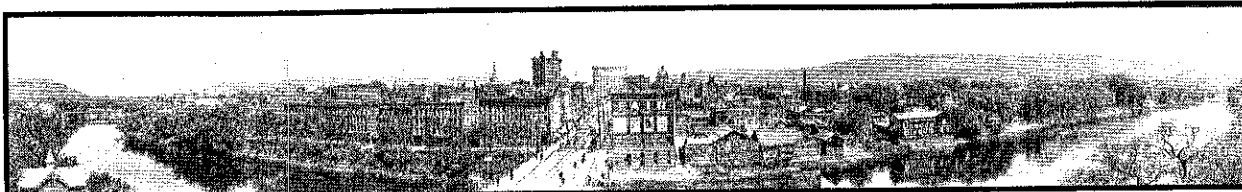


The Cliometric Society

Spring 2006 Vol. 21 No. 1



Binghamton, NY c1909

The Cliometric Society and Economic History Association Sessions at ASSA 2006

By Claudia Rei (Boston University), Jesus Viejo (Boston University), and
Melinda Miller (Michigan)

(Boston) The Allied Social Science Associations held its annual meeting January 6-8 at the Sheraton Hotel in Boston, Massachusetts. The program of sessions for both the Cliometrics Society and the Economic History Association was arranged by Werner Troesken (University of Pittsburgh and NBER).

Robert Margo (Boston University and NBER) chaired the opening session of the Cliometrics Society, "Wealth and Mobility in Historical Perspective." **Howard Bodenhorn** (Lafayette College and NBER) presented the first paper, "Single Parenthood and Childhood Outcomes in the Mid-Nineteenth-Century Urban South." Using 1860 census data from Baltimore and New Orleans, Bodenhorn examines the outcomes of both white and free black children who live in single parent households. Using measures of poverty, school attendance, and the labor force participation of 14- to 16-year-olds, he finds worse outcomes for children in single parent households, independent of wealth status. Margo complimented Bodenhorn's careful data analysis and good grasp of history. He suggested using the death of a woman's husband in 1860 as an instrument for linking to the 1850 census for a difference-in-difference technique.

Next, **Art Carden** (Washington University-St. Louis) spoke on "Trial by Fury: Institutions and Southern Productivity." He theorizes that unstable Southern property rights in the postbellum era led to a decrease in productivity. To proxy for the strength of a state's institutions, he uses the total number of historical lynchings in each state. Under a variety of specifications, his OLS regressions show that increasing the strength and quality of institutions by 10% was associated with increasing productivity by 1.35%. Raven Saks (Federal Reserve Board) pointed out the importance of distinguishing between two stories. Weak institutions could lead to an increase in lynchings, which, in turn, leads to a decrease in productivity. Alternatively, weak institutions

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Executive Director's Notes

A Note from the Publisher:

In the most recent issue of the *Newsletter*, the Society's executive director, Lee Craig, announced his retirement, and he called for candidates for the job to step forward. In response, several members have asked exactly what the executive director does. Rather than give a written description, we asked Lee to describe a typical day. He has done so by sending us a page from his calendar, which is also the official Clio log. Here it is:

7:30-8:30AM

Phone: Spoke w/Kate Moss's agent, re: Clio Ad campaign featuring Kate and Dick Sylla. "Kate in pearls-Dick in tweed." Could hear Kate in the background: "No #\$\$%&ing way"!!!

Project put on hold.

8:30-9:30AM

Correspondence: Wrote "thank you" to all of the Society's members who recently sent donations and gifts.

10:30-11:00AM

Closed office door, made tea, read the *Wall Street Journal*, and checked balance in TIAA-CREF.

11:00-Noon

Photo shoot for upcoming cover of *Explorations in Economic History*.

Memo to self: Next time, remind Bob Margo to wear dark socks.

Noon-1:00PM

Lunch w/big-shot university administrator. Nothing but praise for the Society; good publicity, etc. Ends lunch by asking, "What exactly is a Clio?" Calls me "Steve".

1:00-1:15PM

Check TIAA-CREF balance.

1:15-1:45PM

Phone: Kate Moss's agent, again. Says Kate's definitely available. (Something about legal troubles.) I share my vision: "Kate in pearls..." However, Kate will only work with a current trustee. Dick is out. I propose Tom Weiss. Pause. It's a "maybe" at Kate's end.

1:45-3:00PM

Class! Told them the "Wizard of Oz as a Monetary Allegory." Forgot most of the details; confused the characters. Still, they seemed to like it better than cost functions.

3:00-3:30PM

Oprah called while I was in class. She's P.O. ed I wasn't there to take her call. Refuses to take mine. I spoke to her producer about a Clio episode titled "A Million Little Pieces of Data."

Project put on hold.

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3:30-5:00PM

Went to the gym to work out. Ended up just sitting in the sauna. Steve Margolis came in and told "path dependent" jokes. Not all that funny.

5:00-5:30PM

Paid Society's bills. Phoned home, told the family I'd be late for dinner. Ad campaign and all that. Youngest daughter says, "Mom says you better not be fooling around trying to get Kate Moss." I tell her I don't even know who Kate Moss is.

Memo to self: Smooth feathers later.

5:30-6:00PM

Tidy desk.

6:00-7:30PM

Stop by faculty club for drink and dinner. Lots of superficial bonhomie.

7:30-9:00PM

Phone: Kate Moss's agent, again. Big fight; scheduling; dollars and cents; "artistic control" issues. I hold my ground. Kate, swearing in the background. Finally, we do a deal. Kate in tweed-Tom Weiss in pearls?!?!
Memo to self: Cancel with Wayne Newton's people.

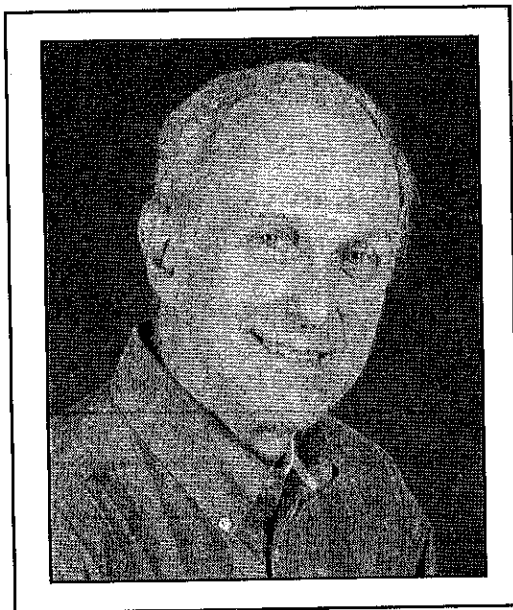
9:30PM-Midnight Home in time to kiss daughters good night. Fell asleep switching back and forth between the PBS fundraiser and "Law and Order" reruns.

Mea Culpa, Mea Culpa, Mea Culpa

The *Clio Newsletter* Managing Editor extends heartfelt apologies to the three authors who wrote a fine report on the 65th EHA Meeting for the *Newsletter's* Fall 2005 issue. In the final rush of putting the issue to bed, their names were omitted from the front page. Better late than never, they are: Linda Carter (Vanderbilt), Heather Howard (Cornell), and Todd Neumann (Arizona). Somehow I feel that as each issue goes by, I miss more rather than less errors. Must be the aging process.

Most sincerely,
Jean Bonde
Managing Editor

An Interview with Gavin Wright



*Gavin Wright is the William Robertson Coe Professor of American Economic History at Stanford University. While he is perhaps best known for his research on the economic development of the American South, he has also studied, among other issues, the role of natural resources in the development of the American economy and the role of race in the labor market. He is the author of three books and co-editor of four. His 1986 book **Old South New South: Revolutions in the Southern Economy since the Civil War** was awarded the Owsley Prize by the Southern Historical Association. He has another book due out shortly from LSU Press based on his 1997 Fleming Lectures, presented at Louisiana State University. He is also the author of numerous articles and book chapters. Wright served as editor of the **Journal of Economic History** (2000-2004), president of the Economic History Association (1997-1998), and president of the Agricultural History Society (2002-2003). This interview was conducted via e-mail in the summer of 2005 by Susan Wolcott (Binghamton University) with the assistance*

of several others, who were Wright's former students. I was a student of Wright's at Stanford University in the early 1980s, and he served as chair of my dissertation committee. It is with pleasure that I present this interview with Gavin.

I wanted to group my questions around several themes. First, there are biographical questions. Would you describe how you became involved in the Civil Rights Movement and the effect that experience had on your subsequent education and career?

Since I have been working on the Civil Rights era as economic history in recent years, I often remark that that my Civil Rights experience was instrumental in pointing me towards economics. But the connection was actually circuitous. As a college student in the early 1960s, I spent the summer of 1963 working on voter registration in North Carolina. That exposure to the segregated South led me to ponder the linkage between racial justice and economic development, a puzzle that has occupied my attention off and on ever since. But I really did not have a single-minded focus in choosing to be an economics major and going on to graduate school. I just knew that I wanted my life to engage with the real world, and it seemed obvious that the problems of the world were economic in some fundamental sense. I thought more advanced study in economics would clarify these matters, and it was somewhat disillusioning to learn (although graduate school at Yale had many benefits) that economics really did not have the kinds of answers I was looking for. I am still looking. It is one of the privileges of academic life, and my own good fortune, that I have been

able to return to some of these issues and retrace my earlier steps as an older and possibly wiser person many years later.

What made you decide to become an economic historian?

There was no glorious inspiration at the start. I took Bill Parker's course my first year at Yale, and he asked me to work the following summer (1966) on the project that was generating the Parker-Gallman sample from the 1860 census. That summer in Chapel Hill, I met Bob Gallman and others (Tom Weiss was just finishing his thesis on services; Don Schaefer was our computer advisor), and I quickly got the sense that exciting things were happening in economic history. By the end of the summer, I had a data set and a leg up on a thesis topic, which counts for quite a lot to a grad student. But it was also true that I did not see an obvious niche for myself in other applied fields of economics, whereas economic history held out the prospect of construing economics in broader and more flexible terms than was possible in other fields. Also, Yale had a particularly good cohort in economic history at that time: George Grantham, Peter Passell, Jan DeVries, Fred Carstensen, Gary Saxonhouse, and Joel Mokyr, just to name a few.

You were part of the debate concerning Fogel and Engerman's *Time on the Cross*. What is your sense of the field's final determination of the validity of their arguments? Has it been resolved?

The assertions in *Time on the Cross*, and the debate over them, were multifaceted, which is why I will resist the temptation to summarize them (much less give a bottom-line verdict) in a paragraph. One would like to think that academic debate is normal and healthy, but, in this case, I do not feel that it

was, because it spilled over into professional reputations, interdisciplinary relations, and even to the wider reading public. The atmosphere was chilly for some time to come. By now enough time has gone by for us to be able to entertain fresh syntheses on the economics of slavery, and I try to provide one such synthesis in a short book coming out next year. In brief, I argue that the key to understanding slavery in a historical context lies in the institution's property rights aspects rather than in its efficacy as a form of work organization, a topic to which so much academic attention has been paid.

Your early papers were quite quantitative, and much of the argument hinged on one estimated parameter and its associated t-statistic. Some of your later work has been very different. In *Old South, New South*, the arguments are much more qualitative. Would you comment on this evolution in your research?

I suppose a certain degree of drift from narrow technical studies to broader and more qualitative arguments is a normal part of an academic career trajectory. One of the distinguishing features of economic history is that the professional maturation process is generally more prolonged than in "straight" economics. Young people may be adept and even brilliant in technical exercises, but there is so much more to learn and absorb and ponder in history, and this takes time. It is also common enough to take a broader cut at a topic when writing a book than when publishing articles. Yet, I would like to think that qualitative dimensions supplement and complement quantitative work rather than substitute for it. At heart, I am still a cliometrician, and my most recent research continues to build on newly developed data sets and to develop analysis couched in theoretical and/or econometric terms. However, it is nonetheless true, as your

question implies, that theoretical and econometric results rarely speak for themselves, and they do not write economic history unassisted. I would say that enhanced appreciation of this truth has been one of the evolving features of economic history generally, not just in my own work.

In William Parker's interview in a previous *Newsletter*, he wrote that he was looking for structure in a historical question. I think you do that in your work. I have heard it said that your gift is to see the key puzzle piece that makes the picture fit together. An example would be the argument that slaves were the main source of wealth for the politically active elites throughout the South, and that one fact can explain much of the development of the antebellum South. Economists find this sort of structure comforting, but, generally, how much should we impose economic logic on history as opposed to providing a narrative of events?

I can recognize this dilemma or balancing act, but it has not been a sticking point for me, perhaps because I have never been an adherent of one of the great, ideal-type theoretical structures. The tension between theory and facts has not seemed troubling, because I see the research challenge exactly as your question implies: to grapple with the "facts of a body of history" and identify the principle or rule that will unify the picture. Of course, as in all science, there is always an interplay between inductive and deductive processes; you could not even assemble the "facts of a body of history" without some prior conceptual structure. But the important point is to be actively looking for a new and better reformulation.

You have often said that Bill Parker, your thesis adviser, was a major influence on you. How should he be remembered?

