exchange rate regime was an important constraint on policy. Floating countries have had far more autonomy than fixed countries. At the same time, looking at the third dimension of the tradeoff, the authors argue that capital controls can insulate an economy from the world interest rate even if it has a fixed exchange rate. Hence, the data appear to support the trilemma. One participant noted that fiscal policy is in no way constrained, thus leaving room for domestic stabilization. The authors agreed, acknowledging that their focus is on the monetary policy constraint.

Cedric Dupont (Geneva) and **Carsten Hefeker** (Hamburg) presented the final paper of the conference, "Policy Tradeoffs and the Choice for International Institutions: The Gold Standard and Beyond." They analyze the relation between monetary and trade integration in historical and comparative perspectives and measure the extent to which governments considered trade and exchange rate policies jointly when trying to achieve higher economic and political gains. In particular, they explore the joint logic of international institutional commitment in these two policy domains. Dupont and Hefeker use a standard political-economic approach with a transaction cost framework to contrast the experience of economic integration and institutional choices under the gold standard and in post-1970 European integration. They conclude that while there was a commitment tradeoff in the late 19th century, with the rise of tariffs under generalized use of gold, the post-1970 European integration has been a case of commitment reinforcement in the two policy domains.

Clio at ASSA *(Continued from page 1)*

household-level census data to a digitized map of the Chicago water and sewer system. Because the 1880 census includes information about morbidity and mortality over the year, the linkage allows them to identify the effects public water and sewer systems had on household health outcomes. Troesken noted that their preliminary results are puzzling, as access to sewer lines seems to be linked to an increase in infant mortality. Melissa Thomasson (Miami, OH) expressed the excited anticipation of all present to see this work in completed form. She suggested that possible solutions to the puzzle may be a selection bias. Was the connection to a sewer line a response to the increasing infant mortality? And, is it possible that infant mortality rates were underreported? Thomasson noted that these rates seem low even relative to 1920s standards. She also wanted to know what the public knew about waterborne illnesses in the 1880s. Were they aware of the dangers of contaminated water? Price Fishback (Arizona) asked whether the sewer lines were private or public goods, and Lee Craig (North Carolina State) wondered how comprehensive the household hookups to nearby sewer lines were. Davie was curious if businesses were hooked to the sewer lines and whether people got sick at work or at home. Finally, Carol Vance (UT-Dallas) inquired about the location of immigrants. Were they disproportionately located in the high-mortality areas, i.e. were immigrant ghetto neighborhoods driving the results?

John Wallis (Maryland) turned the focus from city to state government with his work on "Debt, Default, and Revenue Structure: State Government Finances in the 1840s." Wallis seeks to explain the numerous states that defaulted on their debt. He claims that existing explanations for the defaults do not adequately explain what happened, because they try to generalize the problem for each of the 12 states involved in the default crisis. He concludes that there is no common explanation for why the states defaulted. In his opinion, the mistake of prior work was in the assumption that Pennsylvania was a typical state and that the stylized facts of its situation were an accurate description of what occurred elsewhere. John James (Virginia) noted the obvious relationship between the debt/capital ratio and the default rate. Those states with high ratios defaulted; those with low ratios did not. He lauded Wallis for going beyond this seemingly simple relationship and examining the details, and he encouraged him to spend more time discussing Alabama, the only exception to the debt/capital relationship. He also pointed out with interest that Indiana and Pennsylvania seemed to suffer little for defaulting. Within a decade, they were able to secure credit at competitive rates. He proposed Wallis address this in more detail. Troesken wanted to know if we could really learn anything if all states were different. He also wondered if the federal government forced the states to repay their debts the way state governments do municipalities. Wallis said that in the 1840s the Federal government did not have the political muscle to enforce repayment.

Ryan Johnson (Brigham Young) ended the session with "Welfare Programs and Crime in Cities during the Great Depression," work he is doing jointly with **Price Fishback** and **Shawn Kantor** (both Arizona). The authors use the Great Depression as a background to examine the effects of welfare and work relief programs on the deterrence of criminal activity. They determine that New Deal relief programs did lower property crime, despite the fact that this was not their primary focus. They also state that spending on police was more significant in deterring crime, although their measures are not as precise. Koleman Stumpf (North Carolina) pointed out that prohibition was repealed during their sample period and questioned its impact on their findings. He also raised some issues concerning aggregations. City level relief statistics and individual level decisions to turn to crime are linked but not synonymous. Could crime rates be driven by age distributions within a city? How about migration and crime patterns? And how did noneconomic crimes such as murder and vandalism compare? Wallis commented that once prohibition ended, police had more time to battle other crime, so even with no spending, one could expect to see more efficient police work. Baten asked if the authors could measure income inequality in different cities to see if that mattered. Susan Wolcott (Mississippi) wondered why state employment figures seemed to have no effect on crime but relief spending did.

Howard Bodenhorn and **Christopher Ruebeck** (both Lafayette) led the session entitled "Earnings and Savings in Free and Slave Labor Markets" with "The Economic Consequences of Race and Mixed-Race: Evidence from the 1860 Census." The authors consider the choices and lifestyles of black and mixed black-white individuals in the urban US south prior to the Civil War. Known and visible mulattoes could by behavior and reputation be termed "white." Thus, acculturation was an option for at least some mixed-race people. The authors empirically estimate differences in wealth between blacks and mulattoes and find that mixed-race households accumulated more wealth than black households. Their regression results show that a substantial portion of the wealth gap was due to race. However, the advantages were dependent on the racial mix of the neighborhood in which the mulattoes lived. Mulattoes needed whites in the neighborhood to show them preferences they did not receive from their black neighbors.

Siddharth Chandra (Pittsburgh) followed with his presentation of "Race, Ownership, and Labor Market Outcomes in the Dutch Indies." He uses a data set from the Dutch Indies from 1918-1924 in an attempt to determine whether patterns of ownership have been associated with differential outcomes for labor belonging to different racial groups. He does this by focusing on racial ownership patterns in selected industries. Chandra finds that preferential employment patterns for the minority population, Chinese-owned industries prevailed during this period and speculates on the potential negative impacts these historical roots may have played in race relations in modern day Indonesia.

"The Earnings Gap between Rural and Urban Canada in 1901" was examined by **Alan Green** (Queen's University), **Mary MacKinnon** (McGill) and **Chris Minns** (Trinity College, Dublin). The authors attempt to fill a void in our understanding of the relative earnings of workers in rural and urban Canada at the turn of the 20th century. The extent of urban-rural earnings gaps varied widely across the country, but the reasons are not obvious, suggesting that further research in this area is needed. The earnings gap was large in the northwest, where most workers had migrated to the region and many were still highly mobile, but it was low in British Columbia, with the same factors present. In central and eastern Canada, the authors indicate that there is a major difference between the English- and French-speaking areas. They find evidence that labor markets in English-speaking areas were similar to those for Americans, but the situation in Quebec was radically different. It appears that workers in the rural sector were often stuck there. Living in a province with exceptionally high fertility rates and facing a restricted set of migration possibilities, low-skilled Quebec men earned far less in the countryside than did their urban peers.

Shirley Chenny, Pascal St.-Amour, and **Desire Vencatachellum** (all Montreal) finished the session by discussing "Slave Prices from Succession and Bankruptcy Sales in Mauritius, 1825-27." The authors study the determinants of slave prices in Mauritius during the period in which slavery was legal but the importation of slaves had been banned. They construct a unique data set from slave auction sale results. They find the characteristic concave age-price pattern documented by Fogel and Engerman and identify significant discounted prices for female and nonnative slaves. Moreover, they identify a positive premium on human capital and a strong seasonal component coinciding with the sugar cane harvest season. Finally, they note a sharp increase in the price of child slaves over the period, providing *prima facie* evidence that slave owners were either not anticipating the abolition of slavery in the near future or not expecting important capital losses associated with an eventual abolition, which came about in 1835.

Across the hall, the focus was on financial, not human, capital. **Michael Haupert** (UW-La Crosse) began the session entitled "Money, Banking and the Gold Standard" by presenting work he has written with **Paul Auerbach** (Kingston University) on the response of banks during the Civil War. The authors admitted they are in the preliminary stages of research. Nonetheless, they argue that the issuance of paper currency by banks during the war years was limited because of an existence of a "hierarchy of monies," ranging from an active specie market to Treasury notes, greenbacks, and state bank notes. The public displayed disaffection for bank notes, thus banks were unable to issue their notes in a manner that would make them attractive as either a currency or form of wealth holding. Peter Rousseau (Vanderbilt) was interested in the focus on the multiple currency explanation for the demand for bank notes but urged the authors to also consider the supply side of the market in attempting to explain the apparent anomaly.

Christopher Meissner (Cambridge) continued with "Committee Structure and the Success of Connected Lending in Nineteenth-Century New England Banks." Meissner investigates a managerial decision-making mechanism that helped ensure good lending practices: large decision-making loan review bodies. If many people were involved in the decision, the probability of bad insider projects getting funded was reduced. He formally models bank-lending committees and shows how certain voting rules could limit excessive lending and poor project choice. Jean-Laurent Rosenthal (UCLA) expressed three wishes for this paper. He wanted to see a slightly fuller model, an incorporation of the opportunity cost of making insider loans, and a change in one of the variables from a dividend/stock price to a return to capital. Bert Ely (Ely & Co.) asked if note discount rates provided any useful information, and Richard Sylla (NYU) wondered if the exclusive focus on Boston banks skewed the results.

In "The Effects of Branch Banking on Financial Stability and Bank Competition," **Kris Mitchener** (Santa Clara) and **Mark Carlson** (Federal Reserve System) contend that the standard diversification story regarding branch versus unit banking and stability is inconsistent with the evidence from the Great Depression. The real affect on financial ability comes about through the impact of branch banking on competition. As competition increases in the form of additional branches, marginally profitable banks are forced out of the system, increasing the overall stability of the system. They relate that states adopting branching laws saw more mergers and voluntary liquidations and experienced fewer failures in the 1920s and the early years of the Great Depression. Joe Mason (Drexel) praised the scope of the work but urged them to break it into three or four papers in order to give more attention to some of the detail. He suggested separate research projects could focus on the issue of financial stability and exactly how it could be defined, a comparison of bank performance before and after branching laws went into effect, and on the question of which national banks chose to branch and why given the miniscule number of them in the sample. Ely wanted to know more about where branches were located. Was it in big cities or rural areas? Angela Redish (University of British Columbia) asked how the balance sheets of branch and unit banks differed. Mark Weidenmier (Claremont) inquired if bank stock price data could provide any useful information.

