The Newsletter of

The Cliometric Society

July 1991 Vol. 6 No.2

Summary of Papers and Discussion from the 31st Annual Cliometrics Conference
Bloomington, Indiana, May 17-19, 1991

by Lee A. Craig, North Carolina State University and
Ken Snowden, University of North Carolina-Greensboro

The 31st Cliometrics Conference was convened in the Frangipani Room of the Indiana Memorial Union on a pleasant spring afternoon. Our hosts (George Alter, Fred Bateman, and Elyce Rotella) planned a stimulating program. The festive banquet on Saturday evening featured Fred as Master of Ceremonies with Marty Olney on keyboard and Don McCloskey on guitar.

Gary Libecap (Arizona) and Ronald Johnson (Montana State University, not attending) opened the conference with their paper on the relative decline of patronage workers in the total federal work force, a decline they attribute to the public's demand for better services. In contrast to previous studies that stressed the effects of changes in the types of government goods demanded and the influence of political reform movements on the relative decline of patronage, Johnson and Libecap emphasized the increasing inability of the President and members of Congress to benefit from growth in the patronage system. As population grew, the number of elected representatives and the demand for government services grew as well. Faced with the choice between an increase in the monitoring of patronage workers and slowing the growth of patronage positions, elected officials chose the latter. Johnson and Libecap went on to argue that, although the intention of the first federal labor force reform bill (the Pendleton act of 1883) was not to grant *de facto* lifetime tenure to federal employees, subsequent struggles between the President and Congress over control of the new merit-system employees led to legislation protecting these workers from dismissal.

The discussion began with Jeffrey Williamson (Harvard) asking if the experiences of other countries were similar to America's, and if the same process had taken place at the state and local level. Libecap responded the largest states had shifted from patronage to merit first, which was consistent with their analysis, but he offered no information on other countries. Adam Klug (Princeton), William McGreevey (World Bank), and Gianni Toniolo (UC-Berkeley) offered some information on the British, French, German, and Japanese cases. Price Fishback (Arizona) suggested it was important to document which jobs became merit first.

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What's Inside

NSF Proposals Encouraged on the Economic History of Global Change

The Division of Social and Economic Science at the National Science Foundation has established a new program on the "Economics of Global Change." These changes include the climatic and environmental changes that have been forecast as a consequence of continued industrial development and also the potential changes in the political, social, and economic structure of the world that might take place either as a consequence of environmental change or as part of an on-going process of social evolution. During fiscal year 1991 this program will add $1.2 million of new funding to the existing NSF economics budget, rising to $3.4 million in fiscal 1992, and perhaps $6 million annually thereafter.

The immediate problem facing NSF is to generate high-quality proposals. Two current members of the NSF economics panel, Elizabeth Hoffman and Richard Sutch, suggested that economic history potentially had much to say about the responsiveness of economic systems to global change and the capacity of the global economic system to absorb shocks of the magnitude envisioned by those warning of impending changes. Indeed, Jeffrey Williamson is the first economic historian to receive funding under this rubric; his project is on the evolution of world labor markets since the 1830s. Dan Newlon of the NSF organized a small task force of economic historians to assist in identifying a range of research topics that would be responsive to the new initiative. The task force met April 8, 1991 in Washington at the NSF headquarters. Present were Lance Davis, Elizabeth Hoffman, Peter Lindert, Joel Mokyr, Larry Neal, Richard Sutch, Gavin Wright, and Jeffrey Williamson.

A report from the Task Force on the Economic History of Global Change (May 15, 1991) was handed out at the recent Cliometrics Conference and is available upon request from the Cliometrics Society office. The contact at NSF is:

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The Cliometric Society Newsletter is published three times per year
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Editors' Request

We propose to publish lists of international academic visits by economic historians, so that otherwise distant colleagues might be reached for consultation or to present papers. Of particular interest are visits to North American institutions by economic historians from other continents, and visits by North Americans elsewhere. Please advise us if you know of such visits. We can announce in the October Newsletter those visits beginning early the next year (deadline October 1), and in the June Newsletter we can announce term or academic year visits beginning the following August to October. (deadline June 1)

An Interview with William N. Parker

Editor's Notes:
William Parker is Phillip Golden Bartlett Professor of Economics and Economic History, Emeritus, at Yale University. He was interviewed by Paul Rhode of the University of North Carolina, who first met Bill in 1987 when travelling to Estonia and Russia for a conference on a shared interest, agrarian development. Their connection extends back even further since, through Gavin Wright, Rhode is one of Parker's intellectual grandchildren. Paul says he has been inspired by the humor and literary quality of Bill's work, but above all by his logical and systematic approach to finding structure in complex phenomena, without losing sight of the humanity involved—a preoccupation manifest in much of the interview. Paul prepared a set of guideline questions, in consultation with several of Parker's former students and colleagues, and, in Chapel Hill in January 1991, Bill's responses were recorded not far from his UNC office of earlier days.

I've left you a list of questions ...

Let me talk first, in general, about the thrust of your questions, since they indicate, in my opinion, certain misunderstandings of my misunderstanding of myself. Then I can also comment on some of the specific things that you ask about, especially regarding studying and teaching economic history, what is it and how do I think you do it, both individually, like an old-fashioned scholar, and jointly with like-minded, and sometimes rather different-minded, colleagues in the Cliometrics clan.

The questions are good questions but I almost think they take me too seriously. You may say that that is not for me to judge, but recently I've become conscious that this question of how I take myself, and how I present myself, has been a problem for me all along. I have an instinct to want to seem to underplay things I feel deeply about—including myself. I have wanted to seem to take myself not quite as seriously as one is expected to. A few people have told me this, especially women. Women generally see through a self-deprecating pose, but men, since they view you as a potential competitor, generally are glad to take you at your word. Once, when I was making some irreverent remark, Claudia Goldin said to me, in exasperation, "Don't you ever take *any*thing seriously," and I said, "NO." I mean, what else could I say when directly challenged like that?

My answer may have puzzled her. But at the root of it was a kind of sense of irony, and a self-consciousness that seems to be built in me. I am fascinated with observing myself and observing myself observe myself. I'm doing it right now. Still, of course, one has to learn to carry on simultaneously, on another track of the mind, some objective and impersonal discourse. It is impossible to see one's self as one truly is, or even as others perceive it. But I do think that I am in some sense more personal, more psychological in approach to life than many of my colleagues. I like to look at individual people and really get quite interested in them. I try to learn both about them and about the world and human nature from their viewpoint, and I try to learn to feel empathy.

You can only come to know another person, or yourself, through love and sympathy. Certainly, I am immensely interested in learning about myself, and through that, about other people. But whether my sympathy is in the service of my curiosity, or the other way around, I've never been sure.

Teaching and other duties

Has this self-awareness affected the relationships you have had with students? Could you talk a little about that and about them?

Well, some people say to me, "Your real contribution has been your students," and that always nettles me slightly. Of course I'm very proud of my students. But, Damn! their success might also have to do just a little bit with the content of what I have had to teach them. I consider them all my friends, and I have a very high regard and respect for their individual qualities. But they didn't come to all their views and values just by themselves. Still, I do think that Ph.D. students are not only responsible and trustworthy but also much more inspired than they get credit for.

The director of a dissertation mainly has to help a student to find a topic that taps into his own background in some obscure way and draws on what I call the emotional sources of his research energies. After a student had gotten into his work, I would read his drafts carefully, but I hardly ever made any suggestions until the student had the thing in the bag and was ready to tie it up. Then I would jump in. This way I didn't risk the danger of crushing what may have been some precious insight by premature criticism And I found that by listening, I was capable of learning something myself. It made for an effective, personalized and respectful teaching and learning experience for us both. And I don't mean this just for the "best" students, because they are all good in some sense, if they survive. (If they don't survive, they are also good, conceivably, in some more important sense.)

Yes, for variety and intrinsic quality, independence, and strength, I think that the body of students I've had, they are just the best, and certainly to me that is indeed a major satisfaction.

It's a compliment that you paid to Frederick Jackson Turner that his impact was so strongly felt on and through his students.

Turner's students were, as I said, like the sons or tenants of a great land owner, spread out over the landscape. But I don't quite see myself as the founder of a school, though on the several occasions when a group of the ex-students has come together to read papers, some of them note some common features, some resemblances in approach, emphases, and attitudes toward the subject.Whether this is the result of teaching or of natural selection, I'll never know.

Let me speak a bit too about other levels of teaching—lectures, and seminars, and small classes, undergraduate and graduate. Some years after I had come to Yale, I learned that one of the letters of recommendation for me had predicted that I would be a better teacher in small seminars than in a large classroom. That shows, I think, just how wide of the mark the recommendations we give one another can fall. I've rather enjoyed lecturing and the bit of showmanship that goes with it. I don't say that I'm one of the great performers, but a lecture can give you a real thrill when you can see that you are getting it across. I like public speaking. When I was a kid, my mother had me given "elocution" lessons, declaiming poetry and purple passages from the great orators. And in high school I was "orating" all the time in the student council or before the school assemblies. But in college I had virtually no opportunity to speak. I can remember only once—the time when I got up on a bench on Boston Common and made an impassioned speech for F.D.R., in 1936. On the whole, my career as a public speaker died out with the end of high school, until I came later to give lectures in class and papers and comments at professional meetings—oh, and at faculty meetings now and again. Just before I leave a place, I seem to reveal an instinct to go for the jugular of the President.

Of course, Williams, Carolina, and Yale have all been wonderful places to teach. In none of those places were there big 200- to 300-student classes—at least in economic history. The largest was 90 in the undergraduate course at Carolina. Ordinarily, it was 35–50 there, and at Yale. At Carolina and at Yale, at least half of my teaching was in the required graduate economic history course every year. That's a very different ball game from undergraduate teaching. But even in graduate teaching, I am much more comfortable giving a lecture than trying to lead a discussion. I've found it very hard to say "provocative" things—things I don't think are true—just to get a discussion stirred up. In seminar teaching to a small group, I've not been very comfortable either. I've been most comfortable talking to individual students or, in another mode, in making a public address, rather than in that half-formal, half informal atmosphere of a seminar. If the students aren't prepared or haven't read anything, they just sit there, and I end up lecturing anyway, out of sheer boredom. The trouble is, I think, that I don't like to enforce discipline on other people, making sure students do their reading, quizzing them about it and embarrassing them. That goes against my grain, except in the relative privacy of an oral exam. But my personal approach doesn't always make for an effective class.

The work with the graduate economics students in the required two-term courses in economic history at Carolina and at Yale succeeded, I think, by and large, because it was the students' only exposure to topics with any breadth or much relation to the other social sciences. Some suspicious, ultra-scientific students, carried away by the beauties and rigor of mathematical theory, claimed to find History repellent, loose, and sloppy. No doubt they found real life that way, too. You can't just be smart in economic history; you have to know something, too, so you have something to be smart about. The really strong students liked the freedom it gave them to speculate. It was a kind of therapy for them—a relief from their immersion in theory, especially as the applied courses got more and more *un*applied and more theoretical. I had the support, too, on the faculty, of several other Harvard-trained members of the Yale Department who were sympathetic to economic history in the style we had learned it from A.P. Usher. And Gus Ranis and John Fei in economic development and Joe Peck and Dick Nelson in I.O. bought my stuff. But I felt that the field also had the respect of the mathematical theorists, and of the "old Europeans" in the Department—Fellner, Triffin, Koopmans, Wallich, Goldsmith—and also Mike Montias, of course.

