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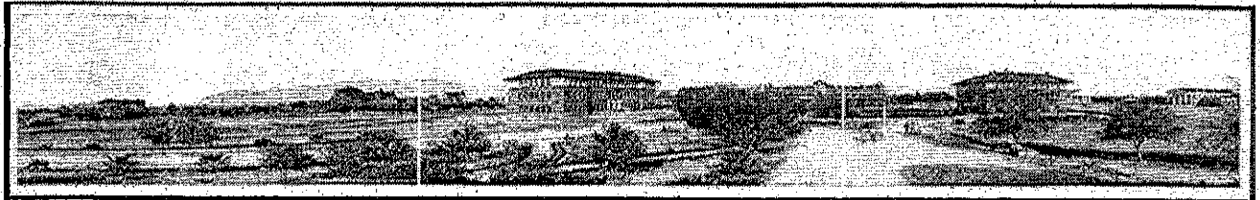

University of Arizona c1909

Report on the 40th Annual Cliometrics Conference

By Joyce Burnette, Wabash College, and Brooks Kaiser, Gettysburg College

(Tucson) The 40th Annual Cliometrics Conference met in this unusually wet desert locale, May 18-20, 2001. The Program and Arrangements Committee of Price Fishback, George Grantham, Michael Haupert, Shawn Kantor, Gary Libecap, Barbara Sands, and Samuel Williamson arranged a conference of 12 fine papers for perusal by 47 participants.

The conference began with a round of introductions led by session chair John Wallis (Maryland). **Michael Clemens** and **Jeffrey Williamson** (both Harvard) brought their paper, "Where Did British Foreign Capital Go? Fundamentals, Failures, and the Lucas Paradox, 1870-1913," to the table to convince us that international capital market failures were not the primary reason for poor countries failing to receive British capital investments. Clemens argues that differing marginal products of capital due to different natural resource endowments may make countries more or less credit worthy. Price Fishback (Arizona) began the discussion with a challenge to the notion that Clemens and Williamson tested market failures rather than government failures.

Several participants expressed concern about the choices of data and implications for the underlying model. Gregory Clark (UC-Davis) proposed capital returns as a more direct measure than capital flows. Kenneth Sokoloff (UCLA) suggested an investigation of the relative importance of private capital versus public capital. Chulhee Lee (Seoul National University) wondered if a test should be devised for differences in countries' internal investment levels.

Tom Weiss (Kansas), Kyle Kauffman (Wellesley College), and Michael Haupert (UW-La Crosse) voiced concern about the role of the poorest of the poor in the paradox, who are (in the case of Africa) or may be (in the case of mainland China and Hong Kong in one variable) excluded from the data set. In a complementary vein, George Grantham (McGill) questioned whether the results were driven by the overwhelming roles played by single nations.

Gerald Dwyer (Federal Reserve Bank of Atlanta), Fishback, Chris Meissner (Berkeley), and Isabel Goedde (University of Mannheim)

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What's Inside

Executive Director's Notes

Greetings Gentle Members:

Let me begin by announcing the election of Bob Margo and Ken Sokoloff to the Cliometric Society Board of Trustees. Bob and Ken replace Jean-Laurent Rosenthal and Dick Sylla, whose terms ended earlier this year. I thank both Jean-Laurent and Dick for their service to the Society. A special thanks goes to Dick who, while serving as the chair of the Clio board, was instrumental in helping me during the first year of the post-Sam Williamson era. I know what you're thinking – You didn't notice any crises during the post-Sam transition. That's exactly my point. Thanks to Dick's wise counsel and administrative skills (skills which, I might add, he honed during his administration days at a certain land-grant college in the Tar Heel state), the transition went smoothly.

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Michael Haupert, Editor
Jean Bonde, Managing Editor
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Turning to the Newsletter, this issue contains, among other gems, the report of the 40th annual Cliometrics Conference. The 40th conference was marked by a transition as well. The Mullah's Obsequious Helper has moved on to wherever such persons of help go when they cease helping. (My guess would be to annuitize their TIAA-CREF balances, but that's just a guess.) For those members who enjoyed the annual reports of the Mullah's Helper, weep not; as you will see, a new replacement was coerced into filing a report. For those members who never really embraced the old helper or his reports, you may take solace in knowing that the market fell dramatically at almost exactly the same time the helper decided to retire; as a result, I hear he will now face at least two more terms of ECON 201, MWF 8:00-8:50AM, just to make up the difference.

The next important event on the Society's calendar is the 2002 annual meetings of the Allied Social Science Associations (January 4-6) in Atlanta. As one of the "Allied Associations," the Society puts together several sessions each year at the ASSA meetings. Kyle Kauffman (otherwise know as "our man at ASSA") and Price Fishback are putting together the sessions, and I hope you will plan to attend.

Remember, every year the Society hosts a party at the ASSA meetings. Somehow, last year's host (Mike Haupert) managed to secure a room atop the Monteleone Hotel overlooking the beautiful Crescent City. I don't know how we'll top that, but we'll try. For some reason, right now I'm thinking … prom theme! Anyway, the exact time, date, and location will be announced in the future, but, if you're like me, you might want to go ahead and start trying to get a date.

Lee A. Craig, Executive Director

An Interview with Deirdre McCloskey

Deirdre McCloskey is Distinguished Professor of the Liberal Arts and Sciences; Professor of Economics, English, and History at the University of Illinois at Chicago; and Distinguished Professor of Economics, Philosophy, and Art and Cultural Studies at Erasmus University, Rotterdam. She has held faculty positions at the University of Chicago and the University of Iowa, where she was Quondam Professor of History, Murray Professor of Economics, and the Director of the Project on Rhetoric of Inquiry. In 1984, McCloskey and Sam Williamson co-founded the Cliometrics Society. She has also served as editor of the Journal of Economic History and as president of the Economic History Association, as well as president of the Social Sciences History Association. This interview took place at the EEA Meetings in New York City in February 2001 and was conducted by Mary Beth Combs (Fordham University).

How did you come to study economics?

From left field, so to speak. I was a bookish child. My father was in political science at Harvard, a student of the history of the Supreme Court, and in our house we took ideas and history seriously. It was not viewed as a strange thing to be interested in the past or the best that has been thought and written; to the contrary, you were supposed to be. My mother studied ancient Greek with a passion, and there were dinner table conversations about Cardinal Newman's *Apologia Pro Vita Sua*. History was the high school social science, so I fell naturally into thinking of myself as a historian. I started at Harvard College in 1960 as a history major. But then I took some actual history courses from Frank Freidel and William Langer (both of them great historians) and found the stuff so hard, because it required so much reading! There was no quick, analytic payoff such as I was used to having as a fan of Prince Kropotkin and Emma Goldman. Furthermore, those same politics implied helping the poor. Then in the summer after my freshman year, I read Bob Heilbroner's *The Worldly Philosophers*, a book that has made so many economists. So I shifted from the left to economics.

Wasn't your initial economics training in transportation economics?

Yes. In my senior year at Harvard, I took a graduate course in transportation, and I worked on a road-building crew every summer. Therefore, it was easy to see transportation as practical and policy-oriented. Fortunately, the teacher was John Meyer (already a famous urban and transport economist and applied econometrician), and the course was a real graduate seminar, with lots of controversy and tough, economic thinking. I remember acing the final exam, because I repeated from memory that "marginal cost pricing is the best way to decide whether to lower a gate on a railway," though I know that I did not then really grasp the function of market prices; I didn't until I started teaching such arguments at Chicago. I was Meyer's RA for a while, which was an eye-opener. John showered me with money, giving me summer employment and term-time fellowships for two years until Alex Gerschenkron took up the task. I learned how to do applied economics by watching Meyer and his colleagues, such as John Kain and the civil engineering professors at MIT, whom

Meyer had brought into his big-money projects. I've always thought of myself as having an engineering approach to economics: How big? And, will it work?

Would you say that Meyers' work inspired you to do the kind of work that you have done in economic history?

As I said, I had always been interested in history, especially English history. Meyer turned out to be one of the founders of cliometrics; he did pioneering work on the economics of slavery with Alf Conrad. What I found more intriguing was that as a graduate student, he had done a paper on English industrial output that used the technique of input-output analysis. I was fascinated and wanted to be like him. Consequently, my senior thesis (a soporific work called *Road and Rail in India*), under John's supervision, used input-output analysis. I was a child, so to speak, of the first generation of such scholars in the way that Richard Sutch and John Wallis were children of North or Claudia Goldin and Rick Steckel of Fogel.

What were some other academic influences in your early shift to cliometrics?

At Harvard all first-year graduate students had to take a full year of economic history. My first teacher of economic history was Barry Supple, the English economic historian, followed by Albert Imlah, who was a professor at Tufts and another well-known student of British history. That suited me fine. I was an anglophile, admired English literature (complements of my father), and had spent my senior year in high school goofing off in England. Fast forward to 1964-65 and the year-long course in history. The students wrote a big paper in the fall and a big paper in the spring, and I enjoyed writing them. Well ... at least I enjoyed them more than the rubbish I was being taught in my other courses. The other first-year courses included an econometrics course and the economic theory course. Both were sterile. So a first-year student's only chance to apply economics to the real world was the course in economic history.

The following year, I joined Alexander Gerschenkron's graduate seminar in economic history in which Tom Sargent, Richard Sutch, Richard Sylla, Knick Harley, Peter McClelland, Barbara Solow, David Loschky, and Stefano Fenoaltea were all students. There we taught each other. Gerschenkron never said anything; he just let the students criticize each other. Our first assignment was to read Fogel's *Railroads and American Economic Growth*, which had just come out. We smart alecks didn't think much of it. Gerschenkron assigned me to criticize David Landes' long, long chapter in the *Cambridge Economic History of Europe* (which became his book *The Unbound Prometheus)* in front of David at a dinner over at Lowell House. There was some tension I didn't fully grasp at the time, since Alex and David had sparred earlier on the significance of entrepreneurship in economic history. I thoroughly demolished the so-called "model" of innovation that David imagined he had. Paradoxically, I am now more in agreement with David.

I also went to MIT to take Peter Temin's course in American economic history the first time he gave it. Peter was a superb teacher. Our class at MIT in 1965 had Sutch, Bob Hall, and other very bright people from whom I learned the MIT style – the blindingly quick draw and the devastating chest shot. Economic history was a very exciting field in Cambridge, certainly the most exciting one at Harvard at the time, because Harvard economics was going through a period of relative intellectual deadness.

When you were in graduate school, you spent time collecting data in the British archives. How did your experience complement your graduate training?

I went to England in 1967-68 on a Harvard traveling fellowship. Times were flush in academic life then, and full-ride fellowships were a dime a dozen. I worked at the Iron and Steel Institute archive on my dissertation. Or rather, I calculated and thought and drank tea upstairs while my wife Joanne collected data downstairs. She couldn't work in England at her trade of nursing, so she helped me. Of course, most of what we collected was irrelevant to the thesis and book I finally wrote. I like to show graduate students the half of my notes I kept, a full file drawer about three feet high (that's a half, mind you!) and set beside it the slender book that resulted. I then say ominously, "This is the correct ratio: six feet to half an inch!" What was more important in that year was my participation in Arthur John and Jack Fisher's seminars at the Institute for Historical Research. Knick Harley, Mike Edelstein, and I were the brash, maybe even ugly, Americans. Especially me. I remember getting into a public quarrel with the great Marxist historian Christopher Hill, who announced that quantitative historians were illiterate; I replied cheekily that qualitative historians were innumerate. Fortunately, I learned a more sensible thing that year: that economic historians not on the cutting edge of economic theory (people like Steve Jones, Dudley Baines, Michael Thompson, Negley Harte) were nonetheless excellent scientists. In fact, they knew more about the world (though less about those chest shots) than I did. It ruined my sneering rights at non-economists.

You took your first job at Chicago.

Yes. In 1968, Fogel called up Gerschenkron and asked who he had, and they flew me over from London. I remember George Stigler attacking me in the workshop, but I attacked back which he liked, the old creep. At lunch, Milton Friedman was appalled that I had not made money on the British devaluation; I thought that confession had cost me the job. Because Fogel and Arcadius Kahan and Earl Hamilton were there, Chicago had a big investment in quantitative, factual science about the past. And then around I'd go to the annual Cliometric Society conferences at Purdue (where they were invented in 1967 and in 1969) and, in later years, at Wisconsin. I just loved the conference. I didn't much care for the American Economic History Association at the time, because it had older people whom I imagined were not as interesting as my crowd and who spent a lot of time honoring each other. The Clio rule was: present your paper, which was preread by the audience, and have it attacked, no holds barred, by people who loved you. That's how we learned to be scholars.

Would you say that the style at the Cliometrics conferences is the same today?

I just went to one this year in Tucson, and it was still in the style of Purdue in 1967. We have been wise in Cliometrics about passing the generational torch. Clio has lots of young people each year. What is crucial is that economists' questions are always in order: What's your model? Do your actors have unexploited opportunities for profit left? How about those second order conditions? But so are historians' questions. It is always considered sensible, relevant, and important to ask the following: Where did you get those facts? How accurate are they? What are their biases? How do you know that they are the important ones? Or, was the political and social context of your paper actually as you are assuming it was?" Or, how does your argument help us in *histoire totale*? Having both sorts of questions in play makes us cliometricians real economic scientists.

What would you say those values are? Is it the mathematical rigor that economists seek, grounded in facts about the world?

I don't much care for sheer rigor. Mathematically expressed rigor in a real scientific argument involving magnitudes is fine. I'm all for it, as long as it has quantitative oomph. You don't find people talking about existence theorems in Cliometrics conferences. If someone said that there exists an equilibrium where a king could charge more than such and such in taxes, everyone would laugh because we aren't interested in existence. That's math and philosophy department stuff, not engineering or history or physics or geology. We historical economists are interested in whether you can make a quantitative case that you have here an actual application to, for instance, the French monarchy in the 18th century. So it's not rigor for itself, as it is in so much of economics, but rather rigor or whatever else is valued in mathematics preparing for what is valued in science, namely, quantitative oomph. It doesn't matter to us whether you do this with simple or complex accounting frameworks, or with econometric inquiries, or with mathematical or theoretical arguments, so long as the latter are always open to the engineer's (and historian's) question of how big. I was always a diagram person, too much so, and Bob Fogel was always an algebra person. We would spend whole lunchtimes at the Quad Club translating one language to the other. His was the sounder approach because he saw, as I did not until he slowly persuaded me of it, that if you are going to do a calculation, you can't confine yourself to qualitative theorems of the sort that Paul Samuelson recommended. You've got to have parameters, you've got to have slopes of lines, and you've got to know how big things are.

Would you say that this method was one of the big contributions of the early Cliometrics conferences?

Yes, and it continues to this day, the scientific stream in our poor, silly, unscientific field of economics. Then, of course, I would go home from the Clio meetings to the University of Chicago; and everybody there, from Milton Friedman to Bob Fogel, would be asking, "How big?" "How big?" "How big?" in every seminar.

How did your involvement in the early Clio meetings contribute to your work?

I have always been a joiner, a club person. I am always trying to find groups to which I can belong. I form intellectual communities if none already exist, like the Cliometric Society with Sam Williamson, for instance. At Chicago I did it in the workshop, and at Iowa I did it in the Project on Rhetoric of Inquiry. For a long time, Clio was the intellectual high point of my year.

Your first two papers to the Cliometrics Conferences dealt with the question, Did Victorian Britain Fail? How were they received?

Approvingly, as far as I could judge. The very first paper I did at Clio was on the British iron and steel industry, and my first important published article was "Did Victorian Britain Fail?" I showed myself to be a quantitative economic arguer about the past in the cliometric style. That's what they liked.

Concerning the paper – "Did Victorian Britain Fail?" – where does this issue stand today, and did cliometrics help resolve the issue?

I didn't push the issue hard enough. I did my dissertation on iron and steel, and I published it as a book. Then I did a few more papers, got bored with the question, and went off to study open fields and enclosures. What I should have done was write ten more papers and a couple more books and just kept on it eight or ten years longer. I would have succeeded in persuading more people, though I would have died of boredom. I still think I am right that it is very odd to think of Britain failing in the 19th century or even in the early 20th century. Britain is a fantastically successful capitalist experiment. But you need to be more than right to persuade people in science; you have to keep at it. The fanatics and the unimaginative often win in science. It's a rhetorical point. I have often thought it would be interesting to someday go back in my extreme old age and read all the work that has been done since (a lot of which I know I do not agree with and some which I do) and write a cool, summarizing book.

If you were to return to the subject now, on what specific areas would you focus?