Angela Redish and **Michael Bordo** (Rutgers) used a team approach to present "Deflation and Depression: Evidence from the Classical Gold Standard." The third author of the paper, **John Landon Lane** (Rutgers), was unable to attend. They argue that deflation has had a bad historical rap due to its association with the Great Depression. Focusing on the period 1880-1913 in the US, UK, and France, they indicate that deflation was reflected both in positive aggregate supply and negative money supply shocks. Weidenmier wondered about the nonmonetary demand shocks and expressed a wish to see the analysis formally extended to evaluate different types of exchange rate regimes during the classic gold standard. Christopher Hanes (Mississippi) was curious what the authors believed caused the depressions of 1893 and 1907, since they appeared to say they were due to supply shocks.

The Clio and EHA sessions wrapped up with concurrent sessions on Saturday. The session on education and intergenerational transfers was opened by **Marina Adshade** (Queen's University), who presented her work on education, technical adoption, and household labor supply. Adshade asserts that the increase in female labor force participation at the turn of the 20th century was driven by growth in the clerical sector. The demand for female workers increased due to innovations that were effectively directed toward the increasing supply of educated women. Carolyn Moehling (Yale) encouraged Adshade to strengthen her story by focusing more on the empirical content. In particular, she wanted her to establish preconditions necessary for the model and then test it in that light. She also believed that a stronger case could be made that increased high school education for women was an exogenous factor, as Adshade claims.

Claude Diebolt presented work he wrote with **Magali Jaoul** and **Gilles San Martino** (all

Universite Montpellier). Diebolt discussed a cliometric analysis of primary education in 19th-century France. The authors base their study of primary education by administrative department on retrospective national accountancy and econometric methods. David Mitch (Maryland–Baltimore County) encouraged the authors to be more specific in defining education. Did their figures include private schools? How are daycare and preschool handled in the definition? Finally, did this degree of "inclusiveness" of enrollment figures change over time?

Diane Macunovich (Barnard) spoke on the "Effects of Changing Age Structure and Intergenerational Transfers on Patterns of Consumption and Saving." She focuses on two strands of literature in this study, using micro and macro level data in the US and the effect of children on family savings rates. Rick Steckel (Ohio State) lauded the important contribution this work makes by looking at the effects of household decisions on macro variables. He wanted Macunovich to use a two-stage analysis focusing on factors affecting age distribution and then its effect on macro variables. Haupert asked if a distinction was made between spending on younger children and loans to older children.

John Murray (Toledo) closed out the session with "The Rise of Southern Literacy: Families, Schools, and Class in Antebellum Charleston." He analyzes patterns of literacy over generations using the records of the Charleston Orphan House and finds that the role of the family, especially mothers, was critical in increasing literacy. Hamilton commented that there were some selection problems in the data set. She also thought Murray should explore further his result that literate mothers have an impact on children's literacy but literate fathers do not. Heather Wynder (Cornell) inquired if parents viewed orphanages as a dumping ground to avoid the cost of raising children or as an affordable training ground for their children.

At the session, "The Nature and Sources of Modern Economic Growth," Chris Minns (Trinity College-Dublin) presented a paper by **Timothy Hatten** (University of Essex) entitled "Can Productivity Growth Explain the NAIRU? Long Run Evidence from Britain, 1871-1999." Hatten uses a longer time series to examine a question that has proved difficult to answer with shorter data runs. He finds productivity growth to be integral to low unemployment in the postwar era, but not earlier. He also states that institutional factors are important. Hanes proposed comparing Britain and the US, citing as an example the *JEP* article by Ball and Mankiw (Fall 2002). In Britain, social policy changes were important, whereas in the US productivity was exogenous to labor. Hanes also proposed breaking the data into prewar and postwar periods and switching the productivity data from hours per worker to TFP, since hours/worker is related to unemployment.

In the second paper, **Alvaro Pereira** (University of British Columbia) asks "When did Modern Economic Growth Really Start?" Growth in preindustrial economies was mainly extensive, although there were a few episodes of intensive growth. Pereira asserts that GDP growth is positively related to literacy after 1820 but not before. By examining specific countries, he determines the "Golden Age" of the Netherlands was a Post-Malthusian economy but did not have all the characteristics of modern economic growth. He concludes by stating that western European economies advanced to modern growth through the triple engines of human capital, technology, and organizational change. Rosenthal made a general plea to all economic historians to stop using Maddison's data, which he believes presupposed slow growth. He advised relating the paper to Wrigley-Scofield. Another concern he had about the data was that it is based on modern nations, some of which did not even exist at the time. He added that preindustrial growth was limited to cities, whereas modern growth is capable of spreading. He also questioned whether literacy truly measures human capital and cited France as a counter example, where literacy increases took place after the Industrial Revolution and fertility decreases occurred before.

Jean-Laurent Rosenthal then spoke on "Organizational Choice and Economic Development: A Comparison of France and the United States During the Mid-Nineteenth Century," written with **Naomi Lamoreaux** (UCLA). They compare the adaptability of incorporation law in France and the US and examine the conventional wisdom that common law is more flexible and responsive. According to Rosenthal and Lamoreaux, the only advantage the US had over France was earlier general incorporation. France allowed more organizational options. Less choice led to US corporations being smaller. The judiciary was the most important constraint due to its conservative objective of protecting creditors, and statute law was the main source of innovation. They conclude US common law traditions were not more flexible than the French system in the area of incorporation. Tom Geraghty (North Carolina) pointed out that US laws were more flexible for corporations in terms of general incorporation but less flexible for other forms. He recommended that the paper should include an assessment of the strengths and weaknesses of each system for enterprise development, proposing they may have been flexible enough given the capital needs of the time, but not adequate for developing countries today. Geraghty wanted them to look at how well alternative organizational forms such as share issuing partnerships served as an alternative to general incorporation in France by looking at the tradability of shares.

In "Evidence on Catch-Up and Convergence Since 1872," **Marianne Ward** (Loyola College-Maryland) and **John Devereux** (Queens College, CUNY) examine a new per capita income data set for the years 1872-1990 (much to the delight of Rosenthal). The income estimates are purchasing power parity adjusted, using detailed price and expenditure data. Using their data set, they contend the western offshoots (US, Canada, and Australia) grew faster and caught up to western European countries earlier than Maddison's data indicate. In conclusion, they look at international price level convergence, where they find price divergence after WWII. This suggests that income and price convergence need not move in tandem. Areendam Chanda (North Carolina State) looked at the differences in the Ward/ Devereux data and the Maddison data. The general direction matches with the exception of 1905, so this discrepancy is worth closer examination. Regarding price convergence, the PPP puzzle is already well established. The question is: Why is the gap so steady? There was also a question about the high French food prices in 1872. Devereux found this puzzling but said the data came from multiple sources. Rosenthal noted the Franco-Prussian War as the probable cause.

The long day concluded with the annual Cliometric Society reception hosted by Mike Haupert. The turnout was robust, the beer cold, and the conversation stimulating. The evening was dampened only by the abysmal performance of the Green Bay Packers, who were on television in the background. Cliometricians will gather next May 23-25 in the sunny state of North Carolina for the Cliometrics Conference hosted by Lee Craig and the North Carolina State faithful.

Report on the First International Conference on Economics and Human Biology

By Marco Sunder, University of Munich, and Alexander Moradi, University of Tuebingen

(Tuebingen) The First International Conference on Economics and Human Biology was held July 11-14, 2002 at the medieval castle Hohentuebingen, located on a steep hill in the center of Tuebingen, an old university town in southern Germany. Organized by Joerg Baten (Tuebingen) and John Komlos (Munich), the conference was financially supported by the German Science Foundation. Participants came from more than 20 nations and from a large number of disciplines, including economic history, transition and development economics, biological and physical anthropology, demography, nutritional science, public health, and sociology. They focused on the various linkages between economic processes and human biology, covering all inhabited continents and virtually all time periods. About one third of the papers pertained to anthropometric history, with a large geographic and temporal stratification.

John Komlos began with a presentation on the anthropometric history of early modern France. On the basis of a sample of archival military records, he has constructed a series of average male adult heights of the French population from (birth years) 1670 to 1765, controlling for the recruitment practices of the military. The spatial patterns in these early data match those published for 19th-century France quite well, and the time trend in human stature resembles those that were obtained in other European countries. The very substantial increase in average stature of 3.8 cm between 1694 and 1706 may be a result of improving climatic conditions after the "little ice age." Surprisingly, the French Revolution was not preceded or accompanied by particularly adverse nutritional circumstances.

Laurent Heyberger (Strasbourg) then examined the correlation between wheat prices and the percentage of French conscripts exempted from military service due to low stature in the first half of the 19th century. He stressed the importance of wheat in the diet of Frenchmen and found that increases in wheat prices resulted in an increase in exemptions. A clear spatial pattern was observed: people in the north were the tallest, whereas those living in the center were the shortest, indicating a great inequality in nutritional terms. Over time, however, a spatial convergence of heights occurred. **George Alter** (Indiana) also used military conscription lists for his study, "Stature in Transition: A Micro-Level Study from Nineteenth-Century Belgium." He focuses on height differences among occupational groups to assess losers and winners of industrialization. Before 1850, day laborers were exceptionally short (160 cm). In contrast, artisans and skilled industrial workers were about 166 cm, while students averaged almost 170 cm. After 1850, most of the groups had made progress, but the largest increases tended to occur among those who were the shortest at the beginning of the century. The convergence of heights indicates that benefits of industrialization were spreading downward to the poorer strata of society.

Deborah Oxley (Sydney) analyzes height records of Irish female convicts transported to Australia. In assessing the "Living Standard of Women in Pre-Famine Ireland," she finds distinct differences between the Irish provinces, as well as a decrease in heights, which reflects the economic decline after the end of the Napoleonic Wars and the union with Britain. "Can Development Be

Hazardous to Your Health?" asks **Michael Haines** (Colgate) in his investigation of the mid-19th-century downturn in average physical stature in the US, England, and the Netherlands. Since real income per capita, real wages, and relative food prices cannot completely explain the internationally similar pattern in heights, he suggests considering the development of urban infrastructure and commercialization, as well as the associated spread of diseases and thus mortality, as important (latent) explanatory factors.

Vincent Tassenaar (Groningen) uses time series data on Dutch heights of the late 19th and early 20th century to investigate their relationship to nutritional intake. He uses the Baxter-King filter to remove the trend in both series and then analyzes the lag structure of the cyclical components of the series. The results indicate that the contribution of nutrition to Dutch heights at certain ages was not constant over the cohorts under consideration. The birth year orthodoxy, which associates final mean height to environmental conditions during the first year, was challenged by **Sebastian Coll** (Santander). In his paper, "When is Human Height Determined? An Exploration in the Lag Pattern of Height's Determinants," he estimates correlation coefficients between age specific mortality rates and final stature of Spanish men born in the first half of the 20th century.