With the Ph. D. students in economic history itself, the ones who wrote all the good dissertations, I was helped by two special lucky circumstances. When I took the Yale job, John Perry Miller, the dean, had instituted a special graduate program between economics and history with half a dozen fellowships from an HEW program to support this idea. So right off I got some very good students—George Grantham in Economics and Jan De Vries in History, for example. There were ten or so of them altogether. That took me through the 60s. Then as that ran out, a second "wave" came along as a result of the unrest and dissatisfaction with standard economics that many students felt in the late 60s and early 70s. Yale had hired one of our own Ph.D.'s to teach the course in the History of Economic Thought—David Levine. He was tough and rigorous, but he explained to students—some of them hearing it for the first time—that there was something out there called Capitalism, whose history could be subjected to analysis. Unfortunately, but inevitably, I suppose, for one whose thought was cast in so Hegelian a mode, he failed to get tenure. When he left, I fell heir to four or five of his students—tough, brilliant, ambitious, independent-minded scholars. I take credit for guiding some of them to some hard-headed, empirical work in their theses. Both these groups, and a number of some of the most able students who came one by one, were wonderful material including the several who found notable careers outside the university. When I retired in 1989, there were still four in the pipeline, of whom two came out with theses and jobs this year, and the other two, who now have excellent jobs, have still only a couple of months' work (I hope and expect) to go. I should mention also at this point the excellent assistant professors whom Yale appointed to work with me in these years. Their names are, I think, well-known, and need no boost from me, but only heartfelt thanks and appreciation, both for their labors and for their personal friendship. I feel that I've been a very lucky guy all round.

Out of my twenty-five years at Yale, I cherish, too, the work I did as Director of Graduate Studies. I held the job off and on for about ten years. It gave me a deeper relationship with all the students, whom, of course, I had already had in class. What a fascinating array of intellectual, social, financial problems they had! And, since no one else wanted the job, nobody would, or could, lay a finger on the graduate Economic History requirement while I was in charge of graduate studies. The relation with all those students, year after year, was very rewarding, and the secretary, Eleanor van Buren, was a wonder in human relations. Yale seemed to me to be a very happy program in those days.

A world before Economic History

How did you come to be an economic historian? What attracted you to the field?

Well, I'm afraid you've let yourself in for a little miscellaneous reminiscing concerning the rather tortuous path that my life and ambition took me down in the years between college graduation in 1939 and 1955, at which time I left Williams for Carolina. It was at Carolina that I began the really concentrated work and career in economic history as a life-long "affair."

In college my humanistic bent won out over social science. It was touch and go. My interests were about evenly balanced between history or politics on the one hand, and literature on the other. But I did enjoy the aesthetic experience of reading literature. I remember a course in seventeenth-century French drama: Corneille and Racine. I'd recite the speeches aloud—probably in an execrable accent— enjoying the music of the language, even on such an imperfect instrument. I suspect that that side of me indulged itself the more because of the almost utterly inactive social and emotional life at Harvard, then a wholly male institution. There were no girls around and I had not gotten to thinking—well ... THINKING, YES. I think a young guy thinks about girls most of the time, but there were no opportunities to think of what was called in those days the "opposite sex" in any objective, concrete way. I was wholly innocent of all that when I was at Harvard. And I didn't have a radical disposition that might have given me some public emotional outlet. I just shut myself up with books and was a good boy. I would get spells of adolescent melancholy, that kind of sweet romantic sadness that comes, I suppose, from frustration. But that did not give me any concept of rebelling at all. The middle class format gave me enough leeway to express myself, and it was all I knew. I was a liberal democrat, but in those days, that had not come to be considered radical. I have gotten more radical as I've gotten older, while the country has gotten more conservative. I see how society shapes young people and how it can oppress or release them.

In college, then, I had this big dilemma about what to major in—political science or English. I followed my heart, I guess. There was all that literature out there that I wanted to read, and this was the easy way to read it. I also wanted to "write"—essays, creative stories, no poetry. I polished my writing skills in a certain classical style pretty far. Dr. Johnson had advised that to develop a beautiful style, a writer should give his days and nights to the study of Addison. So I read the *Spectator* papers and tried to imitate them. I got up at six one term to write out translations of Cicero, just to dissect his style and develop my own. It worked to a certain degree. I've always had great pleasure in working out expository prose. This well served my interest in politics and history in college. But I had a genuine love for literature as an art form. Still, I finally came to feel that literary analysis was a problem either in sociology or in psychology. I couldn't see any way between these that would give any criteria outside of personal taste. In the end, it was not an aesthetic impulse but a kind of socio-scientific instinct that I couldn't satisfy through literary studies.

In 1939, on graduation, I had my fellowship renewed for any graduate school or department at Harvard that would admit me, even Law or Medicine. I went to the chairman of the English Department, a man of the wealthy, gentle-scholar type of that era. I remember telling him that if I went on in his graduate department, I would eventually want to teach English at the high school level, and get into educational administration. This shocked him, I could see, and he told me coldly that for the Ph.D., I would first have to study Anglo-Saxon. So I went over to the Harvard School of Education and talked to a notable educational psychologist. He told me that before long he would have me in the laboratory, testing rats. I didn't want that either.

But when I went shopping to the Economics Department, I came under the spell of John D. Black, the "dean" of agricultural economists. He was a big, heavy-set, rotund man, from Minnesota, and a Jim Farley type of politician. He saw that I was a skinny, idealistic city boy, and he put his arm around me and painted before my eyes a picture of world agricultural development, and described how I could contribute to it. That hit my weak spot. I had always asked myself, "Where is all the world's poverty?" and had answered, "In India and China, among the teeming masses of peasants." Agricultural economics seemed fundamental to every other world economic problem. Agricultural fundamentalism is very strong in me. It was not the virtues of its way of life, but its basic position in economic development, and in the economic history of earlier ages that attracted my attention. I don't know where in the hell this belief came from, because in Ohio I grew up in a city of 300,000. I had no relatives on the farm except for one uncle. Just maybe I had this impression about farming simply because it is true. Agriculture *is* basic to the problem of poverty and social order in most of the world. This fact came to the surface in my thinking again in the 50s, after the war and the short-run post-war concerns had begun to recede from immediate view.

I passed my Ph.D. general exam in the spring of 1941. I did okay for a guy who had been a college English major. With the draft already on, several of us figured we would be drafted soon for a year's service in the army (this was before Pearl Harbor). So instead of registering to begin my dissertation, I took the civil service exam for junior economist, and went to Washington on a government job in the summer and fall of '41. Jim Tobin and I and some other fledging economists were hired by the Civilian Supply part of the OPA. Each of us was given an industry to plan a program to control its output, so as to cut down its demand for steel. I was given the commercial refrigeration and air conditioning industry, and I really had a wonderful time. I was 22. I had the vice presidents of Carrier, Frigidaire, coming in terribly worried, and treating me with great deference. Then one morning in November, 1941, a month before Pearl Harbor, the Greetings came from the local draft board back in Columbus. I resisted as best I could, but the chairman of my draft board, who used to live across the street from us when I growing up, said I was an over-educated ass who had had too much Harvard, and the Army would be the best thing for me.

What happened next?

I went in and stayed for four years. But mid-way in 1943, I escaped from the real army through the good offices of my old college buddy, Ruggles, who was in the OSS office in London. He had a project to estimate German production of war materials from the serial numbers on the captured equipment. I was in London, then in Paris and Germany, responsible only to Ruggles and General Eisenhower. I traveled all over Normandy and Alsace in a jeep with a couple of enlisted men. Our job was to get to the knocked out equipment after a battle and copy down the markings. After the war, we went around the factories to see how close we had come. We came very close, within a few percent for individual items—guns, tanks, trucks, even buzz bombs (V-1's) and rockets (V-2's). Of course, we got the information too late to do much good. There is an article about it in the *JASA*. But the job gave me some interesting war experiences. They were spiced with an occasional bomb that made me feel like a soldier, but it was a very easy and interesting life.

After the war, for about 9 months in 1946, I went to work on Capitol Hill for the Senate Committee on Atomic Energy. That too was an exciting year. I was a major by that time and I still had to wear my uniform because you couldn't buy white shirts. It gave a minor advantage in my first effort at really seriously courting girls. Then the one I became engaged to went off to the United Nations and ditched me. That left me in great despair, and in the fall of 1946 I went back to Harvard to try to write a thesis on the Atomic Energy Act of 1946. But I was too much of an economist by then to take up a political science topic. I just couldn't do it. I'd lie in bed hearing the college bell ring every hour, feeling like a freshman all over again. My morale was just miserable, and it showed in all sorts of ways.

So I gave up and took a research job back in Washington with the State Department in January, 1947, so as to build up to another thesis topic. Stuart Hughes, the intellectual historian, was the chief of the division—a lovely man. The section was also staffed by various émigré scholars—Herbert Marcuse, the famous radical philosopher, was the chief. I always felt that he considered the economics division, which I was in, to be very dull and pedestrian. I wrote some studies on the different Allied zones of Germany—the French zone, and one on the Russian zone on the basis of intelligence reports. I was getting a certain reputation for that in other offices of the Department. But I still felt I wanted to get that thesis done. And when I met this exciting modern dancer with the French name—Yvonne—in the elevator, I really wanted to marry her, and, after a tolerably brief period, I found, amazingly, that she would marry me. At the same time, I used the techniques of economic decision-making to take a long look at economics, and decided my comparative advantage lay in economic history, or possibly in industrial organization. I had been very interested in the post-war economic settlements in Germany, both that of Versailles and the one I saw unfolding around me in the State Department. Back at Harvard, Usher encouraged my idea for a study of the German coal and steel complex in the 1920s. So in 1948, I got married and took off on an SSRC fellowship, supplemented later by a Fulbright, to stay in Paris and Essen for two and half years to do a thesis. We lived in Germany practically free on the occupation economy, in the old Krupp hotel, the *Essenerhof*, and were fed on British Army rations—miserable food, but served with great elegance by the German head waiter, in tails and with a sneer on his face, and we had the use of an Army jeep and driver for taxi service. I was able to bank the G.I. Bill stipends for a nest egg for after I got home and so came back several thousand dollars ahead of the game. It must been one of the few cases when anybody got a bit richer while writing a thesis.

I see in retrospect I really didn't approach the whole job quite right. Usher 's teaching had emphasized raw materials, natural resources, technology; he came at things from the ground up. So I spent a lot of research time unravelling the technical details of coal as a commodity, and its markets. I worked, too, with the structure of the Syndicate and its relation to the steel combines. But I took too physical, too engineering a view of the whole thing. Looking back, I can see, as one does with one's parents, Usher's decided influence, and—I would say now—not all for the good. I never was a student who worshipped a professor, and Usher was not the sort of professor who sought disciples. He was modest in excess, if anything, and rather dull as a lecturer. His personal relationships were couched in an old-fashioned formality. But there is no discounting the power on me of his ideas and his values. They were absorbed like a dye or a disease for which I suppose I must have had a receptive predisposition. Gerschenkron, who had succeeded Usher in 1948, allowed me to pass my final oral in May 1951.