If I was the age I was then and could do it all over, I guess I would take more seriously the qualitative sources (letters of business people, for example) and also try to explain, perhaps in an ideological or a sociology-and-rhetoric-of-science way, why some elements of British life became convinced they had failed. It was largely the immediate post-Victorians who first set out this strange claim that it was a failure. Keynes and others saw this when they looked back on their parents and grandparents in the Victorian Era. I think I could make a good case that it was part of the ideology of Fabianism to claim that this most successful of all capitalist countries was actually a failure even at the time of its alleged peak of success. It's an argument *a fortiori*. It was and is an attractive way of arguing for people on the left, such as Bill Lazonick. Yet, oddly, people on the right, like David Landes, also had their own culturally-based interests in making such an argument.

Setting aside the ideological motivations of the people who have asked the question, who would you say has contributed the most convincing empirical work?

Bill Lazonick. In my opinion, he has done the most interesting anti-capitalist work. I admire people who take on the big questions and really go to it quantitatively. Lazonick has persuaded a lot of people, because he did a lot of serious empirical work. He didn't always do it right, but I didn't always do it right either. All of us (Bill, Lars Sandberg, Knick Harley, Mike Edelstein, Steve Broadberry, Nick Crafts, and myself) knew that you couldn't make the argument on a blackboard. You had to have quantitative magnitudes, and you had to show that things were quantitatively important. That's a tremendous advance in economic history.

On this matter of advances in economic history, you are well known for changing the historical view of open fields.

I wonder. Again I got bored. I said what I still believe is pretty much the truth, got tenure for it at Chicago, and then didn't have the patience to keep saying it over and over again for the rest of my career! My stuff on English agricultural history, which was my preoccupation in the 1970s, can be broken into two simple parts: that enclosures were not an influence on 18th-century economic growth and that open fields can be explained as portfolio balance. I started people thinking seriously about what we now call an analytic narrative of open fields and enclosures. Likewise with Victorian economic failure and likewise with free trade and trade and growth in the 19th century. I think my contribution to

British economic history in general has been the invention of a genre of analytic narrative. That's important, and I'm proud of it. But my contribution to the resolution of actual historical questions has been much smaller. I invented the genre but then didn't push it far enough to persuade many people.

Why didn't you stay with it?

I guess the lack of patience to be boring. I think a lot of scholars have this problem. They think they see how it goes, and so they explain it to people, and the people say, "Well, I don't think so." The doubters haven't got a counter argument, but they don't see the point. I have made an exception to my usual impatience on this score in my attacks on the silly, silly idea of statistical significance and have been willing to become very boring about it, because it matters so much for the science. I can see so clearly the social and scientific gain if people would just grasp the very simple and decisive point I am making. Their whole scientific practice would change, and all sorts of substantive historical conclusions would have to be abandoned. It would be as though someone had noted something radically wrong in the theory of the telescope. It would be a matter of scientific priority to find out whether they were right or wrong, *ne c'est pas*?

Another of your articles – "Does the Past Have Useful Economics?" – is a classic and is required reading for most students in an economic history course. If you were to update that article today, what would you add?

The point of the original article was to uphold the dignity of economic history as a field of study. I was battling George Stigler and other barbarians at Chicago who wanted to eliminate the economic history requirement. In a fuller form 10 years later, the article became a short book I wrote for the Economic History Society called *Econometric History*. I would need to do a lot more now because the book came out in 1987, and we've continued to make good progress. I think of all the brilliant work done by Anderson and Libecap and company on the history of contracts and property, or the big pile of work on women's economic history, or the cliometrics outside the old national centers. I believe what I would now emphasize is that economic history has become virtually the only scientific field in economics. My claim, in other words, would not be as modest as it was in the 1976 article. There I was just saying, "Now look here, you economists, you should pay attention to the past because there's a lot of good factual stuff there that you could use." Instead, I'd be saying now, "Look, you dopes, do you want to learn how to do scientific economics or not? We in economic history are the only ones who do it now, so shut up and stop doing those chess problems you dote on and get back to real scientific work."

You would not be taking your own rhetorical advice if you put it that way!

[laughing] I am not good at coaxing! I do tend to preach, but the fact that I'm not always smooth and agreeable is not an excuse for not grasping what I'm saying, not in the Cliometrics conference at any rate.

In addition to the intellectual aspects, what would you say are your fondest memories of the early Cliometrics conferences?

Well, the people involved were so amusing. Jonathan Hughes was a character and a half, besides being a superb scientist. I remember when I first met him. I found myself sitting beside this older guy at dinner who launched off, without introductions, into an elaborate discussion of his own books. "My book on business cycles, my book on industrialization, my this and that." It dawned on me after five minutes, "Wait a second. I've not read any of his books closely, since they are not on subjects I have worked on, but this must be the notorious egotist Jonathan Hughes." Paul David was there droning on in his brilliant way. Doug North was being craggy and talking about going fishing with his neighbor Perry Como. Lance Davis with his Navy-issue white socks. Gary Walton in his cowboy hat.

You are known to use the terms "econometric historian" and "historical economists" for what many would call "cliometrician." Why is that?

I regard them as synonyms. I was forced by my editor of *Econometric History* to use the silly and misleading phrase "econometric historian," which comes from the subtitle of Fogel's book on railroads and from the Meyer and Conrad book I helped assemble as a grad student. I offer "historical economics" as a term that isn't as obscure, or as cute, as cliometrics, which was invented by the mathematical economist Stan Reiter. Historical economics includes people like Simon Kuznets. Kuznets wouldn't allow himself to be called a "cliometrician." I think he thought it implied some sort of mastery of higher math. There are a lot of economists who are interested in historical events that ought to be included in our fraternity. Bob Gordon at Northwestern is one of them; Tom Sargent is another.

You played a crucial role in introducing cliometrics to Britain. How has the influence of cliometrics held on there?

Not as strong as here, but it has held on and is prospering reasonably well. I organized the first British Cliometrics Conference; it was supposed to be the same thing that was happening at Purdue at the time. It took place in 1970 at Harvard, and then again at Cambridge, England three years later, and then a third time in Cambridge. Out of that came *The Economic History of Britain*.

How was British economic history done differently then? What is the biggest change?

In the early 1970s, it was done without benefit of economics. A lot of good scholars in the field no longer grasped economics even on the level of Thomas Ashton, John Clapham, or M. M. Postan in the 40s. There had been a deterioration in the level of understanding simple things like "relative, not absolute, prices govern allocation," or "MV=PT has to be true," or "trade does not create income," or "comparative advantage is not the same thing as absolute advantage," or "profit opportunities do not lie around unexploited for no good reason," or "national income equals national expenditure equals national production." This is Econ 101 stuff. Because the British had set up departments of economic history independent of both economics and history, they had allowed their standard of sophistication in economics to slip, especially after WWII. I'm not an admirer, as I think must be clear by now, of the chess problem sophistication that reigns in most departments of economics today. But the core ideas of economics are necessary to do very good work in understanding an economy. A business can be understood without requiring the scholar to know a thing about economics, but an industry (and especially a whole economy) cannot. That's changed in Britain since 1970. A lot of the younger scholars actually understand economics, although some of them, I note and lament, have fallen for the chess problems.

Economic history seems to have a higher standing in many European universities

than their American counterparts. Do you think that necessitates having a formally organized cliometrics community here?

I guess they are more favorably disposed towards economic history in Europe, though, in fact, I think a better characterization of what's going on is that certain professors there have a lot of power. Fernand Braudel ran economic history in France and Spain for decades. Things are more fluid in America, less centralized or institutionalized.

You have had long appointments in both history and economics departments at Chicago, Iowa, and now UIC. Does economic history fit better in one department than the other?

By training, of course, historical economists fit better into economics departments, but by avocation they fit better into history departments. The joke is the bumper sticker, "History is a thing of the past." Economists' eyes glaze over when someone mentions facts before last year. I think historical economists trained in economics departments, as soon as they get over being amazed that there are people in the world who don't really know what a demand curve is, find that the values of historians are more like their values as economic scientists than the values of other economists. If economists were scurrying around observing the world and if they were like historians being deeply interested in finding out what happened, instead of doing still another article in some chess-problem literature, there would be less of a gap. I remember the good old Chicago School people (like Ted Schultz, Margaret Reid, Milton Friedman, and Gregg Lewis), who would ask empirically, "How do you know?" It's the best and only scientific question.

Which of your works meets the criteria of being wholly motivated by the world, scientific in a way that makes you the most proud?

My work on open fields. This work exhibits the world intersecting with my Chicago School ideology. I am not claiming that I was dispassionate, and I am suspicious of anyone who claims to be. I was trying to prove that even in 1300 capitalistic behavior was characteristic. As you know, I often engage in these "even" arguments; "even though iron and steel around 1880 is supposed to be the worst case of entrepreneurial failure, it performed well" or "even though the English peasants around 1300 are supposed to be Monty Python-style idiots, they were in fact handling a terrifying world prudently." I like such *a fortiori* arguments, which doesn't make me popular because *a fortiori* rubs it in people's faces. What made that good work is that it was not at all driven by the literature, because the literature about open fields was merely aimlessly confused. It's not that there was an existing explanation or an existing interpretation that I knocked down, as there was in the case of Victorian failure. There was no explanation for why peasants held scattered plots in southern England in 1300 and around the world during other times. There was only intellectual chaos, and I said, "What is going on here?!" And I found out. I find that satisfying. It's what I recommend students do in their dissertations. Find a fact and explain it. I tell them they'll do superb work if they find the answer and very good work if they push the answer along a bit.

You have been an explorer of the boundaries of economics. What were the boundaries of economics in the early years of Clio? How have they changed?

The boundaries (in say 1970) were narrower, because Gary Becker and company hadn't spoken out yet. At the very beginning, we just assumed there were things called law, politics, sociology, and economics; and we thought they didn't have much to do with each other. One of the great achievements of the Chicago School in the 1970s was to say, "Hold it; law and politics and society are part of economics." Historical economists (such as Walt Rostow, Alex Gerschenkron, Simon Kuznets, Anna Schwartz, and Brinley Thomas) actually anticipated this, claiming that history was part of economics. These were the real grandparents of cliometrics, and every one of them was deeply involved in extending economics beyond the conventional boundaries. What I have gotten more clearly since then is that the project, both in Becker-type work and in our earlier work in economic history, was applying prudence only. The way, for example, Richard Posner approaches law is completely different from the way someone who takes the idea of rights and justice seriously approaches it. My opinion is that life in an economy also depends on virtues other than prudence, for example, justice or love. But as soon as we allow the other virtues in, then drawing the boundaries of economics by the applicability of the prudence only model doesn't look sensible.

So would you say that our expertise in maximum utility, prudence only models (our unwillingness to consider the other virtues) limits our field?

Yes. If you say that marriage is a matter of the husband's maximization, which is how Gary views it, the only full character in the story – if Max U is "full" – is the husband, and the wife is merely reacting. You are going to make mistakes in understanding families. You will have some insight but a lot of mistakes. We must deal with a full set of virtues and vices, full human beings. Even in Chicago School, strictly positivist terms, this approach works better.

Are there challenges young historical economists face nowadays that your generation missed?

When I was a Ph.D. student, most dissertations in economics were still empirical nationwide. Empirical work (finding a fact about the world and explaining it) was highly valued. Now, most dissertations in the top departments are theoretical. Finding a fact about the world and explaining it is the last thing that is on many economists' minds. This is a catastrophe for the science and bad for our little band of surviving scientists in historical economics. Actually, though, most grownup economists are interested in the world. Real science is interesting and sells.

What important economic questions do you suggest young economic historians study?

The biggest and most important one: Why modern economic growth? Why in England? Why in the 19th century instead of the 13th century? Why Europe and not China or India? This was the most important event in recorded history and ranks with the domestication of plants and animals. Hundreds of smaller questions can also be asked. Why did the classical world not invent steam power? Was the ability of women in Western Europe to work outside the home an important source of labor in the Industrial Revolution? Were Greece and Rome capitalist economies? Why did the bargaining power of middle class women decline in the 19th century and rise in the 20th century? Why, really, did the Great Depression happen? Has competition increased or decreased since the rise of national and international corporations? And even smaller ones, such as why bread and beer declined in quality in the United States? Was it the immigration restriction? Prohibition? It is endlessly interesting work.

Some of the questions you suggest are those

that the general economist would be less likely to study on methodological grounds. Do you see a difference in the willingness of cliometricians compared with other economists to take on such questions?

Yes. General economists (because they know nothing about the history, philosophy, and sociology of science, not to speak of how science operates in other fields) believe that economics must work by the standard scientific method. So we are supposed to be testing rational expectations or some crazy growth model. But the method that our leaders impose on us is a 1940 philosophy of science. It's nonsense. By contrast, when economists are asked to advise on policy, they get sensible, like we historical economists always are. Here's a little policy question in academic life itself that illustrates the point: How should we teach undergraduates economics? Should we prepare them for graduate school in economics? If the person asking the question is a nonquantitative person, he is at a loss. But anyone with quantitative common sense knows to ask in turn, "Well, how many of our graduates go to graduate school in economics?" The percentage, of course, is about 1%. The answer is clear: Prepare them to be law students, business students, economic actors in the world, lifetime worldly philosophers, but don't prepare them to do Hal Varian's textbook next year. Teach them economic history!

In your definition of what it is to do economic science, stories play an important role. How has our understanding of stories changed in the field?

I think historical economists have become more sophisticated about the role of quantification. In the early days at Purdue, and at Harvard for that matter, we felt ourselves in a bright dawn of numbers: "Bliss was it in that dawn to be alive, / But to be young [and numerate] was very heaven!" We thought that stories would tell themselves if we produced the numbers. I think we've gotten more sophisticated.

It sounds like you are saying cliometricians need to take more lessons from their colleagues in the history department.

Yes. Every historical economist should try to get a joint appointment in the history department. It is a terrible embarrassment for a mere economist to go into a history department, with all those people who know languages and read long books with no equations in them, but it's good for the soul to be humbled! The community of historical economics has more intellectual richness than economics by itself. But you can go further. I've cultivated friendships and read books in anthropology, history, and political science at Chicago; in English, classics, philosophy, and communications at Iowa; and in all of them, with an occasional mathematician or theologian thrown in, at UIC. Believe me, it makes you feel ignorant, which is an experience more economists ought to have. The best scholars know this. For example, if you are going to do sociological economics, you should take it as seriously as Gary Becker did. You can depend on it that Gary has read with care all the classics of sociology. He has long been a full member of the Sociology Department at Chicago, one of the best in the world.

Your work on the rhetoric of economics makes up a substantial part of your career. How did your early involvement with Clio influence your work on rhetorical argument in economics?

There is a very close connection between my experience in economic history and my interest in rhetoric. If I had been a conventional student of mathematical economics, I would never have figured out that there is more than one way to make an argument; whereas among the Cliometrics crowd, I was immersed in an argumentatively rich part of economics. I was debating with people where all kinds of other things were obviously involved: ideology, storytelling, various kinds of anecdotes, personal histories, arguments from symmetry, math, econometrics, accounting. The things that would come up in Gerschenkron's seminar or the Cliometrics conference were seldom the routine arguments in economics. This was because we'd be asking questions beyond those arguments, such as the historians' questions, as I called them, about the sources and the politics as well. Historical economics is more obviously rhetorical than, say, industrial organization. Anyone who thinks that's an attack on historical economics, though, or that industrial organization is not rhetorical, needs to buy my latest book, *Measurement and Meaning in Economics: The Essential Deirdre McCloskey*! It's in the Economists of the Twentieth Century series, a collection of "greatest hits," so to speak. It contains a 50-50 split of economic history articles and rhetoric of economics articles.

Do you have any concluding remarks?

Yes, thanks to the wonderful field of historical economics. I still love it. Historical economics has provided many a person with a social framework for doing real economic science. That's increasingly rare, and we should be very proud of it.

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The Economic and Business Historical Society Meeting

By Laurence J. Malone, Hartwick College

(Albany) The 26th Annual Meeting of the Economic and Business Historical Society was held in New York April 26-28, 2001. Laurence Malone (Hartwick College) arranged the program. Paper presenters came from throughout the United States and several other countries, including New Zealand, Great Britain, Norway, and the Russian Federation.

Baseball was the theme of the inaugural session, "As Close As Some Will Get: One Hour Away From the Baseball Hall of Fame." In "Labor Right and Franchise Performance in Baseball, 1969-2000: The Case of the Oakland Athletics," **Robert Jackson** (SUNY Oneonta) suggests the Athletics are successful despite being in a weak and divided market. In this case, management decision making determines franchise success both in terms of play and profitability. **William Simons** (SUNY Oneonta) provided a noneconomist's point of view with an entertaining biographical sketch of "Hank Greenberg and the Culture of Baseball." Simons is a well-known baseball historian and frequent presenter at baseball conferences.

