

The Cliometric Society

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Summary of Papers and Discussion from the 36th Annual Cliometrics Conference

by Alex Field, Santa Clara University, and
Pamela Nickless, University of North Carolina at Asheville

(Nashville) The 36th annual Cliometrics Conference met under sunny, warm, and humid conditions, May 17-19, 1996 at the Scarritt-Bennett Center near the campus of Vanderbilt University. Conference organizers Jeremy Atack and Robert Margo did a superb job, anticipating and dealing with almost all logistical issues. Travel arrangements, for the most part, were handled flawlessly. Many of the 46 participants also agreed that, during their brief visit, they had enjoyed some of the best – and the worst – food currently available in the United States. The Conference began with welcoming remarks from the Dean of Vanderbilt’s College of Arts and Sciences, Madeleine J. Goodman, who reminded us that the Clio Conference is the longest-running conference funded by the NSF and congratulated the Society on its accomplishments.

The first session, on ROW (a.k.a. “Rest of the World”), was chaired by Alan Taylor (Northwestern). Summer LaCroix (Hawaii) presented the opening paper, which analyzes the economic forces inducing changes in property rights and governance institutions in Java. The paper was somewhat unusual in that, lacking information on wages and capital stock, LaCroix used a model to derive changes in factor prices. He concludes that in 1757 Java had well-defined property rights in man, yet by 1900 property rights in man had weakened and those in land had become more sharply defined. Changes in relative factor prices were not sufficient to explain the pattern of change. Political changes, such as the consolidation of Dutch power in 1830, were particularly important.

Steve Broadberry (Warwick) began discussion by inquiring about several of the tables, particularly expressing skepticism about the figures on population, which imply an improbably high rate of population growth. LaCroix replied that the population data are poor and growth rates may be overstated. In response to a follow-up question about changes in the terms of trade (which also seemed too large), LaCroix said that a substantial decline in transportation costs drives the index. Larry Neal (Illinois) asked about the role of rice as a consumption good in this period. LaCroix replied that Java became a rice importer by 1875, but rice is not included in the terms-of-trade calculation. This is a serious difficulty, and he is looking for references on the consumption of other kinds of food grains in Java.

Joel Mokyr (Northwestern), speaking for the colonizers, complained that the paper is not really about property rights; it is really about Dutch taxation driven

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Executive Director's Notes

At the end of May we began experiencing computer problems. The shareware version of Listproc we used started to crash regularly. We installed commercial Listproc, which required a server operating system upgrade. In turn, the new OS needed numerous "fixes" to make the other programs run properly. A month later, we have recovered and feel better off for our transition.

I was away during these trying times.

The first three weeks of June, I visited Russia, Italy and Spain. I first went to Moscow to meet my host Leonid Borodkin at the Center for Economic History and Theory. His lab had just been connected to the Internet, and the group was quite excited about World Wide Web. There I gave lectures on the history of pensions, Cliometrics, and EH.Net, and also in St. Petersburg, my next stop.

Apparently most cliometric work in Russia is done in history departments: economic history has not yet developed as an important field in economics. It is very hard to attract economics students into economic history, since the opportunity cost for graduates working in private business is at minimum four times the academic salary, and the job market for history majors is worse.

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From Russia I went to Montecatini Terme, Italy, to attend two days of the week-long summer school organized by Gunnar Persson, Giovanni Federico and Paolo Malanima and sponsored by the European Historical Economics Society and the ESTER consortium. This was its third year, and about 40 people attended. Senior "experts" give papers in the morning and students present their work in the afternoon. I was very impressed by the concept and the organization – and the meals and wine. While there I met with John Komlos and others to plan next year's World Congress of Cliometrics in Munich. John is arranging accommodations and excursions, and obtaining funds for the congress. I also addressed the school about Eh.Net and the Congress.

I next went to Seville, Spain, to meet with Gabriel Tortella and Clara Nuñez Romero-Balmas, and the staff of the organization managing the 1998 International Economic History Congress. I have been advising them about setting up Web sites for the IEHA Congress and using electronic forms for registration, hotel reservations, and excursions. The staff are very interested in using the internet for the Congress.

I can report that plans are progressing well, and that those attending the IEHA Congress are in for a real treat. Since the Congress is scheduled during an academic break, sessions will be held at the University which occupies the converted tobacco factory made famous in *Carmen*. It is located at the very center of the old city next to the palace, garden, the river and shopping.

I arrived home just in time to train for a triathlon on July 7th. I finished in 2:04, and was able to come to work the next day.

Economic History Services News

The Cliometric Society membership directory has been updated, so the mail-forwarding service (alias) works again. You can address e-mail messages to Society members at their **first initial last name@cs.muohio.edu**. The EHA and BHC directories will be updated soon.

The new *Abstract Archive* is up, and we hope all members will contribute. *Data Registry* now offers an easy-to-use electronic registration form. [Please see announcements for details.] *Syllabi* has many new contributions; if yours is out of date, please send a current version. In coming months we will publish the program for the IEHA Congress, information about the World Congress of Clio in Munich, and more.

AN INTERVIEW WITH PHYLLIS DEANE

*Editors' note: Phyllis Deane is Emeritus Professor of Economic History in the University of Cambridge, where she held research and teaching positions from 1950 to 1982. She was editor of the **Economic Journal** (1968-75) and President of the Royal Economic Society (1980-82). She became Fellow of the British Academy in 1980.*

The interview published below derives from a conversation in 1993 between Professor Deane and Nicholas Crafts (London School of Economics), that was video-taped for inclusion in the series "Conversations with Historians", produced by the Institute of Historical Research, London. This version is printed with permission of Professor Deane and the Institute of Historical Research. The interview was prepared from a transcript of the tape and edited for publication by Jane Humphries (Cambridge) and Nick Crafts (both of whom were students of Professor Deane).*

*We are pleased to include this conversation with Phyllis Deane in our series of interviews with the "Founders of Cliometrics." While she does not consider herself to be a cliometrician, she was one of the first economic historians outside North America to marshal a broad range of quantitative information about the economic past in a systematic and analytical fashion, notably in her joint work with W.A. (Max) Cole, **British Economic Growth, 1688-1959**.*



Photograph by Dorothy Hahn

I'm delighted this afternoon to have the opportunity to talk to Phyllis Deane, one of Britain's best-known economic historians, author of *The First Industrial Revolution* and many other works. Before we talk about the work itself, I think it might be useful to find out how you started out in life and consider the period before you went into academic research. I believe that you spent a nomadic childhood, ending up in Scotland.

Yes. My father was an employee of the Admiralty and his job took him to Glasgow just at the point at which I could spend the last two years of my schooling at Glasgow, and that put a bit of discipline into my education; from there I went to the University. I originally intended just to study history, but it was a four-year course starting with a fairly

wide first year, and my horizons were opened up to economics. I decided to do a degree in what was called Economic Science and was actually a mixture of political economy and history.

So you became more of an economist than a historian?

I did, as it happens. I think I was influenced greatly by the economics teachers. I certainly had some very good history teachers. I can remember G.O. Sales as a rather splendid lecturer whom I always tried to emulate when I got onto the rostrum. I was also taught by William Robert Scott, who of course was not only an economist but a historian, author of a famous three-volume textbook on joint stock companies at the beginning of the 18th century, and also by Alec Cairncross, who then was very young and fresh from Cambridge with *The General Theory* clutched in his hand which he introduced to us at a fairly early stage. This was in 1938-39. My interest was stimulated by this, and so I did more economics and economic history, although there was a steady stream of political and constitutional history in the course.

*The videotape of the interview with Phyllis Deane will soon be available. Further information about this and the 21 other interviews in the series can be obtained from the Publications Secretary, Institute of Historical Research, University of London, Senate House, London WC1E 7HU, England. Telephone: (0) 71-636-0272; Fax: (0) 71-436-2183; e-mail: j-horn@sas.ac.uk The IHR will supply tapes at a discount through December 1996.

So there was a strong influence of the new Keynesian economics?

Indeed, yes.

Was your degree more theoretical or institutional?

It was fairly theoretical. Not greatly institutional.

But by today's standards not very mathematical?

No, not mathematical. I did statistics, but it was a relatively unmathematical statistics.

Did you do graduate work, in the sense of formal graduate training?

Well, no graduate training, but I went on for a year after my first degree to do research into economic reconstruction. I took my finals when the retreat from Dunkirk was happening, and it really produces a very relaxed attitude to one's own career prospects when that kind of event is happening! Then I went straight into a research project run by Professor J.R. Bellerby – on postwar economic reconstruction.

We all believed at the time that the end of the war was just a year or two away, and we ought to be ready for economic reconstruction after the war. And then a year after that, I was invited to London to join the National Institute of Economic and Social Research, where I undertook a project inspired by Keynes and by Richard Stone and James Meade who had just set up a system of social accounts for the UK. What Keynes wanted was to apply this system to a completely different economy than the UK, for example, to colonial territories. So I sat in London through most of the War using the Colonial Office library and other such sources trying to work out national income for Northern Rhodesia and Jamaica and to see whether I could set the results into a system of social accounts. I was very fortunate in having Austin Robinson as a supervisor. Many well-known economists were working in Whitehall for the war effort, including Austin Robinson, Richard Stone, James Meade, and Arthur Lewis, the last three of whom subsequently became Nobel Prize winners. They used regularly to come over to the National Institute to have a sandwich lunch with me and advise me on my work. So I started out with an advantage that very few research students have. I had interested attention from people who found it a break from their daily grind and were glad to assist me. At the end of the

War, as soon as the seas were open again, I wanted to go to developing countries to find out what it was like on the ground, because I had so far worked with documentary evidence only. So in 1946-7 I went to Northern Rhodesia and Nyasaland and produced national income accounts for them.

Do you think that those national income accounts were actually reliable?

Well, of course it was what one of my colleagues once called the "perpetual invention" method. I had to make estimates all along the line on the basis of rather little data, experimenting all the time with the kind of data I could find. It ranged from actually sitting down in African villages and taking family budget studies to going visiting copper companies finding out what their accounts meant and getting the latest details from them. I was also fortunate enough to be allowed into the census office in Lusaka – this would never be permitted nowadays – where there had recently been a census of the European population. The returns included income data and I was allowed to work through the originals. There wasn't a large European population but I was allowed to make use of the great variety of data in the returns. I did not know how accurate the information was. What was quite clear was that most of Northern Rhodesia's national income was generated by the copper companies. It was also clear that for most people their standard of living depended on agricultural yields; it was an eye-opener as to how uninformative such aggregates are and how important it is to analyze the components. There were a lot of interesting questions that arose as to what the concept of national income meant in a country like that, and what uses it might serve.

So in your early career you really did get your hands dirty?

Yes, very dirty! [laughs]

So you got a grounding in national income accounting and an exposure to development economics, where Arthur Lewis would have been an important influence at the time.

Indeed, yes. This was a period when everybody was interested in the conditions of economic progress. In the early 1940s Colin Clark had produced his book on *The Conditions of Economic Progress*; economists were already looking forward to economic reconstruction and to

stimulating economic progress in developing countries.

So this brings economic growth very much to the centre of the stage; that has been a big feature of your intellectual activities since. But before we pursue your work after this experience, going back to the earlier years, did living through the 1930s – quite a traumatic period in economic and political terms – did that have any serious influence on your later career?

I think living through the 1930s made one much more receptive to the sort of economic policy which was emerging from, say, *The General Theory*, and made one understand what the high level of unemployment meant for a place like Glasgow, where in some areas something like 60% of the people – in Clydebanks, for example – were out of work and you actually saw it happening. So I suppose it gave me a rather strong interest in the importance of income distribution as well as growth. And of the importance of economic policy and the influence of ideas on policy.

So at the end of the spell doing colonial national income accounting, you moved to Cambridge?

Yes. I spent a brief period in the Colonial Office in a department which was said to be a research department, but it did not turn out to be academic research, and so I escaped! I was invited to come to Cambridge to join the Department of Applied Economics, which Richard Stone had just set up. My task was research into regional social accounts of the United Kingdom. I was applying a social accounting system which would show the structural characteristics of the regions.

Was that research published?

It was. But this was a relatively short phase. Meanwhile, I got involved early on in an international research organization called the International Association for Research in Income and Wealth, which was designed to bring economists in government service and in universities throughout the world in touch with each other, to share problems of estimating national incomes and applying the results. Simon Kuznets was one of the initiators and founders of this organisation. It was instructive to bring a great many academic economists in contact with their counterparts in government. We were all learning from each other all the time. With this organisation behind him, Simon Kuznets – who had already been doing national income research in the United States from the

'30s onwards – set out to initiate inquiries into long-term growth, not just national income studies as a basis for current policy, but historical national income studies in all countries of the world.

The purpose of studying national accounts historically or taking the long view was what?

Well, it was to analyze the conditions of economic growth and the reasons for the differences in rates of economic growth the world over.

So this might inform economic development policy?

Yes.

And so it was natural to try to push things back towards early industrialization?

Yes.

And you then got involved in the project to examine British national income historically?

That's right. Yes.

Going back to 1688 I suppose?

That's right. To the Glorious Revolution! To Gregory King!