I went to teach at Williams for five years for Émile Despres, a man who had many of the qualities of greatness. Then in 1954 I was invited by the geographer, Norman J. G. Pounds, to write the last half of a historical study of the European coal and steel industries. It started me on a long career of writing on invitation. In fact, there are only a very few pieces in my bibliography that were not done more or less at somebody's request, or as part of a larger project, sometimes one of my own devising. I like to develop my own ideas, but within a structure of other scholars. Where such a structure did not already exist, I

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Clio Conference *(continued from p. 1)*

Tom Weiss (Kansas) and Lou Cain (Loyola and Northwestern) questioned whether patronage workers were actually less productive than merit workers. Ken Snowden (UNC-Greensboro) added that patronage workers' incentives should have been better aligned with the politicians' and that Libecap and Johnson's analysis suffers by not examining the problems of monitoring a "professional bureaucracy" who may have had their own agenda. Libecap pointed out that, when merit exams were introduced, a large proportion of patronage workers failed. Don McCloskey (Iowa) suggested Libecap and Johnson may actually be seeing a rural-urban dichotomy, since it was more costly to monitor urban workers. Larry Neal (Illinois) noted the paper failed to address the feedback from the federal employees themselves and the effect the Hatch and Pendleton Acts had on their political activities. Lee Alston (Illinois) suggested the benefits of patronage should have been unevenly dispersed among political mavericks and party seniors, and the voting patterns on these issues could be analyzed with this in mind. Susan Carter (UC-Riverside) wondered if the real story had to do with the increasing frustration of big business at the poor service provided by patronage workers, which worked against business' attempt to increase productivity. Libecap claimed "all" business supported merit systems. Michael Bernstein (UC-San Diego) argued the conflict between national and local political parties might explain much of the contemporary debate on patronage.

Jeremy Atack (Illinois) and Barry Eichengreen (UC-Berkeley) noted the large number of abstentions in the analyzed votes and asked if the empirical work could be modified to include them. Edward Saraydar (Western Ontario) brought the discussion back to where it started by asking if either supply- or demand-side arguments alone could explain the shift from patronage to merit. Libecap responded that their supply-side argument was attractive primarily because it explained the timing and specific contractual aspects of the shift.

Mario Pastore's (Miami-Ohio) paper explained the evolution of slavery and serfdom in colonial Paraguay as a result of the rent-seeking behavior of Spanish settlers and the Crown. Pastore argued slavery arose because labor was the scarce factor (land being abundant), and, for colonization to continue, a system of property rights had to be established that permitted the settlers to extract rents from the indigenous population. Employing the fisheries model, Pastore argued serfdom replaced slavery because the Crown wanted to stanch the decline in tax revenues that resulted from the depletion of the indigenous population. Finally, the presence of an indigenous population that could not be legally held in servitude, the Spanish-Indian *mestizo* population, led to the growth of a free peasantry. Thus, the simultaneous existence of slavery, serfdom, and a free peasantry was the result of the state behaving as a discriminating monopolist with respect to the assignment and enforcement of property rights over labor.

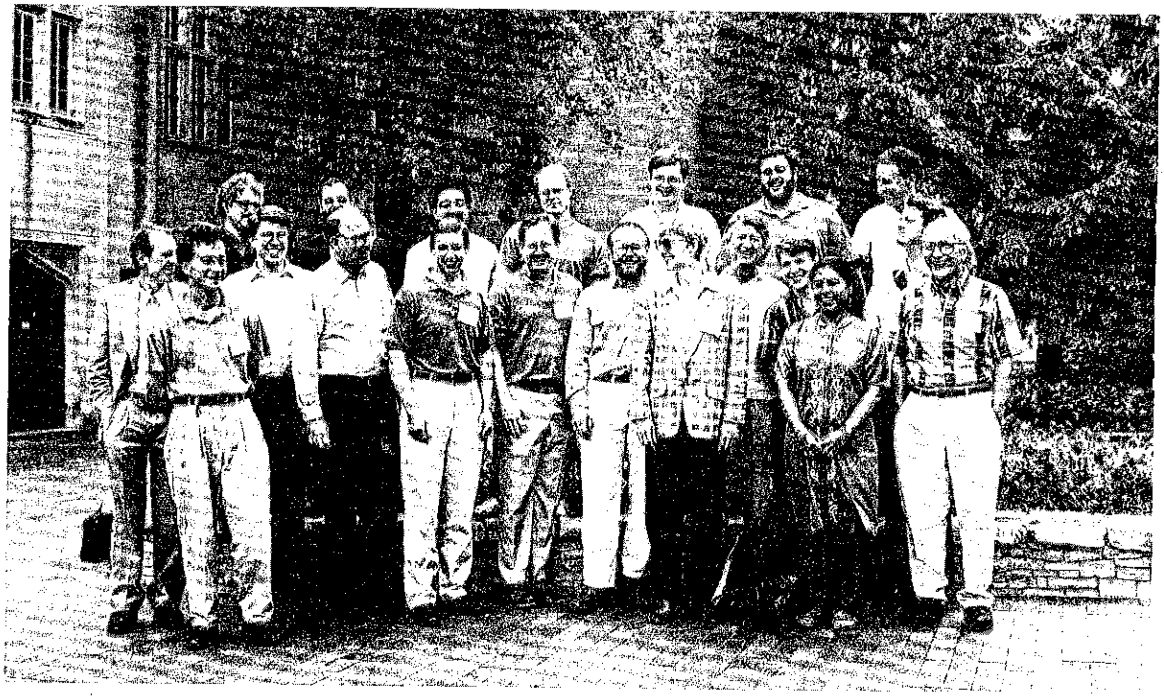
Much of the discussion centered on Pastore's use of the "overfishing" analogy to explain the demise of slavery and the subsequent rise of the *encomienda*. Elizabeth Hoffman (Arizona) and McGreevey asked if the epidemics among the indigenous population after the arrival of the Spaniards did not provide a better explanation of the population decrease. Pastore replied it was the differences in population decreases across labor regimes that provided the most convincing evidence for his argument. George Alter (Indiana) conjectured the violence used in slaving parties was responsible of the decrease in population and not "overfishing" per se, while Nancy Breen (Connecticut College) suggested the fertility behavior of enslaved Indians was the operative mechanism of depopulation. Alston and Sumner LaCroix (Hawaii) challenged "overfishing" on conceptual grounds since the Spanish government should have been able to internalize the costs of depleting the population. Minoti Kaul (Indiana) claimed Pastore should refer to a common "pool" rather than "property" problem since the Indians were humans, but Pastore responded the Indians who did not convert were considered as heathens and property of the state. Michael Haupert (UW-La Crosse) added this made the Christian/non-Christian dichotomy crucial to the argument.

The second theme which dominated the discussion related to the locus of decision-making power during the transitions Pastore described. Charles Miles (Northwestern) asked if Domar's model fit the Paraguayan case since the model implicitly assumes the state has a monopoly on violence. Pastore noted, in places or times where the Spanish Crown had no authority, there was no pattern of change in labor regimes. George Grantham (McGill) wondered if the transition in labor institutions was specifically designed to accommodate conditions in Paraguay. Knick Harley (Western Ontario) and Roger Ransom (UC-Riverside) agreed the individual entrepreneur was missing from Pastore's story, and thought the transitions might be more profitably thought of as "bottom-up" in origin rather than "top-down." Also John Hanson (Texas A&M) noted Pastore's account treats the Crown as an "economic man" without providing evidence in support of such a treatment. Pastore agreed, but noted a plethora of evidentiary material in the form of pronouncements by royal administrators. Leonard Carlson (Emory) asked for additional quantitative information regarding the relative numbers of Spaniards and Indians. Alan Dye (Illinois) wondered if part of the explanation may not be technological, and asked if there were differences in the types of crops raised under the different labor forms.

Charles Miles showed why giving a paper at Clio is a tortuous ordeal; his paper argued that trial by ordeal, as well as outlawry, blood money, and feuds, have been employed commonly throughout history as legal institutions when a central government could not effectively enforce property rights and codes of conduct. Miles argued practices such as these can be explained as rational, equilibrium strategies chosen in a repeated prisoner's dilemma game in stateless societies. Using the tools of the "new institutional economic history" he analyzed these particular practices and institutions because criminal proceedings have been thoroughly documented throughout history. He argued they were important because they present opportunities to discover "smoking guns": explicit rationalizations for the implementation of criminal sanctions and incontrovertible evidence concerning the circumstances under which sanctions were applied. Miles also noted the evolution of legal institutions to enforce property rights can be fundamental to economic growth.

Libecap and Alston began the discussion by complaining the paper had little predictive value. Miles agreed to an extent; however, he claimed there were some broad predictions from his paper, and, furthermore, the data prohibit hypothesis testing. Martin Spechler (Indiana) felt Miles dismissed religion too quickly as an explanation of some of these institutions. Miles replied he was trying to show there was no conflict between religion and rationality. Mary Gregson (Illinois) asked how Miles could explain change in social institutions using his model. Miles replied he was not trying to explain such changes. Pierre Sicsic (Harvard) and Miles then conducted an extended debate on the significance of the year 1215 as a turning point in the decline of ordeals.

Eichengreen, Grantham, Fishback, and Pascal St. Amour (Queen's, Ontario), each for different reasons, questioned whether a game-theoretic approach was appropriate for explaining these institutions. Miles granted game theory was limited, but he felt it was appropriate in this case. Richard Sutch (UC-Berkeley) questioned the relevance of crimes of violence to economics. Miles argued criminal behavior was relevant and again noted data on economic crimes were not available. Hoffman argued the paper confused different types of crimes, and Bernstein felt Miles needed to emphasize why punishment in stateless societies was more severe than in those with states. Miles replied that he was looking into these questions in other work. John Murray (Ohio State) and Carlson questioned the use of feuding as an example, particularly in England, and Miles said he planned to deemphasize feuds in England. Ransom concluded the discussion by asking in rapid succession, "What is a state? Who is the state? What is being enforced?"



Those attending a U.S. Cliometrics Conference for the first time were:(left to right, rear row) Adam Klug, Pierre Sicsic, Swamy Anand, Ilkka Nummela, David Prohovsky, Sumner LaCroix, (front row) Bertrand Roehner, Pascal St. Amour, Charles Miles, John Hanson, Neil Quigley, Werner Troesken, John Treble, Nancy Breen, Bill McGreevey, Va Nee Van Vleck, Minoti Chakravarty Kaul, Paul Rhode, and Edward Saraydar.

John Treble (Essex) and S.J.R. Vicary (Hull, not attending) attempted to identify the sign of the relationship between effort and wages for late 19th century British coal miners. Employing a model of profit-maximizing firms and utility-maximizing workers, Treble and Vicary showed the (Durham) County Average System (C.A.S.), which set different prices per ton of coal at each seam (so that earnings stayed within five percent of the average wage across the entire field) and "cavilling" which randomly assigned workers to different seams for three months (via a lottery), would have been an effective means of equalizing pay rates for differences in the physical quality of coal fields only if the supply of effort was totally unresponsive to changes in the wage rate. They concluded there existed a negative relationship between effort and wages, and this supports the proposition that coal miners in this era had a downward-sloping labor supply curve.

Fishback conjectured the C.A.S. was a market institution to pay a standard compensation difference, while cavilling was a firm-level adjustment. Eichengreen noted the cavilling system meant that workers had the incentive to continue to work hard when assigned to a good seam even when the price of coal was low. In its absence, all workers collectively might have the incentive try to raise market price by restricting output. LaCroix and Alston suggested cavilling might best be thought of as a randomizing strategy to prevent the shirking encouraged by the C.A.S. Treble promised an investigation of the issue would be forthcoming.