The expansion of markets in the North American frontier was addressed in "North American Landscapes and Expanding Markets". **Michael Kennedy** (University of Michigan-Flint) presented "A Landscape Fit for Shipping: The Commercial Impetus Behind Expanding Frontiers in the Colonial Mid-Atlantic, 1683-1800," an account of how entrepreneurs encouraged migration through investments in enterprises for the extraction, processing, and refinement of natural resources in the mid-Atlantic region. Such investments moved progressively westward throughout the 18th century. **Rohit Aggarwala** (Columbia) considers New York City's emergence as a commercial center in the 20 years following the revolution in "The Urban Hinterland: Centralization and the Rise of New York Port, 1780-1825." Aggarwala contends New York's prominence is the most visible aspect of the transition of the nation's ports into a true urban system. In "The Interpretation of Montreal's Growth Over 1851-1901 and the Emergence of an Unusual Core-Periphery Pattern," **Benoit Mario Papillon** (University of Quebec, Trois-Rivieres) observes that the traditional view of Montreal's growth in the second half of the 19th century, which emphasizes trans-shipment, is not supported by census data. Papillon proposes a model of urbanization for Quebec province as a whole, where transportation endowments and access to cheap water transport lead.

In his paper, "Intended and Unintended Consequences in Motor Vehicles, Innovations, and External Shocks," **John Rossi** (Penn State Erie) explores the relationship between innovations and exogenous shocks. He traces rising accidents and deaths in his study of the development of the US motor vehicle-based system of transportation during the 1920s. In addition, he applies systems analysis to explain the difficulties of taking corrective action to solve the problem. **James Smith** (Widener University) also takes a systems-based approach in his paper, "Competing Through Innovation: Toward an Understanding of the Underlying Innovation Systems." Smith describes how high technology firms introduce product and process innovations to leapfrog competitors and create new markets. In "Oil Price Increases as Supply Shocks: A

Reconsideration of the Evidence," **Malcolm Russell** (Andrews University) revisits supply-side explanations of oil price shocks during the 1970s. He concludes that oil prices also reflected demand conditions owing to tight raw material markets, weather difficulties, and the Vietnam War.

Banks, ships, and insurance were the objects of discussion in "During and After Wartime: Inflation, Reconstruction, and Insurance." **Michael Haupert** (UW-La Crosse) presented "Problems in Analyzing Inflation During the Civil War," a joint work with **Paul Auerbach** (Kingston University). In a novel argument, the authors suggest the free market in gold and multiple monies constrained movements in the money multiplier and income velocity, dampening inflation during the Civil War. Multiple monies, however, cause a measurement problem in deriving the price level. The question of measuring inflation in a setting where prices are difficult to determine is considered when the money supply is dramatically increasing but hyperinflation is absent. **Olaf Mertelsmann** (Novosibirsk State University) addresses "The Reconstruction of the German Merchant Fleet and the Shipbuilder Blohm and Voss, 1918-1923" in work based on his dissertation. Mertelsmann describes Germany's loss of its merchant fleet after WWI and how Blohm and Voss achieved reconstruction in an era of devastating inflation by forming a cartel. **Peter Scott** (University of Portsmith) examines interwar insurance investment patterns and practices in the UK using both aggregate statistical data and evidence from companies. Contrary to existing studies of British firms, Scott discloses a major change in asset allocation in the period, from secure fixed-interest assets to securities.

The afternoon sessions on Thursday began with "Looking Backward and Forward in the Fourth Century of American Federal Spending." **Carl Lane** (Felician College) opened the session with his work on federal investments in private canal companies in the

EBHS participants at the Baseball Hall of Fame

early 19th century. He concludes that the investments failed because of macroeconomic conditions and the arrival of the railroad. **Jason Taylor** (Virginia) takes a look at sectoral government spending patterns in his paper, "World War Two Spending and the Southern Economic Rebound." He suggests that despite low per capita spending in the South, the impact of the spending may have benefited the South as much or more than other regions. **Ken Weiher** (University of Texas at San Antonio) takes his readers on a historical tour of federal debt in "The Decline of the Federal Debt: Life Without Hamilton's Blessing?" He asks the intriguing question: What if there was no federal debt?

In an intriguing biographical session entitled, "Three Entrepreneurial Publishers: Edward Bok, Lyman Cobb, and William Buck Dana," **W. David Lewis** (Auburn University) traces the roots of *Ladies Home Journal* in his biography of Edward Bok (the magazine's founder and editor). Bok, a Dutch immigrant, built *Ladies Home Journal* into the first women's magazine to achieve mass circulation. **Charles Monaghan**

(Urban History Press) documents the misfortunes of an early publishing entrepreneur in "Lyman Cobb (1800-1864): Textbook Writer as Entrepreneur." Cobb, a country schoolmaster, authored *Cobb's Spelling Book*, one of the first spelling primers widely used in early 19th-century America. When his publisher went bankrupt, Cobb took matters into his own hands but failed in the emerging textbook market. The final paper featured an account of how William Buck Dana launched the *Commercial and Financial Chronicle*. "Peerless Spokesman: William Buck Dana's *Chronicle*" was written by **Douglas Steeples** (Mercer University) but read by David Whitten (Auburn University) because of the author's recent accident. Steeples is continuing his work on a full biography of Dana.

On Thursday afternoon, EBHS members attended a program hosted by Stefan Bielinski at the New York State Museum. Bielinski coordinates volunteers and students working on the *Colonial Albany Social History Project*, a biographical database of the 16,000 people who lived in Albany before 1800. Because of its inclusiveness, depth, and web-based archive, the project sets a new standard for understanding life in early American communities. EBHS members were impressed by the scale and scope of the project, which can be explored at www.nysm.nysed.gov/albany/.

On Friday, the morning sessions began with "Changing Economic Patterns Among the Amish and Native Americans." **Silvano Wueschner** (William Penn University) describes how the Amish of southern Iowa have broadened their economic activities in order to maintain their community. He offers a dim view of Amish efforts to sustain cultural homogeneity in contemporary American society. **Tom Winpenny** (Elizabethtown College) similarly considers the entrepreneurial practices of the Lancaster County, Pennsylvania Amish in "Recommitting vs. Selling Out: The Changing Economic Role of the Plain People of Lancaster County, Pennsylvania." While Amish bishops fret that commercialization erodes traditional values, non-Amish local business owners worry that the new Amish enterprises diminish the popular conception of Amish life that drives the tourist trade. In "Choctaw Industries Expand Abroad," **Michael Namorato** (Mississippi) extends his work on the Choctaw to explore how exporting has been added to the wide range of economic activities in the tribe.

"The Continuous Evolution of Valuation and Exchange," a session on accounting and finance, began with **Jerome DeRidder** and **H. Reed Muller** (both Salisbury State University) presenting the "Historical Evolution of Income Evaluation Concepts in Accounting." Their work surveys income evaluation methods from the English industrial revolution to the present. In research based on his dissertation, **Michael McAvoy** (SUNY Oneonta) explores the uneasy relationship between the Fed and the Comptroller of the Currency in the two decades after the founding of the Federal Reserve System. Through the First Federal Bank of Bloomington, Illinois, McAvoy examines changes in banking practices and the federal regulatory environment in the decade before the Depression.

During a session on the early days of Hollywood, **Ranjit Dighe** (SUNY Oswego) reconsiders how teachers of economic history relate 1890s monetary populism to L. Frank Baum's *The Wonderful Wizard of Oz*. Although the Oz-populism story is a hit with students, the question of whether Baum intended the metaphor remains. Dighe is expanding upon the topic in a forthcoming book. **Karen Ward Mahar** (Siena College) looks at the early history of the motion picture industry in "'I Really Want to Direct...': The Rise of Hollywood and the First Wave of Movie Star/Producers." Mahar discusses how the promise of Hollywood for female stars and independent female producer entrepreneurs in the 1910s evaporated in the 1920s and did not reappear until later in the 20th century.

Labor market issues and policies were discussed in "Improving the Prospects for Labor: Better Education, Minimum Wages, and Guaranteed Employment." **David Aske** (Colorado) considers background developments that led to the Smith-Hughes Act in "The Smith-Hughes Act of 1917: Progressive Education, Business, and Organized Labor." The session continued with "Monopsony and Minimum Wages: Evidence From the Tobacco Leaf-Processing Industry," where **Andrew Seltzer** (Royal Holloway College) looks at the effects of the Fair Labor Standards Act of 1938 on the tobacco leaf-processing industry. **Jae-Won Sun** (Harvard) sketches the evolution of the postwar Japanese employment system in "Adjustments Prior to Long-term Stable Employment: Preconditions of the Japanese Employment System, 1945-49."

Quirky episodes in economic and business history were intermingled in "Do Things Add Up and Subtract Down? Three Lessons in Economic and Business History." **Antonio Calabria** (University of Texas at San Antonio) led off with "The South Pays for the North: War Finance in the Kingdom of Naples During the Thirty Years' War (1618-1648)." Calabria examines the fiscal offensive unleashed by the Spanish Monarchy on the Kingdom of Naples during the Thirty Years' War. Though the war was fought far from Naples' borders, the monarchy imposed a whole range of new taxes to aid the war effort and protect Milan. With "When Two Plus Two Did Not Equal Five," **Theodore Kovaleff** (Dirks and Company) looks at past divestiture actions, taking on the conventional wisdom that holders of divested securities subsequently benefit in equity markets. Standard Oil and AT&T are considered: the Standard equity parts outperforming the market and the AT&T parts lagging. Kovaleff observes that Microsoft could follow the path of AT&T, with ramifications reaching beyond Wall Street. In "Russian Robber Barons: Moscow Business, American Style," **David Whitten** (Auburn) contrasts illicit contemporary business practices in Russia with similar episodes of lawlessness in the history of American business. As is the case with much of Whitten's work, the subtleties of his explorations are both engaging and entertaining.

Three papers on banking policy were joined under the guise of two rhetorical questions in the session titled "Where Do Banks Come From? What Do Banks Do?" **Jane Ellen Knodell** (Vermont) asks whether the core motivation of the Second Bank (1816-1836) was for profit or the public interest in "Profit and Duty in the Exchange Operations of the Second Bank of the United States." Knodell finds that the Bank priced exchange as a regulated natural monopolist. It earned high profits by realizing returns to scale and scope and creating synergies between its for-profit exchange operations and government fiscal operations. The Second Bank shared cost savings with its customers, because the political conditions for securing renewal of its charter required it to do so. **Rohit Daniel Wadhwani** (Pennsylvania) looks at the evolution of regulations to protect savings banks depositors in "Under the Wholesome Inspection and Control of Government: The Emergence of Savings Bank Regulation and Depositor Protections in the Century Before

FDIC." **Priscilla Roberts** (University of Hong Kong) profiles Frank Vanderlip, president of the National City Bank during World War I. Vanderlip, an advocate of American trade and investment, launched a major initiative to expand National City's overseas activities during the war. Although Vanderlip sought cooperation with Britain, the policy led to conflict with the pro-allied Morgan firm.

"Agents of Industrial Change: The Entrepreneur, the Sales Force, and Barbers and Hairdressers," featured some uncommon episodes in the evolution of industries. **Jeffrey Matthews** (University of Puget Sound) presented "Yankee Enterprise! The Early Rise & Fall of Corning Incorporated, 1851-1871." Matthews chronicles Corning's founder, Amory Houghton, in a biography that moves from farm boy to trade apprentice, skilled craftsman, wharf merchant, corporate investor, and, finally, industrial capitalist. In Houghton's case, bankruptcy preceded success. **Roberto Mazzoleni** (Hofstra University) offers a new look into the importance of sales agents for marketing and distribution in "The Role of Sales Agents in the Diffusion of U.S. Machine Tool Technology in Europe." **Jane Plitt** (Rochester) considers the entry of women into the beauty industry. She provides a detailed account of how the male barber's industry tried to exclude female hairdressers from cutting women's hair and how those efforts led to their demise in women's haircutting.

"The Three S's of Business: Strategy, Structure, and Survival" featured two papers on the airline industry and one on an upscale department store chain. **Juha-Antti Lamberg** (Alabama) opened the session with "ESOP Negotiations in United Airlines, 1985-2000: The Case for Re-Thinking Strategic Shareholder Management," and **Dan Johnson** (Emory Riddle Aeronautical University) followed with "Trans World Airline – Still Searching for a Place in the Crowded Skies." The papers document changing competitive conditions in the industry. In "The Nordstrom Way – Will It Survive?" **Mark Gardner** (Piedmont College) contrasts previous success with the current competitive difficulties faced by Nordstrom.

Harry Kitsikopoulos (New York University) presented "Standards of Living and Capital Formation Among Peasant Holdings in Post-Plague England" in a session that examined "Intriguing Views on the Economic Past." **Harvey Hudspeth** (Mississippi Valley State University) continued with "The Rise and Fall of the Greene Doctrine: The Sherman Act, Howell Jackson, and the Interpretation of 'Interstate Commerce,' 1890-1941." He deals with the evolution of the judicial interpretation of interstate commerce, beginning with the enactment of the Sherman Anti-Trust Act of 1890 and concluding with the Supreme Court's *Darby v. US* decision 51 years later. The centerpiece of Hudspeth's account is an examination of the so-called "Greene Doctrine" and its author, Supreme Court Justice Howell Edmunds Jackson.

Public policies and government intervention in international settings were considered in "Beyond Multipliers: Global Studies on the Effects of Government Policy." **James Stitt** (High Point University) provided another look into the body of his work on the British economy during WWI. He explores how wartime demands for military supplies dramatically changed union-management relations in "How Wartime Governmental Regulations Changed British Industrial Relations, 1914-1916." **Astrid Baker** (Massey University) followed with her study of state-supported business in "A Business and Government Partnership in Post-War New Zealand: Fletchers, Forests, Building and Paper." Baker shows how a single New Zealand company, Fletcher Construction, designed and constructed roads, wharves, sawmills, flourmills, factories, railway stations, university buildings, hospitals, department stores, office blocks, and houses throughout the country and became a potent force in New Zealand's welfare state. The final paper, "Protection Against Financial Crisis and Collapse? The Role of Regulation and Supervision. The Norwegian Case 1914-35," by **Sverre Knutsen** and **Gunhild Ecklund** (both Norwegian School of Management) discusses interwar banking crises in Norway using a theoretical framework based on the financial fragility approach. Knutsen and Ecklund investigate the roots of the banking crises in the economic boom during WW I and suggest that regulatory supervision mattered in limiting financial instability during the early 1930s.

The conference concluded with "Lessons in Commerce: The Kabuli Walla and Capital Flight." Fresh from a meeting of the Social Security Reform Advisory Commission, **Karl Borden** (Nebraska) presented "Stalking the Kabuli Walla," which details an encounter with one of the last itinerant Afghan traders to ply wares in western Bangladesh and northeast India. Most of the Kabuli Walla (literally "Important People from Kabul") remaining during the Bangladeshi War of 1971 either fled or were slaughtered. Borden had arranged a personal interview with Abdul Hazim Khan, who claimed to be 114 years old. **Magdalena Rappl** (Tennessee Tech University) presented preliminary work on a new theoretical model in "Capital Flight Due to Tax Differentials: Some Analytical Thoughts."

On Saturday at the conclusion of the meeting, EBHS members embarked on a pilgrimmage to the National Baseball Hall of Fame and Museum in nearby Cooperstown, New York. It was the trip of a lifetime for those attending, as we enjoyed the ambiance of the quaint village of Cooperstown, James Fenimore Cooper's *Glimmerglass,* and the nostalgia of youth, family memories, and baseball.

Malcolm Russell (Andrews University) is Program Chair for the 27th Annual Meeting of the EBHS in Chicago April 25-27, 2002. Details can be found at the Economic and Business Historical Society website: www. ebhsoc.org.

Telephones and British Economic Growth: An Unwritten Essay in Econometric History

Clio Conference *(Continued from page 1)*

expressed desires to see more rationale behind the breakdown of the sample into time periods for econometric analysis. Meissner suggested more focus on sensitivity analysis and pointed out the potential endogeneity of labor chasing capital and the role of natural resources. Dwyer was curious whether the differing results by time period could reflect connections between risk and property rights and membership in the British Empire. Goedde and Fishback asked for a test for timing issues and structural change. Deirdre McCloskey (Illinois-Chicago) wanted more intuition behind the econometric undertakings. The session ended with Jeff Williamson musing about the importance of audience, a theme that was to be revisited in several sessions throughout the weekend.

Grantham chaired the second session, in which **Jac Heckelman** and **John Wood** (both Wake Forest) presented their paper, "Political Monetary Cycles: The Independent Treasury versus the Federal Reserve." Heckelman opened the discussion by saying that he wrote the paper hoping to show that the creation of the Fed made monetary policy less political but found the data uncooperative. Some participants expressed a lack of surprise at the results of the paper: Meissner, because one purpose of the gold standard was to limit the possibility of discretionary policy, and Wallis, because the actions of the Treasury and the Fed were mainly driven by fiscal necessities. Gillian Hamilton (University of Toronto) asked for more motivation – Why should we expect to see political cycles? Santhi Hejeebu (Iowa) requested more direct evidence on whether the presidential candidate was actually interested in monetary policy and its effects on the election; Wallis cited Bryan's "cross of gold" speech as an example. In contrast to the lack of surprise shown by others, Jeff Williamson found the absence of any mention of Europe unusual and noted that US monetary cycles might be correlated with European cycles.