This is a tough one, because Bill's last years were not very happy. Referring back to the previous question, Bill longed to produce a grandiose synthesis of history and was keenly aware that he had not done so. He wrote wonderful essays and reviews, witty and amusing yet cogent and professional, proving that economic historians can be literate and entertaining as well as rigorous. But there was no masterwork of the scope and power that he hoped for. Maybe one lesson is not to aspire to immortality in that way. To his students, however, Bill Parker meant much more than his writings. Toward the end of his career, Bill remarked somewhat wistfully that he had never had a student who fully bought into the grand schema of society, economy, and history that he had struggled to develop. The reason for this was very simple, namely that Bill understood that a vital part of scholarship was bringing out the creativity and individuality of each student, and he gave this goal high priority in his advising. He never saw his students as extensions of his own ego, much less assigned them to confirm some favored thesis of his or refute some adversary. This is why his students loved him, and these are traits we would all do well to emulate.

When did you attend your first Clio meeting, and what were your impressions?

My first Clio meeting was at Purdue in February 1968, a memory refreshed by Mike Hauptert's write-up in the Spring 2004 *Clio Newsletter*. It was awe inspiring to see so many famous names in the field – Parker, Gallman, Davis, North, Easterlin, Fogel, Engerman – all going at each other in one seminar room. In addition to the Yale grad student contingent (Jan De Vries, Peter Passell, and myself), Bill Parker had persuaded Bill Nordhaus and Joe Stiglitz (then young assistant professors at Yale) to

present papers at the conference. You really had the feeling that these meetings were the place to be.

Does anything stand out in your memory from a Clio meeting – a particular paper, incident, or exchange, for example?

I was struck by the directness of the exchanges among old friends and colleagues. Particularly memorable was Bob Gallman's paper on the self-sufficiency of antebellum cotton plantations. People often forget that the Parker-Gallman sample was collected not to study slavery but to test Doug North's interregional trade hypothesis. Gallman showed that cotton plantations purchased little if any food, casting doubt on the centrality of trade flows between the Old Northwest and the South. North pointed out that Gallman's evidence was for 1859, whereas his thesis emphasized earlier decades, prompting Bill Parker to remark, "Yeah, you're back in the 1830s where we can't get at you."

Have the meetings changed since you first started attending them? If so, in what way, and do you think the changes are for the better or worse?

In some ways today's meetings are better in that you often get a more focused discussion of technical and econometric issues; and, you are also more likely to hear arguments based on "historical" perspectives as opposed to the idea (sometimes encountered in the early days) that the objective of cliometrics was to destroy history and replace it with economics. But what we no longer have is the sense that the leading figures in the field would gather in a room to hash over what were understood to be the most important issues on the subject. Of course, you can never recapture the atmosphere of the past, and cliometric research is now much more

diverse and specialized anyway. But we could still do more to cultivate our ability to address Big Topics connecting economics and history.

Next, I am interested in how economic history fits into economics and how that has changed over time. Using you as a data point, I would like to know what the position of economic history was in the institutions you were associated with. You did your graduate work at Yale, and you did a post doc at the University of Chicago. Then you were at Michigan from 1972-1982. And finally, you have been at Stanford since 1982. What was it like to be an economic historian at those institutions in those years?

The status of economic history at Yale was fairly high, because it combined the strength of tradition – from an era when instruction in economic history was considered an important part of any economist's education – with the novelty and enthusiasm over new quantitative methods and models then being applied to history. Economic history and economic development were also thought to be closely complementary fields. My stay at Chicago was only one year (1971-1972), but probably much the same could be said there. The Fogel-McCloskey economic history workshop was certainly part of the ongoing, cross workshop dialogue at Chicago.

At Michigan the narrative began to change a bit. I was recruited in an effort to create a new field in economic history, the feeling being that modernizing departments would want to get in on the New Economic History action. Alex Eckstein was instrumental in this drive, having decided to return to an earlier interest in European economic history (and calculating not very presciently that studies of China's economy had little future).

We had quite a strong group there, because Gary Saxonhouse and Ron Lee were also assistant professors in economics, while Jacob Price and Sylvia Thrupp were identified as economic historians in the History Department. Despite all of these assets, our success in building up the field was only modest. The department lacked both a tradition and an institutional setup in support of economic history. Alex Eckstein returned to work on China, and we had chronic difficulty staffing a range of courses, especially as budgets tightened in the 1970s. The department was happy to go along with a new field when it seemed to be booming, but its true understanding and appreciation for economic history was limited. I recall one history caucus where Alex Eckstein remarked (regarding the department's view of economic history), "You must remember, we are dealing with Neanderthal men." Ever since then, I have pondered whether our field should identify with the Neanderthals or with their Cro-Magnon adversaries.

I was and would have continued to be perfectly happy at Michigan, but when the chance to go to Stanford came along in 1982, it was too much to resist. I was already familiar with the strength of the economic history tradition at Stanford after a visit during 1977-1978, from which I came away a member of three dissertation committees (Cathy McHugh, David Weiman, and Warren Whatley). Aware of the high esteem in which Moe Abramovitz, Paul David, and Nate Rosenberg were held, I knew that I would have ongoing contact with many more top grad students at Stanford than I could ever hope for at Michigan. So I made the move, and my expectations have not been disappointed. Many things have changed at Stanford over the years, but by and large, I would say that we (and I am including in this Avner Greif, whose addition in 1989 was crucial) have kept the tradition going

successfully. We now have more than 25 people with Stanford Ph.D.s teaching economic history at diverse places around the world. I don't know if that makes us number one, but we do take great pride in our alumni.

When you add our interactions with history-minded colleagues in other departments (such as Steve Haber and Barry Weingast) and with economic historians at nearby universities (such as UC-Berkeley, UC-Davis, and Santa Clara), plus the All-UC Economic History program (which has always welcomed faculty and students from private schools), it would be hard to dispute that Stanford is a pretty good place to practice economic history.

I would like to get your views on how economic history in general differs from other fields in economics. You were chair of the Stanford Economics Department from 1989-1993 and again from 2000-2002. You have had to judge the quality of job applicants and tenure applicants in many fields. What makes a really good economic history paper? How is that different from what makes a really good paper in other fields of economics?

The attributes of a good economic history paper are, in addition to the attributes of any good economics paper, an interesting question to start with, analytical rigor in formulating and processing the question, and (assuming it is an empirical paper) careful attention to issues of data specification and econometric estimation. A good economic history paper hits all of these targets while locating the subject in an appropriate historical and institutional context. A problem in listing these criteria is that the very best economics papers do this too, whether or not they self-identify as economic history. But the prevailing norms are very different on either side of the great historical

divide. Here is one rule of thumb I often use: Browse through an issue of a general economics journal and notice how many of the titles are in the present tense (e.g., "Does School Integration Generate Peer Effects?" from a recent *AER*), implying that the study is testing or proffering a timeless rule or relationship. An economic history paper is much more likely to locate the question itself in a specific time and place and to use this information actively when it comes to assessing the range or implications of the reported results.

My term as North American editor of the *JEH* may have more relevance in addressing this question than my two terms as department chair. I was pleased to find that most *JEH* referees apply what I consider to be higher standards to manuscripts than would be the case in non-history journals. They would write comments such as: "This paper may be OK as 'economics with old data,' but it does not constitute economic history." Of course, I was choosing these referees, but I was not telling them what to say!

Given your own interest in cultural explanations, you are sympathetic to qualitative arguments. But if a paper hinges on one parameter and a t-statistic, it is fairly easy to judge its importance and contribution in terms of knowledge obtained and innovativeness of technique. It is relatively easy to see if the paper "proves" something and communicate that to a non-historian, economic audience. What would be your criteria for judging more qualitative papers, those which hinge on reasoned analysis and nuance? How does one, or should one, convince a non-historian economist that this is a real contribution to knowledge, as opposed to mere "storytelling"?

We may find better-sounding ways to say this, but ultimately I think we have to acknowledge that economic history boils down to storytelling and that is the point of it. We are trying to show, or at least argue, that events fell out across time according to some pattern. Thus, the criteria for evaluation are ultimately subjective. But I do not see this as fundamentally different from other branches of economics. Even a close econometric fit only shows consistency between a model and the data; it does not prove that another model might not do equally well or better. Thus, for us, econometric results are an intermediate step in constructing the story, and we accept the higher standard that goes with this more ambitious goal. The challenge is really the general challenge of all persuasive writing: You need to make your case by showing that your narrative has emerged from numerous sources of information and that it accounts for all the relevant observations. The chief difficulty in practice is that your hypothetical audience member might not have the patience to sit through a complex, historically specific analysis. This creates the temptation to reduce your analysis to simplified, attention-getting form. Sometimes this may be necessary, but it is advisable only if you can back it up in the fuller version.

In his Presidential Address to the Economic History Association in 2004, Joel Mokyr joked that economic historians are a bit separate but we like it that way. At the same conference, at the plenary session, Paul Romer and Nate Rosenberg stressed the need for interactions between theorists and economic historians. Sometimes, however, it is difficult to bridge the divide. Given your experience, what have you found to be the most fruitful ways for economic historians to interact with other economists?

Sometimes we look and sound less like separatists and more like evangelicals out to convert the rest of the economics world. We say they should be more historical, but when they sometimes try it, we often don't like the results much. So we do indeed value the companionship of like-minded specialists in smaller groupings like the EHA or Clio. I often feel that we are simply fated to remain a smallish, relatively elite group within the larger disciplines. But we should not settle for that, at least not without resistance. I think an essential step for economic historians is taking on tough but neglected historical issues that the rest of the economics profession will have to acknowledge as important. One example (among many) is the crisis of the 1970s. Here we have a major historical turning point, a true regime change for economics, for policy, and for politics, occurring midway through the postwar era. Our students and most of our colleagues think of the 1970s as ancient history. Yet, an economic history literature on the 1970s has yet to begin. Unless we can offer interpretations for fundamental historical breakpoints such as this one, economics may have reason to dismiss economic history as a remote specialty.

Most departments recognize that for a credible graduate program, they need a macroeconomist, a microeconomist, and an econometrician. What benefits or externalities does a department get from having an economic historian?

Ideally, every specialist contributes distinctive perspectives, insights, and knowledge of literatures and data sources that will be useful and relevant to colleagues and students. Things may not work out this way in practice, of course. But in principle, an economic historian should be a fully integrated member of the economics faculty,

contributing both detailed expertise from a research specialty (we all have specialties within economic history – labor, finance, IO, agriculture, etc.) and generalized expertise that goes by the name “historical perspective.” Most of us in the field have no trouble seeing that there is such a thing as a distinctive historical perspective, and we believe it should be institutionalized at least to the extent of having an economic historian on the faculty. But it is often hard for economics departments to accept this, because doing so implies acknowledging that there is more than one viable way of construing the economic world.

Stanford still has an economic history requirement as part of its graduate sequence. That used to be much more common. How would you justify this requirement?

You probably recall from your Stanford days that we have a simple tripartite justification that we often have occasion to repeat. Exposure to economic history is essential for graduate students in economics because (1) some economic processes (demography and technology, for example) play out only over very extended spans of time and studying them leads you into history (2) only historical cases compel students to come to grips with institutional settings (slavery, for instance) that are very different from those of the modern economies with which they are familiar and, thus, to consider the institutional assumptions or background conditions for propositions about economic life and (3) certain economic processes display historical path-dependence, which means that contemporary conditions and problems have their origins in historical events and the only way to understand them fully is to study that history.

When you teach a course in graduate economic history, what is the main thing that you would like to impart or leave with the students who will not continue on as economic historians?

What I try to do in teaching economic history to econ grad students is to demonstrate the truth and power of the aforementioned precepts in practice. Path-dependence may be taught as a formal theoretical proposition, an approach with which Paul David has had great success and which works very well with some students. My approach tends more to illustrate the plausibility of path-dependence by exploring historical cases in which outcomes that seem obvious and inevitable in retrospect were actually contingent on historical circumstances and could well have turned out differently. I also cultivate the flip side of path-dependence, which is that if certain structures become locked-in over time, no longer responsive to incremental changes in incentives ("institutionalized," one might say), then when it does come, change will be discontinuous, rapid, and very likely surprising to contemporaries. Economic history is full of examples, but for present-minded econ grad students, the more recent ones are generally most effective, such as the revolution in women's economic "place" in the 1960s and 1970s (as elaborated by Stacey Jones in her 2003 dissertation). My overall objective is to inject historical consciousness into our students' thinking about economics. I am well aware that it does not always "take." Some students fulfill the course requirements and walk away with the same unhistorical mindset with which they arrived. But the greatest satisfaction comes from those who enroll only because of the requirement and then say, "Thanks for the course; I had no idea what economic history was really about."

I also want to ask how economic history fits into history and how that has changed over time. What distinguishes a good history paper from a good economic history paper?

I don't see a basic difference. Not all history is about economics, and many topics do not lend themselves to the kind of formalized structuring that are standard even in economic history. But I really feel that the marks of quality are the same: a carefully specified question or proposition, with arguments clearly spelled out and written in precise language, presented in an interesting manner with rich historical documentation.

In an essay in *Schools of Thought*, in reference to trying to bridge the divide between historians and economists, you wrote that you often felt like "an emissary between hostile tribes, trying to develop a kind of cross-cultural pidgin language through which to inform each one that the other guys are not as bad as they think, and in fact they might actually learn something useful from hearing (or even reading) what they have to say." What do you think may have caused historians to resist listening to economists? Do economic historians typically do a good job of listening to historians?