In "Autarchy, Market Disintegration, and Health: The Mortality and Nutritional Crisis in Germany during the Early Years of the Nazi Regime, 1933-1937," **Joerg Baten** and **Andrea Wagner** (Munich) analyze the hidden costs of a totalitarian regime. They present evidence that morbidity and mortality increased in almost all age groups. Moreover, heights of German school children stagnated. This pattern was unique to Germany. In addition, the prevalence of diseases related to nutritional deficiencies increased, especially affecting the food importing area in northern Germany, as well as larger cities. They conclude on the basis of this evidence that the autarchy policies of the Nazi Regime and the market disintegration due to price fixing were the main causes of these adverse developments. Similarly, **Uwe Jager** and **Konrad Zellner** (both Jena) found decreasing body weight in schoolchildren from Jena between 1932 and 1944 and a reduction in body height of girls only.

An overview of secular trends in human stature in different countries was provided by **Tim Cole** (Institute of Child Health, London). He argues that the increase in stature is due to more rapid growth after birth, since average stature at birth has been more or less stable over the past century. Anthropometric evidence may not only be obtained from archival records but also from skeletal remains. **Nikola Koepke** (Tuebingen) discussed data on the biological standard of living for the provinces of Germania and Raetia under Roman occupation in the 1st-4th century AD. She collected published estimates on body height and age at death of both males and females for her meta study and observes a gradual deterioration of these indicators. With a similar methodology, **Richard Steckel** (Ohio State) closes the chronological gap with evidence on the net-nutritional history of northern Europe over the past millennium. He states that average male heights declined by 6.4 cm between the early Middle Ages and the 17th and 18th centuries. Possible explanations for this phenomenon include the worsening of climatic conditions, urbanization, and commercialization accompanied by the spread of diseases. Human physical stature in Europe did not recover to 11th-century stature levels until the early 20th century. **Christopher Knuesel** and

Marianne Schweich (both Bradford) focus on "Bio-Cultural Effects on Stature and Body Proportions in European Archaeological Populations." Female body proportions are generally less variable through time, and, therefore, apparently more buffered against adverse social conditions and biological stressors.

The experience of transition and developing countries was another central issue discussed at the conference. **Liu Shiyung** (Academia Sinica) presented a paper on Taiwan's anthropometric history in times of Japanese colonialism, 1895-1945. Using anthropometric data from various archival sources, he demonstrates that the population of Taiwan experienced height gains among the birth cohorts of the 1920s and 1930s, attributable to the Japanese promotion of health and infrastructure development. In contrast, **Stephen Morgan** (Melbourne) reports a decline in physical stature among both males and females in mainland China during the same period. **Alexander Moradi** (Tuebingen) examines the cross-sectional and temporal variation in heights of sub-Sahara African women born between 1950 and 1980. The stagnating and decreasing heights after the mid-1960s suggest that almost the whole region went through a nutritional or health crisis. He finds economic failure and a decreasing protein supply to be the main determinants of this development and, surprisingly, a robust negative effect of a country's openness to trade on the biological standard of living.

Speaking on childhood nutrition in Malawi, Tanzania, and Zambia, **Stephan Klasen** (Munich) takes a look at an innovative Bayesian regression framework, allowing for the estimation of nonlinear functions of metrical covariates and for spatial smoothing of residual information on a map of the districts of the three countries. He pointed out that such an analytical tool can help identify omitted (but spatially correlated) variables, such as malaria incidence in the present case. It may also be useful for regional policy targeting. **Ayal Khimi** (Hebrew University) turns to a micro-level

Hohentuebingen Castle

approach and studies subjective and anthropometric measures of health in a sample of 583 Southern Ethiopian households. The results indicate a positive impact of per capita wealth on health, while inequality has a detrimental effect. There is also evidence that nutritional inequality within the household induces a reallocation of resources towards the more poorly nourished. **Alan Martina** (Australian National University) looks at factors influencing the height of Philippine children in two villages. Among significant determinants were the age of children, spacing between births, parental education, household income, quality of housing, and access to a refrigerator.

India is the focus of a study by **Ralph Shlomowitz** (Flinders University). He compares height and weight data recorded in two surveys from the 1880s and the mid-20th century to adduce "Long Term Change in Indian Health." Mean height and BMI (body mass index) were relatively stable for the upper caste, but deterioration was observed for lower caste groups resulting in a widening disparity in health among socioeconomic groups. **Aravinda Meera Guntupalli** (International Institute for Population Sciences, India) draws attention to the current "Health Status of Indian Women." **Sonia Bhalotra** (University of Bristol) evaluates the benefits of the food subsidy provided by the public distribution system by constructing a counterfactual: given the elasticities, what would be the effects on childhood malnutrition if the subsidy would not exist? **Siddiq Osmani** (Belfast) points out in "The Hidden Penalties of Gender Inequality: Fetal Origins of Ill-Health" that inequality may result through undernourished mothers in a high incidence of underweight infants, thereby endangering the survival and development of children of both sexes and thus impairing the interests of male as well as female children.

Recent developments of health in Russia were discussed by **Elena Andriouchina** (Moscow). She finds evidence for a health crisis in the 1990s. The transition process apparently has a negative short-term impact on health. In the same context, **Elena Godina** (Moscow) described "Recent Secular Changes in Russia: What Do They Mean?" She shows a recent stagnation in height by age, as well as a decline in chest and arm circumference, when comparing various anthropometric measures of Russian children born in Moscow in the 1990s with those born two decades before. **Alan Dangour** (London) analyzes micro-level data from children born in the very poor Kasalinsk region of Kazakhstan in the 1990s, a period of relative instability for both economic and health indicators. He says while boys' anthropometric measures stagnated, those of girls declined significantly, arguably a result of rising gender discrimination in the household food allocation. In contrast, the secular upward trend apparently continues in Hungary as discussed by **Gyula Gyenis** and **Otto Eiben** (both Budapest).

Noel Cameron (Loughborough) turned to "Human Growth within Economic Transition in Developing Countries: The South African Experience." Using data on over 4,000 children enrolled in the (longitudinal) Birth to Ten birth-cohort study in 1990, he feels that the growth of white children continues to exceed that of their nonwhite counterparts. Since differences that existed at birth and during infancy have not diminished over time, he concludes that the end of apartheid has not yet affected the growth pattern of black children. Too much food can also be considered a serious health problem. **Roberto Frisancho** (Michigan) points to the increasing prevalence of obesity in the Third World. Extreme body weights can be found regardless of economic status. **Stanley Ulijaszek** (Oxford) then evaluated "Trends in

Body Size, Diet and Food Availability in the Cook Islands in the Second Half of the 20th Century." Based on three population surveys conducted in 1952, 1966, and 1996, he shows a secular increase in height and also in BMI. In 1996, approximately 55% of the females and males had a BMI greater than 30. Since fat intake declined over this period, the reduction in physical activity may be the driving force of this development.

Barry Bogin and **Patricia Smith** (both University of Michigan-Dearborn) had a similar theme on obesity when presenting their paper on the health of children in Mayan immigrant families in the United States. Compared to Guatemala, life in the US offers amenities such as better health access and clean running water. However, less healthy habits can easily be adopted, such as fast food consumption, excessive TV watching, and less physical activity. As a consequence, Mayan children are taller in the US, but they are also more likely to become overweight. A possible solution was presented by **Alok Bhargava** (Houston), who analyzes the behavioral response to dietary education programs among minority group women in the southern US. Using panel data with a treatment and control group, he contends that such education programs work, inducing more healthy food patterns. However, the degree of success depends significantly on the level of education, the concerns about health, and the motivation of the program participants.

Another perspective on health and mortality was introduced by **Bernard Harris** and **Andrew Hinde** (both Southampton). In "Sickness, Insurance and Health: Assessing Trends in Morbidity through Friendly Society Records," they analyze the increasing morbidity in the second half of the 19th century. They find little evidence for increasing age-specific morbidity. **John Murray** (Toledo) examines European health insurance programs in the late 19th and early 20th century. His comparison between compulsory (Germany and Austria) and voluntary (France and Belgium) funds point to the relevance of problems related to information asymmetry. In using data from two hospitals in Utrecht, **Peter Ward** (University of British Columbia) demonstrates that a decrease in perinatal mortality rates did not take place before 1940. Deaths in utero and within seven days after birth were mainly associated with low birth weights and delivery problems. In difficult births, there was shift in personnel from traditional midwifes to medical doctors.

Jim Oeppen (Cambridge) explores the relationship between life expectancy and income in a large panel of countries. Global convergence of life expectancy is addressed in detail within a multilevel model that allows for the estimation of country-specific time and income effects. The results reflect that increases in per capita income resulted in greater increases in life expectancy in Japan than in the US. **Diane Lauderdale** (Chicago) presented her paper on the economic correlates of bone mineral density (bmd) in American women in the US. Bmd is a major determinant of osteoporotic fractures. She uses the NHANES III (National Health and Examination Survey) data in her study, and as a measure of conditions during childhood, she makes a case for a positive association of bmd with height. She also finds a positive correlation between bmd and education (although only for whites) and not for current income.

Virginia Vitzthum (National Science Foundation) argues that ovulation depends on nutritional stress and, therefore, represents a self-regulating mechanism when additional children can not be sufficiently fed. In "Economic Development and the Quality of

Life of Children," **Marcelo Delajara** (Cordoba) presented a theoretical model in which he derives the relationship between fertility, income growth, and the nutritional status of children from a household utility function. **David Meltzer** (Chicago) looks at the role of declining urban mortality in promoting economic growth. Historically, mortality rates were higher in urban environments, in which, he stresses, human capital is generally most useful. Therefore, the epidemiological transition, that converted epidemic diseases of adulthood to endemic diseases of childhood, created more favorable demographic conditions for human investments and fertility decline, which in turn contributed to economic development.

T. Paul Schultz (Yale) gave a lecture jointly hosted by the conference and the Tuebingen economics faculty. In his presentation, he discussed labor force participation of the elderly in Taiwan, which provides an interesting case study due to the absence of tax schemes distorting the individual retirement decision. Schultz reports a positive association between health status and labor force participation, where health turns out to be an endogenous explanatory variable. According to estimation results, the introduction of a public health insurance program did not substantially affect participation rates among the elderly.