Other participants focused on how the institutions examined in the paper conducted monetary policy. Werner Troesken (Pittsburgh) stated that monetary officials of the period did not really understand the quantity theory of money. Shawn Kantor (Arizona) sought more details on the Fed's objective function and on the relationship between fiscal and monetary policy. Fishback and Alexander Field (Santa Clara) both wanted a more detailed description of how the Treasury was able to conduct monetary policy.

McCloskey and Jayne Toman (Yale and Stanford) focused on timing. McCloskey argued that the ending date should be moved to the first quarter of 1933, when the US went off the gold standard, and that the 1879 change, being a matter of law, was fully anticipated. Heckelman replied that using 1875 instead of 1879 does not alter the results. Toman suggested that during the 1914-1933 period the noise was so great it would drown out the signal and that a later period would give a better picture of how the Fed acted.

Haupert chaired the third session, where **Sharon Ann Murphy** (Virginia) reported on 19^{th}-century rural wealth accumulation in the United States. Her paper, "Nineteenth-Century Rural Wealth Accumulation: A Microeconomic Analysis," is part of a larger project examining various types of wealth separately. Her evidence is contrary to the life cycle hypothesis of savings.

A lively discussion focused on the use of land and improvements as measures of

Attending a Cliometrics Conference for the first time (l. to r.)
Back: Sam Allen, Michael Clemens, Jonathan Conning, Isabel Goedde, Jac Heckelman, Brooks Kaiser, Aloysius Siow
Front: John Wood, Jayne Toman, Jakob Madsen, Marianne Ward, Santhi Hejeebu, Sharon Ann Murphy

capital or savings, as well as reasons why the life cycle hypothesis might not be evident here. Fishback, Wallis, Sokoloff, Clark, and Marigee Bacolod (Arizona) all expressed concern about the relationship between wealth accumulation, capital accumulation, and savings. Fishback proposed moving away from promoting a tenuous relationship between rural wealth and savings in order to say more with the great data collected. With the Civil War acting to reduce rural wealth in the middle of the sample period and different savings paths and forms of wealth in the North and the South, Wallis, Fishback, Kauffman, and Goedde stressed the importance of getting more information about personal wealth. Troesken thought that purging the data of capital gains value reduced its interpretability as a measure of savings, and Jeff Williamson agreed, adding that it would be valuable to test differences in responses to capital gains accumulation and other forms of accumulation. Clemens wanted clarification of the wealth-income ratio, and Lee Craig (North Carolina State) felt direct estimation of the ratio would be a valuable exercise.

The possibility of hidden transactions drove the debate about the data's lack of support for a life cycle hypothesis. John Murray (University of Toledo) considered the bequest motive as a possible alternative, and Sam Williamson (Miami University) extended the idea to include implicit contracts with children over land as a mechanism for invisible dissaving. Joyce Burnette (Wabash College) wondered about the definition of a household in the sample, while Grantham and Barbara Sands (Arizona) thought that more information on mortgages should be included. Grantham made a case for using probate data to get at this problem. Hamilton recommended using a different functional form to describe the relationship between age and wealth. Kantor felt tax rolls were another valuable data source. Lee asserted precautionary savings would be much higher in this time period and might mask any life cycle dissaving. Clark and Weiss maintained the disparity between the local and national figures required more attention, and Clark thought that perhaps a restructuring of the general framework of the paper could address this.

Friday evening, vans driven by University of Arizona graduate students Samuel Allen and Ryan Johnson conveyed us safely and merrily into the desert hills. There, participants were treated to a dinner of southwestern cuisine at the home of Gary and Ann Libecap, sponsored by the Eller Center at the University of Arizona.

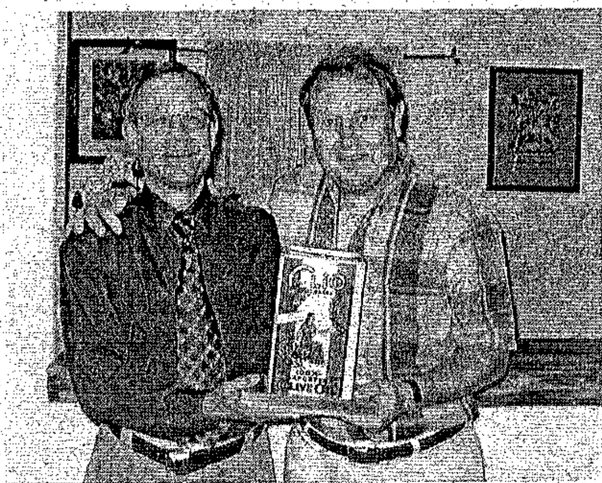
Saturday began with **Ken Sokoloff** (UCLA) presenting "The Evolution of Suffrage in the New World: A Preliminary Examination." His coauthor, **Stanley Engerman**, was not in attendance. Joan Hannon (St. Mary's College of California) chaired the session. In his introductory remarks, Sokoloff focused on the puzzle of why colonies with similar factor endowments had such divergent economic histories. He contends that the answer lies in the differing amounts of inequality in the colonies.

Some participants asked for clarification of the data in the paper. Heckelman questioned the categorization of US suffrage laws, noting that the US was not as liberal as it appears in Table 3. Connecticut had a literacy requirement, many states had a poll tax, and the secret ballot did not begin until the 1880s. He also concluded that the percentage of the population voting was inflated by vote fraud. Sokoloff replied that the "Lack of Secrecy in Balloting" column indicated whether ballots were written or oral and that as early as 1830 states had laws specifying the color and size of the ballot to be used, thus indicating written ballots were used. Lou Cain (Loyola-Chicago and Northwestern) asked how literacy was defined and whether the definition was consistent across countries. Sokoloff responded that, unfortunately, they had to rely on published rates and did not always know the definition used. Jeff Williamson asked for multivariate analysis, to which Sokoloff answered that they had done such an analysis but had not yet included age distribution in those regressions. Sands speculated that dropping the property requirement might have simply resulted in a redistribution of income if the wealthy could buy the votes of those newly enfranchised. Sokoloff rejected this hypothesis, because changes in suffrage laws were followed by changes in public policies.

Other participants pushed for more details on the political economy of suffrage changes in the US. Gary Libecap (Arizona) asked why property requirements were dropped at the frontier, where they were less likely to be binding. He wondered who the opponents of extended suffrage were and whether the concern that some people would monopolize land holdings could have lent support to the opponents of property requirements. Sokoloff said that frontier states did not have disputes about suffrage requirements but did express concern about how to attract migrants. In the original colonies, by contrast, the question of property requirements for suffrage was controversial. He noted that opponents of property requirements tended to emphasize practical problems, such as the inequalities created by drawing a line on a continuous property distribution and the problems of correctly valuing assets.

Moving to comparisons across New World

countries, McCloskey was curious whether federalism contributed to liberalization by creating competition across states. Grantham commented that the Catholic Church might have resisted the extension of suffrage as a potential threat to its own power. Jonathan Conning (Williams College) observed that



George Grantham presents The Can to Lou Cain

while the paper explains why suffrage was extended in the US, it does not provide explanations for why it was extended in other countries. Other participants encouraged Sokoloff to extend his work. Hamilton inquired if there was any plan to examine the extension of suffrage to women and blacks. Jeff Williamson recommended moving the comparison beyond just the Americas and comparing the experiences of other countries in the world as well. To which Sokoloff replied, "A man's got to know his limitations."

Gillian Hamilton and **Aloysius Siow** (University of Toronto) followed with "Class, Gender and Marriage" in a session chaired by Dwyer. The two are working to develop a model that captures the majority of issues present in marriage markets, including assortative matching and market participant concerns about age and fecundity. Discussion centered on requests for greater details about the existing institutional structure and the success of the model at interpreting the data for New France, as well as the transferability of results to other situations.

Many questions arose about the institutional framework of New France. Jeff Williamson and Fishback wondered why New France? How did it compare to other areas? Was it representative or unique and why? Kauffman asked why the female to male ratio was so high, and Cain wondered what alternatives there were to divorce. A host of questions were raised regarding inheritance laws, the incentives for marriage and children, the rates of premarital pregnancy, and the role that social circles and love play in assortative matching.

Goedde and Conning remarked that the results are used to argue that fecundity signals were important, but they do not seem to be important for men and therefore seem questionable. They added that strengthening the institutional story would help the authors support their interpretation of the signal sent by a lack of children in a first marriage. This signal also led Bacolod to ask whether it might be modeled as a search good for second marriages and Murray to suggest that this signal should account directly for age and the length of the first marriage. Meissner and Minns thought that the probability of dying and menopause should be modeled dynamically. Fishback questioned the assumption that high-status people are equally likely to meet high- and low-status partners. According to Haupert, the market should be broken down by birthplace to better understand matching, and McCloskey added that since the sample is really a full population there should be no standard errors. Troesken praised the work for its attempt to build a structural model for historical data, and again, the question of audience was asked by many.

McCloskey chaired the next session, where **Jakob Madsen** (Brunel University) examined "Wage Rigidity, Price Rigidity and Supply during the Great Depression." Madsen opened by stating that he wanted to challenge the emerging consensus on the Great Depression, which emphasized the role of wage rigidity in prolonging the downturn. He asserts that price (not wage) rigidity was the culprit, because real wages were sensitive to unemployment and if corrected for selection bias would probably show a decline during the Depression.

Most of the questions focused on data and estimation techniques. Marianne Ward (Loyola-Maryland) inquired about the sources of the value-added deflator. Heckelman wanted to know why France was an outlier, to which Madsen responded that France really was exceptional because it clung to the gold standard. Fishback speculated as to whether the value-added deflator really is the "correct" deflator. McCloskey noted that the CPI is the correct deflator for labor supply, and value-added prices are the correct deflator for labor demand. Madsen clarified that he was assuming a zero-elasticity supply curve for labor. Lee asked whether the data could distinguish between strikes over wages and strikes over hours.

Wallis expressed skepticism about whether a cross-sectional regression would ever be able to answer the question posed in the paper, and Dwyer wondered where the function estimated in the paper came from. Clark suggested that, since value-added prices are mostly composed of wages, little is learned from the fact that real wages deflated using the value-added wage rise less than real wages deflated with wholesale prices. Troesken asked whether the results were robust to re-specification, and Madsen agreed that more sensitivity analysis was needed.

Field commented on whether the consensus attacked was a straw man, since it is clear that wages in the US declined between 1929 and 1933. Other discussants wanted more theory. Wallis encouraged Madsen to include more discussion of what he thinks is the correct explanation for the long duration of the Great Depression. Madsen replied that ruling out sticky wages and misperception of real wages leaves price stickiness as the remaining possible explanation. Kaiser asked for the micro story behind the rising-markups story. Troesken argued for more information on institutions: Why were prices sticky, if neither the NIRA nor the retreat from antitrust laws seems to have been very effective? Field wanted Madsen to look at other sectors besides just manufacturing.

The afternoon session, chaired by Burnette, began with "Mid-Century Multifactor Productivity in Relation to Current Trends" by **Alexander Field**. Field assured us that he was no longer a dean but instead was working on viewing the 20th century as a whole. In his work, he aims to separate which macroeconomic growth is attributable to productivity changes in the 1930s and which to WWII and beyond. He contends that the second quarter of the century (in particular the 1930s) was actually the period of highest innovation and multifactor productivity growth for the century.

Libecap wondered what the private sector motivation was for research and development in the 1930s, and Clemens questioned where the new scientists were coming from. Haupert suggested a role for increased competitiveness among the firms able to weather the severe economic conditions. Several comments focused on whether or not technological advances in individual sectors could be representative of a larger productivity growth and whether lags from the introduction of electricity and other innovations or federal inputs to infrastructure and development might be driving the data.

The discussion then turned to questions of comparison. McCloskey speculated whether Germany might offer valuable insights. Craig spoke on what would be depressing current levels of growth. Murray considered whether the postulated changes in urban design were visible in the 1930s, but Field thought the low rates of construction would not allow for this visible change. Madsen was curious how these results could be compatible with the share market data.

Next **Chris Meissner** presented "Exchange-Rate Regimes and International Trade: Evidence from the Classical Gold Standard Era," written with **J. Ernesto Lopez-Cordova** (Inter-American Development Bank), who was not in attendance. Kauffman chaired. Meissner began by boasting that the gravity model is good at predicting levels of trade. Some questions centered on interpretation. Wood started the discussion by noting that the results of this paper seem to contradict the results of the first paper, which claimed that the gold standard did not affect investment flows. Sokoloff wanted to know what anomalies in trade the model could explain.

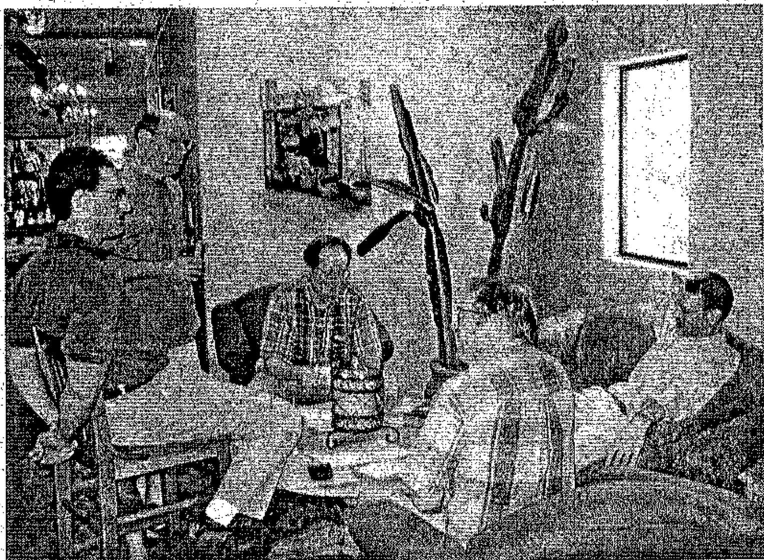
Most of the questions, however, focused on the details of the data and estimation. Heckelman asked why GDP and GDP per capita were both included in the regression. Minns inquired about instruments, to which Meissner replied that he used land area and urbanization rates as instruments to address sample selection biases. McCloskey expressed disdain for gravity models, and Goedde speculated that the fact that the coefficient on the gold standard dummy varies so much from one year to the next might indicate the regression has not sufficiently controlled for the business cycle.

In response to Clark's question about the endogeneity of trade institutions, Meissner examined the two methods he and Lopez-Cordova used to deal with the problem. The first method was instrumental variables: using gold reserves as an instrument for the gold standard and common language as an instrument for monetary union. A Hausman test suggested that endogeneity was not a problem. The second method used fixed effects; the gold standard is still significant, but monetary union has no effect. Siow noted that sometimes countries institute a new monetary regime to control domestic instability and that the relationship between monetary regime and trade may be contaminated with other effects. Meissner replied that when he added dummies for Chile and Brazil to control for domestic stability, he found that these countries actually traded more.

Jeff Williamson claimed the paper was good, but inaccurate. His comments focused on the data used. According to him, the distance variable should measure actual trade routes rather than great circle distances, and transportation costs should be allowed to vary by country pair. McCloskey wanted to know whether distance was measured from the population center of each country.

Sands chaired the final paper of the afternoon, "Government Insurance, Worker Health, and Labor Supply in Turn-of-the-Century Europe" by **John Murray**. Murray uses data from government-sponsored health insurance in Europe in the late 19th century to examine the conflicting economic implications for time at work provided by sick pay and medical benefits. He asserts that the general decline in European mortality rates accompanied by an increase in morbidity rates deserves an economic accounting. Is this a cultural inflation of morbidity or a medical phenomenon whereby lives were lengthened through cured sicknesses of greater duration?

Discussion began with a call by Craig for some process to separate out biological factors from economic factors, since currently absenteeism alone cannot identify the problem. He also worried about the compositional effects of changing mortality rates. Heckelman wondered if there was serial correlation that could be modeled, and Kaiser and Jeff Williamson proposed dose-response functions adapted from the EPA or developing nations to purge the biological aspects of sickness.



An informal gathering of Clioms

The incentive structure of the health care system brought on several concerns. Minns considered whether physicians were acting as gatekeepers to sick pay. Fishback thought sick pay per sick day was probably influenced by institutional arrangements regarding the size and distribution of payments and that learning over time might increase moral hazard. Goedde worried about stationarity and suggested using first differences. This generated a lengthy discussion about the best way to model and interpret interaction over time in the data set. Kaiser and Sam Williamson addressed whether workers earned reputations that affected long-term employment and promotion opportunities that affected their decisions.

Hamilton speculated on what an optimal health plan would look like and was concerned that GDP per capita and year seemed to exert such a strong influence on the regressions. Clark picked up on this point and wondered whether the positive GDP influence on sick days taken wasn't an income effect that allowed workers to adjust their hours worked more optimally. Cain and Murphy talked about comparisons to other data in the US, Canada, and the UK, but Fishback argued these private funds might not add as much to the story as to complicate it.