Thanks for reproducing that deathless quote, which has no doubt been largely overlooked until now by members of the Cliometric Society. History/economic history communications have certainly deteriorated to a sorry state, and I tend to blame whichever group I am with at the moment. Generally historians stopped reading economic history for invalid reasons, believing in (and rejecting) a caricatured understanding of the discipline of economics and the thinking of cliometricians. In truth, economic historians are much broader and

more diverse now than they were 30 years ago, take more responsibility for historical scholarship as well as numbers, and are less likely to dismiss historians' research as ignorant nonsense. But most historians do not know this, and most history grad students get no training or exposure that would help them to read and appreciate, and criticize, economic history intelligently.

Yet, I feel we make a serious mistake if we just accept this status quo as the only way the world can be. We do not put nearly enough effort into writing works of synthesis that are accessible to a general historical readership. Not that we can expect to move the center of the entire history profession. But there are many potentially interested readers and colleagues in various branches of history who are not card carrying post-modernists and who would welcome a better relationship with economic history. I do recognize the obstacles: writing works of synthesis in economic history is not easy (it exposes what you don't know as much as what you do know), and the prospective professional payoff may not be high. But at least some of us, some of the time, should be working on this constituency, since our field is too small to rely exclusively on sales to the domestic market.

Finally, I want to give you a chance to engage in scholarly speculation, since this is the perfect forum for such things. The South is no longer a separate region economically. Certainly labor markets are integrated. But politically, the South is very, very Red and unlike the Blue North. Do you think that the economic history of the South speaks to this apparently enduring cultural difference?

Excellent question! So good, in fact, that it is the very question I am trying to answer in my current research. I have a couple of

working papers (with titles like "Persisting Dixie") that argue that the South has continued to be distinctive in economic as well as political life, not in the old isolationist way (as you point out) but in new ways. The poultry industry, for example, was formerly spread out nationally and is now regionally concentrated in the South – not in any one location, but across the region. Similarly for forest products and pulpwood. But, of course, these new economic centers have close connections to Southern politics as it has evolved since the Civil Rights breakthroughs of the 1960s. Understanding that linkage is my latest preoccupation. Rather than speculate, candor requires acknowledgement that I have not yet solved this puzzle. What would academic life be without the next hurdle to clear?

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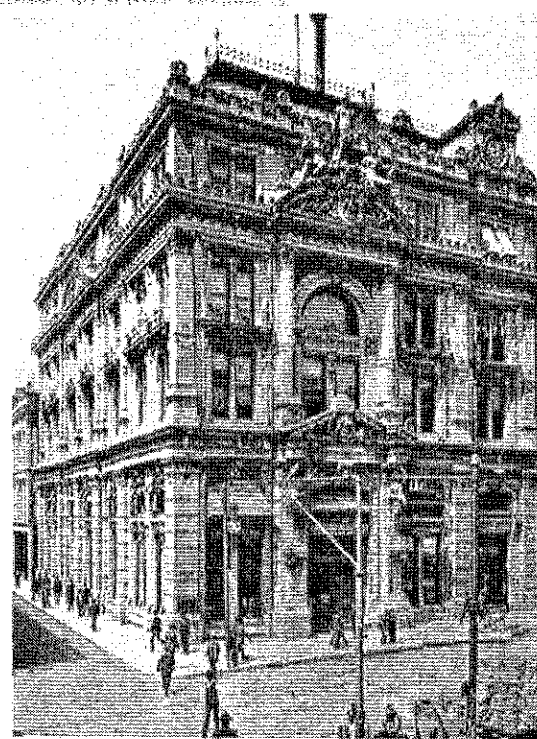
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New Orleans Cotton Exchange, 1903

Call for Papers

Third Iberian Economic History Workshop Valencia, Spain March 23-24, 2007

Iberometrics III will be held in Valencia, March 23-24, 2007. Hosted by the Departamento de Análisis Económico of the University of Valencia, this conference is designed to provide extensive discussion of new and innovative research in economic history.

Those wishing to present a paper should provide a 500-word summary of the proposed paper, their address, phone and fax number, and e-mail address to Iberometrics3@uv.es by **October 15, 2006**. The organizers will cover travel and local costs for all those who are invited to present a paper. More information can be found at www.uv.es/~iberom3.

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| BOOK PREVIEW |
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War, Wine, and Taxes: The Political Economy of Anglo-French Trade, 1689-1860

By John V.C. Nye

*Note: The following is an abridged version of the introductory chapter of **War, Wine, and Taxes** by John V.C. Nye, forthcoming in 2006 from Princeton University Press.*

The idea that Britain was the leading free trader of the 19th century is one of those rare, stylized facts in economic history that resonates with the public as much as it commands the attention of historians and other scholars. In conventional wisdom, Britain's move to free trade was the triumph of the Scottish Enlightenment, an outgrowth of the Industrial Revolution, and a political and ideological movement that converted the whole world (or at least the major European powers) to the virtues of what is now called globalization. Nineteenth-century free trade is at once credited with the success of the Western economy and at the same time vilified for its promotion of the ills of economic integration and its apparent support of the imperialism of European leaders. The subjects that swirl around British free trade and its significance for economics, politics, the history of ideas, sociocultural change, and long-run transformation of all sorts are so numerous that they could (and indeed have) filled hundreds of books and thousands of articles.

It is therefore surprising to learn that until fairly recently no detailed attention has been paid to the validity of the claims of British free trade. Nor have economic historians, eager to quantify most subjects, done much to place British trade policy in a wider international context. There has been little done to separate claims about lower British

tariffs over time, from lower tariffs in comparison to other nations to the uses to which the tariffs had been put and their effects on the economy. Despite occasional forays into quantitative speculation, British claims of trade liberalization were taken at face value.

Previously, I conducted a comparative analysis of British and French trade policy which showed that the standard fables about Britain unilaterally moving to free trade after the repeal of the Corn Laws were misleading if not outright false. This led me to reconsider the conventional wisdom surrounding the political economy of 19th-century trade policy and necessitated further research into understanding why scholars have mischaracterized core features of these policies.

Subsequent work indicated that the problems of 19th-century tariff reform had their roots in conflicts going back to the reign of Louis XIV in the closing decades of the 17th century. The discovery of the ways in which a flourishing trade in French wine was destroyed at the end of the 1600s by a long war between Britain and France caused me to focus more intensively on the political economy of British trade policy and on the long history of Anglo-French commerce. The political economy of customs led me to the more general question of the evolution of modern British fiscal policy. From there, it was an easy step to a surprising story that intermingled commercial and foreign policy with issues of domestic tax policy, the rise of the British excise, the development of the

modern London brewing industry, and changes in British consumption and French production arising from the massive distortions in international trade introduced by the new anti-French tariff structure.

The argument is essentially this: a British government eager to reduce the trade deficit with France at the end of the 17th century was presented with a special opportunity arising out of the period of war from 1689-1713. These conflicts provided the protectionists with the opportunity they sought to cut off almost all commerce with France, thus eliminating the trade deficit. This created a powerful class of protected interests both at home (brewers and distillers) and abroad (notably in the form of British merchants and investors in Portuguese wine) and led to the imposition of prohibitively high tariffs on French imports, mostly on wine and spirits, when trade with France resumed in 1714. The implicit threat of lower tariffs allowed the state to raise domestic excises on alcoholic beverages and other consumables that might otherwise have been uncollectible, thus leading to increased government revenues with almost no increases in tax rates on land and income. The state ensured compliance not simply through the threat of lower tariffs on foreign substitutes but also through the encouragement of a trend towards monopoly production in brewing and the restricted retail sales of beer, which began around 1700 and continued throughout the 18th century.

The net result was an expansive British state with revenues collected by central tax authorities and backed by a cooperative domestic industry shielded from foreign competition that found it easy to shift much of the burden of taxation onto consumers. The centuries-old trade with France was permanently deformed, and cheap wine was kept out of the British Isles during the

century and a half which saw the coming of the Industrial Revolution and the rise of the middle-class British consumer.

In Chapter One, I provide the core empirical findings which motivate this entire book: the demonstration that Britain was not as much of a free trader in the 19th century as has been previously perceived, especially in comparison to France. I go into some detail about the nature of the evidence in support of this thesis and tie it into the conventional historiography of the 19th century.

Chapter Two is a quick overview of British commercial history from the late 17th to the mid-19th century that touches on the received wisdom of the period but also points to the links between foreign policy, trade restrictions, and domestic demands for revenue.

Chapters Three and Four are given over to a more detailed discussion of the history of commercial relations in the 18th century, which show the origins of the trade war between Britain and France that led to the tariffs and prohibitions on French wine, which in turn were to shape policy over the next century and a half.

Chapter Five is a brief counterfactual discussion designed to discuss what might have happened had the British permitted French wines to enter with a more liberal economic policy. The object here is not to precisely rerun history. Rather, the goal is to give the reader some idea of the magnitude of the foregone trade and its potential effects on the structure of consumption and production.

In Chapter Six, I argue that the protection of domestic brewing interests and the technical revolution in the London beer industry led to a situation in which the government and

leading brewers entered into a symbiotic relationship where protection from foreign competition was granted in exchange for substantially higher beer revenues. This led to a surge in total British revenues as well as a shift in the composition of British taxes away from land to various excises that made possible government expansion in the 18th century. I continue this discussion to show how we can better understand domestic politics in the early 18th century. This is accomplished by taking this relationship into account and amplifying the possibilities of studying the political economy of encouraging sustainable revenue collection through protection and the promotion of oligopoly in production.

Because the history of British commercial policy has been such an important



component of debates in politics as well as in history and economics, Chapter Seven focuses on the implications of this work for political economy. Indeed, the story of the so-called Pax Britannica and the British domination of commerce through the promotion of free trade is central to so many different theories that it behooves us to explore how changing the underlying story would change the leading hypotheses. Theorists of political hegemony, international cooperation, or what is known to political scientists as international political economy have relied on a conventional reading of changing trade regimes that is not supported by this historical research. Consequently, I hope that many political theorists will find this historical analysis of some interest.

The final section not only sums up the complete argument of the book but extends the analysis by considering how this might lead us to rethink the existing historical generalizations about Britain's rise to power and wealth. It will also give space to a discussion on how reconsideration of the role of fiscal policy in Hanoverian Britain actually leaves unanswered many questions of great importance to the rise of the liberal state and how misleading conventional wisdom has been. It calls into question the easy link we tend to make between fiscal success and economic development and allows us to question recent attempts to justify mercantile policy on the grounds that state expansion paved the way for modern growth.

It is unlikely that any one work or single empirical test will serve to make people reject theses that have been meticulously built up over the years. In addition to the complex welter of arguments that have accumulated in the literature, all sorts of caveats and cautions will have been worked

into the analysis. Nonetheless, it is my firm belief that no one should be able to read through the material and ponder its particular reading of commercial history without feeling obligated to rethink many of the ideas in international political economy.

The rise of the modern state, the role of fiscal policy in development, the struggle over free trade, the problem of market reform, the political economy of interest groups in times of rapid change, and policy perception vs. economic reality are some of the most important and central problems in history and the social sciences.

The story of British development and the evolution of its trade policy have long stood at the center of these investigations. This is not just a story about the past. It is the beginning of a fundamental reassessment of our understanding of commercial policy in economic history. More importantly, it is my hope that this will serve as a small lesson in how history can inform more static economic analysis and vice versa. The limitations of our models can be overcome by recourse to a narrative that fills in the dynamic elements of the problem. It provides the context without which any analysis is hopelessly obscure, or worse, pointless. And, ultimately, it makes for a more interesting read.