Now that work was published in the form of various articles as you built it up, and eventually formed the backbone of perhaps your best known research monograph, *British Economic Growth, 1688-1959*. Looking back at the work, what's your main memory of how you actually put it together? It seems a monumental task; certainly everyone who's researched afterwards has always used that work as the starting point. It must have looked like an absolute mountain before you started.

Well, it did seem a mountain because only two of us were involved in it in the first instance.

Yourself and Max Cole?

Myself and Max Cole. Yes.

Seems strange in a way. We normally think of a big team these days.

You do nowadays. We were of course really skimming across the surface of the information. We could not collect a great deal of primary data. We used data which had already been either processed or published in some sense.

Would you say the thrust was description as opposed to analysis, description in an analytic framework?

Yes, it was. Yet *British Economic Growth* threw up a lot of analytical problems as we went along.

So you were setting an agenda for future research rather than a definitive set of estimates?

That's right. We were trying to see how far we could develop a set of estimates which were sensible and plausible on the basis of the data we had. But they could at best be only hypotheses.

You set out those hypotheses – I think it would be fair to say – in a not particularly formal way. Was that a deliberate choice or did you just think that actually they were not capable of being formalized?

I think we approached it piecemeal, as it were. Nobody else had done quite that sort of thing before and we just beavered away at trying to produce useful results.

By the time *British Economic Growth* was published in 1962, the big news in this area of economic history and development was the arrival of Walt Rostow. Certainly when I was a student his ideas on take-off, leading sectors, and the stage theory of growth very much ruled the roost.

Indeed, yes. And it was a very important new set of ideas, very striking, very dramatic, with almost a political rather than an economic kind of motivation behind it. By the time he published his 1956 article in the *Economic Journal*, estimates were being produced not only in Britain but in other countries, and Rostow stimulated comparative studies and fresh research.

At the time he was actually writing that article, and then the book, which came out in 1960, was there a lot of interaction between you, or you and Max Cole, and Rostow?

No. We did meet him but the big interaction came in the early 1960s when there was a conference of the Interna-

tional Economic Association – again something set up by Simon Kuznets – which had brought together economists of all degrees of theoretical or empirical bias, economic historians, sociologists, to sit round a table and attack Walt Rostow. Of course, he absolutely flourished under this and it led to the International Economics Association publication *The Take-off into Self-Sustained Growth*. What the round-table did was to make some of those involved in Simon Kuznets's international project combine to fight the more implausible parts of the Rostow thesis and to engage in new research in order to prove their point.

So you were engaged in asking the question: Do Rostow's ideas match the evidence? And you concluded perhaps that they didn't?

They didn't. But these were questions that were really worth asking and, more important still, they were worth answering in detail.

So you thought it important to throw the stones. Do you still think there's any value in the notion of take-off, which was indeed the *leitmotif* of that work?

Not really. I don't think there is any country in the world in which his particular model could be justified.

It was too revolutionary and too dramatic?

Yes. Much too dramatic.

So growth is generally a more evolutionary process?

I would go back to Ashton, who believed that there was evolution and continuity about the business of an industrial revolution, for example, and that it is distorting to try to turn it into a revolutionary process.

Is this one of those cases where the average conceals?

Yes.

So you ended up thinking that the Rostovian programme was misguided but had been fruitful?

I think it was fruitful partly because it was flamboyant. You couldn't avoid taking notice of it.

Do you think Kuznets's contribution is more convincing than Rostow's, then?

Yes, I do. I think it was more careful, less designed to create an instant impression. But perhaps Kuznets's project didn't stimulate discussion so much as Rostow's.

British Economic Growth comprised in a sense a new view of the British industrial revolution – certainly different from some of the earlier strands in the literature which had emphasized revolution. In compiling this work and thinking about it, what were you able to draw from people like Clapham and Ashton, who probably were the most famous of the relatively recent writers at the time you were doing your work?

I think we got more out of Ashton, 'though I'm not sure how far we expressed it in *British Economic Growth*. What one got out of Ashton was a feeling for the fact that there are institutional, social and other factors to be taken into account which are left out of our aggregates.

And from Clapham?

Well, there's a great deal of detail in his work. But I'm not sure that we got more than a mine of information out of Clapham.

Perhaps it's not very exciting to read, either.

It's not just that. He had a descriptive rather than analytical approach to statistics.

Then you came to Cambridge to work in the DAE. I imagine that was very much a research post.

To begin with just a research post. I had no desire to do any teaching. When I left university I thought that there were two things I didn't want to do. I didn't want to enter the Civil Service and I didn't want to teach. Well, I was quite right about the Civil Service. I did go into it for two years and it was not satisfying. But teaching in Cambridge I found rather attractive because I started by supervising, which involves an exchange between equals, rather than pontificating. Eventually I was appointed to a Faculty post in teaching.

That would have been about 1960? Did you find the transition from research to teaching difficult?

I started lecturing in the '60s. It was not difficult, but it was an advantage to have done research because it gave one something to communicate. Lecturing was a different game, but if you're not lecturing to the first year in

economic theory – and I never was – you are lecturing to small groups of people and you can create a discussion.

Did you find the process of writing your lectures helped your research or was it a distraction?

I think it helped the research. It certainly forced me to concentrate on putting my findings into an interesting form.

The lectures led to *The First Industrial Revolution*?

Yes.

In writing that book what did you seek to achieve?

What I wanted to do most of all was to introduce the economist's approach into economic history – much more than had so far characterised existing texts. I wanted to use economists' methods of analysis and bring in enough economic theory to fit economic history into a first year course for students working towards a degree in economics.

So you were trying to make links to things like development economics?

Yes. I was currently teaching development economics to graduate students.

Yet, at the same time, in reading that book I detect an attempt to adjudicate on the well-known arguments of the day. You do have a view on the standard of living or on the population growth controversy. And yet I don't think you published very much in the way of research articles on those questions. May I ask why?

Well, I did not actually do original research in that sphere; my lectures were largely a synthesis of my own and other people's researches.

There has been an enormous amount of work done subsequently. Do you find your views have changed very much? Do you take a view on the renewed debate on gradual *versus* somewhat more dramatic growth experience in the late 18th and early 19th centuries?

I still hold the view that it was a fairly gradual experience. The reason why the opposite result looked plausible was that much of one's information at that time was based on the trade statistics. These were misleading because in

1783 the War of American Independence ended and trade suddenly soared.

So there's a tendency to create an artificial view of explosive growth. What you just said raises an interesting question. You mentioned construction of index numbers. I suppose the first version of national income accounting was done in current prices. In looking at the long-run picture, the index number problem looms very large. Do you think economic historians in general handle that well?

I think they still haven't solved the problem. In the last analysis, the fact that the values of consumers and governments change through time creates difficulties of weighting. I'm not sure that it makes much sense to try to construct index numbers that stretch over very long periods of time.

And yet you yourself were engaged in a very long-run view. Is there a contradiction?

No. It's just a lesson learned about the difficulties of interpreting statistical aggregates in a context of radical structural change!

To finish off on the industrial revolution, the main thing that I got from your work was not to overvalue the importance of cotton. That does seem to be a message which has been resilient and robust in the subsequent period.

Yes, I think it has.

Now let me move away from the industrial revolution and back to working in Cambridge at that time. My recollection as a student is that 1960s Cambridge was a pretty Keynesian kind of environment. That obviously influenced your work. Do you think it limited it?

Yes, it probably did.

Did you spend much time in America or working with Americans who on balance took perhaps a more neo-classical view of things?

I taught in America for a couple of short periods. I probably was limited by my Keynesian preconceptions. You have to remember that I started work in what seemed the brave new world of Keynesian macroeconomic analysis in the early 1940s. It would have been rather difficult

to get that out of my system.

And it inspired a lot of very interesting work in British economic history, much of which was around the idea of the trade cycle or economic fluctuations. One thinks of Cairncross, one thinks of Ford, of that generation. Different probably from what was starting to develop on the other side of the Atlantic. In the early '60s we started to confront the so-called New Economic History – Fogel, the railroad, social saving. How did you react to that? Did it affect your own research programme at all? Did you approve of it?

I approved of it in the sense that I thought these were interesting new questions which produced surprising answers! But it wasn't the sort of activity that I was tempted to imitate.

Let's think how your work progressed in the 1960s. You tended to move more towards the history of economic thought?

Yes, from the late '60s onwards. I found myself stimulated by the recent work that had been done in the history of ideas. It was also relevant that if one was not prepared to set up a research team in the sort of work that I had been doing so far, it was not possible to make much impression on the subject as an individual.

The natural follow-up to your work was, I suppose, the Matthews, Feinstein and Odling Smee volume. You didn't want to participate in that team?

I felt that I had done what I wanted to do in that area, and I was glad that the task was being picked up and moved further, as it were, but I'm somewhat a lone ranger!

You prefer to work on your own?

I prefer on the whole to work on my own, except when, as with Max Cole, I find myself with a very congenial collaborator who is also in a sense working on his own. We worked side by side rather than as a team.

So you chose to work on your own more in the history of economic thought?

To begin with, I was invited to give a course of lectures in the history of economic thought. Partly as a result of being in a faculty which was deeply involved in these

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History of Health and Nutrition in the Western Hemisphere

by John Murray, University of Toledo

(Columbus, Ohio) Thursday evening, 8 March 1996, witnessed the gathering of 40 historians, economists, and anthropologists at the Parke University Hotel for the second "Health and Nutrition" Conference, organized by Richard Steckel (Ohio State) and Jerome Rose (Arkansas). This interdisciplinary project stems from conversations between Steckel and Rose at the 1988 EHA meetings in Detroit. Sensing the synergy that collaboration between historians familiar with written records and physical anthropologists expert in interpretation of skeletons could produce, they organized ways for members of the two disciplines to communicate. A result was a series of meetings beginning in 1990, sponsored by Ohio State and the NSF, that taught historians about the use of skeletal collections as a data source and anthropologists about the historical context of the skeletons. Seventeen research teams were initiated at those meetings. These historians and anthropologists have gathered a standardized database concerning some 13,000 skeletons, organized by period and location. The present meeting brought together members of all the research teams for progress reports (each paper was a joint project, but only the presenters are mentioned here) and for acquiring an all-important overall perspective of trends in the health of people living in the Americas before and after European contact.

The conference began on Friday with a session on methodology. Alan Goodman and Debra Martin (both of Hampshire) presented the information available from the skeletal record and discussed how to quantify it in "Reconstructing Health Profiles from Skeletal Remains." Skeletons provide multiple indicators of stress, and thus allow the use of epidemiological concepts in paleo-anthropology. Estimates of age at death can indicate both stress and adaptation. Subadult growth, measured by the length of long bones, provides estimates of stature, the uses of which are now familiar to economic historians. Enamel defects in teeth provide evidence of disease and nutritional stress at particular ages. Lesions appearing on the cranium, in the eye orbits, and at the ends of long bones are generally accepted signs of anemia, which in turn could result from nutritional (iron) deficiencies or infectious disease. Other bone lesions can be attributed to tuberculosis or syphilis. Some bone fractures can be attributed to blunt object trauma, a clear sign of violence

whether in the household or between social groups. Evidence of arthritis can be attributed to strenuous physical labor. Finally, dental caries (cavities) can be a key to diet and morbidity, with attendant productivity effects.

Daniel Scott Smith (Illinois-Chicago), a historian, opened discussion by noting the complexity of the biological issues, and asked about the extent of agreement among anthropologists on the analysis and interpretation of a given bone. Doug Owsley (Smithsonian) observed that in general there is a high degree of reliability in anthropologists' bone interpretations. Rose described the general process as analogous to the identification of present-day skeletons in forensic work. When recent skeletons are eventually identified and connected with their medical histories, the anthropologist can compare health estimates from the bones with the written medical record of the deceased. Alan Swedlund (Massachusetts-Amherst) questioned the functional meanings of morbidity markers which have causes well outside our own experience: Can we truly extract the actual biological life course from skeletal data?

Bob McCaa (Minnesota) presented the next paper on "Paleodemography: What Do Graphics Show That Statistics Cannot?" He presented two alternative demographic estimators, hazard models and life tables, and compared fertility- to mortality-centered demographic history. He described the problems that accompany what he called the "whopper assumption": that the set of skeletons at any one site was laid down by a stationary population. He concluded that the model life tables of Coale and Demeny did not fit the estimated mortality patterns in New World skeletal populations as well as those from a very high mortality regime, *e.g.*, that found in Liberia among "returned" former slaves. Smith remarked on the seeming paradox that under the "whopper assumption", average age at death is really driven more by fertility than mortality. The extremely high estimated mortality among the over-50 age groups could be overestimated if the population is growing rather than stationary. Philip Curtin (Johns Hopkins) remarked on the difficulties of appealing to data from uniquely disastrous mortality regimes like the Liberian. For example, age at entry into the regime, if the population is not closed, is critical in determining immunity from infectious disease.

Sheila Ryan Johansson (Cambridge) suggested that a more prudent tack might be to assume that a population is growing.