Participants then left for a brief swim and/or informal chat and returned for a salmon dinner, sponsored by Mr. and Mrs. Sam Hunter and Fred and Ann Boice. There was much merriment, partially thanks to Thomas the Wine Guy. Participants were saddened to hear of the retirement of the Mullah, but hopes of future merriment revived when John Murray assumed the responsibilities of awarding the prize for "wisest" saying. George Grantham awarded the Clio can to Lou Cain.

Sunday began with **Werner Troesken's** "The Limits of Jim Crow: Race and the Provision of Water and Sewerage Services in American Cities, 1880-1925," chaired by Weiss. Troesken summarized his paper by answering two questions. Did cities under provide water and sewers to African-Americans? He claims yes, but not as much as you might think. And why? Troesken says because they feared the spread of disease and because residential integration made provision of services to blacks cheap. Some of the comments centered on identifying the causes of under-provision to blacks. Clark and Jeff Williamson were concerned about the extent to which differences between blacks and whites in the provision of services resulted from income differences and whether there was a pure race discrimination effect. Troesken noted that he was careful to label his current findings "under-provision" rather than "discrimination." He also said that he does plan to use census data to control for the effect of individual variables, such as occupation, and to look for pure race effects. Fishback cautioned that we still don't really know what the labor market discrimination equations mean and that trying to identify discrimination in provision is less interesting than finding out the details of how water was provided.

The biology of disease contagion was an important theme. Cain maintained that immigrants and blacks might have similar death rates in northern cities, such as Pittsburgh, because they were both new to the local disease environment and had not developed immunities. Jeff Williamson wanted more information about the diseases, both what we know today (which would help us understand how the diseases spread) and what people thought at the time (which would help us explain their behavior). As a way to investigate contagion, Toman asked for comparisons of death rates for different neighborhoods within a city.

Many participants pushed for expanding the paper in various ways. Jeff Williamson sought more comparisons with European cities, and Sam Williamson wanted more comparisons to cities in the North. Jeff Williamson also requested comparisons to the provision of another important public service – education. Others pushed for more information on the political economy of provision decisions. Murray asked whether cities were responding to any political pressure from blacks, and Wallis wanted to know whether differences in property taxes would support the hypothesis that those who received the services paid for them. Some participants questioned the consequences of service provision. Hamilton asked if the racial composition of cities changed after the amenities were put in place, and Sokoloff inquired about what happened to property values and population growth.

Some questions focused on specific facts, and the author responded to these questions by providing some interesting details. Kaiser asked whether the mixed-race houses on the maps were white families with black servants. Troesken replied that mixed-race houses were either apartment buildings with families of both races or white families with black servants; he guessed that blacks were servants in about half of the mixed-race houses. Sokoloff was curious how much it cost to provide water and sewer systems. Troesken noted that water filtration was relatively cheap but that the costs of water and sewer mains were "huge." As a comparison, he observed that the value of gas mains owned by private gas companies in Chicago accounted for 10% of all the private physical capital in the city. Sam Williamson asked if plumbing was available at a reasonable price, to which Troesken responded that a basic flush toilet would have cost less than the $50 fine Memphis assessed for non-use. Troesken got fewer questions on estimation and theory than other papers, but these issues were not completely ignored. Heckelman wondered whether it was possible to determine which specification of the typhoid rate regressions was the best specification. Field wanted a model that describes the motivations of whites for implementing Jim Crow laws and examined both the costs and benefits of these laws.

Cain chaired the penultimate presentation, "Intra-Household Transfers and Old-Age Security in America, 1890-1950" by **Chulhee Lee**. Lee uses data from household surveys on income and expenditure from 1890 to 1950 to revisit the question of whether industrialization led to a lower quality of living for the elderly in America. Couched in the debate about what influences led to the creation of public welfare and social insurance programs, Lee argues that industrialization did indeed lower the standard of living for the elderly as the expenditure patterns of retirees do not clearly reflect an intra-household transfer of income.

The discussion developed along somewhat diverging lines. First, Kantor contended that placing the paper in the context of the adoption of social insurance laws does not fit the theory of political economy, because there is no motivation to pass such laws in this story. Fishback, Wallis, and Wood supported this contention and spoke on reframing the tale to address the original question regarding changes in the welfare of the elderly over time.

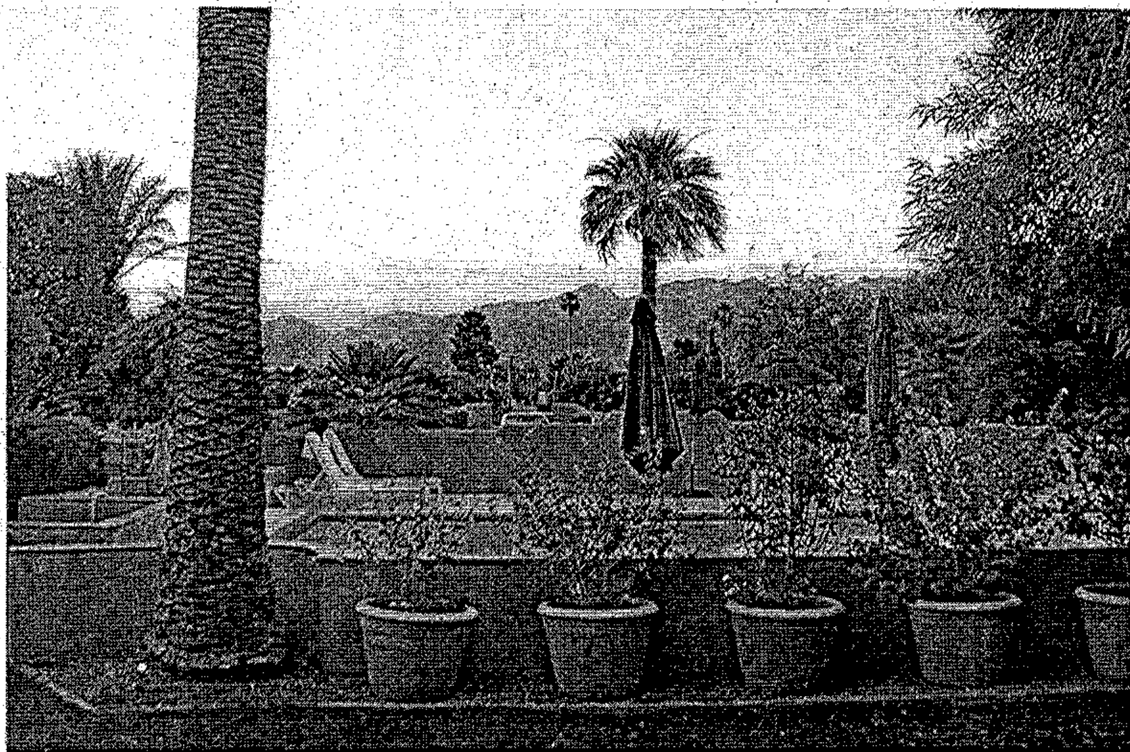
Second, many participants were concerned that the role of children and intra-household transfers was sufficiently obfuscated as to put the results in doubt. Burnette and Sam Williamson said that more details were needed about the data set itself and the role of children at the household level, while Wood and Hannon wanted to know more about household consumption of nonmarket goods. Siow suggested defining a benchmark family strategy to help intuit the story more clearly. Finally, several participants voiced concerns over interpretability of the results and came up with ways to ease the reader into the data set and equation with greater intuition.

The conference ended with **Gregory Clark's** "The Secret History of the Industrial Revolution," chaired by Weiss. Clark began his comments by noting that he was not an "Industrial Revolution denier"; he does believe that there was a fundamental transformation from a world of practically no growth to a world of rapid and sustained growth. On the other hand, he thinks this fundamental transformation has been misdated; the correct date should be either sometime around 1200 or 1870-1900.

After Grantham claimed to have written the paper four years ago, Clark stated that the comments on his paper fell into two categories: those claiming the paper was wrong and those claiming to have written it already. The first category was the larger of the two. Sokoloff remarked that since the productivity data is bad, other types of evidence are needed. He cited three kinds of evidence not consistent with Clark's story: improvements in heights, Bob Allen's data on agricultural growth, and the fact that population shifts away from agriculture. Clark responded that he was skeptical of the height data and that there has been a publication bias in favor of studies that show growth when we expect to find it.

Jeff Williamson was particularly concerned with the assumption that there was free trade between 1600-1869, noting that the period before 1820 clearly did not have free trade. Clark replied that he was simply trying to make the point that before the 18th century we don't need free trade to explain the relationship between population and wages. Field was concerned that the paper makes a distinction between trade and technology, when in fact technological changes in transportation has been the main factor in expanding trade. Clark claimed that transportation costs were not the main constraint on trade, because grain (which is not the most valuable commodity) was widely traded throughout Europe.

Other comments focused on interpretation of the results. McCloskey and Wallis both wondered how Clark could conclude that nothing important happened when Figure 6 clearly shows that Britain escaped from



Malthusian constraints in the 18th century. Fishback claimed to have seen at least 1500 different dates for the Industrial Revolution and wondered why Clark focused on 1770. Clemens and Grantham both questioned why the analysis was limited to Britain and asked what was happening in other countries. Wallis proposed that comparisons with the US would be informative.

Burnette was skeptical about the GDP numbers for two reasons: because profits were omitted and because the wage series is based only on wages of laborers and craftsmen. Clark said profits were relatively small, even in the cotton industry, and that skill premiums remained constant over time. Hejeebu suggested that sensitivity analysis was necessary to convince us that these GDP numbers are superior to Crafts and Harley's estimates. McCloskey asked why it mattered that textiles were able to be traded. Clark explained that the effect of any one sector on aggregate productivity depends on its share. For example, textiles had an impact, because elastic demand and trade allowed the share of that sector to grow. By contrast, productivity gains in printing led to a declining share for books.

Immediately after the last session, without an author to attack, all the participants left to catch their flights. Cliometricians will gather next for a couple of sessions in Atlanta at the ASSA meetings. La Crosse, Wisconsin will be the site of the annual convention in May 2002, where the flooded banks of the Mississippi should prove an interesting contrast to the parched Tucson desert.

Observations on the 40th Cliometrics Conference

By A. Shrub

Good evenin' fellow Americans, and thank you for asking me to judge the entries in the Presidential soundalike context that the Clionomics Conference has been pronunciating. It looks like we have had a bumper catch that makes me proud to have had such an effluence upon our language as we speak it here. Before we move on to consider the nominees, I'd like to thank the Mullah Nasr-ed Din and his obsequious servant for their many years of cataloging neologisms that began back when it was naively considered eccentric [warm applause].

Let's begin the runners-up with a wise observation on the nation's great Civil War, which concluded just before my daddy became President of the United States. One of our hosts observed all too wisely that "Sherman was dissaving all through Georgia." Unfortunately, Clionomicians, I understand, abhor the kings-and-battles version of history, and this view was simply too violent. Soon thereafter, our host remitted himself when he claimed that "Sitting in a foxhole in Italy may not be better than being unemployed." This was, of course, pure speculation as he or I never sat in an Italian foxholes and have not been unemployed but suspect we would much prefer being unemployed in Italy to all three of the options described above.

A series of wisdomicities concerned a mysterious "it," a word I enjoy using due to its easy pronunciation. That fella from the dairy state enlightened us with "The problem with some countries is they don't have anything; people can't even find it." I couldn't agree more, but I believe with a bipartisan effort we would have a better chance of finding it and recognizing it upon finding it. Another fella who has written a lot on globulization responded that "It's good but it's wrong." Now you had me here, but this sounded very smart, and I intend to say this a few times myself, even if I don't recognize it when I see it. This fella's student then spun me around with "If I show you a thousand white swans then what have I shown you? But if I show you a black swan, then you see it." I specially liked that one cause I *could* see it, and I could recognize it even if I didn't see it. I knew then we were speaking the same language. And when someone said, "It is easy to agree when it is hard to disagree," I knew only a rhetorologist could have thunk that one up. I am pretty sure that I myself agree with it.

Two of the finalists encapsulated the differences between historians and economists in how to view the past. And I understand the past, because I was in it once upon a time. The gal who learned us about land wealth (next time study baseball teams, sweetheart) wished that "If only the Civil War had happened sooner it wouldn't show up in my data." But then we would have missed seeing Sherman's march through Georgetown! And I might not have been born in time to see my Daddy serve as Pesident. So I preferred the claim by that fella who knew from his study of political monetary bicycles that "Sometimes the number one is just a number one." Mmmmm ... if only I could speak like that.

The runner up really got me to thinkin' about many a deep thought. One gal (who was Canadian, so I guess she could have voted for my opponent in Florida) proposaled that "Immigrants are usually born elsewhere." Had to think about that "usually" clause ... Pretty sure it's true, but if not, where are those who are born elsewhere born? And where do they vote?

Finally, the winner combined brieferty and that crucial characteristic of coherence with past wisdom emitters. Since the previous year our youngest host had admitted that "Sometimes it's better to do it backwards; sometimes I like it better backwards," it seemed only right to award that bicoastal gal who urged a hapless presenter, "If you want more oomph, you want to get rid of the mushiness." Myself, I will take that as a byword in all my public utterances and promise to urge Congress to produce more oomph that can then be shared with our constituents, our gardens, and biomass-powered electric plants throughout this great nation of ours, which contains absolutely no mushiness.

BOOK PREVIEW

Altruistically Inclined? Evolutionary Theory, the Behavioral Sciences, and the Origins of Reciprocity

By Alexander J. Field

Note: This is an excerpt from the preface to ***Altruistically Inclined? Evolutionary Theory, the Behavioral Sciences, and the Origins of Reciprocity*** *to be published in October 2001 by University of Michigan Press.*

Two strangers meet far from the reach of organized society. Each must decide quickly whether to attack or await the action of the other. Together, they are better off choosing restraint and thus opening up possibilities for mutually beneficial interaction. Desires for both self-protection and possible wealth enhancement, however, impel each of them toward an initial and immediate aggressive move.

The parable of the Good Samaritan reminds us that failure to help can be hurtful. We can easily overlook the point: since we are vulnerable to injury from all but the weakest, failure to harm can be helpful. In holding in check our ability to damage or destroy, we help our counterparts, because they have avoided injury at our hands and because they now face opportunities for gains at our expense that would otherwise have been unavailable. And, in forgoing potential gains and exposing ourselves to otherwise avoidable risks, we have harmed ourselves. A surprising but inescapable conclusion: failure to harm can be altruistic.

Are we altruistically inclined? Are we, in spite of the counsel of prudence and the temptations of greed, often predisposed in situations such as that described above to give up the option of making a first aggressive move? If it is in our nature to be so inclined, how can this possibly be, given what we know of the operation of evolutionary forces?

Discussions of human altruism in the social and behavioral sciences often have a nebulous and ill-defined quality to them. People commonly question what altruistic behavior is and whether it can truly be distinguished from what is selfish. But in a biological context, altruism has a very precise meaning: behavior by an individual organism that reduces its own reproductive fitness while improving the reproductive fitness of at least one other member of the same species (conspecific). Reproductive fitness affects the relative frequency with which an individual's genes appear in the next generation's gene pool.

Like Robert Frank's *Passions within Reason* (1988), this book takes as its starting point the proposition that altruistic behavior is an important empirical category. Also like Frank, it explores the evolutionary explanations of this phenomenon. Unlike Frank, however, it considers the possibility that natural selection (the fundamental motor of evolutionary dynamics) has operated at group as well as individual levels. Group selection occurs when selection differentially rewards members of a group as a consequence of the frequency of some trait they hold, for example, when groups with higher frequencies of those predisposed towards altruism grow more rapidly.

Group selection is not a new idea but has only slowly been reemerging from an intellectual doghouse. The evolutionary models that most people carry around in their heads start with the premise that natural selection operates exclusively at the level of the individual organism. This poses a fundamental problem for the explanation of altruistic behavior, since by definition altruism cannot be favored if selection operates only at the individual level. Much of the history of social and biological sciences since the 1960s has involved attempts to resolve this apparent contradiction.

Considerable progress has been made in understanding altruistic behavior toward kin, for example, the sacrifices that parents make for their children. The theory of inclusive fitness, pioneered by the late William Hamilton, emphasizes that selection occurs ultimately at the level of the gene. Since parents share half their genes with each of their children, sacrifice for offspring may favor genes predisposed to such behavior, even if the sacrifice is not in the material interest of the parent.

The explanation of altruistic behavior toward non-kin is more difficult. The degree of genetic relatedness drops off quickly (second cousins share only 1/32 of their genes). Since altruistic behavior favors the fitness of other conspecific(s) at the expense of the actor, it is hard to see how predispositions to behave altruistically toward non-kin could spread or even survive. Were they to arise through mutation or genetic recombination, such tendencies would seem inevitably to decline in frequency and eventually disappear over time through the operation of natural selection.