McCloskey noted monitoring would have been required to assure the quality of any seam, and asked why workers were not paid a flat rate, then left free to find and work the highest quality seam. Claudia Goldin (Harvard) brought up the "ladder" system (the best workers were rewarded with the best seams) Fishback has described for the U.S. Treble explained these differences might be explained by the information workers had about seam quality in the two countries; he noted cavilling was actually equivalent to a sub-contracting system where property rights were assigned temporarily. Carter asked for more specifics about the history of the regions and communities which used these institutions, and whether any informal institutions existed to regulate work as well. Treble noted the pits and communities were geographically isolated, and the work force was fairly stable in composition. He cautioned, however, the pairings of workers were not stable over time, and little was known about how the pairs divided their earnings.

The evidence presented by Treble and Vicary was questioned on several levels. Fishback argued their regressions measured compensating differences rather than effort. Martha Olney (Massachusetts) noted the efficiency wage hypothesis can be tested only when variation in non-labor inputs can be controlled. Lee Craig (NC State) asked if any narrative evidence could be found to support the paper's conclusion, and Elyce Rotella (Indiana) asked if the workers' behavior was consistent with this conclusion. Treble admitted the evidence was weak in both cases.

Nancy Breen's paper tested the claim that protective legislation for female workers was discriminatory because it tended to lower their employment in male-dominated (typically high-wage) industries and, possibly, to lower their wages in female-dominated industries. Employing biennial data for San Francisco between 1890 and 1922, Breen tested the effects of the California Hours Law of 1911 on the employment share of women and real wages in covered industries. The author rejected the hypothesis the law significantly reduced the employment share of women in non-union industries. In addition, she rejected the hypothesis the law lowered mean real wages of women in either unionized or non-unionized industries. Breen concluded protective legislation did not adversely affect the labor market experiences of women, but this result was specific to San Francisco, a city in where a large number of industries were unionized as a mechanism for whites to exclude Chinese labor.

Libecap and St. Amour began a spirited, and largely skeptical, discussion by objecting to the use of a dummy variable to measure the impact of the legislation— the former because it ignores the dynamic aspects of the labor market adjustment, and the latter because no evidence is given the law was a binding constraint. Neil Quigley (Western Ontario) said the regressions apparently suffered from problems with autocorrelation and multicollinearity, and Hoffman noted the logistic approach should have been used in the share equations. Hoffman concluded the analysis cried out for a structural model. Rotella supported Hoffman and emphasized it was likely some women were helped and others hurt by the legislation.

Atack complained Breen had used industry-level data to measure firm level responses, and Williamson noted the evidence was about weekly wages although the law concerned hourly wage rates. Harley and Fishback wondered why the industries had been categorized according to whether the female share of the labor force was above or below a 23% cutoff. Breen responded these choices were dictated by the availability of data. On that very point, Weiss suggested Breen use Christina Romer's unemployment series, as well as Lebergott's, to see if it made a difference. Paul Rhode (UNC-Chapel Hill) suggested information about San Francisco's business cycle is available for Breen's sample period and could be used to control for labor market effects. Breen promised to act on both of these suggestions.

Carter set the tone of the balance of the discussion by calling for more descriptive information. Pamela Nickless (UNC-Asheville) noted Breen needed to connect her discussion to the protective trade movement among women's groups. Alston asked for more specific details about the jobs covered, and Olney wondered if the labor force sex ratios mirrored those of the general population. Alston also encouraged Breen to examine the political groups for and against the legislation, and Kaul asked Breen to document the conflict among women concerning the legislation. Cain felt the Supreme Court's role in defining "freedom of contract" had not been adequately treated. Consequently, Breen overemphasized Mueller vs. Oregon, a case which is an exception to other decisions in this era. David Prohovsky

(Indiana) closed the discussion by suggesting Breen investigate Florida as a benchmark case since hours legislation had never been enacted there.

Pierre Sicsic's paper examined the city-farm wage gap in 19th century France. Sicsic distinguished between the "reluctant farmer" hypothesis, according to which French industrial development was choked off by a scarcity of labor, and the proposition French rural workers' migration was sensitive enough to wage differentials to supply their services adequately to the industrial sector. He used wage data collected by the French government to analyze the issue and reported small wage gaps (relative to England) between city and farm in mid-19th century. Towards the end of the century, wage gaps increased (to about 25%), but he argued this was driven by the agricultural depression. Sicsic concludes this evidence clearly supports the proposition that labor market imperfections were not a constraint on French industrial growth.

The discussion of the paper began with questions concerning how representative and reliable the wage data were. Harley was surprised Sicsic's data show farm wages were higher in many areas than urban wages, and Va Nee Van Vleck (Iowa) questioned the reliability of the mayors who reported the wages to the French government. Sicsic noted he had used wages from relatively large labor markets to avoid outliers; Grantham observed that the mayors were often chief landowners, who would have been knowledgeable about local labor market conditions. Joseph Ferrie (Chicago) remarked payments in-kind often represented 20% of rural wages in the U.S., and wondered if there were thus a serious downward bias in Sicsic's rural wage data. Sicsic responded that ignoring perquisites would have lowered his rural wage series, and made the gaps larger than they actually were, thus reinforcing his conclusions.

Harley asked if Sicsic's landless labor class was really a potential pool of migrants, or if it was composed of sons waiting for the father to die. Sicsic claimed the group was too large and stable a percentage of the rural population to be explained in this way. Prohovsky noted average wages over heterogeneous groups may not give a reliable picture of the wage gap faced by the "marginal migrant," and Sicsic acknowledged the extent and form of aggregation was a crucial assumption in his work. Neal inquired about the extent of seasonal employment in France relative to England, and Toniolo remarked the number of days rural workers were employed in France (276) was close to full-time employment in Italy. Klug noted 1852 was a special year in which urban workers were facing political oppression, and Sicsic should be cautious in using these data.

Several participants were concerned about the role of international trade and protection on agricultural wages. Grantham wondered if Sicsic was treating England and France as closed economies, while Harley noted a wheat tariff affected French agriculture throughout the 19th century. The discussion also reflected concerns about Sicsic's comparison of French and English wage gaps. Jacob Metzer (Hebrew) suggested absolute wage gaps should be reported as well as relative ones, since the former better reflect the incentives to move. Harley asked for a more thorough description of the English wage series. Toniolo and Snowden urged Sicsic to clarify his position on France's industrial success (relative to England) to complement his comparison of the two labor markets. Breen wanted to see more on the institutional differences in the two markets (such as enclosures). Cain, reflecting on recent work by John Nye, closed the discussion by suggesting a comparison of the returns to industrial and agricultural capital would be useful.

In her paper, Linda Barrington (Illinois) constructed poverty lines and rates for 1939 using the methodology employed by Mollie Orshansky to construct the original poverty line in the early 1960s. Barrington's estimates differed from those of others who have constructed poverty lines and rates for 1939 by extrapolating the 1959 estimates backwards and adjusting for cost-of-living changes. Barrington argued, because other estimates of poverty in 1939 ignored the decrease in the proportion of total income spent on food by non-farm households over time as well as the decrease in the proportion of food purchased by farm households, they overstated the extent of poverty in 1939 on average, and thus overstated the subsequent decline in poverty rates. Further, Barrington's estimates showed important differences among subgroups. In particular, poverty rates for female-headed households actually increased between 1939 and 1959.

Olney asked if the years 1935 and 1936, from which Barrington's food multiplier was computed, were atypical. She also wondered if it made sense to include persons of all income levels when constructing a food share to measure poverty. Craig expressed concern Barrington had uncritically used the 1931-32 USDA food plan, the results of which may have been tainted by politics and allowed for only low consumption levels. Barrington agreed politics were involved in the estimates, but it was the general rule to consider all consumption and income levels when measuring poverty, and the diet she had used in calculating the poverty line was nutritionally reasonable.

The discussion then turned to three major interpretive issues. First, Williamson thought Barrington should stress that her results overturn the traditional story of a major trend towards equality occurring between 1939 and 1959. He found her results so startling, in fact, he wondered how she could rationalize them with the weight of other evidence. Goldin remarked that both income movements and government transfers were certain to have increased income equality between 1949 and 1959.

The second point concerned Barrington's method of comparing the Orshansky measure of poverty with a traditional poverty line measure of 1959. McCloskey suggested Barrington produce an Orshansky measure for 1959 to provide a consistent comparison. Eichengreen thought it would be useful to compare Barrington's 1939 estimates with those from other countries at the same time. Fishback noted newer "basket" measures of poverty might be more appropriate than a food-only approach. He also said these measures show that inequality and poverty trends can move in different directions. McGreevey noted food-only poverty measures could not adequately control for differences in food quality. Barrington acknowledged these problems, but argued poverty was not a pure measurement problem; there is a public policy dimension to its interpretation.

Third, Carter noted Orshansky's measure has been criticized for having been constructed for reasons of implementational expediency. McGreevey countered that the Orshansky method has been used for years precisely because it is simple and easy to evaluate. Nickless asked if Orshansky had ever commented on criticisms of her method. Barrington then recounted personal conversations with Orshansky in which she learned that Orshansky, although troubled by the criticisms, had never responded because of political pressures. Ransom contributed the session's last word by remarking it was the standard of living, not poverty, that was the important issue.

Michael Haupert's paper argued competitive note producers (commercial banks) during the Free Banking period faced the problem of issuing a product that was variable and about which information was costly to acquire; sellers therefore attempted to establish reputations to signal the quality of their product to consumers. He introduced evidence concerning how reputation affected the market value of New York State bank notes (traded in Philadelphia) between 1851 and 1855. Haupert argued the discount on notes could be decomposed into "reputational" and "non-reputational" factors. He emphasized a bank's past record of suspensions played a crucial role in forming the public's perception of its likelihood of suspending in the future. He suggested the American system of competitive note issue may have been more "stable" than critics have suggested, because there was an incentive for commercial banks to establish reputations and not abuse the privilege of note issue.

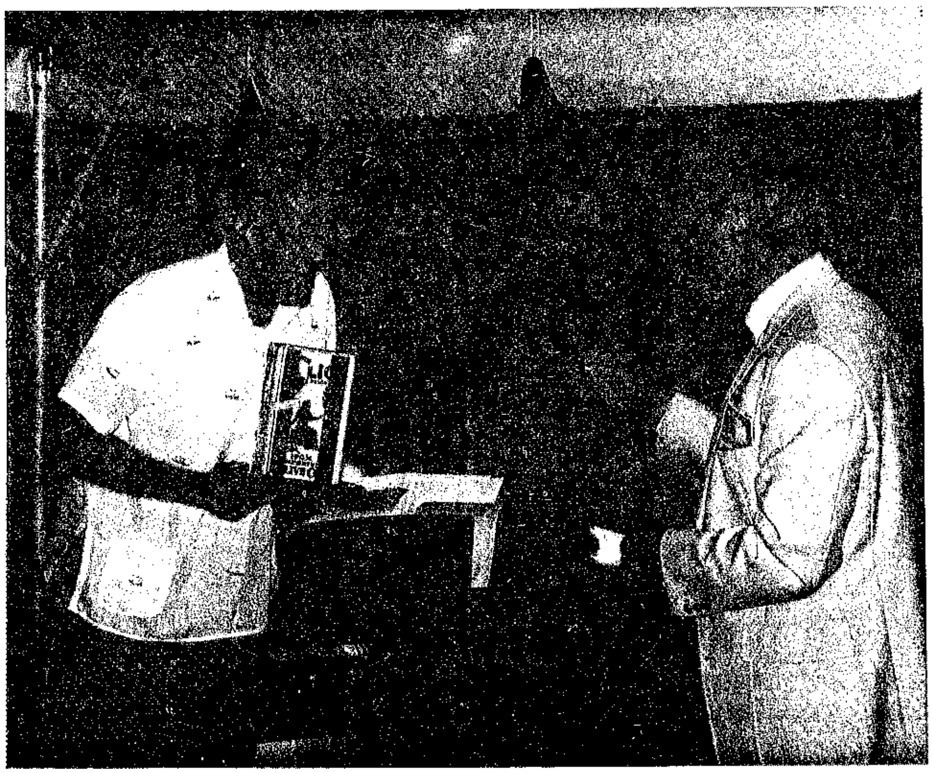
continued on page 25

Vive La Difference

Had Mullah Nasra Din realized that this year's annual Spring rites of the cliometrics tribe would be so reminiscent of Broadway and movie musicals he would have been all the more excited to go. As happens each year, about the time of the Final Four, the Mullah becomes anxious to return to the climos' gathering to encounter those platitudes that only these tribespersons are capable of uttering. While only a few such adages contained much wit or wisdom, the search for those few pearls makes the event what it is. This year, as it turned out, the meetings were graced with the ghosts of Rex Harrison and Maurice Chevalier among others, and the Mullah could not have been more pleased.