The third paper, "A Health Index from Skeletal Remains", was presented by Rick Steckel, who proposed an index to quantify skeletal information based on the quality-of-life indices used in the current health economics literature. His index is a weighted average of health conditions as determined from the skeletal record (the "quality index") in each year of life, which is then summed up over all years of life to get the "health index". He remarked that correlations among all measures (the health index, the quality index, a health index that accounts for the degree of pathology, and average age at death) were upwards of 0.97. Since this is only the Mark I version of the health index, he sought advice regarding revision. Smith proposed that the "whopper assumption" created some problems for the index, as a growing population would have a lower health index through the counterintuitive effect of lower average age at death. He recognized the pitfalls of a socially-constructed notion of morbidity that has resulted in negative correlations between morbidity and mortality. Discussion ensued on the potential differences between chronological age and the skeletally-estimated physiological age. Owsley asserted that up to about age 35 the two estimates would be quite close, and any difficulty in estimating age of older skeletons was offset by their rarity in available samples. Philip Walker (UC-Santa Barbara), Shelley Saunders (McMaster), and Susan Pfeiffer (Guelph) all suggested that differences in the two concepts tended to be greater than Owsley had claimed.

The second session focused on three samples of "Euro-Americans in North America." Shelley Saunders presented a paper summarizing studies of "The St. Thomas Anglican Church Cemetery, Belleville, Ontario", where the congregation's desire to expand its buildings onto a former graveyard resulted in the disinterment, analysis, and reburial of 604 individual skeletons. This site was ideal for inclusion in the present project because extant parish records could be linked to the skeletal data. Linkage shows that this was very much a growing and open population, and that the skeletal sample is fairly representative of Belleville in the mid 19th century. Differences between the written and skeletal records emerged; for example, contrary to expectations, the proportion of infants was greater in the skeletal sample than in the parish records. Agreement between the two sources could be found in the importance of acute infectious disease in

determining mortality, especially among children.

Larry Neal (Illinois) began discussion by noting the role of "immigrant shock" in affecting mortality in a population with many new arrivals. He also pointed to evidence from New England for climatic shocks at this time and suggested searching for the same in the Belleville sample. Clark Larsen (North Carolina) queried the post-1860 decline in mortality, which Saunders attributed partly to the opening of a new Anglican parish in Belleville. Larry Sawchuk (Toronto) said the effects of immigrant shock, in particular, may have declined after 1850, since the proportion of Irish-born in Upper Canada was falling.

Paul Sledzik (Armed Forces Institute of Pathology) then presented "Military Skeletal Samples: 1778-1865." The huge body of historical literature on the military makes even the four small samples of 18 to 30 skeletons described here invaluable for connecting the historical to the skeletal record. Samples used here were recovered from Ohio, Ontario, South Carolina, and New Mexico. Not surprisingly, evidence of weapon wounds was common. Owsley remarked that the Confederate soldiers in the New Mexico sample showed evidence (such as stress fractures and vertebral erosion) of extremely strenuous service in the weeks before death. From the written record, we know that these men were marching long distances and pulling heavy artillery pieces, probably causing their bone deficiencies. Future research will examine issues of selectivity among the military and the effects of health conditions on the survival of soldiers.

Discussant Swedlund referred to the rarity of bone lesions (porotic hyperostosis) in a historical population. Myron Gutmann (Texas) noted the dangers of reporting several significant digits in statistics from samples of only 10 or so skeletons and, initiating a theme to which the conference would return several times, suggested tight limits on generalizations made from small samples. Regarding small sample sizes, Sledzik remarked that very few military samples were likely to become available, since the US government is very reluctant to disinter veterans. Mike Haines (Colgate) observed that stature estimated from some soldiers' skeletons was a function of pre-military conditions, unlike that from the Confederate soldiers who were drilled to exhaustion.

Lorena Walsh (Colonial Williamsburg) and Roseanne Higgins (SUNY-Buffalo) presented their group's paper on "The Poor in the Mid-19th Century Northeast", which was based on skeletal data and written records from the

Monroe County Almshouse in Rochester, New York. This project has benefited from an unusually rich historical literature on the New York poor, on other poorhouses, and on economic conditions during the period of the poorhouse's burials (1826-1863). Mortality in this sample, as well as in Rochester at the time, was quite high, especially among children and infants. The record suggests that disease conditions within poorhouses were probably even worse than elsewhere in Rochester. However, quality of life may have been much better than the rather short estimated life expectancies would indicate: for example, mean height derived from the almshouse skeletons was greater than mean height of a sample of Civil War recruits from New York State. Walsh concluded that the availability of food supplies in the Northeast to mid century was such that even people in the lowest economic strata had moderately good nutrition.



Conference organizers Rose and Steckel

Discussant Neal mentioned the particular importance of recovering infant skeletons in this study, as the condition of the infant skeletons (unlike those of adults) reflects their having spent their entire lives in the poorhouse. Swedlund asked about poorhouse admissions practices, which may have excluded the "undeserving" poor, leading us to a restricted view of poverty. Haines suggested that turnover among poorhouse residents was high, so that skeletal conditions likely reflected conditions elsewhere. Ted Rathbun (South Carolina) asked why the health index of the poorhouse skeletons was greater than that of the Belleville Anglicans. Saunders suggested that the difference was due to differing average ages at death and the swift growth of the Belleville population, at over 5% per year from 1820 to the 1870s.

The next session considered "African-Americans in North America." Ted Rathbun and Rick Steckel led off with a paper on "The Health of Slaves and Free Blacks in the East", focusing on discoveries of skeletons, including some European-Americans, around Charleston, SC, and their historical context. Rathbun mentioned the "gray, semi-published literature" of anthropology, consisting of

reports written under contract for the site owners or state governments. Such documents are not usually available through ordinary library sources and present the scholar with an unusual challenge in discovering what is already known about a site. Rathbun commented that skeletons of African-American women tended to show signs of degeneration in their shoulders, which he attributed to the repetitive motion of flailing rice. These bones also showed a heavy lead burden, possibly from drinking wine sweetened with lead acetate. The skeletons of whites also contained lead, and the use of gold fillings in teeth suggested a high level of wealth. Dennis Van Gerven (Colorado) questioned the use of the term "sample" when we lack information on the population from which the skeletons were obtained. Since these were certainly not random samples in the usual statistical sense, he recommended use of the term "data aggregates".

Jerry Rose presented Friday's final paper on "Diet and Health on the US Western Frontier", which examines an eclectic mix of skeletons of Mormons and Chinese immigrants, skeletons from small family cemeteries in east Texas, African-American cemeteries in Arkansas and Dallas, and two Chicago poorhouse cemeteries. Skeletons exhibited the hardships of frontier life through lesions typical of the anemia associated with a maize-based diet and high rates of parasitism. African-Americans at the Arkansas site were as tall as present day African-Americans but there was extensive evidence of anemia, tuberculosis, and syphilis. During the discussion, several people asked about differential access to medical care, especially if, as Ken Kiple (Bowling Green) commented, some might have benefited from a *lack* of medical attention. Gutmann warned that similarities between the Arkansas and Dallas populations might be due to rural-urban migration, resulting in samples from essentially the same population. The day's proceedings closed with a pleasant dinner at the À la Carte Restaurant near the Ohio State campus.

The remaining sessions examined skeletal records of

various Native American populations. These were organized by geographic region, beginning Saturday morning with "Native Americans in Central America." In "Social Disruption and the Maya Civilization of Mesoamerica", Rebecca Storey (Houston) described the two great disruptions of Mayan history, the first occurring around 1000 AD and the second after European contact. Other than the 200 skeleton sample from Xcaret, the samples she analyzes antedate contact. Most of these samples showed rather mild signs of morbidity combined with early age at death, which was attributed to successive waves of new diseases. In her discussion, Pfeiffer said that the conventional story of the sudden collapse and disappearance of Mayan civilization is not a useful concept - let's dispose of it. Storey's emphasis on continuities in Mayan history presents a more accurate picture. Pfeiffer also emphasized the genetic basis of height differences, since even today average heights of well-nourished Mayans are small by European standards, and suggested that a better measure might be child growth relative to final adult height. Johansson stressed the importance of accounting for migration flows and their compositional effects on measured variables.

Lourdes Marquez Morfin (Pittsburgh) then discussed pre-contact Mexico in "Health and Nutrition in Pre-Hispanic Mesoamerican Populations." The 800 skeletons in this study come from four samples, including one from a relatively urban area, dating mostly from the 14th and 15th centuries. By contrast with other studies, bone lesions here did not always increase with the adoption of maize agriculture, suggesting a greater prevalence of infectious disease. Kiple wondered about the intensive nature of agriculture needed to support an urban area such as Cholula, and suggested that the signs of anemia among coastal dwellers (who had access to plentiful fish supplies) must have been due to high incidence of infectious disease. He also urged that the paper make greater use of Mexican written records which are more plentiful than for the Mayan case. George Milner (Penn State) and Marquez discussed a fascinating issue in sampling problems - the effects of human sacrifice on the skeletal residuum. Milner observed that violent death is not random, and that there is some evidence that victims of human sacrifice in pre-contact Mesoamerica tended to be the halt and the lame. Surviving skeletal sets thus would seem to come from a healthier population than was actually the case. Further discussion centered on the nutrition-stature relation. Saunders said that wasting (thinness) was a stronger influence on mortality than stunting (shortness) in modern populations. Doug

Ubelaker (Smithsonian) said that the nutrition-stature relationship is quite strong.

The next session moved to South America. Walter Alves Neves (São Paulo) presented his work on pre-contact southern Brazil in "Osteology and Prehistoric Economy in the Coast of the State of Santa Catarina." Working with skulls only, he finds no significant increase in frequencies of various pathologies before and after 1000 AD, a time when settled agriculture was thought to have been introduced at this site (because pottery began to appear). Since these people did not show increased rates of caries and tooth wear, Neves concludes that it is unlikely their diet had changed to one typical of settled agriculture. Herb Klein (Columbia and Stanford) remarked that the association of ceramics and agriculture is one of the classic questions of anthropology. A particular danger for this sample was the custom of these coastal people of interring corpses under huge mounds of seashells, which could crush the skeletons of children, introducing another source of bias. Several people discussed problems of presenting data from very small samples, which is rather more common in skeletal studies than in quantitative history. A proposed solution was to publish the entire distribution of certain measures rather than summary statistics only.

Doug Ubelaker then summarized his ongoing research in "Analysis of Health and Quality Scores in the Ecuadorian Samples." He noted that, while much of his career has involved the scholarly study of human remains in Ecuador (of both Native and European Americans), this paper is an effort to systematize his findings according to the project's newly-constructed health index. The health index is highly correlated with average age at death, while the quality scores of different skeletons are more similar to qualitative assessments in his previous work. He concludes that the quality index score shows some reliability, while the current version of the health index is too highly influenced by age at death, and is thus subject to the sampling problems that plague estimates of age at death. Pfeiffer stressed the value of Ubelaker's sample skeletons from a single area for a period of nearly 8,000 years. She described the general problem of precision, in which each stage of analysis introduces more error: choice of sample from population, choice of variables to be considered, quantification of these variables, and their interpretation, as presented here, without standard errors. Linda Newson (King's, London) noted the increase in the quality index as one moved from coastal populations to

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Report on East and Southeast Asian Cliometrics Conference

by William J. Collins, Harvard University

(Honolulu) A room with no view of the nearby beach allowed participants to focus their abundant energies on the eight illuminating papers in the first Cliometrics-style conference specifically to address "East and Southeast Asian Economic Change in the Long Run." Organizer Jeffrey G. Williamson (Harvard) escaped a dreary Boston snowstorm in time to kick off the day-long proceedings at 8:00 am on Thursday, April 11, the day preceding the enormous 48th Annual Association of Asian Studies (AAS) Meetings. The 40 participants included Evelyn Rawski (Pittsburgh) and John Campbell (Michigan), President and Secretary-Treasurer, respectively, of the AAS, as well as economists from the University of Hawaii. For a broader AAS audience Friday morning, a Clio roundtable (it was actually rectangular) consisting of Peter Lindert (UC-Davis), Peter Timmer (Harvard), John Latham (Swansea), Yasukichi Yasuba (Osaka Gakuin) and Williamson briefly recounted themes that had arisen at the conference and suggested some directions for future research on the Asian economies.

Pierre van der Eng (Australian National) chaired the opening session on living standards, which began with a joint effort on interwar Korea by Myung Soo Cha (Yeungnam) and Kenneth Kang (IMF). Confronting the phenomenon of starvation exports (rising rice exports, but falling agricultural consumption), Cha and Kang use a CGE model to explore the relative importance of terms-of-trade shocks and Japanese imperial policies such as the Rice Production Development Program and public health campaign. Counterfactual experiments suggest emphasizing different forces in different periods. From 1920-25, Japanese colonial policy drove down consumption despite improvements in the agricultural terms of trade, by simultaneously promoting population growth and excessive agricultural specialization. Between 1925 and 1931, however, worldwide price shocks turning the terms of trade against agriculture were the overwhelming factor depressing consumption. Their finding highlights the importance of viewing Korea as a small country in a much larger world economy. Finally, from 1931-36, improved agricultural terms of trade along with an industrial boom lifted both real incomes and consumption levels.