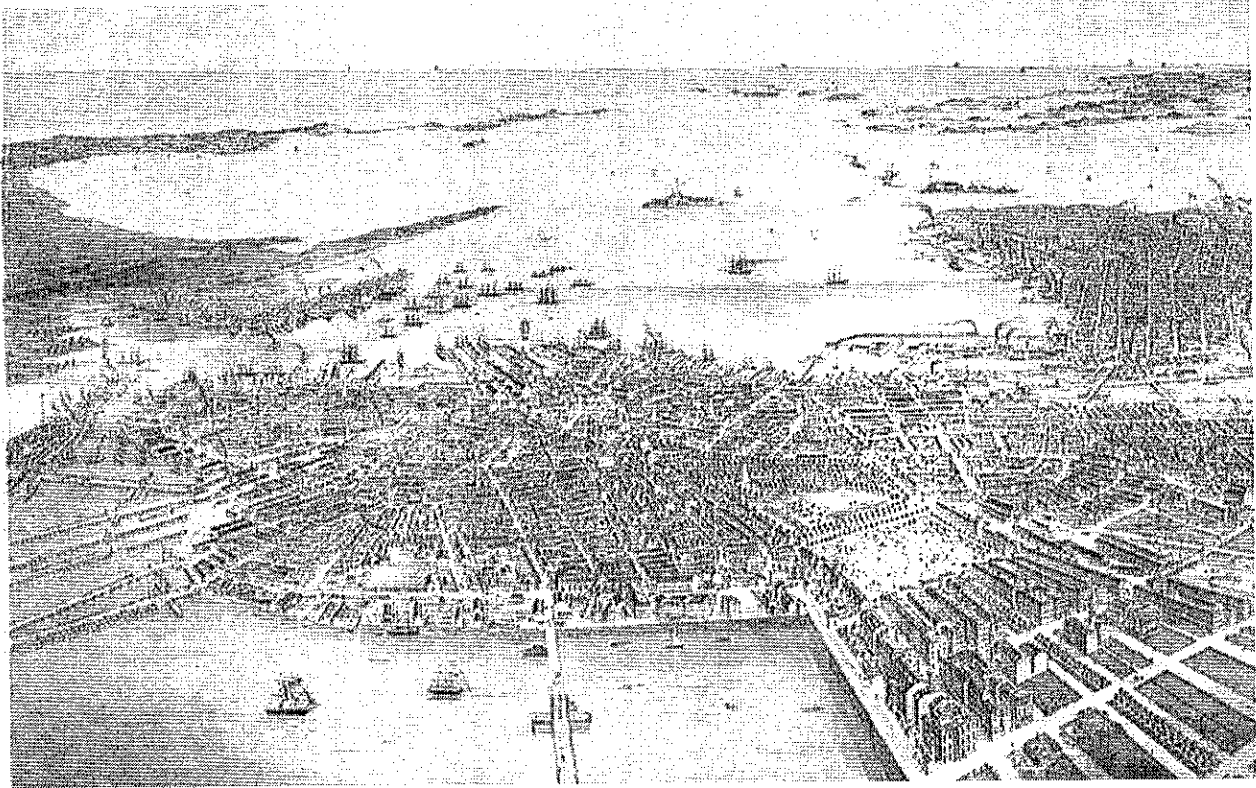
Clio at ASSA *(Continued from page 1)*

could cause both lynchings *and* lower productivity. Carden replied that theory would suggest the latter.

In "Assignment of Military and Post-Service Economic Mobility: Experience of Union Army Veterans, 1860-1880," **Chulhee Lee** (Seoul National University) utilizes the Union Army veterans data set to examine how military rank and duty affected the post-discharge occupational mobility of Union Army soldiers. He establishes that officers were more likely to experience upward mobility and proposes two likely causes. First, an officer's experience may have served as on-the-job training, which increased human capital. Second, those with high ability may have become officers. His regression results support the first scenario. Troesken recommends a two-stage technique to discover who benefits most from officer training. In the first stage, use all the observations to discover what characteristics affect the probability of promotion. Then, restrict the sample to three narrow bands of men who have low, medium, and high probabilities of being promoted. For the

second stage, rerun the analysis with just these people.

Leslie McGranahan (Federal Reserve Bank of Chicago) then looked into why people write wills in "Intestacy and Estate Distribution in Early 20th-Century Ireland." Her sample of residents from County Fermanagh, Ireland who died between 1901 and 1905 includes both people who died with wills and those who died intestate. Using a probit analysis, she states that those who are older, wealthier, or own land are more likely to write wills. Living arrangements have no effect on will writing, while being literate seems to decrease the likelihood of will writing. John Murray (University of Toledo) offered two possible explanations for the interesting literacy result. Interacting literacy with religion might be informative if Catholics and Protestants had different literacy rates. If there was a secular increase in literacy rates, he proposed that the literacy effect might be the result of a cohort effect. He also felt that anyone being sick in the household during the year a member died might increase the probability that the decedent had a chronic condition. Utilizing



Boston, July 4, 1870

this fact could provide a way to differentiate between anticipated and unanticipated deaths. McGranahan said that the literacy result seemed to persist in numerous specifications.

Following the presentation, a spirited discussion of all papers took place. There were several suggestions and questions about Carden's paper. For example, the audience wanted him to convert his data set into a panel to allow for state-level fixed effects and examine other crime statistics.

Lee Craig (North Carolina State) led the second session, "The Economic History of Labor and Health." **Samuel Allen** (UC-Davis) began by exploring "The Political Economy of Workers' Compensation Benefits since 1930." Using data on each state's workers' compensation laws, Allen constructs an index that allows the various components of benefits – wage, injury type, probability of injury, and benefit level – to be

aggregated into a single measure for each state/year observation. He then uses a regression analysis to isolate factors that influence the benefit index. Federal government regulation, workforce characteristics, and state political climate are all important; insurance regulation and unionization are not. Furthermore, he finds that ln wages are positively associated with ln real benefits, while accident rate and firm size have a negative association. Melissa Thomasson (Miami University of Ohio and NBER) complimented the unique nature of Allen's data set. She noted that a 1972 federal law on workers' compensation was an important part of the story. The impact of unionization on benefit levels changed with the law's passage in 1972, and this deserves more investigation. Furthermore, Thomasson asked how premiums were charged to employers and thought Allen should include factors that influence premium levels, such as experience ratings, in the analysis.

Tomas Cvrccek (Vanderbilt) uses data on Czech schoolboys in the 1950s and 1960s to analyze "Food Supply Fluctuations and Cycles of Growth." Unlike many measures of economic activity in post-World War II Communist countries, Cvrccek's height and weight data is an indicator of living standards that has not been altered or interfered with by a central government. He discovers that the growth cycles of boys do not mirror those expected in a healthy population. Before currency reform and the introduction of food rationing in 1953, growth did not exhibit much of an annual cycle. Following the reform, height and weight gain patterns indicate that food availability fluctuated with the harvest cycle. Most family characteristics, such as a father's job, did not have a significant impact on the growth cycle. John Brown (Clark University) believes this paper helps fill a large gap in the existing literature on early living standards in Soviet economies. He suggests exploiting variation in children's residential location (cities vs. suburbs, for example) and draws attention to two potential natural experiments. First, the data are drawn from an area of the former Czechoslovakia with a high number of German and Austrians, causing an interesting variation in birth locations. Second, if the harvest cycle is important, external variations that could influence crop yields could be included in the analysis.

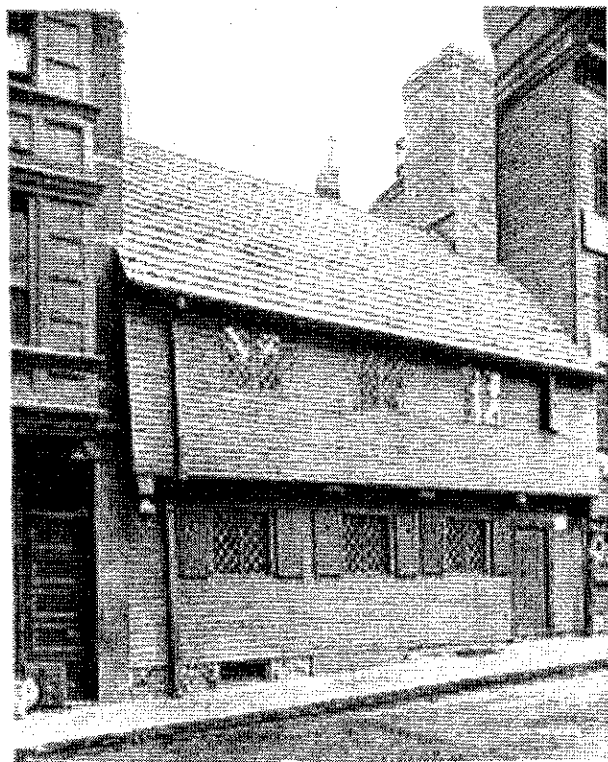
Raven Saks talked about "Historical Trends in Executive Compensation, 1936-2003," written with **Carola Frydman** (Harvard). Their data set is the first to have such a long timespan of comparable data on executive compensation and includes information on salary, bonuses, incentive pay, and stock options. They demonstrate that the average real mean value of compensation was stagnant from the 1940s to the mid-1970s, at which point it began a long and sustained

growth. When comparing the median compensation to the salary of the average full-time equivalent worker, the ratio's time tend exhibited a "J" shape, falling throughout the late 1930s and 1940s, stagnating until the mid-1970s, and then rapidly rising. Why was there an increase in compensation? Saks and Frydman posit that the high level of marginal tax rates held down compensation in the 1950s and 1960s. They estimate that compensation in those decades would have been 35% higher if executives faced the 1990s tax rates. Martha Bailey (Roberts Woods Johnson Foundation and Michigan) praised this data collection project. She pointed out that the high level of manufacturing firms in the sample (75%) may affect the representativeness of the sample and wants the authors to compare the results with other sources. For future work, she said that Saks and Frydman should consider investigating the career paths and wage profiles of each executive.

John Murray spoke on the Progressive Era in "Demand for Private Health Insurance and the Failure of Progressive Reform." During this time period, 30-40% of workers had sickness insurance, which provided sick pay when illness prevented working. The Progressives wanted to transform the insurance from a private benefit people chose to a publicly run, universal system. Their proposals were defeated in several states, and Murray reports on why using data on Michigan iron and agricultural implement workers in 1890. He says that worker participation in these funds increased until age 49, then fell. Using a probit model, he ascertains that workers with more savings were less likely to purchase sickness insurance. His results suggest that older workers were adequately able to save in anticipation of sickness episodes and did not need sickness insurance. Younger workers, who tend to be healthier, also often chose not

to invest in the plans. Workers in their middle years, who supported families and had yet to save as much, insured against illness. Murphy states that the insurance was not universally desired and may explain the Progressive failure to implement universal sickness benefits. Craig believed that the Progressives come across as strawmen whose proposal was doomed to fail. He advised either explaining their plans more in depth to give them validity or perhaps framing the paper around the existence of turn-of-the-century, benevolent societies and dropping the discussion of the Progressives.

The Clio sessions continued with "Economic Shocks and Cycles in the Past," chaired by Marc Weidenmeir (Claremont). **Alexander Field** (Santa Clara) kicked off with "The Impact of World War II on US Productivity Growth." Field's basic argument is that the factors that allowed for post-World War II prosperity were already in place before the War. He carefully analyzes the period of



The Paul Revere House

1929-1941 and contributes new total factor productivity estimates that adjust for the high variation in unemployment rates throughout the time period. John Laitner (Michigan) highlighted the importance of correcting for changes in productivity when such a major shock occurs but alerted for measurement problems in terms of capital and labor quality, namely with respect to the participation of women in the labor force. Field replied that women's participation in the labor force may be relevant for labor quality measurement but only after the time frame in the analysis, given that the US entered the war in 1941.

Next, **Carlos Ramirez** (George Mason) presented "Do Bank Failures Affect Real Economic Activity? State-Level Evidence from the Pre-Depression Era," written with **Philip Shively** (Federal Deposit Insurance Corporation). Their work fits with ongoing research on the real effects of bank failures and introduces a new data set from 1900-1931 (quarterly and state level). To test the hypothesis, the authors use Granger causality and F-tests and find that bank failures propagate commercial failures in half of the states, while there is no systematic evidence of the reverse. They then test for several candidates in order to explain regional variation of the failures and assert that state consumption and investment exposure are major explanatory factors. Finally, they attempt to measure how much bank failures contribute to commercial failures. Christopher Kurz (Federal Reserve Board of Governors) pointed to additional econometric procedures which could strengthen the Granger non-causality results in such a study and proposed looking for a region-level exogenous shock, such as a drought, that could explain the patterns in the data.

Last but not least, **Marc Weidenmeir** spoke on "Original Sin, Default Risk, and

Exchange Rate Depreciation: Lessons from the French Crime of 1873," written with **Michael Bordo** (Rutgers) and **Chris Meissner** (Cambridge). The authors focus on the problem that countries can not borrow in their own currencies, making them more vulnerable to real exchange rate depreciation by reducing their ability to repay. They analyze the French demonetization of silver in 1876, which caused more than a 10% fall in silver prices and study the effect of such an exogenous shock on the countries in the periphery of the monetary system. Their findings show that yield spreads for countries on the silver standard increase following the demonetization in proportion to the country's exposure. Angela Redish (British Columbia) observed that the sample size of 25 countries, of which 8 were on the silver standard, does not give many degrees of freedom and that the results may be driven by the high volatility of interest rates.

At the end of the session, the discussion mainly centered around Field's paper, specifically on the effect of R&D in US manufacturing. Some government programs introduced during World War II, such as the National School Lunch Program and increased R&D spending, may have had productivity effects that were realized much later. Field agreed that R&D had large long-run effects on productivity and was in place much before World War II, stating that Margo shows that the probability of being unemployed in the R&D sector was low during the Great Depression.

The first session of the Economic History Association, "The Economic History of Banking and Insurance," was chaired by Alexander Field. **Mark Carlson** (Federal Reserve Board) delivered "Branch Banking and the Transformation of Banking in California," written with **Kris Mitchener** (Santa Clara). Their motivation draws from

the recent, rapid bank consolidation. Little has been done on how this consolidation and growth in branching affect long-run stability. Carlson and Mitchener study the Californian consolidation experience of the 1920s and 1930s, using micro-level data to test the effect of consolidation on the stability of the remaining banks. The authors conclude that banks who had to compete showed increased stability with a longer expected survival period. The results are robust to endogeneity problems. Eugene White (Rutgers) commented that even though the results are intuitively pleasing, the theme deserves further attention due to large resistance of independent banks to consolidation, which requires more evidence-based studies. White also wanted a sketch on the implications on less developed countries.