David Sahn (Cornell) puts his attention on heights of preschool children in "Decomposing World Health Inequality." He compares the variability of heights in the healthy environment of OECD countries with those of the developing countries. Sahn concludes that 70% of world health inequality could be attributed to inequality within countries and only 30% to inequality between countries. Moreover, he does not find a strong correlation between income and health inequality, which, he stressed, has an important policy implication: reducing income inequality does not necessarily lead to a decline in health inequality. **Lee Craig** (North Carolina State) presented a paper on one of the most important inventions associated with changes in human nutrition: mechanical refrigeration. Concentrating on the experience in the US during the late 19th century, he estimates a 1.25% annual increase in per capita protein consumption attributable to the introduction of the new refrigerating technique. Using coefficient estimates published elsewhere, this effect could be translated into an increase in average stature of about .02 inches. **Marco Sunder** (Munich) presented a paper on the biological standard of living of American children, reporting some intra-family gender effects on height for age. He also argues that anthropometric indicators are of relevance for chances in later life. For example, he shows height and weight influence the odds of having a first date among male and female teenagers. **Mansour Mohammadian** (Madrid) believes that the essence of economic theory and biology should be combined in order to analyze the conditions of "real" equilibria, taking into account monetary aspects as well as sustainability of human behavior.

The conference offered ample opportunities for discussion in a relaxed atmosphere, either during the joint meals in the "Duke's room" of the castle or at traditional restaurants, where the attendees could enjoy the scenic views. Disagreements about some of the conclusions were put aside in the evenings, as all participants became enthusiastic supporters of Swabian food and German beer. The discussions continued on the day after the conference during an excursion to the nearby Hohenzollern castle, the ancestral seat of the Prussian kings. All participants were looking forward to a follow-up meeting in the not too distant future.

BOOK PREVIEW

Rickety Road to Flatbush: The Precarious Life of the Manhattan Bridge

By Thomas Winpenny

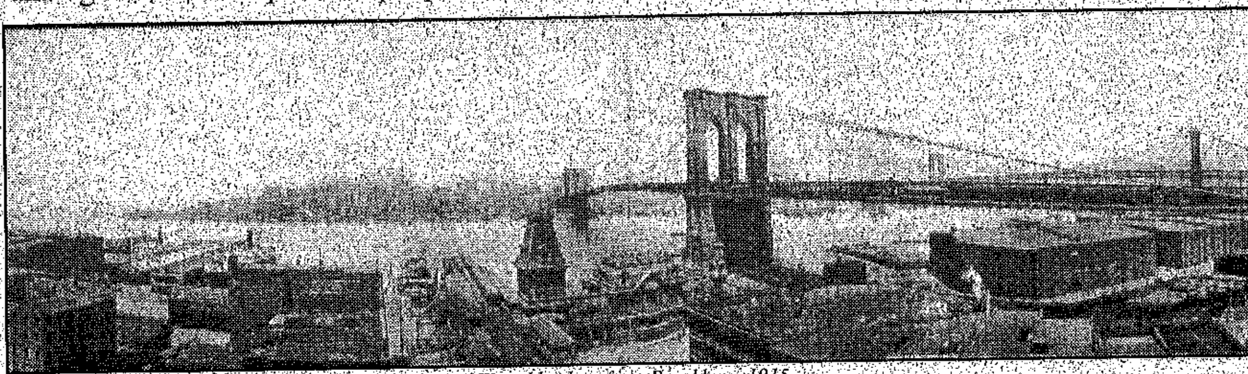
Note: The following preview is from ***Rickety Road to Flatbush: The Precarious Life of the Manhattan Bridge,*** *expected date of publication Fall 2003 by Canal Technology Press.*

The modern American landscape is punctuated by a host of magnificent bridges, from the Brooklyn Bridge in the East to the Golden Gate in the West. Justifiably, many of these structures are admired as works of art and engineering genius. They are revered as national treasures and are sources of great local pride. The artistic are moved to paint, photograph, compose songs, and write poetry about bridges. Lovers stroll hand in hand across their spans, desperate people jump, and the homeless find imperfect refuge beneath. Put in slightly different terms, most great bridges constitute one more American success story. Everyone is delighted to drink in the visual pleasure they provide, motorists and subway straphangers alike appreciate the vista while crossing, and taxpayers understand that they are cheaper to build than tunnels.

Beyond these wonderful generalizations, however, a darker side lurks. Much of this stems from a lack of basic maintenance. A few extremists contend that major bridges cause poverty and depressed land values. Bridges, like most everything else, require care and looking after. Ideally, local governments (sometimes with federal aid) invest in painting, fighting corrosion, and inspecting for cracks and other structural problems. In practice, of course, maintenance is frequently deferred and deferred and deferred until safety considerations finally override budgetary limitations. The net result today is that many of America's bridges are in serious need of major repairs and restoration.

Having noted the general societal inclination to not care for its bridges, what happens when you couple this societal predisposition with original design flaws and bad engineering choices? Enter the Manhattan Bridge. The saga of this East River span is fascinating for at least three reasons. First, similar to the Brooklyn Bridge, which took 15 years to complete (1869-1883), the Manhattan also got caught in a tangled web of Gotham politics and required roughly 10 years to finish. Second, this East River structure, built between 1900 and 1910, was conceived at a time when bridge engineering theory was in flux. As Thomas Kuhn stated in *The Structure of Scientific Revolutions*, they were shifting from one scientific paradigm to another. Unfortunately for the Manhattan Bridge, the newer paradigm was not fully refined until the 1920s. Third, a dismal maintenance schedule superimposed on a heavily used bridge with design problems was a perfect formula for disaster – not a literal disaster in which portions of the span crashed into the East River, but a figurative disaster in which repairing, reconstructing, and maintaining the Manhattan Bridge became a black hole capable of devouring hundreds of millions of dollars with only modest results. It is true, however, that some New Yorkers are quick to point out that the city has never suffered a major bridge disaster. Indeed, the Manhattan

Bridge and its constituency (primarily commuters traveling between Brooklyn and Manhattan on weekdays and "day-trippers" to Coney Island on the weekend) became analogous to two dependent people stuck in a bad marriage. In one sense, they can not afford to stay together, yet they can not afford to be apart. Stated another way, pouring hundreds of millions of dollars into rebuilding the Manhattan Bridge seems to make little economic sense, and yet the quarter million annual riders on the subways which use the structure cannot imagine life without it. This book endeavors to capture the unvarnished (or should it be unpainted?) truth regarding the history of the Manhattan Bridge.



NY and bridges from Brooklyn c1915

Design credit is normally divided among Gustav Lindenthal, Leon Moisseff, and O.F. Nichols, with portals by Carrere and Hastings. Essentially, the superstructure known as the Manhattan Bridge is a modern agglomeration of steel and nickel steel fabricated in Phoenixville, Pennsylvania by the Phoenix Bridge Company, accented with zinc-coated suspension cables. A cynic might protest that it has all the charm of an overly modern structure (for 1909) created by a hyperactive 12-year old with a gargantuan Erector Set.

The second chapter chronicles the opening decade of the 20th century for the bridge: a period of design, steel fabrication, endless delay, construction of piers, and erection of superstructure. Focusing on the entanglements of machine and reform politics, squabbling engineers, and a wide variety of additional actors, the theme that emerges is that business as usual in Gotham proved to be terribly costly and resulted in delays so extreme that observers justifiably labeled them scandalous. Consequently, the public (made up of teamsters, motorists, and straphangers) lost again. Fortunately for scholars, *Engineering News*, *Scientific American*, and *The New York Times* faithfully recorded, and thus preserved, the melodrama.

The third chapter, "The High Steel Worker's World," addresses the labor scene, focusing on issues such as the folklore surrounding it, injury and fatality rates, and who was responsible for the same. Just what did it mean to do high steel work? This chapter also centers on the impact of unions on industry, drawing from sources such as *The Bridgemen's Magazine*, the monthly voice of the International Association of Bridge and Structural Iron Workers (IAB&SIW). Among the interesting issues is the relation between the workers on the Manhattan Bridge and the infamous dynamiting strike conducted by the IAB&SIW during the first decade of the new century. Why did the Phoenix Bridge Company agree to let this East River span be a union job (with a 30 foot union banner), when the firm had a long history of being overwhelmingly committed to the open shop crusade?

The fourth chapter, "Canal Street and Flatbush Avenue Linked Effectively for Forty Years," takes a closer look at the engineering debate, something that went far beyond mere theoretical discourse during the period 1906-1909 when the Manhattan Bridge superstructure was actually being hoisted into place. The collapse of the Quebec Bridge over the St. Lawrence River in August of 1907 killing 75 workers on the south arm was more than just one more grotesque newspaper story. The human carnage and twisted steel reminded both American and Canadian readers that, indeed, great bridges do collapse. What's more, the Phoenix Bridge Company that built the Quebec Bridge was busily at work on a major project over the East River. Did American civil engineers really know what they were doing? Why not recreate the glorious masonry towers of the Brooklyn Bridge? And, who in their right mind ever decided that subway trains (each empty car weighing 40 tons) should run on the outside lanes of the Manhattan Bridge?

The fifth chapter is in some ways the key to the entire book, as it offers an overview of the serious structural problems that surfaced in the 1950s. These problems were so severe that some observers considered abandoning the Manhattan Bridge, deeming it a lost cause after little more than 40 years of service. The difficulties, as noted earlier, stemmed from flawed design and the frequent absence of maintenance. A careful combing of the historical record documents some traces of major concerns as early as the late 1930s. The talk of abandonment in 1953 sounds absurd to any civil engineer or serious bridge historian now. Knowledgeable observers cannot help but be mindful of the iron bridge erected in Coalbrookdale, England in 1779 that currently is doing just fine. Or, if someone wants to argue that the span at Coalbrookdale should be treated as an exception, the fact remains that most any wrought iron truss bridge of the 19th century should serve effectively for at least a century as long as there is at least some maintenance. So says Eric DeLony, Chief of the Historic American Engineering Record (HAER), and one of our nation's leading bridge historians. Bridge designer Gustav Lindenthal once observed that with proper maintenance the Manhattan Bridge could last forever. Having noted all this, through the second half of the 20th century, all conceivable problems seemed to have been compounded. The flow of traffic was almost beyond belief, the resulting torque on the outside of the span created by endless subway trains was extraordinary, and the search for corrosion only led to the discovery of corrosion under layers of corrosion! It is abundantly clear that all of the problems that might have been anticipated made their presence known from the 1950s through the 1970s.

The sixth chapter, "Tapping the Faucets in Washington, D.C.: Federal Dollars Come to the Aid of the Manhattan Bridge, 1978-2000," emphasizes the reconstruction work of Weidlinger Associates (consulting engineers headquartered in New York City), whose work has been underway since the late 1970s and has cost hundreds of millions of dollars. Let the record show that these kinds of expenditures in one decade on the Manhattan Bridge do not mean that a period of modest spending is in the offing. The fact is that discussions about bridge reconstruction seem to dwell on two concepts: decades and hundreds of millions of dollars. Not surprisingly, since The Federal Bridge Replacement Act was passed in 1978, the federal government has paid for 80% of the bridge's major reconstruction projects, leaving the city to shoulder the remaining 20%. With this cozy arrangement, perhaps the lights of the Manhattan Bridge will continue to illuminate the New York night for some time to come.