Williamson led off discussion by suggesting alternative

counterfactuals to bring a more internationally comparative flavor to the analysis than is provided by Cha and Kang's brief comparison with Taiwan. Yasuba questioned the reliability of the production data from which consumption levels must be inferred and suggested augmenting the measures of living standards to include real wages and anthropometric evidence. The authors responded that corrections had been attempted for production measures and noted that real wages fell while stature was unchanged. Lindert demanded a more thorough explanation of the mechanism by which policy is supposed to have affected export trends; in particular, in what sense were rice exports excessive? The authors replied that low-interest loans and subsidies to rice farmers under the Rice Production Development Program promoted rice exports through commercialization and specialization, while lowering overall agricultural consumption by encouraging an inefficient level of rice production. Sumner La Croix (Hawaii) suggested that the colonial government may have been trying to overcome capital market imperfections, thereby moving the economy closer to an efficient resource allocation.

Peter Timmer delves beneath the apparent simultaneity of the relation between agriculture and economic growth in "Food Supplies and Economic Growth in Great Britain, Japan and Indonesia." He draws heavily on van der Eng's pioneering collection of Indonesian historical statistics and also on Fogel's link between increasing caloric intake and increasing productivity. Most importantly, he proposes that a stable food supply is crucial to sustaining modern economic growth. At the microeconomic level, food security promotes human capital investment and "Fogel linkages"; at the macroeconomic level, food security is essential to political and economic stability and, therefore, to capital accumulation and growth. Does this imply that government intervention in food markets can accelerate the growth process, even if stabilization cannot by itself initiate growth? Timmer argues that the Corn Laws stabilized grain prices at a high level in Britain, inducing investment and productivity growth, and that rice tariffs in Japan after 1904 provided a similar impetus to overall economic development. After briefly outlining a free trade model of growth and an endogenous political economy model, he demonstrates that a "food security" model is more consistent with faster growth

than is either of the others. Finally, using van der Eng's time series data and Granger causality tests, Timmer suggests that domestic food supplies are to some extent causally related to economic growth.

Once again, Williamson leapt into the fray, questioning Timmer's interpretation of both British and Japanese history, since, he argues, it was not British trade policy that maintained the high relative price of grain from the late 18th to the mid 19th centuries. Nevertheless, responded Timmer, prices were high and stable, and encouraged agricultural improvement. Williamson also asserted that Timmer's focus on Japan after 1904 is a selective reading of the record and suggested that he address the 19th-century experience as well. Lindert doubted the importance of Fogel linkages in Britain, claiming that improvements in health were driven more by a more benign disease environment than by increased food availability. Timmer agreed that there is more to the story than food intake, but reasserted the plausibility of a link between energy inputs (food) and energy outputs (work). Yasuba suggested a number of refinements to Timmer's sketch of Japanese development, and Timmer again stressed that the Japanese government had been genuinely concerned with food security and took steps to ensure it. It must be noted that van der Eng warned against placing too much weight on inferences made from his somewhat fragile data.

Yasuba chaired the discussion of a thought-provoking paper by David Feeny (McMaster), "Thailand *versus* Japan: Why Was Japan First?" Although both countries were opened to trade at the same time, and both avoided colonization, Japan developed long before Thailand. Seeking an explanation of the divergent growth trends in Thailand and Japan, Feeny explores four broad classes of differences: resource endowments, human capital, institutions and history. Thailand's comparative advantage encouraged specialization in natural-resource intensive products that had relatively little room for productivity growth; resource-poor Japan, by contrast, became an exporter of simple manufactured goods. Literacy and school enrollment rates indicate that Japan had a substantially larger stock of human capital than Thailand. Feeny also argues that both formal institutional arrangements (property rights and local government) and cultural norms (conformity and cooperation) favored Japan's relatively early economic ascent. Finally, Feeny claims that Thailand was more threatened by imperial domination than Japan, and this constrained the scope of government involvement in long-term economic development.

Yasuba pointed out that Japan was initially a large importer of cotton textiles, implying that Feeny needs a more nuanced story of comparative advantage, specialization, and development. Williamson again recommended more comparative analysis, both within Asia and with the rest of the world, to reach a better understanding of the relation between resource endowments and growth. Feeny responded that, while he would like to incorporate more countries into the investigation, the cost would be loss of a detailed, country-specific study. Andrew MacIntyre (UC-San Diego) asked why Thailand took so long to adjust its institutions to accommodate faster growth. Feeny emphasized how the imperialist threat constrained government policy-making; for example, although the long-run value of better educational institutions was clearly recognized, a short-run preoccupation with maintaining sovereignty distracted from such reforms. Latham wondered whether entrepreneurial failure might have played a role in Thailand's relative stagnation, but Feeny asserted that immigrant Chinese merchants were very successful in establishing rice mills, noting also that while comparative advantage discouraged industrialization, poor entrepreneurship did not.

Feeny removed his presentation cap and donned the chairman's chapeau as Jeffrey Williamson and Matthew Higgins (FRB-New York) presented their ambitious joint work on "Asian Demography and Foreign Capital Dependence." They argue that the impressive rise in Asian savings rates since the 1960s is largely explained by a decline in youth-dependency burdens. Criticizing models that invoke a steady-state relationship between dependency and saving and ignore investment behavior altogether, they develop a simple neo-classical growth model, including overlapping generations, to explore the transitional dynamics induced by exogenous demographic events. Econometric analysis supports a view that demographic variables are major determinants of investment and savings rates in Asia. Moreover, *ceteris paribus*, the paper suggests that demographic trends will tend to push Asian countries towards becoming net capital exporters by the year 2025. While the current paper does not suggest where all this Asian capital will go in 2025, the authors hope to explore the issue in a follow-up paper.

After Andy Mason (East-West Center) pressed the authors on some aspects of their model, Lindert asked why the rest of the world had not invested more in Japan in the 1950s and 1960s. Williamson replied that capital controls had not allowed such an influx, and continued by highlighting the potential importance of China's foreign

capital policy in determining the course of future Asian capital flows. Responding to a question from Latham about demographic influences on Britain's 19th-century capital exports, Williamson referred to his work (with Alan Taylor) which views Old World capital exports to the New World as an intergenerational transfer. Most importantly, the authors acknowledged that taking demographic variables as exogenous is problematic and admitted that endogeneity could lead to biased estimates of the relationship between demographic change and saving and investment.

After a lunch break of surfing, snorkeling, and playing in the sand, conferees returned in time to discuss two papers on rice agriculture in a session chaired by Timmer. Pierre van der Eng's latest work, "Production Technology and Comparative Advantage in Rice Agriculture in Southeast Asia since 1870", points out that different Asian countries have grown rice in substantially different ways, depending on their population densities. In thickly-populated areas such as Japan, labor-intensive methods of cultivation were employed, whereas in sparsely-populated areas of most of Southeast Asia, labor-saving methods such as broadcasting were used. This argument has important implications for recent interpretations of Southeast Asia's historical lack of development relative to Japan. Some have argued that labor was made available for Japanese industrialization by increasing rice yields through government-encouraged investment in irrigation and seed-fertilizer technology. Van der Eng argues that such labor-absorbing techniques would not have made economic sense in Southeast Asia, given its land and labor endowments, and the region's pre-war lack of industrialization should therefore not be attributed to lack of government commitment to such programs.

James Roumasset (Hawaii) raised the possibility of induced innovation in rice agriculture, prompting van der Eng to elaborate on the difficulties of adapting new techniques, such as those developed in Japan, to Southeast Asian environments. Williamson stressed potential parallels between Southeast Asian frontier economies and New World frontier economies; Timmer, however, pointed out the dissimilarities of the two regions' experiences and suggested that an explanation of the divergence would be compelling. Furthermore, Feeny recommended the use of land prices or rents to round out the story of relative factor prices and comparative advantage. Van der Eng agreed that such extensions of the work would indeed prove interesting, but, at present, there are simply too many data constraints that need to be overcome.

John Latham's title, "Rice Is a Luxury, Not a Necessity", answers the question with which his paper begins: Why did Chinese rice imports rise from 1870 to 1940? He describes several alternative staple crops including various millets, barley, buckwheat, water chestnuts, yams, and so on. Evidently rice is generally preferred to these crops, and, as income rises, consumption of rice does also, at least up to a point. Latham argues that, since 19th-century Chinese income *per capita* was below this turning point, rising imports of rice suggest that Chinese incomes were rising, contrary to the conventional view that rising Chinese rice imports signify an economy in crisis.

Williamson expressed some doubt about how Latham's current story fits in with his previous work on improving market integration in 19th-century Asia. He suggested that a larger volume of trade was the natural result of lower transport costs and increasing specialization and that increased trade may have had more to do with transport improvement than with rising Chinese incomes. In particular, he noted that no attention was paid to changes in rice prices. Latham then claimed that internal rice prices are hard to come by. Timmer responded to Williamson by asserting that there is no reason to think that trade had been immiserizing in this case; there should have been real income gains associated with the lower transport costs. Steven Morgan (Melbourne) commented that the Chinese rice-price data are not nearly so inadequate as Latham suggested, and pointed out that, relative to domestic production and consumption, the quantities involved in the trade were quite small, implying that inferences about income changes based on import data are necessarily shaky. Referring to Latham's graph of rice imports, Higgins suggested that the sudden and sustained increase in rice imports implies a regime change rather than gradually rising incomes.

Timmer chaired the final session, on "Soil and Property Rights in Agriculture." Sumner La Croix presented the first paper, "Coevolution of Property Rights in Land and Man in Java, 1757-1900." La Croix observes that, at the beginning of the 19th century, Java had well-defined property rights in man but, by the end of the century, these property rights had become diffuse while property rights in land had become more precise, changes driven by both economic and political forces. He analyzes the economic forces with a simple three-factor (capital, labor and land), two-good (agriculture and manufactures) model. In short, changes in relative factor prices, which may be induced by goods price shocks or by changes in factor supplies, generate changes in property rights.

In Java, the 19th-century decline in the land/labor ratio should have increased the relative price of land, as also should the increase in the relative price of agricultural goods. La Croix argues that such movements in factor prices tend to strengthen demand for property rights in land while diminishing the demand for property rights in labor, although property rights in man strengthened up to 1850 in Java before weakening. La Croix emphasizes the importance of the Dutch colonial government's centralization of labor exploitation in 1830 and the political reforms of 1848 in the Netherlands to understanding why the trend in property rights did not quite match the factor price trend.

Van der Eng was concerned about the reliability of the data underlying La Croix's interpretation: for example, under-reporting and changes of coverage over time may give a false impression of both levels and trends in factor supplies, and the terms-of-trade index may not include the price of imported rice (which rose substantially over the period). La Croix replied that he is attempting to reconstruct the terms-of-trade index with the underlying price data, and conceded that, although labor supply estimates are reasonably reliable, land supply estimates are problematic. Williamson suggested a more complex model with three goods, rice, coffee/sugar and manufactures, which would permit a richer picture of changes within agriculture. La Croix responded that a follow-up paper focusing on the cultivation system itself, leaving the property rights issues aside, would be interesting. Other questions concerned the dramatic rise in the terms-of-trade index and the implications of this rise for both colony and colonizer.

Peter Lindert finished off a lively day with a groundbreaking piece of work on Chinese soil quality, "The Bad Earth? Soil Trends and Sustainable Agriculture in China Since the 1930s." Lindert employs time-series data dating back to the 1930s on agricultural land to measure changes in five soil quality indicators: pH, organic matter, total nitrogen, total phosphorus, and total potassium. Organic matter and nitrogen have declined, especially in the eastern regions, but other soil nutrients and pH have improved. To place economic values on the different soil characteristics, Lindert estimates an agricultural production function using cross-section data from China's counties in the 1980s. The results suggest that crop yields depend primarily on the characteristics that have improved since the 1930s; fertilizers can substitute for organic matter and nitrogen. Lindert also ponders the effects of urban encroachment on the stock of agricultural

land. While taking land out of cultivation may decrease the effective stock of soil nutrients, this effect must be weighed against the fact that urban-industrial expansion has increased fertilizer supply, especially for farms nearest the cities. Lindert tentatively suggests that, on balance, urbanization and industrialization have tended to increase the stock of organic matter and nitrogen while depleting phosphorus and potassium. These conclusions should certainly recast the popular debate about soil degradation and Chinese food production.

Roumasett raised several red flags. First, he claimed that Engel effects do not offset soil depletion, to which Lindert reasserted his conviction that rising incomes tend to shift the product mix away from soil-mining foods. Next, Roumasett noted that soil chemistry tests are very sensitive to the level of soil moisture content; Lindert replied that he is sure the scientists carrying out the tests did so in as consistent a manner as possible. Finally, Roumasett suggested comparing the estimated production function with results from experimental plots to check the reliability of the estimates. However, Lindert claimed that while experimental data may provide bounds for the estimates, they do not serve well as checks on their accuracy. The popular suggestion that Chinese population growth, coupled with rapid urbanization, will place enormous pressure on food-producing land (and, inevitably, will lead to soil degradation and import reliance) figured largely in the remainder of the discussion. For example, Timmer claimed that China must either increase yields or increase imports, and expected that China would become a very big player in the world rice market. Lindert offered two ways to view the decline in cultivated acres: the pessimist blames it on soil degradation, while the optimist, peering through the lens of comparative advantage, claims that China has found better things to do with some of its land.