This year too the tribal members seemed especially eager to attend for they would be returning to their ancestral home - Hoosier Land. These annual meetings had originated in the boilermakers' village, at one time the hot spot for cliometric debate but now given over mostly to large drums and golden girls. The change in that village followed closely on the heels of the depletion of the kumquat supply and the discontinued use of DC-3s, and the clioms had not returned to the area since. This year's location, however, was not merely in the hoosier state, it was in hoosierville - the home of the White Knight of the Hoosiers.

Added excitement came from the fact that the meetings would take place soon after the Final Four had completed their proceedings in the urban centre located between hoosierville and the boilermakers' village; the place also where the great silver birds deposited the clioms and their valuable cargo. Some of the clioms had participated in those Final Four deliberations, but as the name implies not all could have done so. The jayhawks and the blue devils have become regular participants at the Final Four, but only the former tribe seems to send representatives to the cliometric rites. Some who live near the DeanDome occasionally attend both events, although they are not usually from the tarheel tribe. The "runnin rebels" are by and large not eligible to



For his contributions to Cliometrics, Jeff Williamson received the Clio olive oil can from last year's winner, Larry Neal.

participate in the cliometric debates, in part for having tampered with fair and random outcomes. They have forfeited as well, at least temporarily, their rights to attend the Final Four. Some, especially members of the wildcat tribe from the Southwest, suffer annual Final Four frustration which helps explain some of their behavior at the cliometrics meetings. Fortunately, the White Knight did not participate in this season's roundball finale, having been pushed over the brink by the jayhawk warriors, so had plenty of time to prepare hoosierville for the clioms' rites.

Prepared it was. Even the infamous Comus had been called up from bayou country to assemble a true and faithful rendition of the epicurean delights and adventures that might be found in hoosierville. Comus did, it seems, rely too much on the opinions of others, and perhaps dated ones at that, rather than his own refined tastes. Mother Bear's pizza, for example, may arguably have been in the top ten some years ago, but seems to have slipped in the ratings. In other instances he was on the mark. The quality of the food at the oxymoronic restaurant Ladyman's left much to be desired, but it is a local institution worthy of exploration. The Uptown Cafe lived up to his predictions, but some guests thought his evaluation should pay closer attention to the waitpersons. Unfortunately, some clioms, perhaps out on their own for the first time, stiffed the Irish Lion, so Comus will be unable to enjoy their fare should he ever return from the country of the bulldogs.

The Mullah, as usual, expressed some worry that no one would utter a proverb to rival the inaugural winner - "never open a can of worms larger than the universe." As has been his wont, his concern was aggravated by the absence of the scholar from the great desert of the Southwest, and the absence of the windy city's wildcat tribesperson who has studied the potato in great depth. As experience has shown, however, something worthwhile would be said in the course of the heated and lengthy discussions, and this year proved no exception. There was indeed a plethora of platitudes; the clioms had not eschewed profundity or obfuscation.

The Mullah was particularly impressed by the parallel between the clioms' proverbs and the sage advice of the nation's leaders. A member of the canuck tribe, for example, did one of those George Bush things, telling other tribespersons to "read my paper." The Mullah was reminded how grateful he was that the perennial contender "if the whole world were Chicago," is not true. There is no telling how much trouble the Mullah would get in if everyone spoke with a Chicago accent. This year one with such a twang had made the Mullah a party to a labor relations suit heard by the Supreme Court (Mullah vs Oregon).

The clioms, it seems, are taking to this adage-generating function with some zeal, and apparently like what they hear. The past proverbial winners are now coming home to roost, as it were, with the Mullah's phrases being quoted in feeble defense of some line of argument or other. As a consequence, the Mullah has once again peered deeper into the barrel of truth to see that the famous orshanskyism "any number you put in print, you will live with the rest of your life," is true for words as well.

In order to sort out the many contending phrases, the Mullah reminded himself that the best proverb was that which contained wisdom for all times and places. It is this universality, or lack of it, which explains why many excellent sayings were not chosen. The winning proverb should also benefit society and must be delivered spontaneously, in the heat of battle.

A sample of quotable quotes that were in contention for the big prize can only hint at the wisdom being spouted. The great hawkeye rhetorician was in the hunt throughout, with advice like "never whore after a theorist," or "I was not persuaded by the fact that there is little mail in the South." The garrulous gourmet from the bay area uttered the sort of quantitative expression that best symbolizes these proceedings "the difference between 20 and 26 is about 5." If only he were right. Harking back to biblical wisdom, the tribesman whose name is not misspelled twice [sic] foreshadowed greater things to come with "it may be easier to enter through the back door than to be pushed through the window."

One performance stood out for its output per unit of time, where time is measured in sessions, a more meaningful unit as we approach the postmodern era of interplanetary consciousness. Clearly an up and coming verbal force, this rookie from the same tribe as the potato analyst would have won a Sessional Achievement Award, if there were such a thing. In one such unit of time he put forth "there are always two strategies," "if I knew the answer, game theory would be a much better discipline,""there isn't really a conflict between religion and rationality..when it comes to the ordeal, you can be an atheist and believe in ordeals," and from personal experience "ordeals today are not painful." While he had quantity, his quality was not up to the final contenders. Moreover, in his written version he had said, "ordeals were used when there was no direct proof of the suspected person's guilt" and his subjecting himself to an ordeal, no matter how painless, suggests that the proof of his wisdom is clouded.

As usual, the Mullah had to dismiss some pithiness that was simply too contrived. The coal king from the southwest, perhaps trying to uphold the honor of the tribe which gave us the inaugural winner, put forth too obvious a campaign to win the prize. It also seems that he is playing with an unfair advantage, for during some discussion other tribespersons kept referring to his minds. How many more does he have than the rest of us? However many, they yielded up "a person in need, the merchant avoids indeed." In a brief lapse into machismometrics he squealed "I'd like to throw myself on the goddess of statistical significance," apparently to enter the discussion about big and little. He tried the quantitative approach, "don't use the ratio 1:1, use a smaller one like 1/2 to 1/2." Finally, and with great relief, he admitted "flush toilets are near and dear to my heart." Frankly we don't need any more of that obvious crap, particularly in light of last year's winning phrase "it is difficult to count all the manure." While none of these phrases would win individually, overall it seems clear that he may be a legend in one of his own mines.

In keeping with the spirit of these meetings which follow so closely on the heels of that which is becoming one of the Mullah's favorite events, the leading contenders were whittled down to the Final Four.

From the musical symbol who carries coal near Newcastle, and who was apparently deeply influenced by Comus's writings came "the best time to go to a restaurant is right after the health inspector." Like many previous contenders this one has intuitive appeal, but may not be true! With rational expectations, perfect information and foresight, one should go just before the health inspector.

From the scholar who emerged from the bastion of postmodernism we heard "If you're talking about what I think you're talking about, we can talk about that." If the Mullah understood what she was talking about, he predicts that she will someday become a distinguished professor of postmodern economic history, specializing on the differences between everyday life and everyday living.

Luckily, a local tribesperson, who is in charge of small stains, made good with "Don't jump into a box with no bottom." A hush fell over the crowd at its utterance. Some thought he had put forth the equal of the can of worms. Truly, it was reminiscent of the inaugural winner, an aphorism so succinct, truthful, and universal that there is no need to investigate its empirical validity, and certainly no desire to do so. It gave pause to think what prize the Mullah would have devised if this had been the first winner. While brilliant, and emulative, it was not the winner, perhaps because it was so similar to the first winner and thus did not chart a new direction.

The catchiest phrase showed keen judgment and related to the empirics of the clioms' body of thought. It reflected a cross between the wisdom of the "wild and crazy guy" and the savoir-faire of Maurice Chevalier. The phrase came from he whose name is not misspelled twice. "French data are too beautiful to be true." While it smacks a bit of nationalism and has an aura of Hollywood, it could nonetheless be true for all time and wherever their data are used. The Mullah is sure that from now on:

> *each time he sees a little number of five or six or seven, he won't resist a joyous urge to smile and say- thank heaven for little data, ...they get used in the most delightful ways...*
> *Thank heaven for them all, no matter where, no matter when; without them what would good clioms do?*

The Mullah was overjoyed to see that the leading general equilibrating gap analyst got canned this year. As the Daily News said when the Dodgers finally succeeded "this is next year!" Fortunately, he lived up to his reputation, offering in the heat of debate "the evolution of bigger plums elsewhere makes smaller ones less valuable." With this year's entry, and some of his previous ones about unrequited love, it seems clear that he had a hand in writing the lyrics to the Fantasticks. This augurs well for next year's proceedings when he will have the responsibility for putting on the stage production, and will have to top "If I Only Had a Can" (from the Wizard of Oz) and "I've Grown Accustomed to the responsibility for putting on the stage production, and will have to top "If I Only Had a Can" (from the Wizard of Oz) and "I've Grown Accustomed to the Can" (from My Fair Illini).

The future looks bright indeed, with the burgeoning musical talent, as well as with the likes of he who won the virgin cabbage award and the other aggressive rookies entering the profession. When the great organizer, he who sometimes travels with four legs and an ice pack, returns from down under he shall be pleased with this year's results. At the same time, the tribespersons will be pleased to have him back among them.

The meetings were brought to an end in a surprising fashion. Thanks to the southwest wildcat tribe, and their sleeping redheaded leader, the jayhawk chairing the last session was provided with a pair of ruby red slippers, albeit slightly used ones. Nevertheless, by clicking their heels together he left for the land of Oz, where he and others will soon do the Wright thing.

Submitted humbly by the faithful and obsequious servant of the Mullah

Larry Neal's Can Song

I've grown accustomed to my can;
I put it in a special place.
I've grown accustomed to the fame,
The can has brought my name.

The white, the green, the red, the sheen-
Are second nature to me now,
Like rejecting and accepting.
I was just an editor, a Clio fan,
Before my can.
Surely, I could always be that way again, but man!

I've grown accustomed to the fame,
Accustomed to applause,
Accustomed to my can.

Parker Interview *(continued from p. 8)*

joined with others to create one. In 1956, when I was invited to North Carolina by that lovely, gentle man, Milton Heath, I was well settled into economic history, with an interest in the long-run history of economic sectors. And when I got to Carolina, I took up my old interest in agriculture as a sector, but now in a historical context. I remained hooked on that cycle of research for the next fifteen years.

The Census Sample

I would be interested in hearing you and Bob Gallman discourse on the Parker-Gallman sample. There hadn't been a lot of work done previously in gathering together samples of this size. And it certainly has had a huge impact.