If group selection processes are operative, however, it is possible within a population periodically dividing into smaller groups for behavioral predispositions to shrink in frequency within each individual group, while they increase in frequency within the global population. Although counterintuitive, this possibility arises when there is a positive covariance between the frequency of altruists within a group and the rate at which it grows. Thus, while altruistic behavior engenders reduced reproductive fitness for the organism that exhibits it within each group, genes that are predisposed towards altruism can increase over time within the global population. This possibility enables us to understand how evolutionary processes could favor altruistic tendencies, even when altruism is a disadvantage to individuals in a group.

Most social scientists admit the relevance of altruism in considering relations among kin. But in relations among non-kin, self-interest seems to reign supreme. Suggestions that altruistic predispositions have a role to play are, if not rejected, greeted with considerable skepticism. This presumption persists despite considerable experimental and observational evidence to the contrary. Part of the reason for this is that we tend to focus on what sustains or maintains ongoing interaction, as opposed to how it evolves.

Continuing scientific progress makes it increasingly clear that the natural sciences do more than define a problem that social sciences must resolve. A more nuanced Darwinian approach enables us to organize and interpret the results of other research in ways that facilitate understanding of biological influences both on universal human behavioral propensities and on the structure of our cognitive faculties whereby we acquire knowledge about the world. These in turn can help us understand the emergence of normative structures without which the origins of reciprocity and complex social organization would be impossible.

Evolutionary, biological reasoning has frequently been misused in the past, sometimes in horrific ways, and many readers approach it with reservation. It is important to enumerate several factors that argue in its favor. First, research and, especially, theorizing in this area is generally somewhat less prone to overreaching than was the case 25 years ago. In particular, there is now more emphasis on understanding genetic influences on human cognitive structure and behavior in the relatively stable ancestral environment of hunter-gatherer existence and less emphasis on attempts to interpret adaptive behavior following the Neolithic revolution. Second, the understanding of and scientific consensus about the levels at which natural selection can and do operate have been refined as the result of observational, experimental, and theoretical research. The same is also true for assumptions about the interrelationships and balance between innate and learned cognitive and behavioral mechanisms. Third, the fruitfulness of inquiry into biological influences on human behavior and cognition has been steadily reinforced by an accretion of observational and experimental data and new ways of interpreting such data.

Overall progress in these areas over a quarter century is striking in comparison with what one observes in much of the social sciences and suggests that research along these lines alone offers the possibility of transcending the most significant and persistent fault line within them. That is the divide separating the sociological-anthropological tradition (with its emphasis on culture, norms, institutions, ideology, and emergent properties) from the economic approach (with its assumption of rational choice and ambivalence toward or outright rejection of all of the above concepts).

Research by heterodox scholars has tried to bridge this divide. But many on both sides remain skeptical that these efforts can lead to a scientifically progressive research program. This book should be of interest to those identifying with either the rational choice or the sociological tradition. In addition, it tries to move beyond the ultimately unproductive opposition between them. Rethinking the implications of evolutionary theory and processes and, in particular, relaxing the assumption that natural selection operates only at the level of the individual organism leads to a rethinking of the strengths and limitations of each. In the context of serious consideration of experimental and observational evidence, it lays groundwork not only for some rapprochement within the social sciences but also between the biological and behavioral sciences. This integration, however, entails a different set of implications than those traditionally drawn by advocates of such unification.

Some background in game theory is helpful in understanding the arguments developed here. This is not because game theory, any more than the rational choice approach of which it represents an extension, provides a universal key to understanding human behavior. But, in recent years, it has become almost impossible to discuss developments in social science and, increasingly, biological science without employing its idiom. The main use of game theory in this book is as a means of organizing our thinking about what would be likely outcomes if interacting individuals were strictly self-interested and/or if natural selection operated only at the level of the individual organism.

Since the main focus is on areas where game theory doesn't predict well, it would not be fair to say that the emphasis here is principally on the application of game theory to the social or biological sciences. This book is wide-ranging in scope, yet focuses on the implications of evolutionary approaches, broadly conceived, for our understanding of essential human predispositions. In exploring these cognitive underpinnings, I also emphasize what has come to be called modularity. Modularity refers to cognitive adaptations, which employ different neurobiological machinery, use different reasoning algorithms, and may lead to different behavioral outcomes depending upon the domain encountered.

As a result of millions of years of evolution, humans possess powerful cognitive reasoning modules that facilitate foraging and its modern equivalents. These include facility at Bayesian learning – necessary for forming rational expectations – as well as competence at maximizing goals, such as caloric yield, and allocating time among alternative activities. The mathematics of constrained maximization, central to economic theory, provides a useful metaphor for modeling the operation of such modules. But in the realm of strategic interaction, as the experimental and observational evidence makes clear, humans possess other algorithms and action inclinations that are at least as important in influencing behavior.

The idea of cognitive and behavioral modularity helps explicate a variety of otherwise anomalous observations. But, like group selection, it is not one that has been widely considered within the social sciences. Consequently, either is likely to be embraced only after the most careful consideration. This book recognizes the appeal of the familiar and that we may be drawn to certain explanatory frameworks because of their expressive qualities or their aesthetic appeal, rather than simply their explanatory or predictive power. However, it is written under the assumption that evidence and argument ultimately matter and that the vast majority of scholars in our disciplines are committed to traditional scientific goals.

Following a prologue, the first chapter develops the main issues and evidence that occupy the study. Chapter Two discusses the logic and mathematics of group selection models. Chapters Three and Four consider other explorations of altruistic behavior, including work by Robert Trivers, John Maynard-Smith, Robert Axelrod, and Robert Frank. Chapter Five covers arguments and evidence underlying the concept of modularity. Chapter Six addresses the heuristics and biases research program and its more limited relevance to the issues addressed here. Chapter Seven considers differences between historical and social scientific explanation and looks to a more integrated future.

Readers approaching this study with a jaundiced view of economics or rational choice theory may question much in the first part of the book as belaboring the obvious. In defense, I can only say that the appeal of the methods associated with this tradition remains very strong in modern behavioral and social science and that those employing them have a justified sense that they explore the implications of some very powerful human predispositions. Only by carefully delineating the restricted applicability of the underlying models can one hope to make headway in articulating the case for alternative and complementary approaches.

Report on the Business History Conference

By Howard Bodenhorn, Lafayette College, and
Lucy Ann Newton, University of Reading, UK

(Miami) The Business History Conference held its annual meeting April 20-22, 2001 at the Biscayne Bay Marriott in Florida. President elect Geoffrey Jones (University of Reading and Harvard) selected Miami, because it represents the crossroads of Atlantic trade in "services and the global economy" (the theme of the conference). An eclectic program with 31 sessions was arranged by Jones, Ken Lipartito (Florida International University), Will Hausman (College of William and Mary), and Mary Yeager (UCLA). The number of papers meant that universal coverage was impossible. Thus, the following reports reflect the personal preferences of the reporters. Readers interested in the broad sweep of papers are encouraged to read both the abstracts and some full-length papers available at EH.Net.

In a Friday afternoon session titled "The Business of Education," **Rolv Petter Amdam** (Norwegian School of Management) discussed the evolution of European business education. Amdam examines how business schools influenced management practice using a linear development thesis. During the first phase, from the 1880s until the 1920s, business schools were in a formative period and were viewed primarily as national institutions. Schools evolved differently across countries. The US followed an MBA path that encouraged close links between academia and practice; Germany followed a diploma model with roots in economics and a strong academic focus; and France used the diploma model with emphasis on mathematics and management. The second phase, from 1945 to the 1960s, was a period when business schools underwent "Americanization." During this period, there was a strong expansion in US business schools, and they were widely seen as the new role model. From the 1960s to the 1980s, Amdam argues that European and national visions asserted themselves, galvanized by the spread of mass education and the rise of business studies as a discipline. In examining developments since the 1980s, Amdam argues that "Americanization" (although relevant to the 1950s and 1960s) was no longer such an important influence on recent developments.

Not everyone agreed with Amdam's thesis, pointing to the ongoing strong influence of the US in all areas of management practice and theory and questioning the success of new pan-European ventures. Yet Amdam defended his assertions by highlighting the continuing existence of differences in management styles, management practices, and management education between the US, Europe, and Asia. He states that the development of the European Union as a trading block to challenge America also encouraged competition in styles and practice between both sides of the Atlantic.

Ken Sokoloff (UCLA) covers a broader timespan and different issues in his paper. The key question he addresses is "Can variations in economic growth be accounted for by examining institutions, such as schools?" Sokoloff's analysis focuses on primary education and literacy rates in which he argues that the establishment of primary schooling strongly correlates to university enrollment and to economic attainment.

In examining the Americas, he maintains that the different societies that emerged from European colonization varied noticeably in their literacy rates. In North America, where the US and Canada were colonized by Britain and France, literacy rates were high. The Spanish colonies of South America, on the other hand, had low literacy rates. Explanations have traditionally centered on the national origins of the colonizers. Those with Anglo-Saxon origins had established public schools and other institutions that promoted growth; therefore, these colonies were relatively advantaged. Sokoloff points out that homogeneous societies with a more even distribution of wealth promote educational attainment, in contrast to highly unequal societies where schools serve the elite.

Sokoloff, asking why inequality is exogenous, examines three factors. First, colonies in locations in which the climate was suitable for sugar growing (the most important commodity in world trade in the 17th and 18th centuries) relied upon slave labor, which created unequal societies. Second, in the Spanish Americas, indigenous Native Americans and abundant natural resources eliminated the need for European immigrants, thus inequality developed. Third, in North America, the population was more homogeneous.

In response to Sokoloff's paper, the audience acknowledged the tremendous work of scholarship that had gone into collating the data for this project and for the broad sweep of its analysis. Individual countries were considered in order to challenge the thesis, including an examination of the southern states in the US.

Coincidentally, a second session entitled "Coffee Capitalism" was offered immediately after lunch. The opening paper, "Family as Foundation: The Social Organization of Nicaragua's Coffee Economy," was presented by **Julie Charlip** (Whitman College). This paper is part of a forthcoming book and addresses self-sufficient coffee farms that rely on family labor. Charlip contends that credit is readily available, is used to add small parcels of land to the family farm, and is repaid through the provision of labor. Discussant Justin Wolfe (Tulane) raised several issues the paper left unresolved: How did emerging state power affect family-based production? What was the relationship between family and social mobility? Was marriage, for example, used as a business practice? From whence did entrepreneurship arise in a family-based business?

Birgitte Holten (University of Copenhagen) presented "From Coffee-Trade to World Market," in which she traces the implications of the 19th-century transportation and communications revolutions for coffee marketing. The question motivating her work is how European consumer preferences were relayed to Brazilian producers. Existing studies focus on both ends of the chain, ignoring the central role played by wholesalers. Holten identifies three distinct periods of coffee marketing. Between 1830 and 1860, before the telegraph and steamships, agents purchased coffee closest to the preferences of European wholesalers. With telegraphy and steamships, information transmission was faster; consequently, the century between 1860 and 1970 saw better matching of consumer tastes and wholesale purchases. Finally, the post-1970 information technology revolution has further improved the matching process. In response, Wolfe commented on the effects of technological advance on the producing country.

Steven Topik (UC-Irvine) offers a look at "How Mrs. Olsen Got Her Full-Bodied Coffee: The Industrialization of the Coffee Service Sector in the US, 1860-1950." He traces the evolution of coffee marketing from home roasting to the emergence of national brands. According to Topik, the growth of national brands depended on the development of a dense transportation network, standard coffee definitions in the 1880s, vacuum-packed cans that first appeared in 1903, and consumer confidence in quality brought about by the Pure Food and Drug Act (all of which converged in the 1930s). Wolfe wondered how tastes developed and why European immigrants accepted American coffee. Topik's assertion that the quality of coffee actually declined after WWII initiated a spirited discussion. Ann Carlos (Colorado) asked what defines a good cup of coffee, while Mary Yeager (UCLA) wanted to know whether oligopolies and standardization trampled on different regional preferences.

Friday afternoon's dissertation plenary session was organized by Jonathan Zeitlin (UW-Madison). **Jennifer Klein** (Smith College) presented "Managing Security: The Business of American Social Policy, 1910s-1960." She points out that while concerns over economic security are currently in eclipse, they were a defining feature of economic and political discourse during the New Deal era. Private welfare schemes developed in tandem with public schemes, and Klein focuses on corporate provision of group health insurance in the 1910s and 1920s. She finds that corporate and public provision were closely linked, thus, state-centered analyses miss the importance of private actors. Public provision of some welfare services increased the demand for privately supplied, complementary products.

Next up was **Christophe Lecuyer** (MIT) with "Making Silicon Valley: Engineering Culture, Innovation, and Industrial Growth, 1930-1970," an analysis of the rapid growth of technology-based industry in the Valley. Lecuyer asserts industrialization began with so-called old economy businesses but quickly emerged as a technology center, firmly entrenched by the 1970s. He asks and answers these basic questions: Why did Silicon Valley emerge as a technology center? and What were the roles of universities and the military in the transformation? Lecuyer traces three waves of technological and entrepreneurial advance. Radio technology businesses arose in the 1930s, were replaced by microwave technology, and subsequently by silicon technologies. He argues that each wave built on, rather than replaced, the advances of the previous generation of technology. The driving force was a growing military demand for electronics.

"The World's Newest Profession: Management Consulting in the Twentieth Century" by **Christopher McKenna** (Brasenose College, Oxford) followed. McKenna relates how at the end of *Strategy and Structure*, Alfred Chandler writes that he had not told how corporate culture was transmitted between firms. If he had, he would have had to tell the story of management consultants. McKenna takes up Chandler's task, focusing on these key issues: What is the institutional history of management consulting? Why did consultants fail to achieve professional status? What effect did consulting have on business, government, and nonprofits? McKenna claims that the first consulting firms adopted an overly narrow view of their mission and failed to survive. The successful ones arose in Chicago in the 1930s and were accounting-based firms that initially provided financial services consulting for large New York City banks. McKenna next turns to the question of why consultants never achieved a professional status similar to that of accountancy or law. He reasons that professionalization usually follows from the actions of a few outspoken free agents. Consultants are seen as part of a larger organization. Finally, McKenna turns to the effects of management consultants. Their principal contribution was that they brought business-like organizational models to government and nonprofit organizations.

Andrew Robertson (Harvard) gave the fourth presentation. In his thesis, "Mobilizing for War," he considers the role of the engineer in Japan between 1935 and 1960. Specifically, he looks at the introduction of quality control procedures and their effects on industrial modernization. Robertson finds that despite considerable effort, industrial efficiency did not increase significantly before or during WWII. Substantial increases came about when the occupational government purged managers and envisioned an emergent labor movement. Engineers saw US technology as both efficiency-enhancing and democratizing and pushed for its introduction.

Saturday morning kicked off with "Multinational Business," featuring an examination by **Jennifer Frankl** (Williams College) of Japanese trading companies operating in the US during the first half of the 20th century. She pays particular attention to the role of Japanese trading companies in facilitating international trade and the reasons behind their growth into such large entities in both scale and scope, in contrast to their western counterparts.

Teresa da Silva Lopes (University of Reading, UK) looks at multinationals in the alcoholic beverages sector. By combining business history literature and theories form international business sources, Lopes considers patterns of foreign direct investment (FDI) undertaken by such firms and their governance structures. FDI took place through wholly-owned companies or collaborative arrangements in production, distributions, and research and development.

David Merrett (University of Melbourne) looks at the development of Australian MNEs from the late 19th century to the 1960s. In this period the Australian economy did not provide the preconditions likely to facilitate a high rate of outward FDI. Indeed, from 1950 to 1980 the ratio of stock of outward FDI to GDP averaged only 1.4%. In contrast, the economy experienced high levels of inward FDI. By studying particular firms, Merrett argues that there was some important FDI taking place by Australian firms.

Saturday afternoon brought a host of interesting sessions, including one titled "Saturday Entertainment." This session had two papers concerning the business of cinema. **Gerben Bakker** (European University Institute, Florence) examines social savings in relations to the film industry between 1890 and 1940 in an attempt to consider productivity growth in this sector of the service economy. By looking at the question of where film had brought society by 1940, Bakker introduced a counterfactual question – What would have happened without cinema? Elsewhere at the conference, the BHC President had encouraged business historians to make more use of analytical techniques such as counterfactual arguments, and Bakker rose to the challenge.

Peter Miskell (Unilever History Project, UK) analyzes the British response to the American invasion of the film industry, in particular, the attempts of the UK government to protect the industry. He set the context for discussion by illustrating the popularity of cinema in Britain, with admissions rising from 903 million per month in 1934 to 1575 million in 1944, dropping only slightly to 1305 million by 1951. Culturally, there were concerns about national prestige and lack of ability by the British film industry to compete with its American counterparts. As a result, the British government intervened. Following the nadir of British cinema in the mid-1920s, when only 5% of films shown in Britain were made inside the country, the 1927 Cinematic Films Act imposed quotas whereby a minimum of 5% of films shown had to be British. By the mid-1930s this had risen to 25%. The Act has traditionally been seen as a failure, with historians arguing that it led to the production of large numbers of cheap, poor quality British films and that these were shown to small audiences. Miskell provides a revisionist argument, asserting that British films produced during the 1930s were very popular and that the protective environment of the 1927 Act provided a seedbed for major British directors and actors such as David Lean and James Mason. Thus, Miskell argues, the 1927 Act was beneficial to the British film industry.