The seventh chapter serves as a conclusion and essentially asks, what are the insights to be garnered from this tale of woe? What does this saga tell us about the history of civil engineering in America? It focuses on the problems of building and maintaining the Manhattan Bridge and the extent to which they were peculiar to that structure. An example is the decision to allow subway trains to use the bridge. In light of the 2,000 plus repairs made annually in the early 1990s, it is worth questioning whether the New York City Transportation Department (who owns the bridge), other city officials, the MTA, the BRT, the commuters, or citizens should turn their backs on the structure and consider walking away. Fortunately, the grand agglomerations of steel known as bridges are not readily nor casually abandoned. In this case, citizens have grown accustomed to the outline of each particular span in the East River landscape. Photographers derive endless delight from grouping bridges in the same photo or capturing the outline of one through the cables of another. Indeed, living with the foibles and shortcomings of the Manhattan Bridge is simply part of being a New Yorker.

The Fourth Summer School of the European Historical Economics Society

By Jean-Pierre Dormois, University of Montpellier-III

(Montpellier) The fourth summer school of the European Historical Economics Society was held in Montpellier in the south of France June 14-18, 2002. Organized by Jean-Pascal Bassino and Jean-Pierre Dormois (University of Montpellier-III) and financed by the French Ministry of Research, 21 participants from different European and non-European countries attended. Meetings were held in the old medical college (originally founded in the 13th century) next to the Cathedral; the massive medieval masonry kept the atmosphere very cool and insured complete discretion. The central theme of the summer school, "Globalization in Historical Perspective," was originally submitted by Giovanni Federico (EUI Florence) and focused on trade issues in the European context. This was no small feat of courage in a city prone to rally in support of its local hero and archenemy of *mondialisation*, Jose Bove. Due to its relatively small size, the program was especially intense and relaxed at the same time, and it is hoped that participants drew as much benefit from it as they would have from a larger gathering.

A special guest speaker, **Michael Kitson** (Cambridge), opened the session on the first day with a lecture on "Trade and Growth: A Historical Perspective," which surveys the European experience of the past 150 years, summarizes the conclusions that can be drawn from it, and points to some unresolved issues. Among lessons from recent experience and scholarship, Kitson emphasizes the need for reducing the imbalances in trade and growth between world regions and for international exchange rate coordination.

Pedro Lains (ICS Lisbon) followed with "The European Periphery and the World." He sets out to identify and quantify open economy forces in the period 1870-1930 by comparing economic performance and trade openness indicators in southwestern Europe (Portugal) and southeastern Europe (the Balkans). Lains shows that the poor performance of these countries up until WWII cannot be exclusively assigned to their respective trade policies: free trade was imposed on the successor states of the Ottoman Empire in Europe, while Portugal remained strongly protectionist. **Giovanni Federico** turned next to Italian protectionism in war and peace, reaffirming his belief that, despite their apparent magnitude, Italian tariff duties had played a minor role in Italy's economic growth before WWII. In "Do Frontiers Give or Do Frontiers Take?" **Guillaume Daudin** (Stanford) discusses his findings on the impact of the transatlantic, or infamous "triangular trade," on the French economy in the 18th century. Among other things, he shows that profits from the slave trade were not widely out of line with other comparable activities and that, contrary to the enclave economies of the third world today, profits from trade were reinvested in the traditional, domestic, and urban economy. After a well-fueled discussion, participants adjourned to lunch in a garden restaurant nearby.

During the afternoon session, junior participants were invited to present their completed or in process Ph.D. research. **Ignacio Briones** (IEP Paris) revisits Chile's free banking experience in the last third of the 19th century. He contends that the truly competitive free banking episode lasted only until 1878, when the Chilean peso was made inconvertible and the state resumed the emission of "fiscal notes." While Chile's free banking experience was fraught with much publicized crises, its economy converged favorably with that of the leading European and American powers. Next, participants heard **Vincent Carpentier** (Institute for Education, London) present his thesis on "Education and Economic Growth in the UK, 19th-20th Centuries." He argues that Britain is another case of successive bursts of public investment in education (PIE) following economic downturns. In this respect, the UK's experience over the past centuries is parallel to other European countries. **Sunniva Engh** (Oxford) took our small party to the Indian subcontinent, at least in our imagination, to examine the different approaches of the Swedish and Norwegian governmental aid agencies in providing family planning and birth control. The Norwegian approach proved more efficient and longer lasting than its Swedish counterpart. The original design played a part, as did domestic political factors, in successfully adapting to local conditions and needs. This contribution, which included considerations about policy and public health (some alien to the economic historian's mindset) attracted numerous questions from the audience and sparked a lively debate. After this extra-European excursion, globalization made a comeback in the guise of a paper by **Emanuele Felice** (Pisa and LSE), "A Missed Opportunity: Southern Italy and Globalization in the Long Period, 1860-2000." Building on the already copious literature, Felice identifies the key factors which impeded several of South Italy's provinces from taking advantage of the open economy forces which pulled other regions in and outside Italy out of underdevelopment. The discussion, launched by Federico and Antonio Tena (Universidad Carlos III), centered on the explanatory factors which, on the basis of the available data, would lend themselves best to quantification tests.

After the exertions of the day, participants were able to relax. Sunday was dedicated to the discovery of Montpellier's hinterland, with a visit and lunch in the old town center of Pezenas, where Moliere made his debut. They also toured the former royal woolen manufactory of Villeneuvette (founded by order of Colbert in 1677 and in operation until 1954). The day ended with a visit to Valmagne, a Cistercian abbey, famous for its wines. A wine-tasting session duly followed the visit, expertly led by Guillaume Daudin.

The next morning, **Kristine Bruland** (Oslo) opened the first session with a presentation of the results and reflections on her 20 year-long research on the role of technology transfers in European industrialization. This contribution was especially welcome, as knowledge and technology often receive far less attention in debates on globalization than do factors of production. Bruland discusses particularly revealing examples that illustrated the channels of international technology transfers, as well as the obstacles to such transfers, in the 19th century. Turning to another facet of globalization, **Jean-Pascal Bassino** (Montpellier III), in his work with **Hironobu Nakagawa** (Aoyama Gakuin University), addresses the issue of why and how French Indochina succeeded in maintaining a silver standard until WWII, when the colonial power (France), and indeed most of the rest of the world, had already adopted the gold standard. Although questioned on repeated occasions between 1864 and 1940, a silver standard made sense for a small open economy that was a major rice exporter with China as its main trading partner. Archives reveal that local businessmen clearly favored this system. Counterfactuals confirm the sound basis of such policy, which was eventually reversed when French authorities decided the gold-silver ratio put French trade at too much of a disadvantage. Masahiro Hayafuji (WTO Geneva) saluted their work and wondered whether the use of dummy variables would significantly alter the regression results.

In the next presentation, **Antonio Tena** returned to the role of 19th-century tariffs in European growth. Surveying the various methods for measuring protection overtime, he shows the distorting effects implicit in the use of aggregate protection rates. Building on recent literature, he breaks new ground by investigating the use of differentiated protection rates. It appears that different conclusions can be drawn depending on the methodology used, something which is not always explicitly stipulated in the literature on the subject.

After this taste of Spanish scholarship, it seemed fitting to get a taste of Spanish cuisine, so participants left to enjoy tapas at the La Fuente restaurant nearby. Sangria flowed freely but in reasonable quantities under the blazing sun. When the assembly reconvened in the cool environs of the summer school, **Juan Flores** (IEP Paris) continued with an account of the causes of the Argentine bubble of the late 1880s; it burst in 1890, taking no less than Baring in its downfall. One fascinating aspect of this episode is that the acceleration of the flow of FDI into Argentina seems to have been prompted by an attempt by the government to reach a settlement with seven major European banks to stem the country's mounting indebtedness. The author maintains that the protagonists' changing perceptions of their interests and the opportunities at hand accounted for the temporary reversal of the country's financial situation. During the discussion, a commentator suggested the author could draw an interesting parallel from Mexico's similar experience prior to the Zapata uprising. **Gabriel Molteni** (LSE) retraces the career and ideas of the prominent economist Alejandro Ernesto Bunge, the author of *La Nueva Argentina* (1940), and outlines the impact on his thinking of the policies introduced by Presidents Castillo and Peron in the 1940s.

Next, **David Gilgen** (EUI Florence) examines the use and circumvention by German chemical firms of the Reich's first patent law of 1872. The liberal character of the law induced engineers, and later laboratories, to register their findings with the UK patent office so as to ward off the exploitation of their findings by their most serious competitors while collusion and market-sharing arrangements preserved the German domestic market. **Clemens Jobst** (IEP Paris) attempts to assess the credibility of the 1892 switch to a fixed exchange rate regime in Austria-Hungary, as the crown replaced the florin. He asserts that it was not the reduced independence of the central bank but the commitment of the government to a policy of high expenditure and high indebtedness which accounts for the success of monetary policy in the dual monarchy. Sweden also managed to maintain its specie standard in the face of a structural capital account deficit in the 19th century. For most other countries on a bimetallic standard, **Anders Ogren** (HHS Stockholm) argues the formal switch from silver to gold in 1873 did not alter the underlying soundness and adaptability of the monetary system. Trade flows and the resulting capital movements continued to serve as a main adjustment device to guarantee the external value of the currency.

On the last day (which coincided with the 187th anniversary of the battle of Waterloo), the sole morning session returned to the central issue of the program: European trade. **Patrick Verley** (Geneva) presented his extensive examination of the structure of world trade between 1840 and 1870. Drawing on his familiarity with the composition of domestic demand for consumer goods, especially textiles, he first stresses that windows of opportunity for exports had been few and narrow up to 1850. Hence his contention that it was nearly impossible for outsiders to compete once British export industries had taken their cut. Expanding on the work of S.B. Saul, he further establishes that France, and in particular Paris, had played a pivotal role as a clearing market between Britain and the US and between central and eastern European markets. This was due to France having adopted a bimetallic standard, of course, but also to the diversity of its trade and partners. One could go so far as to say that it was French merchants rather than French bankers who contributed most to maintaining the stability of the world monetary system during this period. **Philippe Guillaumet** (Aix-Marseille-II) offered his own contribution to the already abundant literature on tariffs and growth. He presented an array of econometric tests which he has conducted on French trade and growth statistics. His research covers the last two centuries, but here he concentrates on analyzing the results for the period of the Great Depression, which seems best to illustrate the trade and growth paradox. To conclude the session, **Jean-Pierre Dormois** (Montpellier-III) took the stand to uphold (along the lines adopted by Antonio Tena earlier) the opposite conclusion and reassert the validity of orthodox trade theory. Rather than working with aggregate growth and tariff rates, he has conducted panel analysis of labor productivity indices and effective protection rates by industry for three continental countries (France, Germany, and Italy) relative to the UK at three intervals between 1873 and 1913. His evidence clearly points towards a vindication of the standard implication of comparative advantage: commercial protection hampered productivity catch up in most cases or else was superfluous, a fact reinforced at a greater level of disaggregation.