Well, so far as I know there hadn't really been an effort before to apply any sort of scientific sampling to the Census materials, except for the population censuses. The historian who came closest was, I suppose, Frank Owsley, but sampling simply wasn't in earlier historians' tool bag.

Did you think it would have such wide use?

I didn't think much about that, one way or the other, but I could see I was on to a good thing. I could see infinite bodies of data that could be exploited in this way. The quantitative study of slavery had gotten a big boost with Conrad and Meyer's paper at the EHA meetings in 1957. Racial integration was just barely beginning, and sociologists were showing a new interest in labor systems in underdeveloped countries. But my interest was simply in the conditions under which cotton was supplied to the world market in the nineteenth century. Tom Cochran had asked me to write a paper on large management units in American agriculture for a session of the International Economic History Association at its first (1960) meeting at Stockholm. Plantations were the only largish scale enterprises, in terms of labor employed, in the American experience. Some wheat farms in the Red River Valley in Minnesota and in California in the nineteenth century were large land holdings, but not large bodies of year-round labor. Of course, these and the plantations, too, were peanuts as compared to East European estates with serf or hired labor.

But in my paper on the slave plantation in American agriculture, I tried to think out the different aspects that could illuminate, and be illuminated by, the economist's natural questions—demand, regional balance of trade, capital inflow or outflow, internal self-sufficiency, even the bias against industrial development. I think it hit on the main lines along which the treatment of the subject indeed did develop. Certainly I had in my head an implicit economic model. I did considerable quantitative research before I wrote the paper, though I didn't include any numbers explicitly. But I went over from Chapel Hill to the Duke Library, where the nineteenth-century manuscript census records of Louisiana, Arkansas, North Carolina, and (I believe) Mississippi were held. A very sturdy graduate student, Don Schilling, helped me, and we dug out by hand some of the 1860 records on large farms.

Then we got greedy. I began planning a large sample of the manuscript returns from the Census of agriculture, matching the farm productions, by name of farm operator, with the farm labor force, as reported for each slaveholder in the Census of slaves and for free family labor in the Census of the white population. We planned to do this for the counties in the Census of 1860 harvesting 1000 bales of cotton or over, with selection from all the major soil-type regions. Before the work could begin, I had to get a grant from the University grants committee at Carolina. The dean of the School of Business wanted to earn respectability with the liberal arts college, and he endorsed the proposal with enthusiasm. The chairman of the grants committee was Fletcher Green, a southern historian who had produced more Ph.D. theses than any man in the world: 500–600, it was said. He didn't know what I could do with all these numbers, but he could see that they concerned farmers, and I suspect he was a bit of an old Populist. In any case his committee gave me all the money I needed in order to explore. Next Jim Blackman, then gone from

Carolina to NSF, helped us to get an NSF grant to get the data filmed from about a dozen state libraries in the South. Franklee Gilbert (now, Whartenby), a very good thesis student at Chapel Hill, was awarded an SSRC grant to go down to South Carolina and several other places to collect data from the plantation records for the 1830s and 1840s for her dissertation. She didn't use the census records themselves, but she tracked down where they were. When we got the films together, we set up three microfilm readers, side by side, to try to match names in the census of agriculture with those of slave holders and the heads of white famiilies.

All of this fitted into a larger structure of the study of American agriculture that was my part of a sizable Ford Foundation grant, shared with Ross Robertson, Moe Abramowitz, and Jack Sawyer—a general grant for the economic study of American economic history. In my portion of the work, I divided agriculture up by crops, and tried to get labor input in each operation on each crop. The work on slave plantations was a by-product of this scheme of measuring the contribution of agriculture to American development overall. After the project got started, I made the move from Chapel Hill to New Haven, and Bob Gallman and his students re-worked the sampling and improved it and then brought out a series of fundamental studies. At Yale, Gavin Wright and Peter Passell drew on it for their dissertations. The recorded result of much of all our work was published in a book, T*he Cotton Economy of the Ante-Bellum South*, in Wright's beautiful book, *The Political Economy of the Cotton South*, and, supplemented by their own exhausting investigations, in Fogel and Engerman's *Time on the Cross*. Its influence moved out in many directions, both in the study of Southern economic development, and as the predecessor of similar researches in the nineteenth-century census manuscripts of agriculture and manufactures.

The middle-sized "big picture"

Let's move on to look at some more general issues of methodology. You've always had sort of a structure or schema in your teaching and writing. Where do you think that habit of mind comes from? You were saying that you were trying to find a "structure" when you studied literature.

Well, it seems to me to be the way anybody has to think. Idealization is involved in it, a kind of theorizing. A tension or ambivalence is produced between ideal structures, —ideal type structures, Parsonian structures—on the one hand, and the facts of a body of history. You are to try to explain historical change within a structure, as it is observed in operation, but you also have to explain how the behavior at a deeper level of structures creates and alters the economic structures themselves. And so on, *ad infinitum*. That tension between the general and specific is what moves historical and sociological research down into ever deeper levels. If you get pulled too far in one direction, your thinking steams up into clouds of philosophy, and if you go the other way—down toward the particular—you get buried in the dust and don't say anything of general interest. You have to hold steady on a middle ground.

Again, that is a moderate attitude—part of that shying away from radical thought that is in my bones. Perhaps that is an English-American intellectual trait. Usher had it quite strongly and articulated it. He never had much use for "ideal-type structures," as he called the theories of Weber and Marx. But he did deal in large topics—technology and population. He seemed to think that there was a sort of optimal size of topic that could be handled. If you went beyond that, it got too complicated, and if you got below that, the work seemed trivial and antiquarian. That is the name of the game in economic history—to work at an interesting, yet sustainable level. It is an engineering problem really, though, oddly enough, the history of technology with a few well-known exceptions, has itself rarely been handled with this kind of balance.

You don't favor some of the structures that people typically use to organize their studies, such as the growth of the national incomes of nation-states.

Well, I think the national income framework has been very useful. But I would like to see explorations at both lower and higher levels of aggregation.

As a way of analyzing an economy, you seem to emphasize regions and resources.

Yes, Usher's emphasis on resources, geography, technology produced in my mind a kind of opportunity/response framework. When I came to organize the graduate course in economic history at Carolina, I picked this up as a way of handling the material. Three natural forces combine to create an opportunity framework for an economy: resources, technology, and demand. The response to opportunity is a problem of human organization—a political problem rather an economic one. It is not about wresting a living from the earth's materials, or feeding hungry mouths. It is about power and contrivance and how individuals control one another mutually, how some are better at the game than others, and what are the different economic results of the different combinations of roles and actors. It was only after ten years or so at Yale that I began looking past the "opportunity" part to this other element, where culture, society, and a collection of individual personalities all come into the structure of explanation, piled on top of one another in layers.

But I had no really formal training in the other social sciences to help me. In the early years at Yale I read sociology—the German historians, Weber, Veblen, Sumner, and a few of the more attractive moderns, Riesman, Parsons, Merton, Mills—and, to even more profit, the older anthropologists—Malinowski, Furth, and some of Mead, Benedict, Frazer, Herskovits, and later, Sahlins. I felt great sympathy with the French *Annales* school and their peasant studies, and particularly with the wonderful books of Marc Bloch. I never talked about *histoire totale* much and I never wrote about it, but I soaked it up. My mind and imagination were very receptive to it. I think that that strain of interest goes back partly to college. I remember the sophomoric bull sessions we had about understanding the world. We all aimed at a total comprehension, a totality, a Hegelian "holistic" concept, though we had never heard of Hegel. We had a phrase—"Knowing what it's all about." As Harvard men, we thought we "knew what it was all about." (About the rest of mankind, we were not so sure.) In a way, we were talking in college about a social equilibrium of different character traits affecting every item of behavior and culture. I remember reading Burkhardt's *Civilization of the Renaissance*—the section on the state as a work of art—and Huizinga's *Waning of the Middle Ages* and Eric Erickson's books, especially *Young Man Luther*. Books like these—and there are not very many of them—I really sopped up. Sometimes with these great books it will be a decade or longer before you really realize what you had read.

Then, in U.S. economic history, I had to come to grips at last with the relation of the industrial culture of New England and the Midwest to an underlying ethos or mentality. I read some on the Puritans—not just Weber, but some about the actual doctrines. (The Yale Library is a great place to do that.) I'm really deeply interested in the psychology of all that. I don't have too much respect for historians who ridicule its importance by producing counter-examples from the capitalism of the Mediterranean or Japan. The relevant question is—what is the sum total of factors that are present and how do they interact? There are many factors, but in the Western context, Protestantism is surely one. Just because three and two make five does not mean that four and one also couldn't make five. The economic response to a production or trading opportunity will be organized in one way in one social group and in another way in another. If the opportunity is very wide, then strong individuals pursuing a variety of goals may come to fit together in a market framework. Tidy, bureaucratic social organization of the response may be the most effective response if you already have well-disciplined individuals. But the measured outcomes of two different combinations of individual characteristics and organizational form may be very similar. I had an example of this in the growth of the iron industries in German and French Lorraine after 1870. Lessons like this come from attention to comparative economic history, particularly when, as in Al Chandler's latest book, Germany, with its special organizational and characterological features, is one element of the comparison. So in studying both European and American agrarian structures and industrialization on the two continents, I have tried to suggest, at least a little, how the human side of organization fits with its natural and technological constraints. Consequently I feel very uncomfortable in bull session talk about individualism and "collectivism." There isn't any simple weighting that fits all cases. It's a day by day confrontation, as life develops, between human impulses transmitted in different forms and with differing relative intensities.

So I have remained calling myself an economist. Besides, I have great sympathy with the policy side of economics, the possibility of some really useful contributions to the functioning of a democracy with the limited economic knowledge that we have. It always seemed to me a bit self-indulgent to enjoy reading about primitive tribes simply because their societies could be imagined to form such pleasant, esthetically pleasing wholes. I suppose you will say that *my* Puritanism is showing.

Economic history in graduate education

What do you think about the relationship between economic history and the economics profession?

I am bemused to think that the people who favored the economic history requirement during my years at Yale were not always the traditional applied economists, but rather the development people and the mathematical economists. With a few exceptions, like Joe Peck, applied economists, if they think about history at all, tend to think about it mainly in relation to current issues in their own fields. The fact that history is a synthesis of many areas was something you always had to keep up in front of them.

In my editor's postlude to *Economic History and the Modern Economist,* I claimed that an economics program has many different uses for economic history. But from any view, it is a healthy thing for students to be exposed to. For one thing, it attaches at the ends to all the other social sciences. If you are of a naturalistic, physical-science bent, you still have to see where demography, or politics, or sociology must be brought into economic studies. History leads you out of the strict, narrow economic maximizing paradigms to the rest of God's creation. For economists, it offers all the benefits of foreign travel.

Alongside quantification and model-testing, history's narrative techniques still undeniably make some kind of sense, even though you cannot prove every interpretation or calculate the statistical probability of its truth. And for students to get the "feel," the intuitive feel, of the actors in an economy—putting themselves in the place of people in a different culture—this is an exercise of imagination and thought that economists need, both in framing hypotheses and in making policy recommendations. After all, where do hypotheses and assumptions come from? They are impressions arising in the mind from the cursory examination of a record. Narrative economic history is a tissue of untested hypotheses. Sure, most of them are untestable, but they are nevertheless powerful stimuli to the imagination, and to the mind's effort to learn and explore.

The new economic history and the old

Would you call the so-called "new" economic history the result of a "rebellion" against the "old"?