After an early morning swim on Friday, Williamson convened the roundtable to report on the previous day's proceedings and to suggest directions for future research to a group of about 50 scholars drawn from the large AAS pool. Never at a loss for new research projects, Lindert launched into a discussion of Asia's relatively low levels of social spending and suggested that relatively young countries tend to redistribute income less than older ones. Will social spending rise as Asia ages? Timmer followed up with a query about how institutions matter in the world rice market. Noting the volatility of rice prices, Timmer suggested that rice markets were better integrated in the

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EH.Net Report

by Robert Whaples, Wake Forest University

On April 27 and 28, 18 EH.Net group members met in Chicago for the inaugural network conference. Temporary advisory board members Samuel Williamson (Miami), David Mitch (Maryland-Baltimore County), Joshua Rosenbloom (Kansas), and Robert Whaples spent several weeks planning the extensive agenda, which included adopting an EH.Net charter, defining responsibilities, creating new positions and projects, evaluating current resources and planning expansion. Many decisions made about reorganizing EH.Net lists and improving the Economic History Services fileserver have been carried out since the meeting, and others are in various stages of implementation.

Drafts of the charter had been circulated to all list editors and editorial board members prior to the conference. After minor revisions, the charter was approved unanimously. [The charter can be viewed at <http://cs.muohio.edu/charter.html>] In accordance with the charter, conference attendees elected Williamson Director and Whaples Associate Director, and EH.Net editors and board members elected additional Executive Committee members Greg Clark (UC-Davis), Patricia Denault (Harvard) and Mitch by electronic ballot in May.

Plans for an abstract service were discussed at length, with the intention that this service fill a niche different from recently-announced commercial services. EH.Net will encourage individual submissions, with the proviso that a completed work must be available in hard copy or on line. At the request of Editor Larry Neal (Illinois), *Explorations in Economic History* will initiate posting abstracts of articles; other journals will be asked to submit abstracts to complement their publications. Clark and Don Schaefer (Washington State) volunteered to be editors, and Denault agreed to serve as copy editor. Abstracts can be sent *via* the internet or by mail. [See announcement on page 35.]

Details of the long-considered book review project were worked out. Using the H-Net procedure outlined by guest speaker Wendy Plotkin, reviews first will be posted to EH.Net lists, then archived in the H-Net Book Review

Library to which the EHS server is linked. Whaples assumed the position of Book Review Editor, and expects to begin posting completed reviews soon. Readers interested in writing reviews are asked to send an e-mail message to whaples@wfu.edu.



L. to r.: Row 1: Santhi Heejebu, Patricia Denault, Robert Whaples, Lynne Kiesling, Louis Johnston, Don Schaefer, Debbie Morner Row 2: Larry Neal, Greg Clark, Alan Taylor, Kyle Kauffman, Ross Emmett, Jonathan Bean, Sam Williamson, Joshua Rosenbloom, Joseph Ferrie, David Mitch, Jeremy Attack

Data Registry Editor Santhi Heejebu (Iowa) reviewed the project status, emphasizing that researchers who make interesting data sets available might motivate econometrics students to explore historical data. A large number of available sets would benefit both experienced researchers and students new to the field. Conferees agreed that contributions from established researchers should be invited, and Neal will require authors of *EEH* articles to register their data sets. [See announcement on page 29.]

In her session on Web design, Denault stressed that the presence of scholarly content on the page and the use of links to related educational material may attract significant academic resources to electronic publishing, which is slowly but steadily gaining parity with print. Ross Emmett (Augustana) reported, for example, that in Canada electronic articles and book reviews earn authors credit for publication. List editors, editorial board members, and others involved receive at least the indirect rewards of promoting their names in the field and building their resumes.

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“La Reine est Elevé; Vive la Reine” – or “From the Mouths of Babes”

The Mullah debated for a long time trying to decide whether to attend this year’s meeting of the Clioms. As always he looked forward to the wisdom that would gush forth during the deliberations and to the camaraderie that prevails. Nevertheless, he had been reluctant to attend because he worried about how he and others would react to his being among these seekers of wisdom after his other-body experiences of the past two years.

His reluctance to participate began to ebb as he realized this would be the 10th anniversary of the search for pithy wisdom. Appeal grew as he contemplated the site, Nashville, Tennessee, which, after all, has been home to some of the greatest purveyors of pith, whose tidbits would certainly be in the hunt for the Mullah’s prize. Willie Nelson, for example, would be high on the list with “Mamas, don’t let your babies grow up to be cowboys” (or perhaps clioms). An even more serious contender, however, would have been the King of the Road, Mr. Doo Wacka Doo, who had many pearls such as “You can’t roller skate

in a Buffalo herd” and “You can’t take a shower in a parakeet cage.” The Mullah envisioned as well the prospect of sighting or hearing Johnny Cash, a legend the Mullah had once had the great fortune to hear in the era before political correctness. He doubted that the man could now sing his self-parody “I hold my pants up with a piece of twine,” but perhaps he could hum a few bars.

The decision to attend was sealed by two events. The first was the passing of Larry La Prise. Perhaps in Nashville the Mullah could learn the secret of the Hokey Pokey, or

at least learn the words that followed those momentous phrases “You put your right foot in, you put your right foot out.” The second was the good omen provided by the Miss Universe contest. There the finalists were asked to expound extemporaneously on the question of “what can women teach men?” Miss Venezuela, the last to answer, exclaimed that “women can teach men how to enjoy life. After all, look at all the men in the audience who were having a good time just looking at beautiful women.” She elaborated further, but there was little need as she walked away with the trophy.



Nominees Wolcott, Taylor, and Clark examine a local artifact

The Mullah was, of course, most pleased to see that the scholar from the great desert of the Southwest would be in attendance. Over the years she has uttered some of the finest of aphorisms, most especially “Never open a can of worms larger than the universe”, but also “I didn’t know I was going to know the answer to the question, but now it’s a different question,” each of which won the grand prize in its year of utterance. Moreover, she would appear early in the pro-

ceedings and, with her inimitable style, could set the tone for the entire conference.

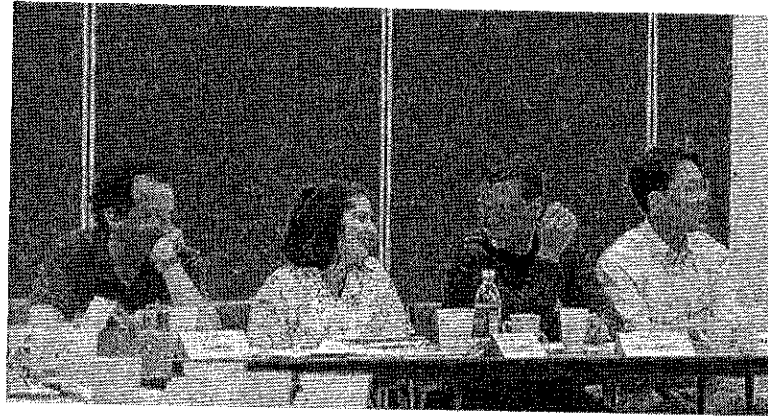
As it turned out, there was a bumper crop of pithiness. The Mullah would venture that there were more aphorisms per minute than in any of the previous meetings. Like the grapes for wine, however, quantity is not always the most sought-after characteristic. Although he heard many to choose from, the best, as good as they were, did not quite measure up to those of the great vintages, except perhaps for a small and late harvest.

One appealing strain of the wisdom flowing forth this year was that which alluded to great musical compositions. He of the Hawaiian tribe, for example, began the annual affair with the notion that in all studies of Indonesia there is a constant refrain, and the Mullah could only imagine it was "Java, Java, java-java, jing, jing, jing." The long-term Tarheel tried to croon a phrase from Old Blue-eyes, "If you'd do it my way", while he who sees little difference between servants and convicts sang that old country hit "O Loathsome Me." One of the apprentices from the Cambridge tribe reminded the Mullah of his old favorite by M. Chevalier, "Thank heaven for little piecers, for little piecers get bigger every day." Perhaps the finest musical rendition was that by the shark of the conference, who lamented that "First the tide rushes in, steals the life from the banks, then rolls out to sea, and one claims he turned it round."

The crop was so bountiful that the Mullah had to create directories in which to file the truisms. Among the "Obvious" he put "Some points may be interesting, but have little value", "If there isn't a decline after the zenith, then it wasn't a zenith!" and "Things may not make sense when you are trapped in the wrong mind set." There were of course the traditional phrases, "It is hard to distinguish Java from Nashville" and "We're not like Alabama, we're Massachusetts," and the personal ones, such as "I have sophisticated ideas, but no sophisticated data" and "I'm not unhappy, just surprised."

As always there were many methodological offerings, which seem to flow naturally from the mouths of the clioms. At least four were nearly contenders for the grand prize: "Anything or nothing is consistent with the institutional approach", "Using general equilibrium means you have to make some assumptions", "You should know what you are looking for in the data, then go find it", and "Rational expectations explain everything." The last was manifest by she who resides among the tribe of close-set plants who insisted she was inspired by what she thought someone was going to say.

There were also those wonderful offerings about specific countries that contained abundant wisdom. He who has studied the potato at great depth claimed that "Anyone as smart as the Dutch might have thought of the same thing", while the chief punster of that same tribe on the reclaimed land of the great lake said "The retreat of capital from Egypt had to do with the collapse of a pyramid scheme." With its sphinx-like delivery, this marvelous phrase was missed by many in attendance. Both of these were following in the great tradition of the 1991 winner uttered by



Young clioms delight in the wisdom of the elders

he whose name is not twice misspelled, "French data are too beautiful to be true." Unlike that platitude, this year's utterances seemed too nation-specific to claim the prize.

As usual, the Mullah consulted with some of the elders of the tribe. This year he listened most carefully to she who is not sure that an inch or two matters much. He reminded her of the ground rules that the winning phrase must contain wisdom for all ages, must be pithy enough to be easily remembered, and above all must have been uttered in the heat of battle – and, of course, that the Mullah is allowed a certain degree of literary license to refine crude utterances. There is naturally no finer example than the inaugural winner. This endearing phrase is recalled readily by any cliom, and the entire assembly reaffirms its truth and beauty each year by chanting it at the appropriate moment as the rites wind down.

There were four finalists worthy of serious consideration. Two were emitted by the Scottish member of the Davis tribe, and one has to wonder – Is that an accident? "Immovable chairs have to yield to unstoppable discussants." This is clever, even a bit witty, and very appropriate for the clioms' annual gatherings. It is nearly universal enough to win, but unfortunately there are some tribes around the world (the World Federation of Wrestling for one, the followers of Rush Limbaugh for another) who simply are unfamiliar with the concept of "discussant." His second offering, "All dead bodies get cold", seemed like a sure winner, until its author was hoist by his own petard: wives of Indian manufacturers who throw

themselves on their husbands' funeral pyres prove the exception – and his undoing.

The disciple and co-author of that same Scottish chieftain, a member in her own right of the temple of brotherly love, caught the essence of much of the meetings' discussions when she said, "It is always nice to look at the raw stuff." Such voyeurism goes on all the time at the annual meetings, and intine between meetings, but, without some further specification as to the realm of its applicability, it seemed to fall a bit short of universality. Does the rule, for example, apply to just data, or does it also refer to text? Does it hold for graphics called up on the World Wide Web, and, if so, would it include the picture of Lois Lane? Would it cover as well the King of Clio line dancing at the Wild Horse?

The winning phrase turns out to be not only universally true – so much so that one need not carry out any empirical investigation of the matter – but also *apropos* of the setting in which these meetings were held. To remind those who forgot to put their linen out: there was to be no smoking, no drinking, no carrying on, no late night talking, and, of course, each should lie down in the bed of their own making. Some of these may seem unnatural or deviant, even if, as Old King Coal said, one doesn't do them oneself. Thus, the cliché that "Everyone who follows aberrant signals will die" walked away with the prize. Can anything be closer to the truth? The related platitudes that everyone who doesn't follow aberrant rules will die, or everyone who follows non-aberrant ideas will die, get no closer to truth.

This marvelous phrase could only have flowed from the tongue of one participant – the scholar from the desert of the Great Southwest. Not content to rely on the logic or theory behind such truth, she collected a sample of 38 million observations to prove the point. It would seem no accident that she who is responsible for the founding of this great endeavor to find truth wherever it rears its head, and who is personally responsible as well for a goodly portion of the banality contained in the little book of

wisdom, has added yet additional material.