Before retiring to the hotel and hence to the airport or rail station, participants agreed that the last 96 hours had been well spent, that exchanges had been fruitful, and that the conference logistics had been satisfactory. As there was some time before lunch that day, a delegation which still had some time to spare paid a visit to the municipal art museum, which possesses rich collections of European art and was currently hosting an exhibition of American paintings from the first half of the 20th century, a welcome reminder that it is not just McDonalds and cliometrics that come from America.

For more information on the EHES Summer School, you can go to the following website: www.univ-montp3.fr/ehes-summer_school. In addition, information and papers can be obtained with the consent from their authors by contacting Jean-Pierre Dormois at jean-pierre.dormois@univ-montp3.fr. The Society's 2003 Summer School is due to meet in Paris and will focus on financial aspects.

Clio in Retrospect: 1969

By Michael J. Haupert, UW-La Crosse

Individuals in bold type are currently listed on EH.Net

(West Lafayette) It was youth on parade at the 1969 Cliometrics meetings, held on the campus of Purdue University Jan 30-Feb 1,1969. The young turks of the economic history profession with their freshly minted Ph.D.s dominated the presentations and discussions. Larry Neal, Richard Sylla, Franklin Mendels, Donald McCloskey, George Grantham, and Gavin Wright all represented the future of the discipline, showing off their novel approaches to financial history, econometrics, and agricultural history. Their findings were presented before a feisty crowd, definitely on the young side, including neophytes **Fred Bateman** (Indiana), **Lou Cain** (Loyola), **Tom Weiss** (Kansas), and two fellows everyone assumed were brothers: **Sam** (Iowa) and **Jeff** (Wisconsin) **Williamson**. Some of the old guard in attendance included Alice Jones (Wash U.), Rondo Cameron (Wisconsin), **Lance Davis** (CalTech), and Harold Williamson (Northwestern).

On campus, the students wore bellbottoms, long hair, and sunglasses. They chanted anti-war slogans and preached love and peace, spreading their gospel through rock and roll, free sex, and drugs. The Clioms, in all their sartorial splendor, arrived in plaid pants, black-framed eyeglasses, and pocket protectors, ready to debate the relative merits of counterfactuals, statistical significance, and the latest dispersion models.

Larry Neal (Illinois) opened the proceedings on Thursday, presenting his research on "Investment Behavior by American Railroads, 1897-1914." This would appear later in the year in the May issue of *The Review of Economics and Statistics*. Neal's major claim is that American railroad investment behavior in this period is best understood by emphasizing the role of financial factors. His argument rests on the significant changes that he observes in railroad finance during this period. The changes permitted easier access to funds from sources external to the companies and likewise permitted more internal funds to be used for capital formation.

Next on the docket, **Richard Sylla** (North Carolina State) discussed "Banking Market Structure and Capital Mobilization after the

Civil War." The *Journal of Economic History* would publish this paper in December of 1969. Financial history had been the subject of his recently completed dissertation at Harvard, but Sylla assured his colleagues the topic was only a passing fancy and that he planned to spend the rest of his career on more traditional pursuits. In this study, Sylla deals with the origins of the development of American banking, which increased capital mobility, making it a critical determinant of both the timing and pace of industrialization in the late 19th century.

Larry Wimmer (Brigham Young) continued the theme of financial history with "The Gold Crisis of 1869: A Problem in Domestic Economic Policy and International Trade Theory."

Raymond Goldsmith (Yale) discussed his broad-based research program on comparative financial development since the late 19th century. He would summarize his long-running study with the publication of "The Quantitative International Comparison of Financial Structure and Development," in the March 1975 issue of the *Journal of Economic History.* Goldsmith argues that the quantitative international comparison of financial structure and development involves three levels of problems: identifying the purpose of such comparisons, the concrete features of the financial system to be compared, and how the comparisons are to be carried out. He acknowledges that this will be a long run project for which he does not currently have many answers, but notes that nobody else seems to have any answers either.

Four Clioms found time between their presentations to join a student protest on the Purdue campus.

The Thursday sessions were closed out by Alexander Korns (Harvard) with his presentation of "Real Wages and Foreign Trade in the Civil War Era."

Talk at the dinner table that night focuses on the papers delivered that day. The Korns presentation on real wages during the Civil War sets Clioms to comparing wages a century later. They wonder how much longer the best of them will be able to resist the call to more lucrative fields such as accounting, where mere clerks are hauling in \$500 a month, or finance, since Wall Street is currently luring new graduates with \$9000 starting offers. The agricultural historians in the crowd note that farm labor is lagging behind the wage run-up, averaging only \$3650 per year.

Raymond Dacey (Purdue) discussed one of Clio's favorite new toys, the counterfactual, when he opened the Friday sessions with "Aspects of the 'Counterfactual Controversy.' " Thomas Craagh (North Carolina) followed with another novel idea, which he debuted in "A Model for the Dispersion of the Migrant Labor Force and Its Results for the United States, 1880-1920."

Franklin Mendels (Wisconsin) presented research from his dissertation when he discussed "Population Pressure and Rural Industrialization in Early Modern Europe." Mendels would eventually publish his dissertation summary in the *Journal of Economic History* (March 1971) and a chapter from the dissertation a year later, also in the *JEH* ("Proto-Industrialization: The First Phase of the Industrialization Process"). Mendels notes that during the century preceding the beginning of modern industrialization, several regions of Europe experienced an impressive growth in traditional rural handicrafts and, concurrently, acceleration in their rate of population growth. In investigating the relationships between "proto-industrialization" and the acceleration of population growth during this period, he finds *prima facie* evidence that in 18th-century Flanders, industrial growth was induced and promoted by population growth.

George Grantham (Yale) followed with his paper: "French Agricultural Productivity – 1852-1882." This research project would prove to be long-running, resulting in a *JEH* publication of "The Diffusion of the New Husbandry in Northern France, 1815-1840" (June 1978). Grantham discusses agricultural innovation in early 19th-century France. The core of his argument is that the diffusion of the new intensive mixed husbandry in northern France was delayed by the slow growth in demand for meat and dairy products before 1840, which reduced the advantages to be gained from adopting forage-intensive crop rotations.

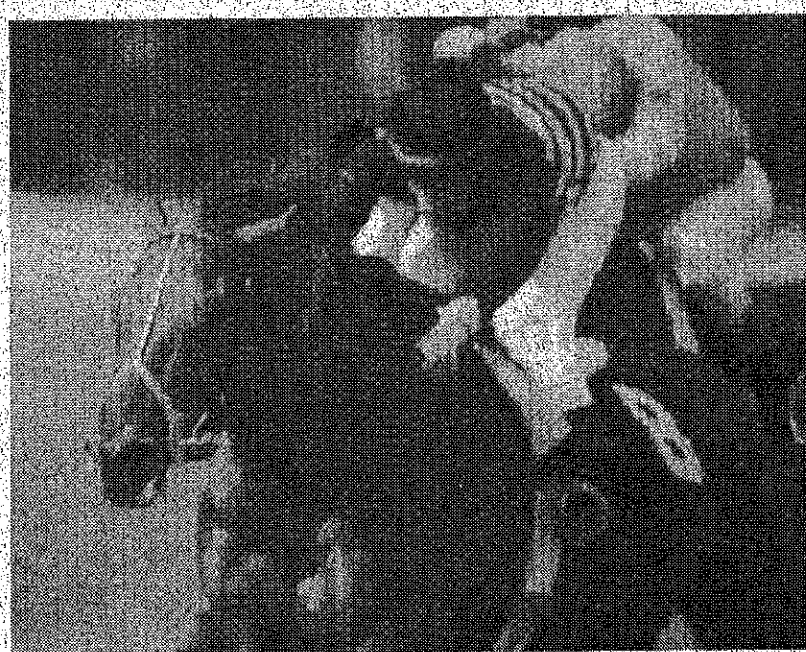
"The Explanation of Slower British Growth after 1870: Criticism and Measurement" continued the themes of youth and novelty at this year's conference. **Donald McCloskey** (Chicago) presented his research, which would eventually lead to a legion of publications on the topic of Victorian England. This paper was an outgrowth of McCloskey's recently completed dissertation, "The British Iron and Steel Industry, 1870-1914: A Study of the Climacteric in Productivity," summarized in the forthcoming *Journal of Economic History* (March 1969). McCloskey remained at the center of this debate for years, publishing numerous articles and books on the subject, including a follow-up to this presentation, "Did Victorian Britain Fail?" in the December 1970 issue of *The Economic History Review*.

The theme of novelty that dominated the day carries over to discussions at the dinner table. Clioms chat about some of their newest toys, such as the Polaroid instant camera (a steal at $50), and the latest rage in lawn ornamentation, the plastic pink flamingo. You can bring Florida into your yard even in the winter chill of Indiana for a mere $3.69 at the local K-mart. A whole flock can be tastefully dispersed throughout your yard for less than the cost of a new typewriter. The hottest discussion, however, is saved for the revolution sweeping the recording industry. Cliometricians prove that they are prognosticators of the finest degree when they confidently predict a long and glorious future for the 8 track stereo tape player. Several have upgraded their stereo systems by adding this component, which is widely available for about $70.

The final day of the conference was opened by **Gavin Wright** (Yale). Wright presented "The Distribution of Agriculture Wealth in the South and Economies of Scale, 1840-1860." This proved to be more than a passing fancy for Wright. He would go on to publish numerous articles on the southern economy, including an outgrowth of this paper, "An Econometric Study of Cotton Production and Trade, 1830-1860." This would appear in *The Review of Economics and Statistics* in May 1971.

Diane Lindstrom (Eleutherian Mills-Hagley Foundation) closed out the meetings with her research on interregional and intraregional grain flows in the antebellum South.

The conclusion of another meeting of Cliometricians kicked off a big year. 1969 would see a man walk on the moon, the debut of *Penthouse* and the demise of *The Saturday Evening Post*, and the retirement of Sheriff Andy Taylor from the Mayberry, North Carolina force. Some were heard to suggest as they departed West Lafayette that a fitting tribute would be to move the meetings to North Carolina. Everybody agreed that this was a great idea, but nobody suspected that it would take 35 years to do it.



Majestic Prince – 1969 Kentucky Derby Winner

Walt W. Rostow, 1916-2003

Walt W. Rostow, Rex G. Baker, Jr. Professor Emeritus of Political Economy at the University of Texas at Austin, died February 13, 2003 at the age of 86. He is survived by his wife, two children, and one grandchild.