Well, not exactly. I don't think of "new" economic history as really a "rebellion." Except for Carter Goodrich, Hal Williamson, and Chester Wright, the "old" economic historians in American Economic History of the 1940s and 1950s had been trained as historians. Kirkland, Shannon, and Faulkner, for example, had written the three principal texts, and they—and their economist counterparts—were all very solid scholars indeed. It is true that a lot of loose talk on capitalism came out of the followers of Veblen. I think Veblen was a great thinker, a great intuitor as well as a great writer. The institutionalists who followed him—Ayres, Brady—tended to be a bit windy. I didn't have much respect for that as a school of careful thought.

I never used the phrase "new economic history," until others took it up. It always made me squirm a little because I was sensitive to the continuity of the effort with the writers of the 1930s—Clapham, Heckscher, Usher, and Bloch. Those earlier scholars had different ways of going about history, but it was all wonderful scholarship. In the United States, certainly, Beard was pretty extreme sometimes in his willingness to paint a big picture. It made your flesh creep a little, but it was inspiring. I didn't want to throw it out. The book in agrarian history I most admired was Webb's *Great Plains;* it is so original, and seems so thorough, so honest, and true. Of course, Webb was a vastly spirited and entertaining writer. Shannon's *Farmers' Last Frontier* at places swings into that mode, but Shannon struck me as probably a narrower man, without Webb's scope. I found the mastery of detail and the sound economic judgement in L.C. Gray's *History of Agriculture in the Southern United States* admirable. Parts of Phillips's books on the South, too, I was affected by. I knew he had his biases, but he had his sympathies as well. I liked Malin's books, too, and I admired greatly Bogue's really fine book, *From Prairie to Corn Belt.* I think Gavin Wright's books on the South carry on in this tradition, with more, and more exciting, technical economics in them. Those older agricultural historians were my heroes, even though I worked in a statistical, "counting" sort of way.

So I felt a great sense of joy to break out from the numbers far enough to write the chapter on agriculture in our 12-author textbook *American Economic Growth,* which we subtitled "An Economist's History of the United States," and also the chapter about the American farmer in a book on peasants that Jerry Blum edited. It gave me great satisfaction getting my information and intuitions together and saying it in nice language. With those pieces, and with that chapter in the *Cambridge Modern History,* Volume 13, on European industry before 1850—which, being based on my lectures, flowed out of me like a novel—I felt I hit a stride. I felt that with the quantitative work, too—the piece on grain which Judith Klein and I did, for example. When the data—so painfully gathered and sorted—began to fall into place, they outlined a logical puzzle which gave real intellectual satisfaction to work out. But I enjoyed, too, the emotional satisfaction that came out of sticking a little sociology beneath the agricultural history, as I did in the two survey essays. In these ways, work in the field came to satisfy both the intellectual and the emotional sides of my nature.

The second volume of your collected essays really forms an outline history of American economic growth. It is dedicated "to Doug and Dick and Lance and Bob and Stan and Bob and Stan and Al and Paul and Peter" and to your joint efforts. Can you say a few words about these people?

You want me to tell you what I think about my colleagues? Incidentally, you must have seen a typescript copy, because in the published version someone at the Cambridge University Press has cut out the second "Bob and Stan" that I had put in the manuscript. If they should read this, I hope they will take note, so as not to spend time guessing which "Bob and Stan" I had left out. But the need for this explanation only serves to emphasize that colleagues are a sensitive matter. Nobody ever completely approves of anyone else's personality and work except his own, and if he is any good, usually not that either. I was moved to make that dedication because I felt—well, it's what you feel when a department is working well, when people are getting along well together. Some joint product was coming out. I really did feel a sense of intellectual communion among that group of guys with their different talents and emphases. I thought that altogether we had really got the subject organized, and I take a good bit of credit for my part in organizing a sub-set of us into a reasonably harmonious group for our textbook, which came out in 1973.

About the textbook?

I had signed up with Irwin to write an economic history textbook when I was still at Williams in 1954. At Carolina in 1958–59, Dean Lee gave me a Ford Faculty Fellowship to spend a whole year in the Library of Congress. I worked pretty hard. I had an outline for forty chapters and I got two chapters written, one, on geographical discovery, and a second chapter on minerals discovery. By that time the year was up and I had written two out of forty chapters. I said to myself, "This is not a game you know how to play." Working full time, I would be another 20 years finishing this outline, and you can't get fellowships for that long a period. So I just set it up on the shelf while various joint efforts began to materialize, especially the National Bureau volumes. Along with Dorothy Brady and all the authors, I put a great deal of thought and effort into both Volumes 24 and 30 of the *Studies in Income and Wealth.*

By 1968, I felt that we really were quite a little group. I had given up writing a textbook by myself, but it did seem we could do a good job working together. Lance Davis had the same sort of idea, and, with Dick Easterlin as critic and consultant, we went down a list otopics and a list of people. Some topics were not covered by anyone in our group. But the two lists corresponded quite closely with one another. It was as if the natural division of labor, enforced by the Invisible Hand, had made us steer clear of one another's areas. Putting everyone together, the fit was quite good. There was Lebergott on labor, Gallman on national income, Easterlin on population, Davis on capital, Fishlow on transportation, Rosenberg on technology; I had resources and agriculture--twelve new economic historians altogether. It was subtitled "An Economist's History of the United States," and it was a damn good collection, which nobody bought. I think it was because professors took their lectures out of it and didn't want their students to read it first.

The famous Purdue seminars, which turned into the Cliometric Society, had been, of course, a looser format. When we were able to squeeze ourselves between the covers of a textbook, we had gone about as far as you could ever go to get these fellows to pull one wagon. That's what I meant when I made that dedication. I think they were all intellectually in the book. Bob Fogel, Stan Engerman, and Paul David did not write chapters, but they were obvious people we all counted on and looked to for intellectual support.

A look ahead

Do you have any closing words of wisdom?

O.K., I get the hint. Yes. Let me make one last industrial statesmanly statement—a feeling which I would like to express and to propagate. This has to do with your mention of the nation and the nation-state. American economic history is the history of a continent. Why isn't European history the history of a continent? Why do we keep all such heavy emphasis on national histories? It seems to me that over the next 20–30 years, if the study of European economic history is going to be of any use or interest, in much the same way that Kuznets's comparative cases were to students of national development, it is going to have to have a different format, one in line with a common market, a history of transnational trends and development in which the political units were set. That, too, was largely Usher's emphasis.

Even the homogeneity that resonates from one state to another—the general adoption of liberal policies in the mid nineteenth century, for example—meant that many of those states were abandoning mercantilism for 50 to 80 years, at least until the 1920s, and allowing a freer market and freer trade. World War I messed that all up, and that is what I would gather that the bureaucrats in Brussels, and the liberal-minded intellectuals—as well as some business and banking interests that support them—are trying to restore. Can't historians help this effort in some way? Of course, we talk about Western Europe and "Western Culture" as if those terms were not simply an artificial creation of the Cold War. Europe is really three cultures—North , East, and South, that is, loosely, Germanic, Slavic, and Latin, with enclaves of even older ethnic groups. The *West* of Europe is the United States, with all its ethnic diversity.

But a suitable organization of the world's nations and ethnic groups into a peaceful, prosperous and joyous community is a subject rather larger than what Usher would have considered to be of optimum scale. Still, I'd like to give it a whirl with all that blessed irresponsibility that a scholar can show in his seventies. Maybe I can imitate Scheherazade (or, Shevardnadze) and keep telling my story to put off the day when the Sultan cuts my head off.

Come to think of it, that last remark is a good example of what I meant in my opening remarks as I tried to explain myself to myself, before your questions began.

But this sort of talk takes us beyond even the capacious bounds of economic history, much less of Cliometrics.

Editor's Note:
From Bill Parker's writings on European and American economic history, a selection, made to form an overview of his approach to the field, has been recently published as Europe, America, and the Wider World, London and New York: Cambridge University Press, Vol. 1 (1984); Vol. 2 (1991).
See also:

Lance Davis et al. American Economic Growth: an Economist's History of the United States. New York: Harper and Row, 1972, Introduction, Ch. 4, 11.

W. Parker, ed. National Bureau of Economic Research, Conference on Research on Income and Wealth, Trends in the American Economy in the Nineteenth Century, Studies in Income and Wealth, Vol. 24. Princeton: Princeton University Press, 1960.

W. Parker and Eric Jones, eds. European Peasants and their Markets. Princeton: Princeton University Press, 1975.

W. Parker, ed. The Structure of the Cotton Economy of the Antebellum South. Washington: The Agricultural History Society, 1970 (reprinted from Agricultural History, XLIV: 1 (Jan. 1970))

Gary Saxonhouse and Gavin Wright, eds. Technique, Spirit, and Form in the Making of Modern Economies: Essays in Honor of William N. Parker. JAI Press: Greenwich, Conn. and London, Eng., Research in Economic History, Supplement No. 3, 1984.

W. Parker, ed. Economic History and the Modern Economist, Basil Blackwell: Oxford and New York, 1986. (Italian edition, tr. by Albert Caracciolo, Laterza: Rome-Bari. 1988. Libri del Tempo, No. 214)

W. Parker, "Liberalism and Modern Reality: Do American Formulae Fit France?" The Tocqueville Review: 7 (1985-6), 299-307.

Clio Conference *(continued from p. 14)*

Bernstein and Klug began the discussion by stating they were troubled by the use of New York as the primary example. Haupert agreed and added he only presented the weakest case of his argument. Quigley, Snowden, and LaCroix all felt the definition of "reputation" was unclear. Haupert justified his definition by granting "the econometrics were not sophisticated." Hoffman, Treble, Alston, Carlson, and Libecap each commented on various aspects of the model, with Hoffman summarizing this part of the discussion by suggesting the econometrics should either be more sophisticated or dropped.

Toniolo put the discussion on a different track asking whether the notes in circulation were really "money." Haupert argued they were, by today's definition. Miles suggested the use of game theory, and Van Vleck wondered if there was a measure of the "ebb and flow" of reputation. Haupert replied he was not sure how much damage one could do before a reputation was completely destroyed. Rhode and Cain were troubled by the treatment of location and the transportation variable in the model. Haupert emphasized the use of one state avoided having to deal with differences in state regulations. Elmus Wicker (Indiana) argued the paper said nothing about the stability of free banking institutions. Haupert replied he had not tried to do so. Wicker, supported by Eichengreen, followed up by arguing this was the important question.

Martha Olney's paper argued that, to the extent consumers use credit to smooth purchases, consumer credit could dampen business cycle swings, but the opposite effect was also possible. If collateral or a down payment is required, if the extension of future credit is predicated on the ability to service earlier loans, or if a recession leads to costly default and a reduction in household wealth, consumer credit could actually exacerbate swings. Using data from a 1918 Bureau of Labor Statistics survey, she divided consumer credit into three categories: merchant, borrower, and installment credit. Black families relied much more frequently on installment credit than did white families everywhere but in the South. Whites, on the other hand, were much more likely to use merchant or borrowed credit than installment credit. Olney constructed predicted credit-use probability profiles for several household characteristics. For white families, the probability of using merchant and borrowed credit declined rapidly with income and peaked during the prime spending years of the family. Installment use by whites, on the other hand, was of relatively constant likelihood across income levels and declined with age. For black families, the probability of using installment credit increased dramatically with income and declined rapidly with age. Merchant and borrowed credit use by black families, on the other hand, changed very little with income or age.

Ferrie, Fishback, Gregson and Dye raised questions concerning the underlying structural model. They pointed out there were underlying demand and supply equations, but the regressions in the paper looked like demand equations with the supply equations being neither reported nor discussed. Olney replied she would look into these problems. Snowden questioned the use of cross-sectional data to understand disruptions or changes in credit markets over time. Olney conceded this point, but she felt her cross-sectional results were encouraging enough to proceed.