Andrey Ukhov (Indiana) presented "British Investment Overseas, 1870-1913: A Modern Portfolio Approach," written with **William Goetzmann** (Yale). The authors evaluate the pattern of British foreign investment in 1914 and ask whether it was excessive relative to domestic investment. Using Edelstein's time series of 1870-1913, they use mean variance analysis in order to construct an optimal portfolio. Ukhov and Goetzmann deduce that the best subsets of available investments always include foreign asset classes, and, therefore, there is no evidence against capital market equilibrium. Michael Edelstein (Queens College) thought that there were weaknesses in the data which should be taken into account, and felt an investigation of the causes of diversification was in order to better account for how the portfolio decision was being made.

Chris Kingston (Amherst College) continued with "Marine Insurance in Britain and America, 1720-1844: A Comparative Institutional Analysis." During this time period, Atlantic trade was undertaken in

unsafe ships, in risky weather conditions, and when the risk of naval warfare was common. The agency problems of maritime insurance encompassed not only moral hazard on the merchants' side but also adverse selection on both the merchants and the insurance underwriters. Before 1720, Britain observed a pattern of private underwriting, while after the South Sea Bubble, chartered marine insurance corporations were created. Whereas the latter generally failed to succeed in the old continent, in the US, after independence, there was a proliferation of such corporations. Kingston develops a model to explain the multiple equilibria. Eric Hilt (Wellesley) questioned whether in addition to corporations and private underwriters, there may have been informed merchants/entrepreneurs as suppliers of insurance, which in turn may have influenced the final equilibrium.

Anders Ogren (Stockholm School of Economics, visiting at NYU) wrapped up the session with "Commercial Bank Note Issuance and the Development of a Liquid Capital Market." Ogren analyzes the Swedish banking system of the late 19th century and contributes to the debate related to the efficiency of Joint Stock Banks and Enskilda Banks (the commercial banks of Sweden with issuance power at the time). Using the annual balance sheets of the Enskilda banks, he notes the importance of note issuance in the possibility of establishment of banks in illiquid areas and finds support that increased liquidity raises the likelihood of interest bearing assets. Mitchener made comments on the structure and content of the paper and felt that the use of regional variation may be useful in testing the importance of the Enskilda Banks.

Robert Margo chaired the session titled "The Economic History of the Middle Ages and Early Modern Europe." The first presenter,

Albrecht Ritschl (Humboldt University of Berlin), explored "Making Financial Markets: Sovereignty, Contract Enforcement, and the Engagement of Tradable Assets in Late Medieval Europe," written with **Lars Boerner**. The authors highlight the role of communal responsibility in the emergence of tradable assets in Europe. Their results show that communal responsibility was critical for the enforcement of contracts and that tradable assets emerged early on, not just informally but as collateralized debt contracts across jurisdictions.

Yadira Gonzalez de Lara (University of Alicante) continued with "The Secret of Venice's Success: The Role of the State in Financial Markets." Gonzalez de Lara uses a game-theoretical framework and historical records to investigate the institutional environment facilitating financial intermediation in Venice during the late Middle Ages. She uses information from nearly one thousand transcribed notary acts and secondary studies on Genoese notary records to build and generate predictions from a context-specific game. According to her theoretical and empirical analysis, the state, rather than non-state, reputation-based mechanisms, provided the institutional backdrop which enabled Venice's commercial success.

Alvaro Pereira (University of York) shed light on "Natural Disasters and Economic Performance in History." He looks at the impact of the 1755 Lisbon earthquake, the largest recorded natural disaster in European history, on the main aggregate variables of the Portuguese economy. In the aftermath of this earthquake, GDP contracted about 10%, the skill premium on wages increased (thus affecting subsequent inequality levels), and both trade and fiscal imbalances widened. Pereira argues that the evidence he provides is suggestive of a much greater economic

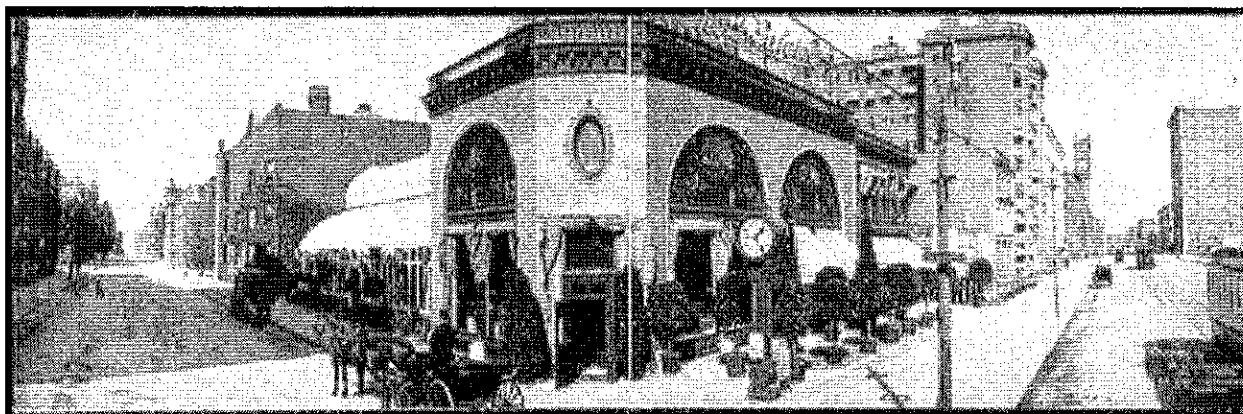
impact than previously acknowledged; for instance, he points out that, at least indirectly, the earthquake is to blame for important economic reforms in the 1760s. During the discussion, Gary Richardson (UC-Irvine) complimented the paper's ambitious agenda and thought that perhaps Pereira should divide the present study into several shorter ones focusing on specific issues, such as long- vs. short-term economic effects of the earthquake.

Bringing us back to Venice, **Dean Williamson** (US Department of Justice) presented "The Financial Structure of Commercial Revolution: Financing Long-Distance Trade in Venice and Venetian Crete." Williamson utilizes a data set of 1633 principal-agent contracts, many of which are unpublished and were unearthed directly from the State Archives of Venice, to illuminate the question of contract selection in long-distance maritime trade. In particular, Williamson studies the conditions under which agents were more likely to choose equity-type contracts or *commendas* over debt during the Late Middle Ages. Two patterns emerge from his analysis of these contracts: equity risk sharing schemes are used in information rich environments and debt contracts are selected in frontier situations where information is poor. In addition, the fact that almost all contracts

involving some sort of family relation were *commendas* and the ships were traveling in a convoy is used to argue that rather than just alleviating information asymmetries, ventures among relatives served the purpose of training younger generations of merchants. Williamson concludes that while formal institutions existed at the time, his evidence suggests mechanisms enabling long-distance trade beyond the formal reach of these institutions.

Following the presentations, each author had a chance to briefly respond to the discussant's comments and suggestions. Also, an interesting discussion took place, primarily between Gonzalez de Lara and Williamson, on their somewhat distinct views on the role of formal institutions in Venice's commercial revolution.

On Saturday evening, the Executive Director of the Cliometric Society, Lee Craig, held a three-hour reception in his suite at the Four Seasons Hotel. All participants from the Clio and EHA sessions were cordially invited to attend. In the fine tradition of Clio, the reception provided a pleasant, relaxing time, where the discussion of ideas was present and new contacts were made. The next ASSA conference will take place on January 5-7, 2007 in Chicago, Illinois.



Galvin's store with view of Fairfield Street and Boylston Street, Boston c1903

Call for Papers

Business History Conference

Cleveland, Ohio
June 1-2, 2007

The 2007 annual meeting of the Business History Conference will take place June 1-2, 2007 in Cleveland, Ohio at the Weatherhead School of Management of Case Western Reserve University.

The theme for the conference is "Entrepreneurial Communities," defined broadly in scope and scale. We are interested in papers that consider how firms and other groups (within, between, or outside particular firms) and society as a whole have organized themselves to foster or inhibit entrepreneurial activity. In keeping with longstanding BHC policy, the committee will also entertain submissions not directly related to the conference theme.

Potential presenters may submit proposals either for individual papers or for entire panels. Individual paper proposals should include a one-page abstract and a one-page curriculum vitae. Each panel proposal should include a cover letter stating the rationale for the session, a one-page abstract, and author's cv for each proposed paper (up to three), as well as a list of preferred chairs and commentators with contact information.

The deadline for receipt of all proposals is October 15, 2006. Notification of acceptances will be sent by January 2007. Presenters will be expected to submit abstracts of their papers for posting on the BHC website. In addition, presenters are encouraged to post electronic versions of their papers prior to the meeting and to submit their papers for inclusion in our on-line proceedings publication, *Business and Economic History On-Line*. The BHC also offers graduate students who are presenting papers grants to offset some of the costs of attending the conference.

Please send all proposals to:

Dr. Roger Horowitz, Secretary-Treasurer
Business History Conference
P. O. Box 3630, Wilmington, DE 19807
Phone: (302) 658-2400; Fax: (302) 655-3188
Email: rh@udel.edu.

Personal Reflections

Time on the Cross: How and Why Not to Choose Between Economics and History

By Marc Flandreau

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

I. Clio-Me-Tricks

I never intended to become an economic historian. I thought I had had enough in my early university years, with the mandatory courses in humanities dealing with the industrial revolution: where had it happened first? When did it begin? And had it really existed, after all? All the coal accounting, number guessing, vagueness, aggregation, etc., not to mention the description of innovations, was obviously the least exciting thing one could think of, and I felt about it very much the same enthusiasm one would feel for modern articles on the 'new economy', and how the internet is going to revolutionise economics and society at large.

On the other hand, there were two sister disciplines in which I found intrinsic beauty and appeal, which I decided to study separately. These were economics and history. The seduction of economics was its abstraction, its ability to operate a fairly sophisticated conceptual machine capable of moving at high speed and to land it, as a helicopter, in the tidy glades that can be found in even the most inaccessible jungles. The seduction of history came from qualities that are exactly opposite. The same jungles are explored on foot with a duty to collect every single exotic flower along the way, taking the petals, leaves and roots together, writing where they picked in a booklet, and studying them back in the office both for themselves and in relation to each other. While in history elegance and scholarly

achievement is often a thick book, in economics, it is a lean one. But how to choose between, say, Arrow's *Social Choice and Individual Values* and Braudel's *Mediterranée*? I think that is it this continued refusal to choose that led me, in large part unwillingly (or at least unknowingly), to become an economic historian.

This however took time. The bridge between economics and history is by no means easy to erect, and in fact I did not feel any urge to build one. Our legacy of the post-war triumph of technocratic-scientific administration in France has been the ascent of the engineer-economist: an improbable offspring of nineteenth century Comte style positivism and of the ideas that produced the Soviet system (except if you consider that positivism created the Soviet system, which is possible). This type believes (a) that society, as any other physical body, is subject to the laws of nature and that (b) if only she or he were given decision making powers, she or he would improve society by making it conform to her or his idea of social good. Their scholarly ideal is a Minister of Figures, crunching numbers *sine ira et studio*, for the glory and advancement of Science and Mankind. The position of French historians in French society on the other hand had suffered some blows with the political default of the Third Republic. They had not been able to recapture their former role as a source of inspiration for pragmatic policy

making. Their move to the far left after the Second World War had been one more dead end as the Fall of the Berlin Wall eventually showed, mimicking in a slower way what Berliners had done overnight, they could only offer to 'deconstruct'.

How to connect Malinvaud and Derrida? There was obviously no way, and obviously no interest in doing it. Moreover, economic history was by the 1990s in dreadful shape in France. One lacked a starting point. It is true that in economic history as in many other things, France had its glorious time, long, long ago. One could still hear in some quarters the word '*Annales*' being whispered in due respect. But this respect was more akin to the one students of Ancient Greece experience when coming across a reference to the mysteries of Eleusis: nobody knew what was in there. Were the *Annales* dead and buried, or had they survived somewhere? The *Ecole des Hautes Etudes* had a claim on that and argued its case on *Jus Soli*. But in effect it had become, after Braudel's death, a place where heirs fought over inheritance. It still gobbled huge amounts of resources and lots of energy but did not produce much light.

To be entirely fair, there were, in French universities, a few exceptions to this doomed picture: they urged me to pursue my doctoral studies in a place where my uncomfortable balance, 'between two cultures', as Carlo Cipolla so nicely put it, might find some way to be resolved. To tell the entire truth I should add that these voices cautioned me against cliometrics. As I understand it now, the resistance was in part religious (isn't it sacrilege to measure a Muse?), in part philosophical (can one really gauge events that have occurred using others that haven't?), and in part French (wasn't this discipline predominantly Anglo-Saxon?). But there was also a fascination: hadn't

Ernest Labrousse himself developed, way before cliometrics were even born, what can be called a fully fledged 'model' of the effects of wheat crises on agricultural economics? If practised with the required dose of Cartesian doubt, cliometrics could after all have their virtue. Included in a broader framework of interpretation that would make sure that over-simplification would be resisted, counterfactuals were conceivable. And in the end, my attempts at arguing that the rise of the gold standard in the late nineteenth century was by no means preordained and that another course of events might have been possible, suggested that I had already crossed the Rubicon.