Rostow was born October 7, 1916 in New York City. He received his B.A. (1936) and Ph.D. (1940) from Yale University and attended Oxford University as a Rhodes Scholar from 1936-38. He joined the economics faculty at Columbia University in 1940 before leaving for a long and distinguished career in government service.

LBJ consults with Walt Rostow

He served as a Major in the OSS during WWII and joined the State Department after the war as assistant chief of the German-Austrian Economic Division. He later became the assistant to the executive secretary of the Economic Commission for Europe. He returned to the classroom for a decade before joining the Kennedy and then Johnson administrations in a series of appointments beginning as deputy special assistant to President Kennedy for national security affairs in 1961. This was followed by appointments as counselor of the US Department of State and chairman of the Policy Planning Council of the Department of State. In 1964, he added the position of ambassador of the Inter-American Committee on the Alliance for Progress before returning to the White House in 1966 as special assistant for national security affairs. He returned to the classroom for good in 1969, joining the faculty at the University of Texas at Austin.

It was during his appointment as professor of economic history at MIT from 1950-1961 that he wrote his most famous book, *The Stages of Economic Growth*, which went on to become a classic, reprinted for the third time in 1990. In it, he characterized the process of modern growth through five stages and introduced the term "take-off into sustained growth" to the economics lexicon. This work continues to have an impact on the development policy literature.

Rostow received many honors during his lifetime, including the Order of the British Empire in 1945 (honorary, military division), the Legion of Merit in 1945, and the Presidential Medal of Freedom in 1969 (with distinction). He was a member of the Board of Foreign Scholarships (1969-71) and served as chairman of the board (1992-98) of The Austin Project, which he helped to found in 1992.

He remained active in the classroom until his death, teaching an undergraduate course in the fall and a graduate seminar each spring. He was also still active on the lecture circuit, recently speaking about the population problem, and had continued to publish scholarly articles and editorials.

Personal Reflections

Confessions of a Card-Carrying Economic Historian

By Jan de Vries

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

The ongoing appeal to me of economic history as an intellectual practice is located in a cluster of interrelated characteristics. This is an academic practice that is truly part of history and truly part of economics, and it links these identities in a 'middle kingdom' that is at once recognisable and porous, with its own tradition but open to syncretic adaptations of all sorts.

Interdisciplinarity - We hear a great deal about interdisciplinary and multidisciplinary studies. Sometimes it appears that the interstices between every disciplinary boundary have been colonised by bands of academic pioneers, exiles, squatters, etc. Not many of these enterprises flourish for long, and still fewer possess the characteristic that gives economic history its strength and appeal. That is, our field of study, at its best, is not an *escape* from discipline but entails an *embrace* of two disciplines - two demanding and very different disciplines. The economic historian joins an inductive and a deductive, an idiographic and a nomothetic discipline in what necessarily must be a difficult embrace. This is the source of its interest to me, and, I would add, of its value, not least to the two disciplines with which economic history must remain in continual dialogue.

Understanding the past - There are, of course, many mansions in Clio's house, and more than a few provide valuable historical knowledge. And, only a naif of the first rank would now propose that a historian of any sort can uncover the plain and unvarnished truth about the past, to know *Wie es eigentlich gewezen*. But economic history - its undeserved reputation for unrepentant positivism notwithstanding - recognised long before most types of history the futility of supposing that research into the past could yield unmediated historical knowledge. It was, indeed, the engagement with economics that trained historians to seek out repeated and patterned behaviours, make comparisons and measure. Because economic life is so evidently the joint and often unexpected product of many people, the economic historian has long known that one cannot embark on research without first being equipped with organising concepts, measurement tools, and theory. We do not all cast our work in the 'hypothetico-deductive models' that the New Economic Historians had called for in their Young Turk phase, but more than in other fields, the reader of our work is informed of the assumptions and given the means to replicate and test the findings. Does this 'scientific' apparatus mark the economic historian as a deluded positivist or is it the attribute of a scholar acutely aware of the contested nature of all historical claims, as someone eager to engage in the debate rather than painfully to construct a terrain of exclusivity, accessible to no one but the author?

The unavoidably 'constructed' character of most economic history offers what for me is a highly satisfactory sort of knowledge. At its best it reveals how (a bit of) the world works. Three aspects of research in the field come to mind that, I believe, contribute to this satisfaction.

First, it is *intended* to be tested, challenged, and improved upon. Most historians today deny the notion that knowledge in their field is cumulative. The current weakness of the impulse to empirical refinement undercuts the basic motivation for maintaining a broadly-based discourse, leaving historians isolated, talking only to their closest soul-mates. Economic history remains an 'open' discipline, seeing this as the guarantor of improved historical understanding.

Second, it is an inherently inclusive field of study. The theoretical material most of us bring to economic history does not readily allow for a discourse of 'otherness' and a sentimental savouring of the impossibility of understanding the behaviour of distant peoples, past times, or other genders or races. Our personal biases can limit the field, to be sure, but the intellectual foundations of the field must lead us, sooner or later, to overcome such limitations. There are, of course, those who suppose economic historians are unavoidably apologists for the rich and powerful. Perhaps we are more inclined than other historians to seek to understand the challenges they face, but the same should apply as we approach inventors, workers, peasants, housewives, pensioners, slaves, and all the other socio-economic categories of historical humankind with which we are concerned.

Third, and finally, economic history leads to the creation of complex reconstructions of the past. Rarely can we come to grips with a problem by focusing on an individual or by presenting a simple chronological narration of events. Our field recognises the importance of what Fernand Braudel called the 'plurality of social time', if only in a consciousness of the distinction between short-run and long-run processes, and it is inclined to reject the 'pernicious humanism' that limits so much historical research to a sterile anthropocentrism. These holistic impulses and the capacity to develop complex models of historical change release us from the banality of the narrative form that is the conspicuous mark of the larger discipline's failure of nerve in this generation.

Understanding economics - The interdisciplinary and social scientific character of economic history gives it a special place in the discipline of history, and thus far I have emphasised the role it can play in returning that errant discipline to its full vocation. It has a role no less important to play in dialogue with the discipline of economics.

For many years this role was defined by the New Economic Historians who emerged in the 1960s and quickly came to dominate the field in the United States. Economic History was destined to be a type of applied economics, testing with historical data models founded on neoclassical theory. The challenge to history was clear: replace stories and narration with testable hypotheses and analysis; reveal all assumptions and biases. However, these bold, reforming claims went yoked with a major weakness: reliance on an ahistorical theory whose impressive power is restricted to tests of market rationality, efficient allocation, and similar static concepts. Dynamic models could incorporate locomotion, but not irreversible, contingent change. In short, ahistorical theory can give convincing answers only to relatively minor historical questions. And what, in fact, does such work contribute to economic theory? Robert Solow may have been unduly severe when he answered 'It gives back to the theorist the same routine gruel that the economic theorist gives to the historian. Why should I believe, when it is applied to thin eighteenth century data, something that carries no conviction when it is done with more ample twentieth century data?' In fact, imaginative use of these limited tests have often supported arguments concerning larger issues of dynamic economic change. But an economic history limited to neoclassical theory remains a tethered beast.

This New Economic History, tethered or not, has gained adherents internationally and remains influential today. Indeed, I am enough a child of this movement to view its achievements with respect and admiration. But from its earliest days there were critics who urged a restoration of focus on long-term economic change. Instead of a social scientific history, we should aspire to develop an historical social science. This impulse has led in various direction, ranging from the new institutionalism of Douglass North, to the 'total history' concept of Fernand Braudel, with its aim of crowning history as the 'queen of the social sciences'.

A history equipped to give depth and meaning to the superficial, event-focused disciplines remains a dream as noble as it is distant, but there is an unmistakable growth of interest in concepts that seek to restore time and space - history and geography - to economic theory. The new growth theory, the concept of path dependence, and models of *homo aeconomicus* that add fear and love to the familiar impulse toward greed, all act to convince the economist that 'history matters'. Indeed, the more general drift away from Newtonian toward biological metaphors opens new possibilities for fruitful interaction between history and theories of complexity.

Boundaries - The appeal of economic history to me lies primarily in the satisfaction that comes from the special effort required to join two different and demanding disciplines. If the cost-benefit ratio seems attractive to me, perhaps this reflects a taste for boundary crossing. And this, in turn, may explain why I find the most attractive current areas of research to be those that also feature boundary issues. They are general historical issues, but I believe that economic history can play a leading role in their exploration. Let me mention the two most general: the boundaries between 'early modern' and 'late modern' history, and the (multiple) boundaries that limit deployment of a world history perspective.

Longevity has given Western history's conventions of periodisation a venerability that will prevent their speedy dismantling. But there are unmistakable trends toward a 'deconstruction' of the historiographical assumptions that under-girded the boundaries between ancient and medieval, and between late medieval and early modern. Economic historians have played, at best, ancillary roles in these developments. Now it is the great eighteenth century revolutions that guard the portals to modernity that are under discussion. They form a monument of periodisation that, more than the others, is implicated in the most important models of economics, political science, and sociology. There is little doubt in my mind that our current reassessments of what the future might look like will force a continued re-examination of the concept of 'modern history' that was so deeply implicated in the justification of a future that has now vanished from view. The French Revolution, as it was cultivated for over a century, is over; the British Industrial Revolution is also on its way to becoming something other than what we have known for most of the twentieth century. Undoubtedly, new principles will be advanced for the compartmentalisation and analysis of historical time; I suspect the years around 1800 will fade as a boundary, and that this will allow fruitful new questions to emerge, especially in economic history.

The second boundary issue concerns the effort to construct a foundation for a non-Eurocentric world history. Economic history, because of its long interest in comparative method, has much to offer this project, but much of what passes for world history is still transparently a form of colonial and imperial history. Without wishing to minimise the difficulties ahead, I believe that economic history has real opportunities to lead in the development of suitable models for integrated and comparative studies that transcend historiographical traditions.

A useful example is found in current debates about the concept of 'early modern' as a world – rather than a European – historical category. Some specialists in the field, such as Thomas Brady, argue that the very fact that most traditional European claims for the modernity of the Renaissance and Reformation have been decanted from the term 'early modern', it is now suitable as a vehicle to explore intercontinental – at least Eurasian – commonalities in the first era of continuous and direct commercial and political interaction. Others, most recently and vigorously Jack Goldstone, remain concerned by the 'Eurocentric' baggage that unavoidably is carried by the term. He advocates a new label, but shares the view that European and Asian societies in some 'pre-modern' era yet to be given a name can be studied with a common tool kit. At present these useful conceptual contributions and some rich, substantive studies of comparative history are mingled with no small amount of politically-charged rhetoric. But I am inclined to the view that all of it together constitutes a sign that our 'inter-discipline' has much to contribute at these boundaries.