Goldin noted the data were from non-representative years, and Hoffman questioned the accuracy of the data. Olney argued people kept more account books and better records during the early part of the century. Haupert, Cain, Nickless, and Ransom raised questions concerning the relationship between white merchants, white agents, and black debtors, while Treble wondered if the sample of black debtors was biased. Olney replied she did not know whether a particular merchant was white or not, and the blacks in the sample were from a relatively high income group. Bernstein wondered whether or not literacy played a role. Olney thought this was intriguing and promised to consider it. Rotella suggested running

At the 'Awards Banquet,' participants were serenaded by past winners of 'The Can,' Roger Ransom, Tom Weiss, Larry Neal, Richard Sutch, and Don McCloskey, in this year's version of The Can Song.

separate logit equations for blacks and whites. Olney said she had done this, and the results were not different. Alter questioned the use of age in the diagrams, wondering why age remained important after Olney controlled for family size. Finally, McCloskey observed that t-statistics were superfluous with 12,000 degrees of freedom.

Bertrand Roehner's (Paris) paper examined the integration of the 19th century French wheat market between 1825 and 1900, during which time the mayors of every market town reported market prices twice a month. Roehner concentrated on four meth ods of analyzing integration: measures of price dispersion, contemporaneous correlation in price series, contemporaneous price correlations weighted by distance (the correlation "length"), and multivariate ARIMA models of prices among markets. He was interested primarily in understanding how the different measures of integration related to each other, how they differentiated between the real economic structures through which wheat and information flowed and the nature of the shocks which hit the system, and how they differentiated between local and global measures of market integration. He reported there was not necessarily a relationship between price dispersion and contemporaneous correlation. In some cases they both increased over time, while in others the more "normal" pattern (where dispersion decreases as correlation increases) evolved. His measure of correlation length among several markets, however, appeared to provide a more robust measure of long-run integration. He provided evidence that market integration improved noticeably after 1800.

Harley remarked the paper omitted the history of the era. Roehner replied a model including all of the specific points to which Harley referred would be unmanageable. Sicsic and Snowden questioned the treatment of transportation costs and distances between markets. Roehner replied he included the effects of the railroad but did not find the correlation one might expect.

There were several comments concerning the objectives of the paper, Neal felt the concepts were poorly explained, particularly the statistical techniques. Snowden asked if the paper was primarily a methodological piece on how to measure integration. Roehner attempted to clarify his concept of integration, but some questions still remained. St. Amour raised the point that unintegrated markets could have similar movements. He also questioned, along with LaCroix, the lack of dynamics in the definition of integration and the omission of first-differencing. A lengthy discussion took place between St. Amour and Roehner on these issues. Rhode and McCloskey commented the diagrams showing the relationship between location and prices did not appear linear. Roehner replied other specifications were possible. McCloskey also argued integration was not an absolute concept but must be measured against some standard. McCloskey and Fishback also questioned the concept of causality and the emphasis Roehner placed on "dominant" and "satellite" markets. Several discussants, in particular Snowden, disagreed with this point, and the discussion ended without a consensus.

In his paper Dennis Halcoussis (Penn) argued the reason Kansas farmers became increasingly irritated during the 1890s and turned to the Populist Party to solve their grievances was that their losses from incorrectly forecasting market prices and crop mixes increased dramatically during the decade. Unlike DeCanio, who assumed the crop mix (of corn and wheat) actually produced by farmers in any year provided an unbiased estimate of their expectations (at the time the crop was sown) of the relative prices of the crops at harvest, Halcoussis argued price expectations need not be inferred in this way for the late 19th century, because a well-developed futures market was operating. He showed the agricultural supply model of 73 countries performed much better using futures prices rather than spot prices, and from these models he estimated the magnitude of the errors farmers made each year. He concluded over the period 1881-1907 the forecasting error of future relative prices increased. The error in choosing a crop mix based on price expectations, however, rose until the mid-1890s after which it fell. Together these two determined the total economic loss from forecast errors, and they peak in the period 1888-1897. This was, of course, the same period in which the Populists gained their greatest strength. DeCanio found a similar temporal pattern, but Halcoussis' estimates are substantially larger and equal 2-6% of total income.

Alston commented the paper offered almost no bridge between economic distress and the politics of the era, and Hoffman noted, while the paper made a strong claim for an economic-political relationship, no evidence of this link was offered. Halcoussis recognized this weakness but was not sure how to remedy it. Libecap was unconvinced by the explanation of why the loss declined in the latter part of the period. Halcoussis claimed farmers improved their forecasting. Libecap wanted to know how they improved, and Halcoussis argued information, as indicated by the widespread citation of the corn-to-pig ratio, either became more available or was better. Fishback commented, in this, Halcoussis was merely updating the work of Bob McGuire and others. Halcoussis agreed, but thought such an update was important nonetheless.

Hanson and Metzer questioned whether Halcoussis's view of farmers was a naive one. Halcoussis felt his view was not that farmers were naive, but rather that their participation in world markets increased dramatically at this time. Snowden asked about the length of the futures contracts, and Haupert wondered if farmers in fact participated in futures markets. Halcoussis responded farmers did not have to participate to derive information.

Eichengreen noted the paper's framework only accounted for loss relative to the best that a farmer could have done, while Harley was troubled by the act of putting crop mix at the tip of the farmer's concern. Gregson suggested Halcoussis use farmgate prices since the railroad made a difference over time and space. In response to Sutch's request for an explanation of why farmers opposed futures markets, Halcoussis commented farmers thought speculators were part of a conspiracy to obtain money due farmers. Finally, Williamson suggested the inquiry be expanded to address what was novel internationally about the Populists.

Alan Dye's paper examined the role of technological change in the dramatic increase in Cuban sugar production in the late 19th and early 20th centuries. According to Dye, the implementation of multiple grinding rollers in sugar plants and the integration of a variety of innovations in the ancillary tasks undertaken in the plants explain the increase in output. Dye argued the temporal pattern of innovation was well-approximated by Salter's vintage capital model in which technological change is embodied, but co-exists for a time with older (embodied) technologies as they depreciate. There was a wide range of scales used in sugar cane processing throughout the period, but, while the mean of average plant size increased over time, the dispersion of plant size around the mean was stable. Dye explained that the newest plants often began operating at the low end of the size distribution of plants, reaching optimal scale after several years had passed, by positing substantial adjustment costs which prevented an immediate investment in a plant of optimal scale. Getting the plant physically in place quickly was costly and required a larger and more reliable supply of raw cane from the local area. This supply problem itself required substantial investment and development of the cane fields. He measured these adjustment costs by calculating the profits foregone by not operating new plants at optimal scale until several years after they were put in place.

Saraydar questioned whether soil conditions made a difference in scale in different areas. Pastore argued Dye left out the adjustment costs of extending the acreage in cane. Dye replied the adjustment of the size of mills and acreage will proceed together. Pastore and LaCroix raised issues concerning additional acreage and the use of the land. Dye replied in general land was abundant.

McGreevey claimed Dye's paper had "too much metrics and not enough Clio." Harley commented Dye had only offered readers the "easy part" of the story with little mention of the agricultural details or descriptions of the institutions involved. Quigley and Snowden raised issues about the lack of discussion of changes in international finance and investment in the 1920s. Dye responded he would consider these more closely in the future. Snowden also asked if there was any strategic behavior with respect to "jumping in" on a small scale just to get into the business. Dye explained the difficult part was not mill size but obtaining the land. Prohovsky argued what may have been going on was that an optimal mill size was planned all along, but it just took a few years to achieve this optimal size.

Fishback pointed out Dye's estimates of lost profits due to failure to employ the optimal scale conflict with his main point of no "entrepreneurial failure." Contributing to this issue, thereby closing the session and the conference, Rotella asked if anything remained of "entrepreneurial failure." The consensus was, though it may not be dead, it was (at the least) comatose.

Call for Papers and Dissertations The 38th Annual Meeting of the Business History Conference

Pasadena, California, March 6-7, 1992

The theme of the 1992 conference is "The History of Business and Public Policy." Proposals for papers on other topics are welcome as well. The goals of the thematic papers, offered in plenary and concurrent sessions, include explorations of the ways in which business firms and business associations have influenced public policy, and the ways in which government policy has influenced the behavior of business firms and associations. Proposals that seek to explore the theme in an international perspective are especially welcome.

In selecting the program, the committee will give preference to those who did not offer papers at the 1991 meeting. Strong preference will be given to proposals received by September 10, 1991.

To propose a paper, please submit a one-page abstract and a brief curriculum vitae to:

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Those who have completed dissertations in business history in the last three years (1989-91) are eligible to submit their work for inclusion in the dissertation session. The Herman E. Krooss Prize is awarded annually for the best dissertation presented at the meeting. An abstract and copy of the dissertation should be submitted by October 1, 1991 to Professor Wayne Broehl, Amos Tuck School of Business, Dartmouth College, Hanover, New Hampshire 03755.

Soviet/American Exchange in Agricultural History

by Richard Sutch, University of California, Berkeley

In March 1991 a symposium on The Transformation from a Rural to an Industrial Society, hosted by the All-UC Group in Economic History, was held at Stanford University and the University of California. About 60 academics and community members at four California sites participated in the program. along with the five Soviet scholars who were their guests: Andrei K. Sokolov, Michael A. Svishchev, Tatyana L. Moiseenko, Leonid I. Borodkin, and Leonid Milov.

Until recently, a forthright analysis of economic development, of transferring resources from rural to urban settings and comparing the experiences of the United States and Russia/USSR by Soviet scholars would have been difficult for political reasons. The history of forced collectivization, the over-taxing of agriculture, the destruction of private property and private initiative, the lack of financial and marketing systems linking the agricultural and urban sectors are not well understood by Soviet historians and yet at the same time are the source of many of the country's current difficulties. The objective of the exchange was to provide an opportunity for analysis and comparative perspectives on these issues at a time when it may fundamentally alter the course of Soviet scholarship and policy.

The program included formal presentations of papers by Americans and Soviets followed by roundtable discussions; intensive discussion of research and policy issues; discussion of computerized data sets; tours of agricultural research facilities; and excursions to art museums, historical sites, and repositories of scholarly research materials.

The program began informally in San Francisco on March 16 and 17 with an orientation and exchange of papers. On March 18 the proceedings were opened formally at Stanford by Leonid Borodkin, who presented his paper Macroanalysis of Migration Flows Structure of Rural Population in Russia; a lively discussion followed. On March 19 and 20, the symposium moved to UC-Davis, with presentation of papers on California agricultural history by Morton Rothstein (agricultural trade), Brian Thompson (railroads), and Bob Witter (Sacramento Valley farming), and visits to computerized data facilities. Michael Svischev presented a paper asking Was the Great Break-Through Historically Inevitable? Finally, the visitors moved to UC-Riverside for further exposure to computerized data sets, and, on March 23rd, for an all day symposium which included presentation of papers on The Russian historical process (Milov), rural-urban migration during the Soviet industrialization drive (Sokolov), and on aging and male employment in the late-19th century US (Sutch and Roger Ransom).

I am certain that the entire visit was extremely valuable in furthering lines of communication among Soviet and American scholars. The structured symposia created a stimulating academic environment, with open and vigorous exchange of ideas. The less formal social interactions prompted spontaneous dialogue of equal significance, and provided the Soviet visitors with a view of American culture and everyday life.