Larry Neal (Illinois) chaired a second dissertation session on Saturday afternoon. Unlike the Friday session of completed dissertations, this session focused on works in progress and encouraged participants to provide constructive comments. **Gunhild Ecklund** (Norwegian School of Management) opened the session. Her dissertation focuses on two issues: How did the Bank of Norway change and implement policy between 1945 and 1970? And, how did this compare to other central banks in policy formation and implementation? The conventional view is that the Bank lost its independence after WWII, becoming an arm of the Ministry of Finance mostly because the citizenry lost confidence in the Bank during the tumultuous interwar period. Ecklund concludes that political control over the bank did increase significantly and that it changed from traditional monetary policy concerns to a credit policy more concerned with nominal interest rates and credit availability than inflation.

Alison Parkinson (Nuffield College, Oxford) considers options open to English women from the mid-18th to mid-19th century when they reached an age to leave home. The traditional choices were often captured in the phrase "marry, stitch, or die." This suggests an effective withdrawal of women from market activities, because most chose the first option. Parkinson points out, however, that 19% of adult women had to survive without a husband, and others found that husbands failed to provide economic security. Many of these women turned to small business self-employment to support themselves. Parkinson's data on small business ownership are fire insurance contracts intiated between 1747 and 1861. She finds that women contracted for about 10% of all policies and that 85% of these policies contained a female name only. Parkinson also finds that self-employment in the food, drink, and hospitality sector declined during the century, while employments in sewing and laundry increased.

Serguey Chiekhetov (UC-Santa Cruz) is working on a cultural history of Soviet entrepreneurs during the brief New Economic Policy of the 1920s. Despite developing in vastly different environments, there are close similarities in the value systems adopted by US and Soviet entrepreneurs during this period. He argues that both sets of entrepreneurs developed value systems that drew sharp we-they distinctions across many dimensions. In particular, both systems held similar attitudes toward women, labor, and appropriate entertainment outlets.

Siddhartha Singh (Cornell) is working on a history of agricultural biotechnology. Singh notes that the so-called Green Revolution has improved the standard of living through the introduction of pest and drought resistant hybrids. Moreover, he claims genetically modified foods will be necessary to forestall an impending Malthusian crisis. Despite resistance to genetically modified animals and plants, they have a long history. People have been relatively quick to accept genetically modified animals, but slow to accommodate new plant hybrids. He suspects that this may change as the lines between agricultural biotechnology and pharmaceutical industries blur and that history tells us people will eventually accept genetically modified plant foods.

Late Saturday afternoon, Tony Freyer (Alabama) introduced an eclectic session on "Law and Organizational Choice." In his paper, **John Wertheimer** (Davidson College) traces the connection between free enterprise and free speech. He notes that most free speech appeals prior to WWI lost. Newspapers instigated most pre-Civil War cases. However, between the Civil War and WWI free speech cases came from other sources, including political and labor activists looking to organize and libertarians who wanted a freer discussion of sexual practices. Another important group was corporations, who used free speech appeals to overturn various restrictions on their behavior. Prohibition, advertising restrictions, and licensing rules were all challenged under free speech appeals. Corporations were typically more successful than other appellants, because they had greater resources and received more sympathetic hearings from judges willing to overturn restrictions on free trade and property rights.

The session continued with research by **Adam Winkler** (UCLA) where he argues that at the beginning of the 20th century the law accepted the corporation as a natural entity or individual. In the law of the electoral process, however, the corporation was seen as an association. A 1907 law prohibited corporate contributions to political candidates, because politicians thought that the corporations could corrupt the electoral process. By mid-century attitudes changed again, with the corporation emerging as a salutary association, because it increased political participation.

In the session's third paper, **Howard Bodenhorn** (Lafayette College) presented an economic interpretation of partnerships in the late antebellum South. He divides firms into six sectors and shows that in four of them only about 25% of all firms operated as partnerships. In professional employments, however, only about 10% of firms were organized as partnerships. Wholesalers were the other outlier, with 65% of all firms organized as partnerships. Firms are a nexus of contracts that include agreements about provision of specialized services, direction of production, and length of association. Focusing on the latter two, he shows that partnerships typically involved a combination of individual discretion in production decisions and relatively long-term associations. This combination was amenable to wholesale transactions where profits depended on trust and on arbitraging small price differentials. Other processes typically require more direction or different expected length of association.

Vicki Woeste (American Bar Foundation) was the discussant for this session and instead of providing detailed comments on the individual papers thought that they provided an opportunity to discuss a meta theory of how they fit together in the larger agenda of interdisciplinary research. All three papers raise issues about law and economics, but do they challenge us to rise above disciplinary constraints? She argues that they do, because the law pervades every social science discipline from economics to feminist studies. Economics is perhaps the only one not co-opted by law. Coase stripped the firm of its legal form, and Posner's Chicago School approach believes that the point of law is to facilitate efficient transactions. These papers, to different degrees, incorporate the law into their approach, which is a good sign because law matters.

Sunday morning opened with "Insurance Companies in a Worldwide Perspective." **Mae Baker** and **Michael Collins** (both University of Leeds, UK) began by examining British insurance industry investments between 1900 and 1973. They consider public sector investments first. The period before WWI saw insurance companies making 21% of their total investments in public sector securities. Even allowing for the distortions of two world wars, such securities remained popular throughout the period. The authors then take a look at the holdings of market securities by insurance companies through the 20th century and identify two main trends. The mix of securities became less risk-averse, and market securities as a whole became the dominant part of the life companies' portfolios.

Dalit Baranoff (John Hopkins) studies insurance boards and the control of risk in the American fire insurance industry between 1873 and 1906. Her analysis includes four events which had a major impact on the US fire insurance industry: the Chicago fire of 1871, the Boston fire of 1872, the Baltimore fire of 1904, and the San Francisco earthquake of 1906. During this period, local insurance boards acted as cartels. Previous attempts to establish boards and operate cartels had failed, consequently Baranoff asks why agents and companies continued to organize cartels after 1870 and why these cartels worked successfully in the post-1870 period.

The rate setting function of the boards meant that they could manage risk and uncertainty, whether it was a particular risk on one building or the fundamental risk for a whole town. In the 19th century, such fundamental risk was common and resulted in high costs or even bankruptcy for many insurance firms. This provided a stimulus for the establishment of cartels. Moreover, external conditions and internal growth of demand also stimulated the founding of cartels. Accelerated US economic growth in the late 19th century and the 1880s boom provided an external environment that stimulated demand and spurred on changes in practices that allowed local boards to succeed.

This paper provoked debate concerning the lack of state regulation or federal legislation. There was also some discussion about the impact of higher rates upon customers. Overall, the paper was lauded for its historical detail, rigorous analysis, and perceptive overview of an important industry.

This brief report fails to do justice to the breadth of ideas discussed at the conference. The organizers put together a number of stimulating panels. The setting, too, was conducive to a relaxed yet energetic atmosphere. With a marina outside the hotel's back doors and a view of the Venetian Causeway, it was easy to grab a few moments of fresh air and clear one's head between sessions. Beyond the causeway, the enticements of South Beach undoubtedly led minds (and bodies) to wander in less than scholarly directions.

Report on the Economic History Society Conference

By David R. Stead, Nuffield College, Oxford

(Glasgow) The 75th Anniversary Conference of the Economic History Society was held in Scotland March 29-April 1, 2001. Because most sessions were held concurrently, this represents only a sample of the program. Negley Harte (University College London) organized the first session in commemoration. He opened with a statistical overview of the three phases of the Society's history as reflected in membership numbers and publications. In the Society's first phase (1926 through 1945), total membership drifted downwards, however, the decades immediately following WWII were characterized by rapid growth. Membership peaked in 1976 at over 2500. After that, there was a slow decline to the current level of 1500. The total stock of publications on British economic history since 1926 is now over 60,000. In recent years, the annual number of new books on this subject has exceeded that of articles, a publication pattern more characteristic of the humanities than the social sciences.

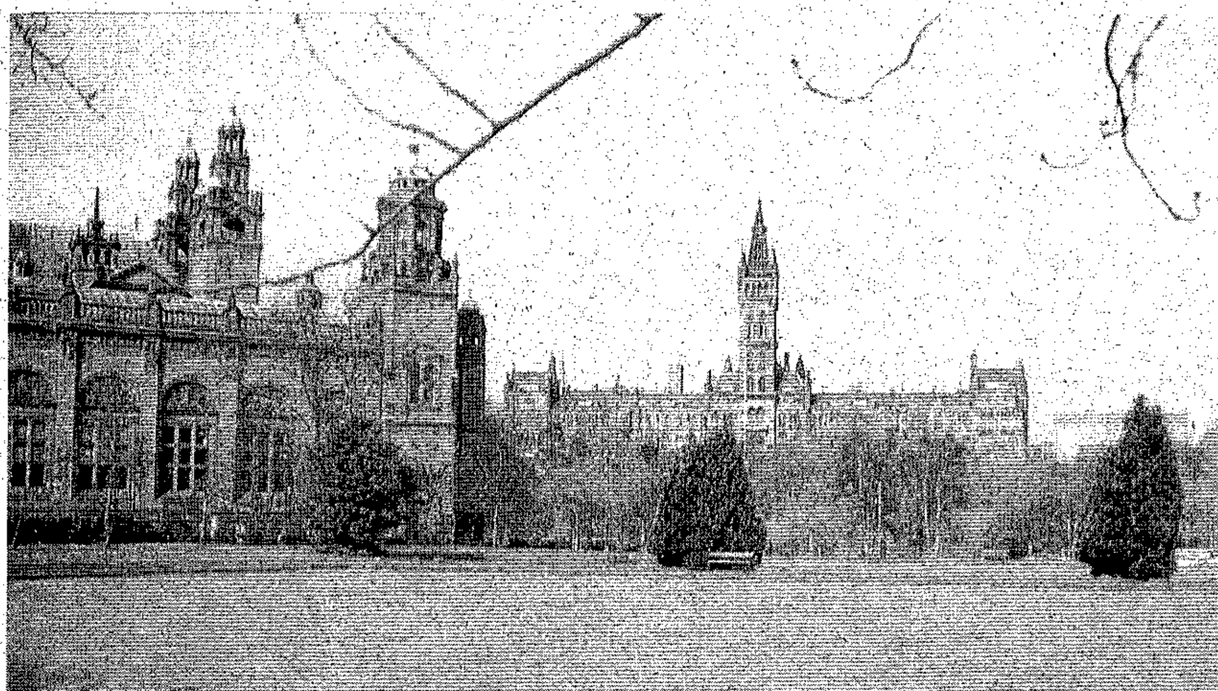
Various members then shared their memories of the personalities and research undertaken throughout the Society's history. Participants in the discussion included François Crouzet (Sorbonne), Eric Hobsbawm (Birkbeck), Jim Potter (Institute of United States Studies), Walt Rostow (Texas), Margaret Spufford (Roehampton), and Barry Supple (Leverhulme Trust). The group also listened to extracts from interviews with Maurice Beresford, Sir John Habakkuk, and Max Hartwell. Patrick O'Brien (London School of Economics) and Maxine Berg (University of Warwick) shared their research on the founders of economic history in Britain. The session provided both a fascinating and amusing oral history of the Society.

The first session, "From Manor to Mill," focused on Britain during the 18th century. **Craig Muldrew** (Queens' College, Cambridge) discussed the emergence of the concepts of capital and saving in contemporary writings and presented estimates of the amount of credit in the economy. Muldrew's estimates suggest that in the mid-17th century Britons held at least £15 million in bonds. Furthermore, credit expanded over the next century as mortgage and stock markets were further developed, and by 1760 interest-earning wealth may have totaled £200 million.

In the next paper, **Julian Hoppit** (University College London) criticized the conceptual drift in constructing a unitary state out of what actually was a complex amalgam of multiple authorities. For example, initial changes to the operation of the Poor Law were made at the local level. The pluralistic state, which sought consensual decision making and listened to arguments and initiatives, was useful to the makers of the Industrial Revolution, but did not make the Industrial Revolution.

Nick Crafts (London School of Economics) and **Knick Harley** (Western Ontario) then presented counterfactual simulations of the static and dynamic effects arising if peasant agriculture had survived in Britain. Their model, calibrated for 1841, indicates that peasant continuance would have had major implications for the economy, such as a doubling of the share of the labor force in agriculture. But the transition to capitalist farming that did occur would only have made most families worse off if the change had been accompanied by significant population growth.

In the brief discussion that followed, Pat Hudson (Cardiff) felt that adding cultural variables to the Crafts-Harley model would enrich the analysis of the welfare implications. In her opinion, modeling regional variations would be another useful exercise. Crafts acknowledged the loose language involved in assessing movements in utility solely in terms of real wages and thought that other factors could be captured because the model now incorporates heterogeneous agents. Harley added that some writers believe that peasants valued their independence.



University of Glasgow, host of the Economic History Society Conference

The round table discussion on British entrepreneurship over the last three centuries began with **Pat Hudson's** paper on entrepreneurs during the Industrial Revolution. Hudson finds that those who succeeded in the inherently risky business conditions of the period included immigrants and families working from home. An important source of competitive advantage was localized industrial clusters that facilitated trade networks and the transfer of knowledge.

Alfred Chandler (Harvard) criticized Britain's organizational structure of personal capitalism, which, unlike the United States, failed to realize economies of scale and scope. However, he made an important concession by acknowledging that one great British success, the branded packaged goods industry, was run by family firms. Preoccupation with organizational structure may be a "red herring." Significant improvement in marketing techniques was one advance made during this period.

Terry Gourvish (London School of Economics) addressed a more pessimistic picture of entrepreneurship between 1870 and 1914. He argues that "incumbent inertia," especially in the traditional sectors of the economy, led to complacency and a stalled response to the new challenges of the age. Gourvish cites the move toward limited liability as a possible defensive move to protect inheritance and avoid taxation rather than as an expansionist strategy to stimulate investment.

In his paper, **Mark Casson** (Reading) examines enterprise culture in postwar Britain. He explains that until 1973 the British political and business elite favored striving for scale economies and the active use of industrial policy. Attitudes subsequently changed and flexibility from small and privately-owned companies became the preferred form of business. Casson argues, however, that reality and rhetoric may diverge. For example, it is inappropriate to assume that small firms are necessarily more flexible than larger ones.

Some audience members, including Bill Rubeinstein (Aberystwyth), felt that Casson had neglected some important changes in postwar enterprise culture, such as reductions in taxation and the revival of the industrial clusters that were so important in earlier centuries. Casson acknowledged flexibility, which he had stressed, was a subset of wider movements toward a more liberal economic agenda. Districts such as Manchester and South Wales have undergone a recent renaissance, but these recoveries were based on foreign direct investment, where the initiative lies outside Britain. Rubeinstein also wondered about the success of financial services. Gourvish's impression was that the city generally had been effective in providing capital to domestic entrepreneurs. He maintained that the service sector as a whole usually had been successful in grasping new opportunities. Stephen Broadberry (University of Warwick), nevertheless, suggested that Britain's relative decline actually was driven by loss of leadership in services. Rostow remarked that he was more comfortable disaggregating the historical record rather than making generalizations.

Oliver Westall (Lancaster) attempts to reconcile Church's assessment of entrepreneurship around 1850 with Gourvish's portrayal half a century later. He notes that change in market structure from the mid- to late-19th century, such as the growth of collusion, generated market power for incumbents and therefore blunted entrepreneurs' incentives. The final strand of the discussion focused on the possibility that entrepreneurs actually could generate conditions conducive to lock-in. Potential examples include the development of fixed procedures for dealing with various contingencies and a preoccupation on selling the same products rather than attempting to meet new consumer demands.

Michael Thompson convened four sessions on the theme of "Land and Rural Society." The first focused on medieval Britain, with **Chris Dyer** (Birmingham) and **Jane Whittle** (Exeter) delivering papers on the crisis of the 14th century and the importance of serfdom. **Mark Overton** (Exeter) opened the second session, devoted to the agricultural revolution, with an historiographical review. Overton explains that a passionate debate has raged over the timing of and mechanisms generating any agricultural revolution, as well as the methods and data employed by the various protagonists. In response, Liam Brunt (Nuffield College, Oxford) called for more data gathering and microeconomic modeling to pinpoint the revolution, which he stated is best measured by movements in total factor productivity (TFP). Furthermore, Brunt noted that the real rent method is a valuable tool for tracking productivity trends over time, but the results are very sensitive to the assumptions employed. Last, Brunt voiced his concern about the quality of rent data that indicates substantial drops in agricultural TFP.