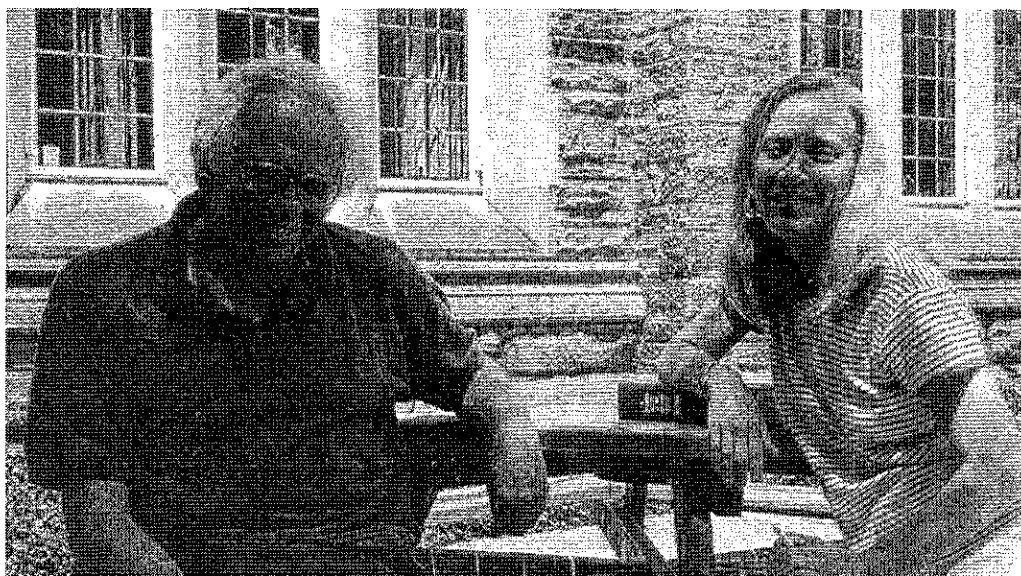
Sports fans are by now hearing the famous "threepeat" reverberate through their minds. And, yes, this is her third prize in this annual competition. Such an accomplishment deserves special recognition. The Mullah, after having visited the Country and Western Hall of Fame and observed sundry and assorted paraphernalia of the great and otherwise forgotten, has decided to establish the Pithy Wisdom Hall of Fame for Clioms and others of their ilk. Furthermore, he has ensconced therein the cliometric Queen of Aphorism. With this elevation she will no longer be eligible to win the annual prize, although the Mullah hopes she continues to speak out and to inspire others to utter memorable phrases.

Inspiration may have been on her mind the last day when she offered to relinquish her prize to a late utterance by the newest disciple of he who has studied the potato at great depth. "Salvation and Calvinism are orthogonal!" This is the kind of phrase one can carry around easily in the recesses of one's mind, pondering endlessly its true meaning and significance, without interfering with more serious deliberations. The youth of this orator, as well as others, bodes well for the future; wise utterances shall persevere even though the Queen of Aphorism may no longer enter the fray.

Carrying on with tradition, the final event of Saturday night's proceedings was the passing of the can. She who had recently reached the half-century mark elevated this presentation to new heights, with numerous references to gods and goddesses all laid out deftly in what must be the first Ode on an Empty Olive Oil Can. Whether it will endure as well as Ode on a Grecian Urn, or even Ode to Billy Joe, remains to be seen. It does, of course, increase the burden on the winner to follow in such tradition for next year's delivery. There seems little doubt that the great ante on-line bibliographer is up to the task.

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

Ode to the Muse Clio and Her New Caretaker



*Can-holders Engerman and Goldin
musing outside Scarritt-Bennett Center*

*I was canned in Kansas just last May
Not canned at Cannes as film buffs say
But canned I was and with my can
Took leave by plane – on American*

*I lugged my can back to my seat
And stored it safely at my feet
The flight attendants never suspected
A Muse named Clio boarded undetected*

*Muses, like gods, eat mainly ambrosia
Not having any, I offered Pepsi-cola
After one sip, Clio gave me a poke
From now on, she said, 'It's got to be coke'*

*Thus friendship was sealed between Clio and me
Girl from the Bronx; daughter of Mnemosyne
I took Clio home and made quite a fuss
Put her up high, as on Olympus*

*I devoted the year, omitted no detail
Fetched pizza and nectar, put her on e-mail
Sad but true she's at the end of her stay
I'm told I must give my Muse away*

*The next can-holder required no oracle
A choice so clear it will be historical
He's no Apollo, Hermes, or Zeus
Just the finest we mere mortals produce*

*The next can-holder will give Clio a home
Near Ilium and Carthage, Syracuse and Rome
Closer still are Homer and Greece
Where Gods and goddesses seek their peace*

*Clio's new master has knowledge so grand
That even the Muse will understand
From him she'll learn the mathematical
And add to her facts biographical*

*The new can-keeper is a student of slavery
Admired for wisdom, not for his bravery
Though learned in war, he's not in the Iliad
Knows facts aplenty, a countless myriad*

*So join me in applause to echo like thunder
To make the Gods know of his wonder
And not be jealous that Zeus's daughter
Goes to just some ordinary suitor*

*For there is nothing ordinary about this man
He is, of course, the extraordinary Stan
Who has been my muse for 25 years
Before on-line catalogs he had no peers*

*So leap, dear Clio, from your oil can
Into the arms of the Engerman
And spend a year along the Genesee
Where you'll wait till June for Persephone*

*Before I take my leave of you
I must reveal advice that's true
Some words for Stan, the Muse is moody
Don't tell her of your spouse named Judy*

– Claudia Goldin

Clio Conference Report *(continued from page 1)*

by the Dutch fiscal system. They wanted taxable cash crops, so the crop mix cannot be explained by what happened to factor prices in Indonesia. LaCroix countered that we need to know what happened to factor prices even if the cultivation system had been driven by Dutch policy. A discussion about the distinction between slavery and tax collection ensued, with LaCroix emphasizing that this was a system of coerced labor where people were removed from their villages to work on export crops. The discussion ended with Farley Grubb (Delaware), Barbara Sands (Arizona), Winifred Rothenberg (Tufts) and others expressing concern about the definition of property rights – what does it mean to have “increasing” and “decreasing” property rights? Are we concerned about property rights in man or property rights in labor? LaCroix agreed that the paper would benefit from a clearer articulation of the concept.

Barbara Sands' paper shifted the focus to China during the Great Leap Forward. In the late 1950s and early 1960s, China experienced one of world's largest demographic disasters, in part because of widespread famine. While the famine has been blamed on the policies of the central government, Sands and her co-author, Dana Weber (Arizona), find its effects were regionally distinct. The largest number of deaths occurred in four of China's 28 provinces. These four were the most “radical” and may have enforced central government mandates most effectively.

Chulhee Lee (Chicago) suggested that factors other than central government policies may have caused the differentials. Since middle-aged men appear to have suffered the highest mortality, it would be helpful to know the causes of their deaths. The disease environment may have varied from province to province. Sands responded that they had looked at the epidemiology, particularly since some diseases like TB tend to hit middle-aged men. Unfortunately, there is no information on cause of death. Grubb worried that the migration model was too simplistic, since it treats each province independently. Refugee migration may be determined by factors other than fleeing to a safe haven; people may have been returning to the provinces of their ancestors. Sands replied that the information on migration was not detailed enough to tell where people were going. She believes the migration category may have been treated as a residual; local authorities filled in the birth and death rates, then calculated migration to make the numbers come out right.

With respect to migration, Lucia Tsai (UC-Berkeley) wondered how the enforcement of government control varied. Did all cities have high growth rates or just the ones specifically mentioned? She also thought the jump from evidence of higher death rates in the radicalized provinces to the conclusion that those provinces were more successful at enforcing government policy was a bit premature. Sands replied that, by 1955, the central government had mandated large migrations throughout the country; all China's cities grew rapidly. She reminded us that most explanations of the famine are about the failure of central policy at the national level, while this paper focuses on regional differences to point out that China may have not have been as centralized at the time as it is thought to have been.

The balance of the discussion focused on the high death rate for middle-aged men. Both Broadberry and Claudia Goldin (Harvard) expressed amazement that, amid all this death, there were no descriptions of disease. Sands noted that some interviewing is being done now, but it is being done in the wrong provinces. Richard Steckel (Ohio State) and Ann Bell (Vanderbilt) wondered if these deaths had been due to dietary stress or exposure. Sands commented it was central government policy to substitute labor for capital; men may have been shoved into more onerous work (earthen dams and backyard steel furnaces), which would explain not only their high death rate but also the decrease in the crude birth rate. Tom Weiss (Kansas) and Goldin suggested that the data may be inaccurate. A look at this cohort through time would help to determine whether as many had died as the early reports would indicate. A discussion ensued concerning the interesting problems presented by Chinese data.

The final paper on Friday afternoon was by Tarik Yousef (Harvard), who explains Egypt's failed development during the interwar years as the consequence, in part, of a change in the willingness of lenders, particularly in Britain, to supply capital. He attributes a good deal of the absence of research on Middle Eastern economic history to methodological preferences. Middle Eastern studies have been dominated by a nonquantitative tradition where economics has been secondary to political and ideological concerns. Yousef's paper is a first step toward integrating the history of an important region into a broader economic history of the developing world.

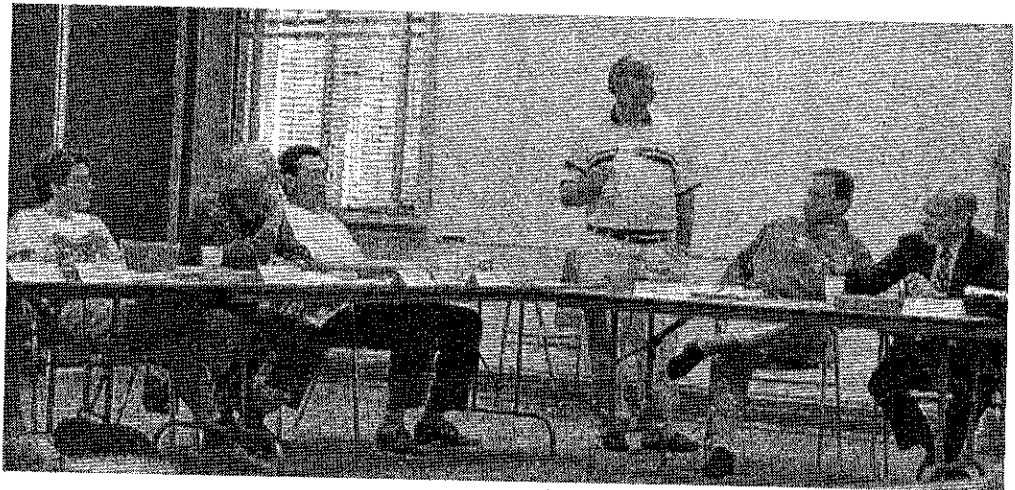
Price Fishback (Arizona) began discussion by suggesting that the dearth of capital flowing into the region before 1940 might have been related to political as well as

economic factors. Yousef responded that capital flight could be linked to internal problems in Egypt, but it was also related to external events. Up to 1907, perhaps to 1914, Britain took the lead in sending capital to Egypt.

Those countries to which Britain had sent capital before World War I experienced capital outflow after the war. Greg Clark (UC-Davis), in his inimitable fashion, suggested that Harvard is perhaps afflicted by a cargo cult: a worship of capital leading to the belief that if only there were enough investment in these countries, growth would occur. Why didn't it? Clark suggested it was because of inefficiency: Egypt tried to establish a domestic textile sector, but it wasn't profitable. India, by contrast, had an inflow of capital. What was the rate of return in the Egyptian economy? If the profit rate had been little greater than in Britain, then you know why there was no inflow. Yousef fully agreed. Mokyr asked what kinds of opportunities went unexplored because of the purported shortage of capital.

Broadberry addressed the dynamic structure of the savings function and asked whether it was stable. Were capital flows exogenous or were they endogenously induced? Why did Egypt do so badly in the 1920s? Yousef defended his basic savings function as robust; he had tested for structural breaks, and found the basic results stood up. Conditions in Egypt must be linked to conditions in Britain. Clark noted that the actual change in demographic structure is minuscule compared with what is observed in Taylor's study of Argentina. Yousef replied that travelers from the US and Britain complained about a huge population problem that was not being addressed. Emily Mechner (Harvard) noted that the retardation story seems not to match the narrative, since retardation appears to have begun even before World War I. To deal with recovery in the 1930s, it may be worth looking at the military role played by Egypt in the European conflict. The recovery of investment in Egypt coincides with the beginning of rearmament. Tim Leunig (Oxford) asked what happened to exports during the interwar period, and Sam Williamson (Miami) asked about the composition of the import bill. In this spirit,

Alex Field suggested looking at the relation between the capital and current account, noting that the industrial sector consumed a relatively small amount of resources compared to infrastructure.



Mary MacKinnon, Bob Gallman, John Nye, host Jeremy Atack, Joseph Mason, and Alex Field.

Saturday's first session, "Threads", was chaired by Mary MacKinnon (McGill). Susan Wolcott (Temple) presented her paper, written with Greg Clark, on the Indian cotton textile industry in the early 20th century, observing that India's GDP *per capita* departed from Japan's after World War I owing to differences in labor productivity growth. There were some improvements in capital productivity in Japan, but the differential is smaller. A lot of excess labor was used in India in comparison with Japan. Was this just a movement along an isoquant, a response to differential factor prices? Even factoring in the poor quality of Indian cotton, Indian workers, with a strong distaste for work, were working only 10-12 minutes per hour. In a slowly growing economy, Wolcott and Clark argue, Indians collectively restricted their productivity to preserve their jobs.