II. Changing Places

I first stopped over in London, still believing that I would turn, *nolens volens*, to macroeconomics. In many senses this stay helped in a decisive fashion to make my way to economic history. Most modern French economists, for all their definitely Gallic taste for maths and economics-as-an-exact science, are more or less living with a complex vis-à-vis their English speaking counterparts. Cases of cultural self-rejection are not uncommon and can reach the more neurotic point of an adverse relation to one's own language, which tends to be misused, often in seminars, sometimes even at home in the intimacy of family life. These phenomena are quite remarkable and probably not discussed enough. In any case, for a student of economics, meeting the original proto-types that were so far only appraised through the mental reactions which they have induced on French scholars is a defining experience. It is a bit like a young adult who has only heard about his or her distant, impressive, grand parents through his or her parents and happens to meet them in person. The danger obviously is for the young to find a way to challenge his or her parents by seeking the praise of the grand

parents. If such is the case one is bound to inherit the parents' neurosis. Alternatively, one may suddenly realise that the reasons for the parents' difficulty with dealing with their own parents belong to a time and history with which the young has nothing to do. In such a case, the experience may be quite liberating. This is how I decided that I would *NOT* become an economist, and how I landed in California, helped by a grant and an invitation from an American professor who was kind enough to offer to guide my next (or first) steps in economic history.

The early months, and in truth, the following ones too, were something not unlike the experience of the characters of the famous David Lodge novel. There was the sun and the weather of the Bay Area, the ambience of the campus, the extraordinary library facilities, and the rich supply of the seminars, where everybody was open and relaxed, focused and concise, tolerant and attentive. In Euphoric State University, I gorged myself on reading, something which I had never been able to do on such a scale in the past, overexploited the possibilities of interlibrary borrowing, and discovered the charms of the 'government documents' library where all the official publications, statistics, etc. were so conveniently gathered. I also discovered that cliometrics was a subject that was respectable enough to be taught to (final year) undergrads. Economic history moreover featured as a full subject within the requirement of the PhD programs, and some students within the economics department would choose a historical topic for their dissertation.

III. Trick or Treats?

At the same time, while the mass was still being said, and while there was a substantial supply of bishops and archbishops who knew their liturgy well, one had a sense that, as far as the original Clio program was considered

(i.e. use ideas from neo-classical economics and apply them to revisit defining episodes of (American) economic history), the heroic times were over: a soon to be awarded Noble prize (a distinction that honours the victors of old controversies whose whereabouts everybody has forgotten) would indeed be the R.I.P. of the first clio movement. It is not that there was no claim on direction. The Davidians announced that 'history mattered' and told stories about keyboards. It was not clear how useful such theories could be since the majority of economists anyway believed that history is 'bunk' (I had to look up the word in my dictionary). Historians, on the other hand did not need to be convinced: so why should they sound apologetic? The Northians on the other hand said they had discovered the role of institutions in economic development and wrote about British-style parliamentary systems and property rights. But any French high school student knows from the textbook that the problem with Russian rural development before World War One was that the Mir's communitarian structure did not provide people 'with the right incentives'. To a large extent, Clio seemed to owe much of its survival to the forces of inertia: there were syllabuses, journals, students and thus a tendency toward replication. After the original take-off of the clio revolution, growth was achieved through absorption of generations of technological progress. In the same fashion neo-classical economics had been applied in the past, and new waves of innovation, originating in economics, could be processed on historical matter. The new economics of information was (and still is) especially trendy. Bankers of the past became 'financial intermediaries' in charge of resolving 'informational asymmetries', those who did not get access to loans were being 'credit rationed'. The mill could run at full speed, and the economic historians were precisely those intermediaries who derived a

rent from knowing both the economists' tool (with a standard delay) and the historians' facts (with a standard error).

IV. Time on the Cross

It is an important question to decide on intellectual grounds what economic history should be at the turn of the century. What was path-breaking 40 years ago cannot remain so forever, and there is no longer much novelty in the application of economics to history. The only possible renewal that would come from this continuing process would involve more narrowly defined historical issues: i.e. it might contribute, topic after topic, to change our insights and views about things past, with each new application of economic ideas. This natural trend would end up making economic history a junior planet in the economic galaxy. Located downstream in the intellectual process of investigation, economic history would derive its institutional status from providing illustrations to the glory and power of economics. The challenge would be for historians: a new swarm of techniques and findings would invade their fields every year. In this sense, the trend in economic history would be just that experienced by other social sciences – such as political science – which are being gradually transformed by the instillation of economics, to the point where they sometimes seem to lose their specificity.

Is another course of events, (desirable and even possible?), the current evolution of economic history may be an appropriate response from the point of view of institutional strategy: it may be better to be the poor cousin of economics than to experience a thorough and irresistible decline such as has occurred in France and in fact in many other European continental countries. At the same time, if economic history becomes a mere plaything for economists, it will certainly lose much of its appeal, and

much of its usefulness. For again, it is the beautiful (or deadly) flowers that history throws up and that do not fit into the square explanations of economics which should provide the thrust and the energy for new research efforts. It is precisely because we deal with facts more than with stylised facts, with observations more than with introspections that we can advance the state of knowledge in social sciences. The first cliometric revolution did a wonderful job in proving that economics was an adequate tool to explore history. But isn't it time to show that those explorations have in turn something to tell us about economics? And wouldn't this be much more exciting than endlessly replaying the same old tune?

After years trying to strike a balance between economics and history – a balance that could in turn be called economic history – I have come to the conclusion that the essence of economic history is not about the appropriate proportion, the optimal dose which each part should have been in the final product. It is rather in the very attempt at striking a balance, in the continuing sense of discomfort that one has as long as a clear and systematic explanation has not been found, and in the renewed sense of discomfort as soon as such an explanation – suddenly all too clear and systematic – has been found. This is probably why a perennial bridge has not been and shall never be built between the two cultures: because both banks of the river are moving or constantly changing, so that any bridge is bound to have its foundations weakened and washed away at some point. And because economic history is about the effort at building the bridge, about crossing the river on the provisional construction, and about the view one gets from there – not about the bridge itself. I suppose that's how, unknowingly and involuntarily, by refusing to choose between economics and history, one may end up an economic historian.

Marc Flandreau (b. 1967) was educated at the Ecole Normal Supérieure, Paris, the University of Paris – Sorbonne (1986-1990), and his PhD was jointly awarded in 1993 by the Ecole des Hautes en Sciences Sociales, Paris and the London School of Economics. He has been Fulbright Fellow at the University of California, Berkeley; Visiting Professor at Stanford University; Research

Fellow at the Centre National de la Recherche Scientifique and is currently Professor of Economics and the University of Lille (since 2000). His research interests are money doctors; history, economics and politics of international financial expertise 1820-2000; the political economy of the Habsburg Empire; private responses to international financial instability pre-1914.

Presenting at Clio: A Comprehensive Analysis, 1961-2005

By Michael Hauptert, UW-La Crosse

As this issue of the *Newsletter* goes to press, the program for the 2006 meetings is in the process of being put together. It is an annual rite of spring for Cliometricians. Since 1961, we have met annually, first in January and ultimately in late spring or early summer. Beginning in 1988, we added winter meetings under the umbrella of the ASSA and, in 1985, began meeting on a quadrennial basis, semiregularly, under the nom de plume of World Congress of Cliometrics.

The inaugural World Congress was held in Evanston in 1985 and next convened in Santander, Spain in 1989. In lieu of a World Congress in 1993, Clio had a session at the IEHA meetings held in Milan in 1994. Since resuming the Congress in 1997 in Munich, Clio has gone to Montreal in 2000 and Venice in 2004. The next meeting of the World Congress will take place in 2008 in Edinburgh.

The World Congress and the ASSA sessions, while sponsored by the Cliometric Society, are not run in the same format as the annual meetings. As anyone who has ever attended a Clio conference knows, the papers are mailed to all participants in advance, with the expectation that they will be read before the conference. During the conference, presenters are limited to a mere five minutes

to summarize their research, while the remainder of the session is devoted to questions and comments from the discussants. In addition, there are no concurrent sessions. In contrast, the World Congress and ASSA sessions resemble more closely a traditional conference atmosphere. Even though substantial abstracts of the papers are circulated in the fall *Newsletter*, there is no control over the attendance at the ASSA sessions, so the five-minute presentation format is not used. Instead, the majority of the time is lent to the presentation of papers. A referee then comments on the paper, and a few minutes are left for the floor to weigh in. The World Congress often has concurrent sessions and is much larger than a traditional Clio meeting, thus the control of the program is not as tightly run as the spring meeting.

From 1961 until December of 1988, the only time Cliometricians met was the annual spring conference, originally held in January, now in May or early June. From December 1988 through December of 1990, Clioms closed out the year with a second meeting, held in conjunction with the ASSA as a number of sessions within the larger organization's meeting. When the ASSA switched their schedule from the last week of December to the first week in January, the

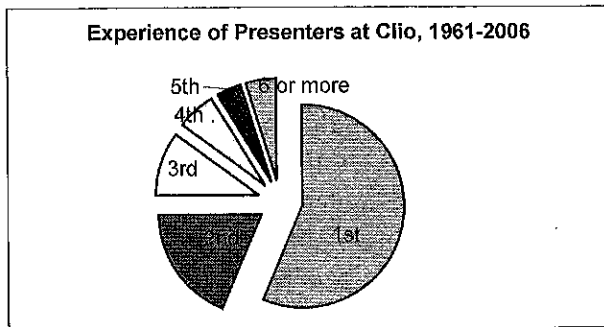


Figure 1

ASSA Clio meetings became the first meeting of the calendar year. This change meant that Clio met only once in 1991. ASSA met in December of 1990 and then in January of 1992.

When the World Congress first met in 1985 in Evanston, these meetings replaced the spring Clio meetings, as they did in 1989. However, in 1997, Clio experimented with holding its regular meetings (in Toronto that year) in May in addition to the World Congress in Munich in July. The experiment has never been repeated – and will not be in 2008.

Throughout the history of the Clio meetings, covering all sessions in all venues, there have been a total of 899 papers presented by 687 different scholars, 459 of whom only presented one time at Clio. (Note: All authors of a paper were counted as having their work presented. It was not possible to determine who among the authors of multi-authored works were actually present at the meetings each year). On the other end of the scale, the reigning veteran of Clio presentations is Robert Margo, who has presented 11 papers.

The meetings are heavily weighted towards young scholars, or at least scholars with little history of presenting their work at Clio. More than 85% of total presentations have been by authors with three or fewer presentations, with 56% of all presentations

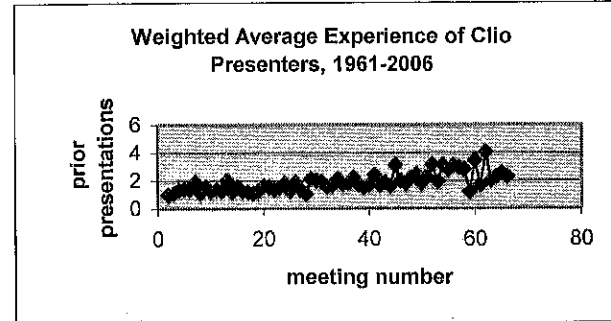


Figure 2

made by first time participants (Figure 1). Grizzled Clio veterans are a relatively scarce commodity. Only 4.8% of all presentations have been made by Clioms with six or more previous presentations. As Clio has increased the frequency and size of its gatherings over the years (the early meetings featured only six papers, while recently the standard has been 12), the trend has been toward more experienced presenters. One measure of this level of experience is presented in Figure 2 as the weighted average number of presentations. The upward trend is obvious, although it has been highly erratic in the past few years. For example, the first meeting, held in 1961, was the only one in which all presenters appeared for the first time. In every year thereafter, Clio has featured a mix of newcomers and veterans, with the minimum percentage of first-time presenters 21.1% in 1999 at the ASSA Clio sessions.

While the site of the annual meetings has moved regularly in recent years, that was not always the case. The first two decades saw the meetings take up lengthy residences at Purdue (eight years) and then Madison (ten years) before moving to Chicago for two years and Iowa City for three. Thereafter, Miami University in Oxford, OH became a semi-regular site, hosting the meetings in 1984, 1986, 1988, and 1992. Since then, the meetings have returned to a site only once. Tucson hosted the meetings in 1994 and 2001, although they did return one more time

to Oxford in 1999. A complete listing of meeting sites is presented in Table 1.

Besides an emphasis on fresh faces at Clio, the meetings have also seen an increase in cooperative research. During the first half of Clio's history, the majority of the presentations were solo-authored works. In 20 of the first 34 meetings, 80% or more of the papers were single authored, including four times when all of them were. In the 31 meetings since 1990, that 80% mark has been reached only seven times, and never has it reached 100%. Viewed from the other end, each of the first 34 meetings had a minimum of 60% of the papers written by one author. Since then, more than half of the meetings have featured fewer than 60% of the papers by only one author (Figure 3).