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[1]Robert Solow, 'Economic History and Economies', *American Economic Review* 75 (1985), p. 330.

[2]Thomas A. Brady, Jr., 'Introduction: Renaissance and Reformation, Late Middle Ages and Early Modern Era', in Thomas A. Brady, Jr., Heiko Oberman, and James Tracy, eds., *Handbook of Early Modern European History*, (Leiden, Brill Publishers, 1995), Vol 1, p. xxi.

[3]Jack A. Goldstone, 'The Problem of the "Early Modern" World,' *Journal for the Economic and Social History of the Orient* 41 (1999), 249-84.

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Making buckaroo biscuits in a Dutch oven

The Cliometrics Ph.D. Program at the University of Munich

By Michael Haupert, UW-La Crosse

Economic History plays an important role in the training of economists; Milton Friedman's classic treatise on money, as well as Simon Kuznets's path-breaking work on economic development, for example, were, to a considerable degree, based on historical analysis. We analyse the dynamic processes of development over time by formulating explicit formal models and econometric methods. We test hypotheses formally in order to enhance our understanding of such major determinants of the way we live today as the industrial revolution, industrialization and the information revolution. We use historical (often archival) data to test the extent to which economic theory can be validated or improved upon in a wide array of ways, spawning totally new perspectives, such as counterfactual history. The granting of the 1993 Nobel Prize in Economics to two economic historians, Douglass North and Robert Fogel, is a clear recognition of our unique scientific contribution to the discipline.

– **On The Role of Economic History,** http://www.vwl.uni-muenchen.de/ls_komlos/index.html

John Komlos has held the position of Professor of Economics and Chair of the Institute of Economic History at the University of Munich since 1992. He was also chair of the Department of Economics from 1997-99. During the summers of 1994 and 1995, he offered a seminar entitled "The Industrial Revolution in Comparative Perspective" for American economists, funded by the National Endowment for Humanities. John was kind enough to answer some questions regarding the Ph.D. in cliometrics offered by the University of Munich.

How does a doctoral program in cliometrics differ from a traditional economic history program?

It differs in the application of economic theory and data-intensive econometrics to historical issues, which is not expected in a traditional program. This is essentially what distinguishes cliometrics from traditional approaches in general.

What are the essential elements of the cliometrics program in Munich?

I initiated the program soon after my arrival here in 1992 as a natural outgrowth of my own interests. It has a three-tier structure. At the undergraduate level, students can take a minor concentration in the field and also write their senior thesis on cliometric topics, which is usually a substantial piece of work 60-100 pages long. At the graduate level, there are two teaching assistantships dedicated to cliometrics. This gives the students an opportunity to work on their doctorates while receiving a decent salary. Teaching one recitation section is part of their obligations. The third element of the program is a postdoctoral fellowship, also funded by the department, but, of course, the funds ultimately come from the Bavarian Ministry of Education. Postdocs have a four semester-hour teaching load. We have had two American postdocs here. Brian A'Hearn in Italian economic history from Berkeley, an Eichengreen student, has gone on to win the prestigious T.S. Ashton prize for one of his articles and is currently teaching at Franklin and Marshall College. The other postdoc was Douglas Puffert, a Brian Arthur and Paul David student from Stanford, who is working on path dependence and is now at Warwick. He has a book coming out with the

University of Chicago Press. So all in all, the program has been able to support a variety of work in cliometrics.

We do not specialize in any one historical epoch, as our current postdoc in medieval cliometrics, Ulf Ewert, indicates. By the way, actively inviting visitors is an integral part of the program. We have had a very large number of mostly American cliometricians coming through Munich for extended stays of up to one month or longer. The list includes Gregory Clark, Lee Craig, Timothy Guinnane, Michael Haines, John James, Richard Steckel, and Richard Sylla. James Dunlevy spent a year here on a Fulbright fellowship. Many others have given workshop presentations. Our Nobel Prize winners have visited us for shorter stays. Patrick O'Brien will give a set of lectures on globalization and Boris Mironov on Russian economic history next summer. And, of course, we hosted the Third World Cliometric Congress here in 1997.

How is the program funded?

The teaching assistantships are permanently funded by the department. In addition, there are fellowships available through the Department of Economics funded by the German National Science Foundation for which aspiring cliometricians are eligible to compete. (http://www.vwl.uni-muenchen.de/ls_rady/kolleg.html) Although students also have the opportunity to work on a doctorate without a fellowship, this is not very attractive within Germany because of the very high opportunity costs involved, even if the German welfare state does not charge tuition for students at either the graduate or undergraduate level.

What do you expect the job market will be like for these graduates *vis-a-vis* traditional economic history Ph.D.s?

Insofar as graduates have substantial training in quantitative analysis as well as in economics, they are quite sought after by the private sector, in stark contrast to traditional historians. The study of econometrics is not as widespread in Germany as in the US, so cliometricians also have some comparative advantages *vis-a-vis* economic students, who may or may not have had training in quantitative analysis.

What type of research do you foresee these dissertations covering? How might it differ from economic history dissertations?

Work here has, in the main, reflected my own tastes in research in being iconoclastic by using new approaches in cliometrics, such as path dependence and anthropometric history. Moreover, dissertations have applied, and I suspect will continue to apply, new theoretical insights to historical topics. Krause's dissertation applied, for the first time to my knowledge, Krugman's new trade theory to a historical topic: the German soda industry. His paper came out recently in *EREH*. Stogbauer's dissertation has applied the traditional cliometric tool of a counterfactual model to the German elections preceding the Nazi takeover in order to ascertain if the takeover could have been prevented by a budget-neutral fiscal policy that reallocated unemployment spatially. He has also published in *EREH*. Jorg Baten's dissertation on the anthropometric history of Bavaria has just received the best dissertation award from the International Economic History Association awarded in Buenos Aires. All in all, there has been a tendency to work on German topics, but this is changing. Marco Sunder is working on the "antebellum puzzle" on the basis of US passport data, and Marielouise Baur is using the NHANES data set to try to figure out why the physical stature of Americans has fallen so far behind relative to West Europeans in the second half of the 20th century after having the tallest population in the world for two centuries. This is obviously quite an anomaly worth investigating.

What do you tell a student who asks "Should I get a degree in cliometrics or economic history?"

Cliometricians, as Krause and Stogbauer have shown, can find good jobs in the private sector. They are quite sought after. That is not the case for traditional economic historians. Cliometricians also fare well in the academic job market in Germany, as Jorg Baten's experience testifies. He has recently become the youngest full professor in economic history in Germany, in no small part due to his cliometric training.

Does economics need another doctoral program? In what way that is not currently being covered will these graduates contribute to the discipline?

It is common knowledge that cliometrics has not fared so well on the Continent, including in Germany. Richard Tilly has made considerable inroads beginning in the late 1960s, and the Munich program will hopefully succeed in finally establishing cliometrics in Germany on a solid academic footing. I suspect that this goal will become easier now that there are two other cliometrics programs in Germany: the one in Tubingen established by Baten last year and the one at the Humboldt University in Berlin established at the same time by Albrecht Ritschl. Ritschl is also from Munich and has worked with my predecessor, Karl Borchardt. These are signs that cliometrics will have a permanent and more visible presence in the German academic scene.

For more information about the Cliometrics Ph.D. and the Seminar fur Wirtschaftsgeschichte at the University of Munich, visit the Institute webpage at http://www.vwl.uni-muenchen.de/ls_komlos.

Fifth World Congress of Cliometrics

Venice, Italy
July 8-11, 2004

The Fifth World Congress of Cliometrics will be held July 8-11, 2004, at Venice International University on the island of San Servolo. Please visit the VIU website *http://www.viu.unive.it* for additional information and pictures. A call for papers will be issued later this year. A program from the proposals will be posted on the Cliometric Society website early in 2004. Complete information about registration and lodging and the Congress Registration Form will also be announced on the Cliometric Society website soon.

All members of sponsoring organizations will be invited to attend. The organizing and program committees consist of Lee Craig, Price Fishback, Albrecht Ritschl, and Gianni Toniolo.

A Letter from the Editor

Gollllly!

This year, CBS will celebrate the 43rd anniversary of the incorporation of the town of Mayberry, North Carolina with a reunion of the cast of the Andy Griffith show. From May 23-25, Cliometicians will celebrate the 43rd reunion of their clan with a gathering in Raleigh, North Carolina, the virtual backyard of the bustling metropolis of Mayberry. Consider that the Clio retrospective in this issue focuses on 1969, the first year of the decade without Sheriff Andy Taylor protecting his streets, and this confluence of events takes on an aura beyond mere coincidence.

While pondering the deeper meaning of this series of events, it occurred to me that Clio and Mayberry are really two parallel universes. The population of Mayberry could quite easily be cast with Cliometricians. In particular, the personalities of the leading characters are mirror images of one another. Many of you must remember the main cast of the show: Sheriff Andy Taylor, whose relaxed attitude, down-home wisdom, and strong moral character saved many a Mayberryian from the clutches of big city slickers. Time and again, Andy's mere presence calmed the storms around town. And there was the sheriff's deputy and cousin, Barney Fife, who was as well-meaning as he was incompetent. Who could better personify these two guardians of the town than our current executive directors of Clio and the EHA?

Watching over the widower Andy was his Aunt Bea, the consummate 1950s stay-at-home-and-take-care-of-the-men-in-her-life television matron. Also under her watchful eye and excellent cooking (most especially that award winning apple pie, whose secret ingredient she took to her grave) was Andy's little boy, Opie. Rumor has it a founder of Clio (and a past EHA president) bakes a mean apple pie, using a secret ingredient whose origins she has never revealed. Coincidence? I think not.

Opie himself was the picture of wide-eyed innocence and optimism. He clearly benefited from the wise tutelage of his father, as his understanding of human nature defied his youth – just like a certain second generation Cliometrician who has graced our presence and led the Cliom's for many a year.

The town also featured a gas pump jockey named Gomer Pyle. Gomer never met a stranger, and no topic was too great or too small for Gomer to expound upon at length. It isn't easy to find anyone who pumps gasoline anymore, especially one ready and willing to dispense copious amounts of advice and assistance to unsuspecting customers. And as many of you are aware, you had best set aside some extra time whenever you ask a question of a certain trustee and long-time Cliom. And then of course, there was Floyd the barber. Floyd knew everyone and everything, as any good barber should, and he never failed to share it – all of it, all the time.

I've never been to Mayberry before, nor for that matter have I ever been to Raleigh. I think I'll keep my eyes and ears open while I'm in the neighborhood and see who stops by. Enjoy the summer; maybe I'll see you at the fishin' hole.

Michael J. Haupert
Editor

Separated at birth?