Tsai asked what managers had done about this. Apparently not much, for the authors find excess staffing lasting 30 to 40 years. Leunig asked why in British weaving, it was typical for one person to tend four looms, but in India only one or two looms per person was the norm. Mary Gregson (Knox) asked, if the profit rate fell by half after 1907-18, why hadn't firms exited? Wolcott responded that textile wages in Bombay were enormously sticky, despite a declining cost of living. Investment continued, in part because contracts for machinery were fulfilled three to five years after orders were placed. Disinvestment did not occur because managers and owners kept thinking things would get better.

Grubb wondered why they assumed that machinery lasted 50 years without replacement. Wolcott responded that, as long as profits are positive, you don't scrap. She also noted that one could purchase machines with different possible configurations from Lancashire. Some of the machines permitted larger package sizes. The larger the package size, the less labor used, but the more floor space required. India did not buy the smaller packages, despite its labor surplus. Mechner asked whether a low utilization rate was a management variable. Wolcott responded that you can't find evidence that there was any real managerial failure – either domestic or foreign. Scale didn't matter, mules didn't matter, and age didn't matter. Mills that succeeded in shedding labor paid higher wages to the remaining workers.

Mokyr told about his stay in a Delhi Hotel, where he realized that the ability to hire a second worker at the front desk, a worker who appeared to do very little, was in fact part of the compensation package of the first worker. In the mills, it appears that workers were imposing constraints on management. John Nye (Washington U.) asked why free labor behaved this way, and Field asked why there had been no action to change labor's ability to act collectively. Noting the possible role of education and nutrition, Steckel asked whether these workers were simply too tired to work a full day.

Broadberry wondered if the accounting data were reliable. Wolcott responded that she has read the *Indian Investors Handbook*, as well as directors' reports, and believes it is unlikely there were hidden reserves. They found no correlation between any particular strategy and higher profitability. It would not have been obvious to managers what they should have been doing to restore profitability.

Tim Leunig then presented his paper on the early 20th-century Lancashire cotton industry. He argues that although mule spinning has a transport cost advantage over ring spinning in a vertically specialized industry (because bulky wooden ring bobbins must be transported back and forth from spinning to weaving firms), transport cost considerations played no role in the choice of technique for more than three-quarters of the spinning sector. He shows econometrically that the regional variation in the proportion of rings to mules can be explained by variations in transport costs and the relative costs of skilled and unskilled labor. Finally, he compares productivity in Lancashire with that in New England, finding that Lancashire enjoyed a substantial productivity advantage

in both ring and mule spinning. Leunig accompanied his presentation with a demonstration of the two methods of spinning using models built almost entirely of Legos. (Tim proudly wore his Lego Builders Club membership pin).

Much of the discussion centered around whether Leunig had measured his variables correctly. Labor cost savings were not, according to Clark, since the calculation was based on the average wages of mule and ring spinners. He claimed that earnings depended on the vintage of equipment, so one must use labor costs on new machinery. Leunig responded that uniform wage lists for ring spinning were standard across Lancashire. Grubb asked about the calculation of transport costs and whether they included terminal costs. Leunig responded that point-to-point transport costs were subject to government regulation and, therefore, are known with great accuracy. Rothenberg inquired about the effect of the Casablanca system and electrification on spinning techniques. Leunig responded that the Casablanca system was invented by a Spaniard in 1918, after which no one installed mules, and that there was no electrification in Lancashire mills before World War I.

Clark remarked on the "astonishing revisionism" reflected in Leunig's productivity comparison with New England (Clark's own work shows a 50% productivity advantage for the US). This is a remarkable vindication of the English who, according to Leunig's data, had both lower wage costs and higher output per worker. Discussion ensued about the appropriateness of the data sources used in the different studies. Leunig noted that Britain was much better at spinning than weaving. In 1939, cotton was still England's greatest export; indeed, Keynes had chaired a cotton commission which assumed Britain would always export cotton goods.

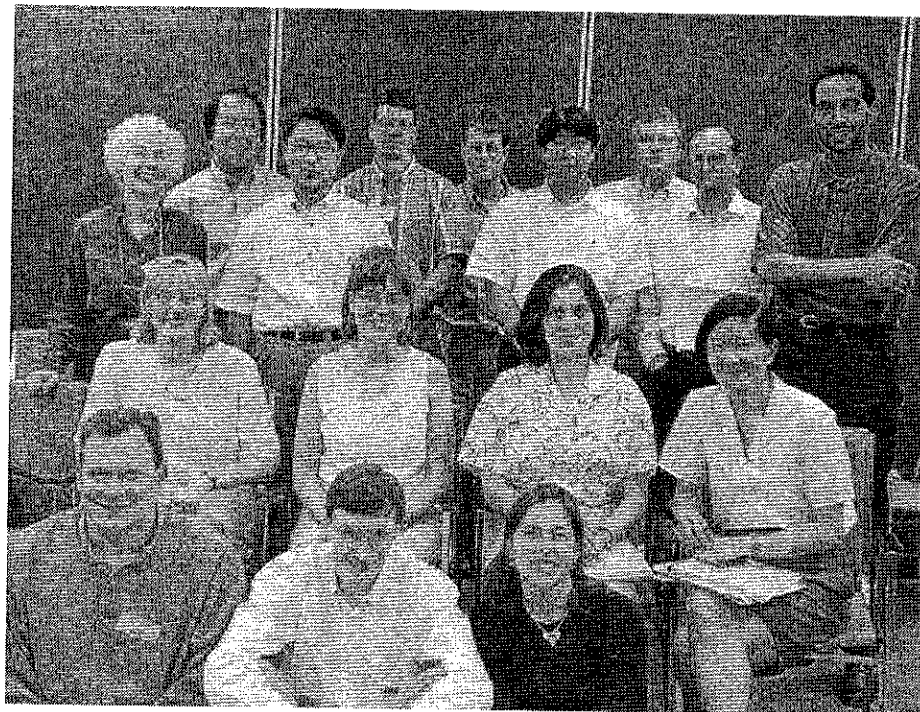
The third session, entitled "Protective Legislation", was chaired by Price Fishback. The paper by Carolyn Moehling (Northwestern) looks at state labor laws and the labor market participation of children using state-level data from the 1890 and 1900 Censuses. She also examines household data from the 1900 Census to explore the effect of state labor laws on family labor supply decisions. Her results indicate that, although state laws prohibiting the employment of children did not lower overall participation rates for children, they did reduce the likelihood that a child would participate in the labor market. These contradictory findings appear to result from substituting the labor of older for younger children.

The discussion centered on two main themes: the appropriateness of the econometric model and the question of the impact of laws on the allocation of family labor. Bob Margo (Vanderbilt) and LaCroix argued that the model was not really a difference model, although that term is used loosely in the literature. LaCroix suggested a two-stage approach so that male and female labor supply could be estimated separately. Margo argued that it was possible to construct a natural experiment using the birth month and state of residence, similar to the model he and Finegan have used for their work on compulsory schooling. Michael Pammer (Johannes Kepler Universität) and Joan Hannon (St. Mary's) were both concerned with the measures of industrial mix: Pammer that they ignore the impact of agricultural labor, and Hannon that the industrial mix itself might have affected state laws. Moehling responded that one measure captures variations due to changing employment of children in industry at the national level, while the other looks at the change in industrial mix if the employment of children remains the same. Combining the variables has little effect on her results. While the impact of agriculture should be captured in the industrial mix variables, to the extent that the law changes the industry in a state, this model will underestimate the impact of the law. Larry Neal (Illinois) advocated use of a variable to control for household mix across states, since household mix should play an important role for decisions made at that level.

Several people had questions about the family allocation of labor decision. MacKinnon commented that the information on girls seems suspect, since this is information for children living at home and many girls at work were likely to have been live-in servants. Moehling replied that the data do not provide any information for girls in service and that the R^2 for girls is very low. Sands, Tsai, Gregson, Hannon and others all worried about the substitution of older for younger children. Hannon argued that an increase in employment of 14 year olds and a decrease in employment of 11 year olds would indicate the law

was working. Moehling replied that, when the anti-child labor movement saw the high rates of employment among 10-15 year olds, they thought the laws were ineffective, but they may have been wrong. Gregson, Wolcott and Sands all suggested using data from the manuscript census to determine separate labor force participation rates for 11, 12, 13 and 14 year olds.

The other paper in this session was by Stefanie Schmidt (Milken Institute), who explores the role compulsion



First-time Conference participants, l. to r.:
 Row 1: Joseph Mason, Timothy Leuning, Leslie Moscow
 Row 2: Stefanie Schmidt, Carolyn Moehling, Emily Mechner, Lucia Tsai
 Row 3: Winifred Rothenberg, Chulhee Lee, Hajime Hadeishi, Michael Pammer, Tarik Yousef
 Row 4: Mark Kanazawa, Richard DePolt, Michael McAvoy, Craig Pascoe
 Not shown: Luciano Pezzolo

played in the rise in high school attendance. Did compulsory education laws result in an increase in high school graduation rates? Her two-part study looks at cross-state variation and variation within one state. Data from New York show there can be substantial in-state variation. Enforcement makes a big difference; continuation laws had big effects. These laws required part-time attendance if a student was under 18 and working. Schmidt estimates such laws contributed to a sixth of the increase in high school graduation rates in the US.

Kerry Odell (Scripps) was concerned about assigning children to their state of birth and asked whether migration might affect some of the cross-state results. Schmidt

responded that regressions (unreported) had also been run on people who resided in their state of birth, with results largely unchanged. Wolcott noted that enforcement was an exogenous variable and asked whether regional attitudes or differences in economic structure might explain variations in enforcement. Broadberry inquired whether the quality of education varied across states, noting that in Britain there is a lot of regional variation. Williamson asked whether a better product could have attracted more students. Gregson suggested that it may be difficult to distinguish econometrically between the effects of carrots and sticks: suppose more people chose to go to high school voluntarily. Schmidt replied that attitudes would have had to change coincidentally with the passage of specific laws.

Lee pointed out that a high school diploma would be needed and required in white-collar occupations, but not necessarily in manufacturing, which is used as the industrial variable. Rothenberg suggested that labor markets had experienced contrapuntal movements, leading both to de-skilling and an increase in the demand for skills, as well as the effects of an influx of foreign labor. Much discussion concerned how Schmidt's results should be interpreted. Goldin asked whether these results would hold for shorter periods. Joe Mason (Office of the Controller of the Currency) noted that the decade before the 1940 Census was macroeconomically turbulent: Nevada schools were closed in 1932-34, and Chicago teachers were paid in scrip. Goldin argued that education for most people was unaffected by the Depression.

Session IV, "The Great Depression and Beyond," was chaired by Alexander Field. The first paper was presented by Randall Kroszner (Chicago) whose co-authors are Clifford G. Holderness (Boston College) and Dennis P. Sheehan (Penn State). The paper looks at the evolution of corporate ownership and governance since the Great Depression and explicitly tests the Berle and Means assertion that the separation between ownership and control of firms has grown wider over time. Kroszner *et al.* compare managerial ownership for 1935 and 1995 and find that managerial ownership of publicly traded firms has increased. They then hypothesize about the reason for this change.

Several questions were raised about their data. Schmidt asked what percentage of firms was privately held in 1935 as compared to 1995, and whether 1935 was unusual in any way. Kroszner replied that there is simply no information on privately or publicly held firms prior to 1935.

Berle and Means used information for 1929 from *Wall Street Journal* articles. LaCroix was concerned the 1935 data might be biased and questioned whether compliance with the new regulations would have been particularly good. Kroszner replied that the SEC believed at the time (he thinks correctly) compliance was quite good. In any event, if stock holdings had been manipulated to avoid the new regulations, such biases would work against the current findings.

Others suggested that the paper did not really test the Berle and Means hypothesis. Wolcott thought the relevant comparison should be 1900 with 1932, not 1935 with 1995, and wondered if the test adequately reflects the demise of single proprietorships. Kroszner replied that Berle and Means only provide data for 1929 and make it clear that the trend toward managerial control was growing over time. Further, Berle and Means' focus was not on the demise of the single proprietorship, but on the proportion of the economy's total output that was controlled by managers. Field and Broadberry were both concerned that the trend to increasing managerial ownership was a recent one. Field argued the shift may have occurred in the '80s, while Broadberry wondered if the increase in hostile takeovers in the '50s hadn't been responsible. Kroszner pointed out that other studies of the '70s and '80s are consistent with their findings.

Several audience members were skeptical that managers were motivated by owning larger stakes in their firms. Grubb and Mokyr both felt the story about motivation did not fit with the data, while Steckel and Clark suggested that there was little or no connection between motivation and ownership. Kroszner admitted that this was one of the problems with the ownership literature. It is clear that between 1935 and 1995 the market value of equity owned by officers and managers had increased, and their research shows that peak performance was at about 5% insider ownership for both 1935 and 1995. Many in the audience remained skeptical, and the session ended with a lively discussion of risk aversion, ownership, firm performance and the question of whether the managerial ownership share is endogenous or exogenous.