Table 1: Location of Annual Clio Meetings

| Years | Location |
|------------------------------|----------------------|
| 1961-68 | West Lafayette, IN |
| 1969-78 | Madison, WI |
| 1979-80 | Chicago, IL |
| 1981-83 | Iowa City, IA |
| 1984, 1986, 1988, 1992, 1999 | Oxford, OH |
| 1987, 1990 | Champagne-Urbana, IL |
| 1991 | Bloomington, IN |
| 1993 | Evanston, IL |
| 1994, 2001 | Tucson, AZ |
| 1995 | Lawrence, KS |
| 1996 | Nashville, TN |
| 1997 | Toronto, ON |
| 1998 | St. Louis, MO |
| 2002 | La Crosse, WI |
| 2003 | Raleigh, NC |
| 2005 | Lake Tahoe, NV |

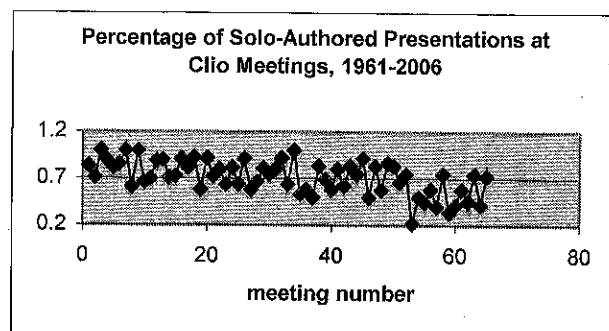


Figure 3

Only 3% of all papers presented have been written by three or more authors together. Interestingly enough, two of those papers were written by the same troika of Atack, Bateman, and Weiss. Even more curiously, both were presented at the same Clio conference, held in Evanston in the spring of 1979. Apparently, the program committee was so impressed by the two papers, "The Manuscript Census as a 19th-Century Data Source" and "The Diffusion and Adoption of Steam Power: Risks and Returns in 19th-Century Manufacturing," that they could not decide between them. Another possibility is that they were thrown off by the fact that the order of authors was changed on the two

papers. Either way, it was a rare event. Only three other times in 65 meetings of the Cliometric Society have the same author(s) presented two papers at the same conference. Stan Engerman was the first. In 1968, he presented "Regional Incomes in the Nineteenth Century," while his coauthor, Bob Fogel, presented their work titled "A Model for the Explanation of Industrial Expansion during the Nineteenth Century with an Application to the American Iron Industry." Eugene White presented two papers at the 1988 ASSA Clio sessions, and Ulrich Woitek presented "Integration of Grain Markets in Pre-Industrial Southern Germany" and his paper with Rees and Long, "The Puzzle of Slave Heights in Antebellum America" at the 1997 World Congress.

The greatest length of time between a Clio member's first presentation and their last is 42 years. Richard Easterlin presented at Clio for the first time at the 1962 meetings and most recently appeared at the 2004 ASSA Clio sessions. In between, he presented at the 1964 and 1966 Clio meetings and the 1996 ASSA sessions. The only other member of this exclusive 40-year club is Peter Temin, who first appeared on the Clio

Table 2: Longest Presentation Tenure at Clio Meetings

| Name | Span (years) | First presentation | Most recent presentation |
|-------------------|--------------|--------------------|--------------------------|
| Richard Easterlin | 42 | 1962 | 2004 |
| Peter Temin | 41 | 1964 | 2005 |
| Fred Bateman | 38 | 1966 | 2004 |
| Gavin Wright | 36 | 1968 | 2004 |
| Paul David | 36 | 1962 | 1998 |
| Stanley Engerman | 36 | 1965 | 2001 |
| Lance Davis | 35 | 1962 | 1997 |
| Richard Sutch | 34 | 1970 | 2004 |
| Jeff Williamson | 33 | 1971 | 2004 |
| Sam Williamson | 33 | 1971 | 2004 |
| Knick Harley | 33 | 1971 | 2004 |
| Alexander Field | 33 | 1973 | 2006 |
| Rick Steckel | 31 | 1973 | 2004 |
| Roger Ransom | 30 | 1964 | 1994 |
| George Grantham | 30 | 1969 | 1999 |
| Nick Crafts | 30 | 1975 | 2005 |
| Michael Bordo | 30 | 1969 | 1999 |

scene in 1964. Interestingly enough, Temin's only other appearance was in 2005 at the ASSA Clio sessions, giving him two presentations separated by 41 years, a record for abstinence. There are several members whose presentation career has spanned 30 years (Table 2). This longevity, however, has not always translated into frequency. Only four of the 17 members of the 30+ club are among the top 18 most frequent presenters (Table 3). In fact, half the members of this latter list made their Clio debut after 1980.

The first Clio conference featured six papers, including presentations by Robert Fogel and William Parker, who would both become regulars on the Clio scene. Fogel presented five more times – 1963, 1965, 1968, 1972, 1989 – featuring such notable works as "The Social Savings Attributable to American Railroads in the Interregional Distribution of Agricultural Products in 1890: An Application of Mathematical Models to a Problem of History" (1961), "The Position of Rails in the Market for American Iron 1840-1860: A Palaeontological Reconstruction" (1963), and "The Market

Evaluation of Human Capital: The Case of Slavery" in 1972, with Stanley Engerman. Parker appeared again in 1962, 1964, 1966 and 1989. Until his last presentation in 1989, the two would never again appear together on a Clio program. Half of the 1961 program (G. W. Bertram, J. H. Mc Randle, and John Snyder) would never present at a future Clio conference. The final paper was presented by George Murphy, who came back to present once more in 1970.

Among the many presentations that would go on to become landmark contributions to the literature were Robert Gallman's "National Output in the 19th Century" (1962), "Trends in Ocean Freight Rates" by Douglass North (1963), Richard Easterlin's "Economic-Demographic Interactions and Long Swings in Economic Growth" (1966), Gavin Wright's "Production Functions in Cotton Farming in the Nineteenth Century" (1968), Donald McCloskey's "Did Britain Fail?" (1969), and Claudia Goldin's "Women in the American Labor Experience: Issues, Life-Cycle Participation, and Earnings Functions (1979)."

Table 3: Most Frequent Presenters at Clio Meetings, 1961-2006

| Number | Name | First | Most Recent |
|--------|------------------|-------|-------------|
| 11 | Robert Margo | 1988 | 2004 |
| 10 | Greg Clark | 1984 | 2001 |
| 9 | Jeff Williamson | 1971 | 2004 |
| 9 | Gary Libecap | 1977 | 2000 |
| 9 | Eugene White | 1980 | 2000 |
| 8 | Joe Ferrie | 1983 | 2004 |
| 8 | Mike Hauptert | 1988 | 2005 |
| 7 | Fred Bateman | 1966 | 2004 |
| 7 | Richard Sutch | 1970 | 2004 |
| 7 | Michael Bordo | 1973 | 2003 |
| 7 | Alexander Field | 1973 | 2006 |
| 7 | Joel Mokyr | 1975 | 1997 |
| 7 | Lee Alston | 1978 | 2000 |
| 7 | Jeremy Atack | 1979 | 2004 |
| 7 | Price Fishback | 1982 | 2003 |
| 7 | Howard Bodenhorn | 1990 | 2006 |
| 7 | Werner Troesken | 1992 | 2004 |
| 7 | William Collins | 1997 | 2004 |

An interesting pattern emerges when looking at the percentage of first-time presenters at Clio meetings. A break point seems to have occurred in 1988 when the Clio sessions were added to the ASSA meetings. Before 1988, the percentage of first-time presenters was much higher, averaging 74.8%. Admittedly, there were fewer meetings, thus fewer experienced presenters around. However, these are percentages, and the meetings tended to be smaller. After 1988, the percentage of first-time presenters dropped to just over 55%. Meanwhile, the ASSA average has been only 43%, falling below 40% in seven of the past eight years. It seems that the Clio presentations are graying. In part this could be due to the fact that more sessions are held and they are held more often. In addition, with the increase in papers written by multiple authors, more authors are getting presentation opportunities. Another, less positive,

explanation for the trend is the decrease in the number of new economic historians on the market. In an earlier essay, I noted the recent decrease in the number of new PhDs and jobs in economic history.

Clio is nearing the half-century mark. There have been many changes in the Society in that timespan. The meetings are larger, more frequent, and, some would say, less confrontational. The computer has advanced the original concept of the use of quantitative methods in economic history to levels beyond what anybody could have imagined in those first few winters at Purdue. One thing that remains constant, however, is the high quality of the research presented at the meetings and the use of the Clio meetings as a springboard for the dissemination and publication of leading work in the field of economic history.

2005 Annual Conference of the Economic and Social History Society of Ireland

By Gaye Ashford, St. Patrick's College

(Dublin) The 2005 Annual Conference of the Economic and Social History Society of Ireland (ESHSI) was hosted by the History Department of St. Patrick's College, Drumcondra, Dublin on November 11-12. At the opening night reception, Pauric Travers, President of St. Patrick's College, warmly welcomed everyone to the College and noted that 12 years had passed since they last hosted the ESHSI.

Over the two days, delegates to the Conference heard 12 distinguished scholars discuss a wide range of topics on the theme of "Economy, Settlement, and Society in Ireland." They also enjoyed an excellent selection of postgraduate research presentations by: Annette Overland (NUI,

Galway), "An Upland on the Beara Peninsula: Charting Land-Use and Landscape Change over the Last Two Millennia;" Brian Gurrin (NUI, Maynooth), "Challenging Times: The Impact of Subsistence Crises on County Wicklow, 1720-1740;" and Jennifer Kelly (NUI, Maynooth), "Secret Societies."

On Friday night, the keynote Connell Lecture was delivered by Gabriel Cooney (UCD, College of Arts and Celtic Studies), who discussed the relationship between prehistory, history, and archaeology.

In the opening presentation, **Finbar McCormick** (Queen's University, Belfast) discussed his research on "Economic and

Agricultural Change in Early Medieval Ireland," which is also the topic of his forthcoming book. McCormick interprets evidence from archaeo-zoological studies on the remains of cattle bones and examines the grounds for a currency and value system in 7th- and 8th-century Ireland. He argues that cattle as a value system in Ireland declined during the 8th century, when it was replaced by the expansion of cereal production. He makes a case for this transition by pointing out the large numbers of known horizontal mills constructed between 801 and 825. During the same period, Irish cereal production expanded from a domestic yield to "industrial" proportions.

Kevin Whelan (Keough-Notre Dame Centre) talked about the relationship between the environment and history in eight regions of Ireland in "Regional Cultures in Eighteenth- and Nineteenth-Century Ireland." He took delegates on a lively descriptive tour of Ireland using examples from literature, poetry, music, place names, surnames, culture, and Gaelic games to outline the influences, similarities, and differences found in each region. Whelan explores the economic and cultural flows westward from Ireland to Newfoundland and towards Europe, noting the distinctive regional survival of Gaelic, Cambro-Norman, and Old English traditions. He argues that the topography and geology of each area was important in either assisting the flow or acting as a flow block on these regional cultures, which led to distinctive and particular regional traditions.

On Saturday morning, **Matthew Stout** (St Patrick's College, Drumcondra) reported on "Cattle Economies after the Great Famines circa 540 and 1845," which draws on his research on ringfort classifications. He proposes that the resultant exploitation of cattle in Ireland following a suggested

climate downturn circa 540 led to a famine and gave rise to a unique economy centered on the ringfort. According to Stout, the resultant value system surrounding cattle shaped Ireland and its society and affected all facets of Irish life. He also states that following the potato famine of 1845, tillage decreased while cattle numbers increased. Stout uses examples from Early Irish Law and modern statistics to argue that ruthless farm management enabled farms to be kept sustainable and viable following both famines.

David Dickson (Trinity College, Dublin) uses information from three newly discovered sets of records kept by Garret FitzGerald of Lisquinlin and Corkbeg in County Cork in "Garret FitzGerald: A Gentleman Farmer in Seventeenth-Century Cork." The records cover a 25-year period, which spans times of war and peace during the Commonwealth and Restoration, including the period during the Cattle Acts. The records, which are principally account books and records of butter contracts, build a picture of direct proprietorial farming that provide insight into the rise of dairying in the south of Ireland. The records also shed light on FitzGerald's dealings with small farmers during the 1650s – dealings that highlight a relatively low displacement of tenants but higher proprietor displacement. Dickson believes that these records will also help to bridge the gap between the Civil Survey and the 1641 Depositions.

Colin Breen (University of Ulster, Coleraine) followed with "Economic Landscape Change in the Territory of the O'Sullivan Beare." Breen, who recently published a book on the subject, uses evidence from historical and archaeological sources to reconstruct the economic base of the O'Sullivan Beare territory between 1400 and 1640. He indicates that the core of the

O'Sullivan Beare territory depended on fishing, marine communication, agriculture, and church lands. Breen finds that during the post-1420/1430 periods, clusters of settlement grew up in and around churches and shifted economic power toward a new mercantile planter elite with London as the central perspective. The expressions of wealth ownership and settlement patterns of the new planter elite were different from other locations in Ireland and Britain, and by the 1620s, these changes resulted in a more formalized economic structure.

W. J. Smyth (University College, Cork) presented "William Petty and the Construction of Cromwellian Ireland," based on his forthcoming book *Map-Making, Landscapes, and Memory: Colonial and Early Modern Ireland*. He looks into William Petty's keen understanding of the triangular relationship between power, knowledge, and action. Petty played a key role in the collection of information about Ireland for the Cromwellian administration; this information resulted in most of the subsequent plans for those areas of plantation. Smyth highlights Petty's role in developing the first Field Mapping Techniques, which are still followed in modern day field mapping. He observes that the importance of Petty to this period is perhaps best illustrated in his work, *Down Survey*, which laid the foundation stone of the Cromwellian land settlement in Ireland that lasted until the Land Acts 200 years later.