In the lively discussion that followed,

Gregory Clark (UC-Davis) and Avner Offer (Nuffield College, Oxford) contended that it is inappropriate for Brunt to criticize data on the basis that it shows large declines in TFP. Clark and Offer noted that TFP can change, as in cases where technology becomes obsolete or where it takes time to learn new working methods. John Broad (North London) supported Clark and Offer by providing examples of the current and 1860s cattle plagues, which suggest that knowledge may not be easily retrievable. In both cases the government of the time had "forgotten" how the previous outbreak had been combated. Thompson added that one value of the Agricultural Revolution debate was in teaching students about TFP.

Clark and Overton said that the time-path of the ratio of milk to hay prices, which provides some impression of conversion rates, does not indicate significant productivity progress in the livestock sector. John Chartres (Leeds) observed that a modern expert had been amazed at the net carcass weights of 18th-century, distillery-fed pigs, but Clark thought size was a dangerous measure of productivity. Clark remarked that the puzzling coexistence of rapid population growth with modest rises in farm output could be explained by a shift in production structure of English agriculture. By 1860, England was importing goods that had previously been produced by the home agricultural sector (such as cotton, coal, and timber) allowing domestic farmers to focus on food production.

The 17th century was the topic of the third session. **Joan Thirsk** (Oxford) reviewed the literature on early modern rural Britain over the last three generations. While much valuable work has been done, particularly on regional farming systems, poultry and fruit production and land tenure remain neglected areas. In her paper, **Elizabeth Griffiths** (East Anglia) considers the employment of sharecropping contracts in late 17th-century Norfolk and finds that the contracts usually were last resort transitory responses to farming difficulties. Eric Kerridge (Bangor) and Thirsk contended that the scattered instances of sharecropping in southern England they had encountered were often utilized, because (as Broad suspected) a family wanted to produce food as well as preserve their capital for nonagricultural trade. Paul Brassley (Plymouth) outlined the standard economic case against sharecropping – it halves the marginal revenue product, thus reducing incentives for farmers. For Griffiths, another disadvantage was that the landlord found it demeaning to be exposed to the tenants' demands and complaints.

The final session on land and rural society focused on another under-researched topic: British agriculture during the interwar period. **Ted Collins** (Reading) argues that although contemporaries saw an industry in crisis, agriculture actually was characterized by the highest sectoral TFP growth in the economy. Performance in the 1930s was superior to the 1920s, which was partly a function of state intervention and mechanization rather than agricultural science. **Peter Dewey** (Royal Holloway) then discussed the interwar market for farm machinery. After a postwar boom, the industry lost its export markets, and a process of cartelisation failed. From the late 1920s on, performance improved markedly due to elements such as the development of marketing boards and the introduction of protectionism.

Offer noted that the results of both papers depended on factors external to British farming. He thought that a variety of other economic and social elements needed to be incorporated into an analysis of the interwar agricultural depression, including improvements in human capital and the dramatic decrease in land prices. Other audience members stressed the need for disaggregation. Thirsk stated that alternative agriculture, such as fruit and vegetables, usually becomes prominent when the cereal sector declines. Collins confirmed that the depression fell unevenly across different products, regions, and years. The myth of crisis, as put forward in contemporary poetry and by the National Farmers' Union, was important in facilitating the passing of legislation favoring agrarian producers. Jeremy Burchardt (Reading) added that the most dynamic sectors in this period were those that were the least easy to mechanize, such as pig keeping. Dewey said that the labor saved from the purchase of tractors, for example, was probably redeployed elsewhere on the farm.

Paul David (Stanford and All Souls College, Oxford) closed the conference with the Tawney lecture, where he replied to the critics of his belief that the QWERTY keyboard layout is an example of suboptimal technological lock-in. Three principal areas of criticism had been raised. First, QWERTY would not have slowed the speed of touch-typing, because most typists used the four finger "hunt and peck" method. David argued that contemporary writings confirm that the keyboard was deliberately designed to reduce jamming. Second, the modern ergonomics literature indicates that the improvement with the Dvorak Simplified keyboard is small at best. However, this evidence is not relevant to the optimality issue, because modern systems embody past technological developments that have reduced the cost of defects embedded in the original designs. Third, the vigorous competition among contemporary typewriters suggests that the best model survived. Yet this selection process was not between keyboard layouts but the whole product; manufacturers advertised other features of their typewriters, including bells and the ease of inserting paper. David concluded that the QWERTY story has at least two wider morals: the dangers of lock-in suggest that there may be public value in the designers of products characterized by network externalities delaying their rush to market; and the current academic, legal, and journalistic discourses over QWERTY demonstrate the force history has in the modern world.

The Economic History Society will hold its next conference April 5-7, 2002 in Birmingham, England. For details regarding session and paper submissions, visit the Society's web page at www.ehs.org.uk.

Personal Reflections

Formal Estimates of Personal Income are Really Personal

By Lance Davis

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

Between November 3, 1933 and November 3, 1939, as I aged from six to 12 years old, my total income was higher than my father's. Although he was a highly skilled bullbucker, he was employed for only a few weeks over that period; on the other hand, I had a paper route. During those years I was introduced to the importance of per capita income. Over the next dozen years, between two stints in the navy (1945-48 and 1950-52), I managed to collect a bachelors degree in economics. By that time, Keynesian and post-Keynesian developments had led most economists to believe that violent short-term fluctuations in income were a thing of the past (it was another decade before they became disabused of this idea); but my government-funded visits to such ports as Tsingtao, Manila, Pusan, and Singapore had convinced me that there were still important questions concerned with per capita income and long term growth and development.

Economics, however, had changed. Developments in micro theory during the 1930s (John R. Hicks, Joan Robinson, and their contemporaries) had provided a set of powerful theoretical tools that permitted economists effectively to examine short-run behavior and even to suggest some rational government policies. As a consequence, economists became increasingly concerned with issues that could be attacked by the 'new tools'; and, as a result, institutional economics (typified by the work of economists like Thorstein Veblen, Clarence Ayres, and John R. Commons) – a field that had been an important part of the profession's research agenda – was pushed farther and farther outside the mainstream of economics.

Instead, economists tended to focus their attention on short run issues, in part because the institutional structure – the rules that are observed or enforced that govern the ways in which economic agents can compete or cooperate – can be treated as exogenous and fixed. When those economists have been unable to avoid issues associated with institutional change, they have tended to wave their hands and mutter something about transaction costs. They have, however, seldom attempted to explore the nature of those costs, although on occasion they have mentioned the word 'institutions'. Any economist who really attempts to understand the process of long-run economic growth and development, however, must immediately confront the problem of institutional change. In the long run, the institutional structure does change, and the changes are at least partly endogenous. Any successful long-run analysis must explicitly include assumptions about the nature of institutional development, but we still know little about the relationship between the institutional structure and the more traditional economic variables, or about the way changes in the external environment – economic, political, social, and cultural – affect the institutional structure.

In the mid-1950s, modern cliometrics was born. And, while a part of the work focused on using economic theory to explore historiographic questions (slavery, for example), some of the 'new' economic historians, following in the steps of Simon Kuznets, turned to questions of long-term growth and development. That group included Richard Easterlin, Robert Gallman, Douglass North, and William Parker. Increasingly, their work tended to focus on the role of institutions, but even these pioneers were slow to begin to think in terms of a theoretical model of institutional change. In principle, such a theory could become the basis for a model of long-term economic growth and development that would be as powerful as the present short-run microeconomic models. Much of what we do know about institutional change comes from the work of Nobel Prize winner Douglass North. To North, 'the economies of scope, complementarities, and network externalities of an institutional matrix make institutional change overwhelmingly incremental and path dependent'. Since 'the static nature of economic theory ill fits us to understand that process we need to construct a theoretical framework that models economic change'.[1] Although he clearly understands the nature of the problem, we are left with a warning, an admonishment, and a number of examples. Clearly, we are not yet ready to specify a theory of institutional change; however a taxonomy – a formal structure of classification and description – is a logical first step toward the development of any theory; and such a taxonomy is within sight.

What is needed in order to develop that taxonomy is a series of parallel case studies drawn from different times, different geographical regions, and different social and political structures. It is only recently that I have come to understand that, if there is any underlying structure to my lifetime research agenda, it has been an attempt to provide a number of such case studies in the hopes that, when combined with the works of others, it will yield such a system of classification and description. Applied micro economists, despite great gains in econometrics, still focus almost entirely on the short run. Experimental economists have produced some useful policy proposals, but almost all are based on experiments that depend critically on some form of an auction – and few laboratory auctions can be structured to cover a real time period as long as a decade, let alone a century. Only economic history provides a laboratory that permits economists to study those long-term changes in the structure of institutions and the implications of those changes for an analysis of economic growth and development.

Thus, at an intellectual level, what economic history means to me is that it is the avenue that may make it possible for me to contribute productively to a gradually emerging systematic analysis of the process of institutional change. Nor am I any longer one of a small number of voices 'crying in the wilderness' of existing micro-economic theories. The emergence in the past half decade of ISNIE (the International Society for the New Institutional Economics) and the list of distinguished economists who have joined (a group clearly not limited to economic historians) indicates that these important issues have again begun to move into the mainstream of economics.

At a personal level, economic history has, over the years, also provided the foundation for a number of friendships and intellectual partnerships that have made my life much fuller. There was (and is) the ever narrowing circle of the original cliometricians (Douglass North, Jonathan Hughes, William Parker, Robert Gallman, Richard Eastelin, and Stanley Engerman), then there was the second and third generation of scholars who are currently at the forefront of research (a group that includes, but that is not limited to the likes of Claudia Goldin, Ken Sokoloff, Jeff Williamson, David Galenson, and Naomi Lamoreaux), and finally there are students that I have helped to train (Jean-Laurent Rosenthal and Robert Cull, to name only two).

Thus, to me, economic history means both intellectual and personal warmth and fulfillment.

Lance Davis (b.3. 11. 1928) was educated at the University of Washington, Seattle and Johns Hopkins University. He has taught at the California Institute of Technology since becoming Professor of Economics there in 1968. He is currently Mary Stillman Harkness Professor of Social Science. He has published widely, mainly on American economic growth. His research interests are the evolution of international capital markets; the economic efficiency of naval blockades; and the impact of the microstructure of rules on the economic efficiency of formal markets (New York, London and Paris).

[1]Douglss C. North, 'Some Fundamental Puzzles in Economic History/Development'. Paper delivered at the Von Gremp Workshop in Entrepreneurial History, UCLA, February 21, 1996, pp. 7& 30.

A Two-Day Interdisciplinary Conference

Why Economic Growth? The Meaning And Measurement of GDP

This conference will be held August 30-31, 2001 and is sponsored by the Kingston University School of Economics, Faculty of Human Sciences, and the Centre for the Understanding of Society and Politics. Among the presenters will be Nick Crafts on Day One: Measurement and Measures and David Dollar on Day Two: Happiness and the Politics of Growth.

For booking details, please contact:

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Report on the All-Ohio Economic History Seminar

By John E. Murray, University of Toledo

(Columbus) On April 27, a beautiful spring day, economic historians from across the state converged in Columbus for the latest installment of the All-Ohio Economic History Seminar. **Ed Glaeser**, perhaps best known for his prolific work in urban economics, presented a paper that he wrote with **Andrei Shleifer** (both Harvard) entitled "Legal Origins." Attendance undoubtedly was bolstered by the announcement in that morning's *Wall Street Journal* that Glaeser had been short-listed for the John Bates Clark Medal.

Glaeser and Shleifer address an issue of great interest and familiarity to economic historians: the rise of efficient institutions. In particular, the paper discusses reasons for the development of common law in England and civil law in France. Common law is a legal system characterized by principle and precedent (such as the reliance upon *stare decisis*), while civil law depends on what are termed "bright line" rules as enumerated in legal codes (such as the Code Napoleon). Further distinctions appear in the structure of court proceedings. For example, civil law systems depend on a judge who acts as advocate and jury. Common law systems, on the other hand, depend on a jury to decide cases, although judges can adjust the magnitude of the penalty imposed on the wrongdoer. Glaeser and Shleifer note that in the early 12th century the English and French legal systems were similar in their reliance upon trials by ordeal or battle, but by the 14th century differences in civil and common legal systems had already emerged. So it is to the 12th and 14th centuries that we must look to discover the source of these differences.

The authors note that the fundamental difference in the two polities was the freedom of English juries to reach a decision without undue outside influence, whereas local French nobles who were beyond the reach of the crown could easily coerce a jury with threats of violence. The response of the French crown was to institute civil law with its "bright line" rules in order to remove legal discretion from subornable juries and maintain central regulatory power. Since no such problem existed in England, the English crown found no need to take discretionary power from local juries.

Howard Marvel (Ohio State) asked how French judges were "persuaded." Glaeser explained that murder threats were eminently credible and usually had the desired effect. Lars Sandberg (Ohio State and Uppsala) wondered how Sweden fit into this model. Glaeser noted the anomalous state of Sweden's most famous strong king, Gustavus Adolphus. Randy Olsen (Ohio State) suggested that the legal systems could have been driven by religious factors that were isomorphic to political structures. Glaeser remarked upon the usefulness of Scotland as a counterexample, a country with a civil law tradition that accepted reformed Protestantism quickly.

After the seminar, participants met at the Faculty Club for dinner. All look forward to the fall meeting at which David Landes (Harvard) will discuss world economic growth.

A Letter from the Editor

It is a lovely June day here in La Crosse. Sweat is dripping down my nose, and my fingers are slipping on the keyboard as I put the finishing touches on the summer issue of the Newsletter. I would like you to think my physical condition is a function of my hard work and toil. In reality, however, it is a very atypical June day in Wisconsin, with the mercury well into the 90s and the humidity level close behind. In a nod to California, the university has decided to cut back on air conditioning this summer, "browning out" several times each week. As luck would have it, those are the same three hours I work each week.

This issue debuts another new feature: personal reflections on economic history by cliometricians. These essays were originally published in *Living Economic and Social History*, Pat Hudson (ed.) Glasgow: Economic History Society, 2001. We have secured permission to run a selection of them, which we will do over the next several issues. I think you will find them interesting. They lend a more personal touch to economic history and historians than we normally encounter.

We continue our book preview with a look at *Altruistically Inclined? Evolutionary Theory, the Behavioral Sciences, and the Origins of Reciprocity* by Alexander Field. As usual, let me remind you that if you have a book forthcoming in the next year, please send an introductory chapter or overview so that we can highlight it in this feature.

This issue also features reports from numerous conferences, highlighted by a summary of the annual meeting of the Cliometrics Society, hosted this year by the University of Arizona. The conference was well-attended, featured intriguing research and lively discussions as well as the usual array of witty sayings and malapropisms.

A bit of somber news relates to this latter point. The Mullah, a regular attendee and reporter for the annual conference, has hung up his quill. After several years, he has turned his duties over to a younger, hipper equivalent. In this issue you will find A. Shrub's coverage of the territory once ruled over by the indomitable Mullah. Let us know what you think. He is on a short-term contract with an option for next year, so it's not too late to renegotiate before the big free agent frenzy this fall.

Let me close by once again thanking you for your comments, which allow us to improve the newsletter, and ask for your assistance in continuing to do so. Remember, we are always in need of reporters for conferences. If you will be attending a conference of interest to our membership and would be willing to write a summary, let me know. I will be only too happy to send you a set of the author guidelines. While we cannot pay for your services in cash, the warm glow you feel from having done a good deed should take you a long way. If that doesn't work, remind me of your efforts next time you see me, and I'll buy you a drink.

Calls for Papers

Economic History Society Annual Conference
University of Birmingham
April 5-7, 2002

The EHS Conference Program Committee welcomes proposals for papers in all aspects of economic and social history, covering a wide range of periods and countries. They are particularly interested in papers of an interdisciplinary nature and invite proposals for entire sessions (of 1.5 hours duration), as well as for individual papers.

For each proposed paper, please send (preferably by email) an abstract of 400-500 words to the local organizer:

Dr Katherine Watson
Department of Medieval and Modern History
Birmingham University
Edgbaston
Birmingham
B15 2TT
Email: k.e.watson@bham.ac.uk

For full consideration, **proposals must be received by September 28, 2001.** Acceptance notices will be sent to individual paper givers by November 2, 2001.

Economic & Business Historical Society Conference
Chicago
April 25-27, 2002

The Economic and Business Historical Society welcomes proposals for papers or sessions on all aspects of business and economic history, domestic and international. The host language is English. The president and program chair, Malcolm Russell, invites proposals for individual papers as well as panels. Requests for places on the annual program should include abstracts (no more than two pages) of the paper or papers to be presented, CVs, USPS and E-mail addresses, and telephone and fax numbers for each proposed participant. Graduate students and non-academic affiliates are welcome. **Please send all proposals by January 15, 2002 to:**

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