Jane Grey (NUI, Maynooth) drew on her recent publication, *Gender and Industrialization in Ireland during the Long Eighteenth Century*, for her presentation on "Gender and Deindustrialization before the Famine: The Puzzle of Fermanagh." The themes addressed in Grey's work focus on the decrease in weaving in County

Fermanagh, which stands in contrast to the increase in weaving in west Ulster and northwest Connacht. Grey finds that, while there is evidence for some increase in weaving output in the Fermanagh region after 1800, weaving in Co. Fermanagh presents as a negative case study and may help to expand the explanatory capacity of existing theories for household participation in rural domestic industry before the factory age. Grey suggests that weaving in Fermanagh reflects some of the differences in women's labor, power distinctions, and status in the home.

In the closing session, **Cormac Ó Grada** (University College, Dublin) reported on "Irish Jewry a Century Ago: Economic-Demographic Perspectives." In the late 1860s, members of a Jewish community in Lithuania immigrated to Ireland, where they were known as the Litvak Community. Ó Grada examines a few important characteristics of the Dublin Litvak community, such as its demography, infant mortality rates, and occupational profiles at immigration and as the community became more settled and developed. He determines that the Dublin Litvak community thrived and eventually acculturated. He believes that the community numbered more in Dublin than in other parts of the country, such as Belfast, because Dublin offered greater job opportunities for the self-employed and highly entrepreneurial Litvak community.

Organizers concluded this year's conference by thanking Ms. Maura Sheehan, Secretary to the Department of History, St. Patrick's College, Drumcondra, for her flawless and enthusiastic organization of the event and Ms. Bernadette O'Dwyer for organizing rooms, refreshments, and acting as liaison with the excellent catering staff. The ESHSI will meet again November 17-18, 2006 in Belfast.

Clio in Retrospect: 1976

By Michael Hauptert, UW-La Crosse

The site was Madison, but the spirit was Philadelphia. Cliometricians gathered on the campus of the University of Wisconsin in the spring of 1976 for their 26th annual conference. The mood was festive and the dress patriotic. Several members donned minutemen regalia, authentic down to the bootless, swaddled feet. Others chose to exhibit their sartorial splendor in a more contemporary manner, donning red, white, and blue platform shoes, bellbottoms, and liberty bell jewelry, whose sparkle was outshone only by the presentations.

The program was selected to feature youth. The veterans were two second-time presenters known for their youth and vigor: Joel Mokyr (Northwestern) and Alexander Field (Stanford). The other ten presenters were on the program for the first time. Seven of the twelve would not present again.

Joel Mokyr and **Eugene Savin** (University of British Columbia) presented "Stagflation in Historical Perspective: The Napoleonic Wars Revisited," research which stemmed from Mokyr's recently completed dissertation. They constructed a supply side model with an exogenous technology endowment in an effort to offer a quantitative description of the divergent paths of the economies of the Low Countries. Mokyr and Savin then sought to explain the reasons for the divergent paths they observed.

The other Clio veteran on the program, **Alexander Field**, also presented work, "Occupational Structure, Dissent, and Educational Commitment: Lancashire, 1841," in the same vein as his recently completed dissertation on educational reform and manufacturing development in mid-19th-

century Massachusetts. Field seeks to outline and document the dimensions of the historical coincidence between the rise of the factory system and that of public schooling, as well as explain those developments. His dissertation focuses on America, but his Clio presentation extends his findings to the wider world.

Michael Haines (Cornell) shared part of his recently completed dissertation on "Industrial Work and the Family Life Cycle, 1889-1890." His work focuses on the economic and demographic reaction of the agricultural sector to the forces of population pressure and rapid modernization in Prussia. It would be published in the *JEH* in June of 1976 as "Population and Economic Change in Nineteenth-Century Eastern Europe: Prussian Upper Silesia, 1840-1913." His "Fertility, Nuptiality, and Occupation: A Study of Coal Mining Populations and Regions in England and Wales in the Mid-Nineteenth Century" was one outcome of this research. It was published in the *Journal of Interdisciplinary History* in the autumn of 1977. Haines shows that occupational fertility and nuptiality differences could be explained by economic factors, although the general social norms surrounding each occupation continued to play a role.

David Galenson (Harvard) gave an account of "British Servants and the Colonial Labor System: An Analysis of the Length of Indenture," which he would eventually publish as "British Servants and the Colonial Indenture System in the Eighteenth Century" in the *Journal of Southern History*, February 1978. He uses evidence from 18th-century British records to examine the important but overlooked role that indentured servants



Clio Meetings are not as hostile nowadays

played in the origin and growth of slavery in the American colonies.

"Toward a Quantitative Description of the New Orleans Slave Market" was presented by **Laurence Kotlikoff** (Harvard). In June of the following year, it was published in the *JEH* as "The Old South's Stake in the Inter-Regional Movement of Slaves, 1850-1860." Kotlikoff attempts to illuminate the theoretical determinates of the East-West slave migration and resolve the issue of the migration's impact on the Old South. His results constitute a strong refutation of the historical claim that the Old South needed western expansion and slave exportation to prosper. He claims the East had little if anything to gain in the economic sphere from the expansion. More probable is the notion that Eastern wealth declined substantially as more slaves moved west.

Gloria Main (York College, NY) discussed "Inequality in Early America: The Evidence of Probate Records from Massachusetts and Maryland," which she would ultimately publish in the *Journal of Interdisciplinary History* in the spring of 1977. Main

investigates the inequality of wealth-holding in antebellum America. Her work contributes both to the history of the distribution of wealth in American and an understanding of the nature of inequality itself. The growth of wealth at the top rather than the expansion of the propertyless provided the impetus for the sudden jump in inequality in both states.

Besides bicentennial fever, which led to a general hoarding of change in a desperate search for the commemorative bicentennial coins in circulation, there was also disco fever. Groans of protest arose from the crowd when it was announced that the disco dance tentatively planned for the evening was to be cancelled. Instead of strutting their stuff to the top hits of the day – "Don't Go Breaking My Heart" (that crowd pleaser by Clio favorites Elton John and Kiki Dee) and "50 Ways to Leave Your Lover" by Paul Simon – Clioms were left to linger after dinner and catch up on shop talk and reruns of *Happy Days* and *Charlie's Angels*.

Howard Marvel (Ohio State) argues that the Factory Act of 1833 was designed to place a differential burden on a subset of textile manufacturers in "Factory Regulation: A Reinterpretation of Early English Experience." He finds that the act was not enacted and enforced solely out of a compassion for factory children. It was, instead, an early example of a regulated industry controlling its regulators to further its own interests. It was eventually published under the same title in the October 1977 issue of *Journal of Law and Economics*.

Michelle McAlpin (Tufts) reported on "The Demographic Effects of Famines in Bombay Presidency, 1871-1931." She went on to publish it under the title "Dearth, Famine,

and Risk: The Changing Impact of Crop Failures in Western India, 1870-1920" in the *JEH* in March of 1979. She addresses the issue of the frequency of crop failures in western India between 1870 and 1920 and the declining instance of famine during the same period, concluding that deficient rainfall led to crop failures, which led to increased trade in agricultural products and increased labor demand. Coupled with increased credit and relief services from the government, this significantly reduced the probability that a crop failure would result in widespread famine.

Talk during the break turned to the recent trend of singing stars taking a bullet. Jerry Lee Lewis accidentally shoots his bass player while using the hotel door for target practice. Fortunately for the bassist, he lived. Unfortunately for Jerry Lee, he sued. Then, reggae star Bob Marley was shot when seven gunmen burst into his home in Kingston, Jamaica. He too survived, but, unfortunately, he could not locate any of the seven gunmen to sue them.

Philip Mirowski (Michigan) shared part of the dissertation he was working on under the supervision of Gavin Wright. In "The Plague and the Penny-Loaf: The Disease-Dearth Nexus in Stuart and Hanoverian London," he focuses on business cycles in the early stages of England's modern economic order. The notion that pre-modern economic fluctuations were due mainly to wars and harvest fluctuations, he argues, is belied by the fact that Walrasian theory predicts that there is no a priori expectation about the direction of their effects, while there is an expectation that they will largely cancel out in the aggregate. In fact, the only logical periodization in Walrasian theory is no periodization at all. The modern period should be coterminous with the historical existence of the market. According to

Mirowski, this is just another way of saying that Walrasian theory is truly ahistorical.

Steve Easton (LSE), the only non-American on the program, presented "Able-Bodied Relief in England and Wales, 1855-1910." This was a precursor to his March 1979 *JEH* article, "Aggregate Aspects of the Poor Law, Unemployment Insurance, and Unemployment in Britain, 1855-1940." He concentrates on several questions associated with the late Victorian Poor Law program, including whether or not the number of people receiving relief was significantly affected by the amount of the benefit paid. He also looks at whether the rate of unemployment significantly affected the number on relief and whether the rate of Poor Law assistance affected the rate of unemployment. He uses a model that disaggregates the rate of pauperism per thousand into the proportion on indoor relief and outdoor relief to answer these questions.

Last but certainly not least, **Terrence Thomas** explored "The U.K. Full Employment Budget Surplus during the Interwar Period: A Measure of the Deflationary Impact of Fiscal Policy." Happily ever after was how the Clio conference ended, unlike the dominant theme in Hollywood that year. Rocky lost his fight with Apollo Creed, Carrie did in her classmates when they didn't play nice, and the Bad News Bears didn't win their Little League baseball crown. The year had gotten off to a somber start with news that Agatha Christie died at age 85 on January 12th. Nine days later, however, Emma Bunton, who would become Baby Spice of Spice Girls fame, was born, thus maintaining a sort of cultural cosmic balance. It wouldn't take anything as powerful as a cosmic force to reunite Cliometricians. They would gather again in Madison the next year for another round of explosive elocution.

A Letter from the Editor

Welcome Aboard Cliometricians,

Spring is here, which naturally means that a middle-aged editor's fancy turns to baseball. And when baseball is the topic, two things follow: steroids and the Hall of Fame. It occurs to me that Clio has neither a policy regarding the former nor a venue to house the latter. I think the time has come to address both of these shortcomings.

A hall of fame is a surefire winner. It generates interest, draws tourists, and can be a cash cow for the management thereof. It is also a fine way to recognize the achievements of Clio pioneers and record holders. Imagine the thrill young Clioms-in-waiting could experience by seeing an original copy of the first Clio program, complete with margin notes made by G. W. Bertram, or the hairpiece worn by a noted Clio session chair during one of the Purdue meetings. I imagine a wing of the Hall would be dedicated to oral history. An exhibit where visitors could hear the many fine contenders for the Mullah Award (and its successor, the Tattler) articulate their pearls of wisdom would be popular. Of course, the first few years would have to be recreated, but this should not be a barrier.



I have personally visited halls of fame devoted to some of the more important endeavors of mankind: baseball, football, bowling, bread, postal history, and sex (in no particular order). Cliometrics would fit nicely on that list.

And a gift shop. Don't forget the impact of a gift shop. Books, posters, earrings, magnets, and celebrity bobblehead dolls only scratch the surface of possibilities. Spreading the wisdom of Clio and reaping millions for the Society simultaneously – what could be better?

Of course, before Clio can open a hall of fame, it would be wise to establish a policy regarding steroids. Such policies are all the rage these days. What good is a hall of fame if you don't have some controversial policy from which to exclude someone? Good examples of policies can be found for all credible institutions that also have a hall of fame, ranging from the aforementioned baseball and football to cycling, competitive spelling, and gardening.

Since we need a policy to deal with steroids, what better way to influence those who are using or considering using steroids to get that competitive edge than to announce that anyone caught using them will be permanently banned from induction into the Clio Hall of Fame? The publicity we could garner by sending our Executive Director to the Larry King Live show to defend our policy would be priceless. After all, better to have our Executive Director defend a steroids policy to Larry King than a comma usage policy. Heck, I bet even Wayne Newton would agree with that.



Mike Hauptert
Editor

Call for Papers

Economic History Society Annual Conference

Exeter, United Kingdom

March 30- April 1, 2007

The 2007 annual conference of the Economic History Society will be hosted by the University of Exeter, March 30-April 1, 2007.

The conference program committee welcomes proposals on all aspects of economic and social history covering a wide range of periods and countries and particularly welcomes papers of an interdisciplinary nature. Preference may be given to scholars who did not present a paper at the previous year's conference. Those currently studying for a PhD should submit a proposal to the New Researcher session; please contact Maureen Galbraith (ehsocsec@arts.gla.ac.uk) for further information.

The committee invites proposals for individual papers, as well as for entire sessions (3 speakers, 1.5 hours duration). The latter should include proposals and synopses for each paper in the session, although the committee reserves the right to determine which papers will be presented in the session if it is accepted. If a session is not accepted, the committee may incorporate one or more of the proposed papers into other panels.

For each proposed paper, please send (preferably by e-mail) a brief c.v. and a short abstract (including name and postal and e-mail addresses) of 400-500 words to:

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For full consideration, proposals must be received by September 18, 2006. Notices of acceptance will be sent to individual paper givers by November 17, 2006.