Session IV concluded with a paper by Joseph Mason on the role of the Reconstruction Finance Corporation in the early 1930s. Many have argued that the RFC, through its loans and preferred stock programs for banks, administered an effective open-bank assistance program. Using state-level data from Illinois, Mason's paper tests the hypothesis that the RFC was able to

prevent bank failure. The results indicate that the RFC could not; in fact, RFC loans seem to have increased the probability of failure.

Leslie Moscow (Northwestern) began the discussion by suggesting that, if loans and preferred stock were an either/or option, it might be more appropriate to use a multivariate logit equation. Mason replied that was not appropriate since loans and preferred stock were issued over two distinct time periods with little overlap. Moscow and Mark Kanazawa (Carleton) then raised the issue of the relationship between the probability of failure and the probability of assistance. Moscow suggested that perhaps a simultaneous approach estimating first the probability of assistance, then the probability of failure, would help correct for the bias toward failure. Mason said there were two issues here: first, a bias from not observing applicants who failed to get loans, and second, a self-selection bias for discouraged borrowers, who did not apply. Furthermore, while we would expect the probability of failure and the probability of assistance to be inversely related, this does not appear to be the case. Generally the RFC gave about 10% in assistance regardless of the probability of failure.

Kroszner worried about the performance of the net worth variable which did not help predict failure in the early period but is significant in the later period. He suggested there were environmental changes in 1933 that created fundamentally different regimes; Mason agreed this is an appropriate area for future research. Odell asked what changed in October of 1933 to cause an increase in RFC applications. Mason replied that, in his haste to reopen banks after the bank holiday, Roosevelt asked the RFC to make sure banks were sound. The RFC recapitalized around 2,000 banks with preferred stock in a six-month period so that the banks could join the FDIC. Jones (the head of RFC) appealed to the American Bankers Association for weak banks to apply to the RFC, and this apparently caused the jump in applications. Williamson raised the possibility of political favoritism and wondered if the funds were distributed accordingly. Mason replied that he has tested that hypothesis elsewhere and finds no support for it.

Several members of the audience expressed amazement at the high percentages of banks that closed, particularly in California. Bob Gallman (North Carolina) reminded us of the reason Will Rogers gave for the decline in bank failures in the mid-30s – we were running out of banks! The session ended on that note.

The fifth and final session, "Labor Economics," chaired by Rick Steckel, took place on Sunday morning. Leadoff speaker was Farley Grubb, who presented his paper on the trans-Atlantic market for British convict labor. This work is part of a larger effort to reintegrate the analysis of indentured and slave labor, with a particular focus on analyzing differences in contract prices and terms of servitude for convicts and servants.

Kyle Kaufmann (Wellesley) began the discussion by noting that Grubb's analysis assumes a competitive market, but the people who shipped convicts had a government monopoly, and they had no choice but to take whomever the British government ordered to leave. He also inquired whether all convicts were sold to private individuals as laborers. Grubb replied that all convicts were sold to planters. Throughout most of the period there was a nominal shipping monopoly, but he is not clear exactly how much competition there was for those monopoly rights. For Australia, about a third of those sentenced were not transported, so perhaps jailers had some discretion.

Neal remarked Grubb's paper reminded him of Ralph Davis' estimate of the cost of passage at £10, an estimate without a very firm foundation. Grubb estimates the cost at £9 but with convicts, wouldn't there have been a problem in terms of costs of provisions, guarding, *etc.*? Grubb granted that there could have been some variability in the costs of passage. Moscow asked why the government regulated convict transport. Grubb replied that almost all servants served terms of seven years or less, so seven years contract length simply wasn't much punishment. Why didn't the government feel it had the option to inflict more severe punishment? It appears that selling Englishmen as slaves would have violated deep cultural norms. Still, it's a puzzle why the government gave out a monopoly on shipping, then subsidized the shippers, even though they would have made profits without the subsidies.

Mokyr observed that a large number of these regulations had been created by the state. He suspects that a large number of rents were dissipated by bribery, and that there was probably some rent-sharing, but was not persuaded it made sense for American masters to discount the price of convicts by one-third. Wolcott noted that convicts who had broken rules would have been thought likely to run away before their full seven years of service, thus explaining the discount for convict labor. Rapists and murderers would have been hanged; transportees were guilty of less

serious crimes, and many were children. MacKinnon asked whether masters knew what crimes convicts had committed. Grubb answered that this should have been stated on the indenture documents.

The second paper, by Joan Hannon, is an extension of her earlier work on poor relief. She finds rising dependency over time in the antebellum relief records, which she attributes to increases in periodic spells of unemployment. Her paper responds to criticisms of her earlier research by using a simultaneous equation model to account for the endogenous nature of policy changes. She finds dependency on poor relief continued to increase until 1870. The declines after that date, however, are due largely to the transfer of the poor to other types of institutions, not to a decline in dependency *per se*. Her work also attempts to control for changes in family structure over the life-cycle. In general, demographic variables perform poorly. When they do perform well, results are counter-intuitive.

Margo began the discussion by suggesting that Hannon look at generosity as defined by Lebergott. He then described his relief policy model that allows for an interaction with other communities and for a trade-off between a higher dependency rate and the supply of recipients. Hannon replied that she tries to capture the same thing, but the key policy variable is use of the poorhouse as a deterrent, not the benefits/earnings ratio. MacKinnon suggested that, given the quality of the data, it might be best to look at the results for reduced-form equations before proceeding to more sophisticated econometric work. Hannon responded that she had estimated the reduced-form, and results were not improved. Broadberry wondered if including New York City creates a non-linear relationship, but Hannon replied its inclusion does not.

Several participants asked how the relief system functioned. MacKinnon wondered if counties made the decisions, Schmidt inquired about the role of orphanages in reducing dependency, and Rothenberg wanted to know what caused the dramatic change in the numbers on relief after 1875. Hannon explained that legally all decisions were made at the county level, but some counties could opt out. In some localities, towns provided outdoor relief, the county provided the poorhouse; in others, towns decided who would receive relief, but the county paid. Differences in the use of orphanages across counties cannot be identified from the data. The drop in 1875 is directly related to laws requiring counties

to place children in orphanages rather than in poorhouses. Although localities paid for the orphanages, such expenditures were not reported as relief, and Brooklyn and other places as well cut back on outdoor relief or stopped providing it. Lee inquired as to the effect of the Civil War; Hannon said that the Civil War was almost never mentioned as a cause of pauperism. Females on relief outnumbered males both during and after the war, and there were pensions and a Soldiers' and Sailors' Home for veterans.

The final paper, by Leslie Moscow, explored charity and the bequest motive through a study of 17th century wills from Suffolk County, England. Suffolk County had lots of arable land, and was among the first to be enclosed. It had two major cities with populations of 5,000 - 10,000. A very religious and strongly Protestant region of England, it was loyal to the rebel cause in the Civil War and a major source of immigrants for America. Most bequests were to the poor, not to the church.

Gallman observed that Moscow's results show that George Stigler was wrong (for many good reasons), and that the existence of an altruistic motive can be confirmed. Hajime Hadeishi (UCLA) raised questions about the religious variable, suggesting that those higher in social status may have written longer preambles. Moscow replied that, while this is true to a certain extent, she believes the regressions adequately controlled for wealth. Leunig inquired whether scribes just included religious boilerplate when writing wills for illiterate people; Moscow answered that many preambles were formulaic. Leunig suggested looking only at literate people to see if the religious variable behaved differently.

Kroszner commented on the table of correlations of various variables and raised the concern that some are proxying for others—for example, religious variables may proxy for wealth. He suggested interacting religion with other variables. Moscow replied that she had tried these interactions, but the results showed no relationships. Mechner noted that other friends and family mentioned must be a proxy for wealth. Wealth is a very strong predictor of charitable bequests, consistent with modern findings. Moscow agreed.

Clark suggested an alternative hypothesis to explain the apparent causal relationship between religiosity and generosity. Perhaps there are people who are warmer, more generous and more expressive than others—people who are more loquacious about their relationship with God—but they expressed their generosity by giving to the poor

rather than to the church. Local authorities disapproved because they perceived the poor to be lazy and shiftless. It might be interesting to look at bequests that were limited to the poor and were more niggardly. Clark encouraged Moscow to consider a wider political economy of donations, looking at who left money and who did not, given the incredible data at parish levels about donations of different types and major changes over time. Moscow replied that most donations were anonymous. Furthermore, no one gave to the church. Some said give to the poor who attend my funeral; others said give only to those who do not. Wolcott objected that the observed behavior was not necessarily altruistic, since it is perfectly consistent with giving as a form of consumption.

Discussion was closed by Rothenberg, who observed that in Massachusetts there was very little giving to the poor

until around 1800. There was a difference in wealth between those who wrote wills and those who died intestate. Moscow replied she would like to look at 17th-century Massachusetts, and asked, were there then no poor in Massachusetts?

The conference concluded Sunday afternoon with airport vans arriving like clockwork to whisk conference-goers to their appointed flights. All participants, including your rapporteurs, left intellectually enriched. Perhaps the conference's most astounding revelation occurred during a dinner conversation on Saturday night. Robert James Waller, author of *The Bridges of Madison County* and other masterpieces, was Fred Bateman's first dissertation student. He served as dean of a business school for a decade before embarking on his new career. There are grave cautionary notes in this historical vignette, but it shall be left to you, gentle reader, to draw them out.

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Deane Interview (continued from page 8)

controversies, I became interested in trying to analyze the debates and see how they had developed through time.

One of the better-known histories of economic thought was written by Mark Blaug. How would you differentiate your work from his?

Well, my work was more descriptive and less critical. I was more interested in the way the ideas evolved than in discussing their analytical deficiencies.

So in a sense it is more historical?

It is more historical. I would regard my work as not at all competitive with Mark Blaug's but as complementary – another way of looking at it.

In both your recent books, *The Evolution of Economic Ideas* and *The State and the Economic System*, there seems to be quite an emphasis on the world prior to 1914. Is that an unfair reading?

I don't think it's unfair. Do you think it's an undue emphasis?

Well, it's a matter of choice, but clearly figures like Ricardo have a considerable interest for you. That might be part of the Cambridge environment, of course. And yet another part of the Cambridge environment would be to dwell rather more on Keynes than I think you did in those books.

What I was trying to do was to write about subjects which I thought had been inadequately dealt with, and perhaps I was satisfied that there were more competent people to talk about Keynes in Cambridge than I.

What should the history of economic thought contribute to a study of economic history? I think many economic historians neglect history of thought. I'm certainly guilty of neglect myself!

I'm not sure what history of thought should do for economic history, but there's no doubt that economic history has made a great contribution to the development of economic ideas. People have generally tried to answer questions which reflect the spirit of their age. Economists have tended to produce theories which they find acceptable to current policy-making authorities. That is the sort

of thing that I was really interested in when I developed my latest book, *The State and the Economic System*. I was interested in the way economists' theories and researches were defined by problems of policy as they arose and by the political attitudes of the authorities that might make use of economic conclusions.

The most obvious example I suppose is Keynes responding to the unemployment of the inter-war period, and perhaps the demise of Keynesian influence during the inflation of the 1970s. Were you trying to suggest that policy makers set the agenda for economists?

I think they do. I think that economists in government service, for example, who are most directly in touch with the policy makers, need to be aware of the fact that only certain kinds of advice and certain kinds of research will be acceptable to policy makers. If you're really serious about having an influence on policy, then you have to recognise those constraints.

Is that what makes the 19th century more interesting – there were more mavericks because there was less pressure from the government of the day or financial pressure? Despite that greater freedom, the intriguing feature of the writings of people like Smith and Malthus is that they failed to foresee in large part the economic growth which was just materializing. Economists were clearly poor forecasters then as now!

They're always poor forecasters!

Do you have a view as to why these eminent writers failed to see what was happening?

I think because all the evidence which lay behind their researches is historical evidence.

Backward looking?

And the world changes. It doesn't change totally but it changes all the time and the changes are often themselves unpredictable. Who would have predicted what has happened in Eastern Europe, for example, in the past few years?

So economists are better at solving the problems of the past than the future.

Well, yes.

I think the biggest continuing debate is about whether it is better to have rules or to allow policy makers a lot of discretion. My generation would have interpreted Keynes as suggesting that more discretion should be given to policy makers than the 19th century had thought wise. Would you share that view?

Yes, I think I would. If you fix very tough rules in a changing world you have no flexibility.

That, I think, is definitely a Keynesian view. You said when you started out in the 1940s you saw yourself as very much influenced by the new Keynesian economics. Are you now a "neo-Keynesian"? How have the passing years changed the youthful Keynesianism which was very optimistic and confident that there were better ways of running the world?

I'm not so confident or as optimistic. I'm not so confident that one can be sure that economists will produce the right answers or even that the policy makers will. I think it is still important to allow a fair amount of flexibility to economic policy making. It is also important that there should be a rather large expenditure on research on the way the system works and is changing. I'm not sure that this need was recognised in the original Keynesian project, because it was rather assumed that the economists knew how the system worked.

Would that research be econometric?

Also institutional and sociological, I'm afraid. I mean what one is trying to describe is economic behaviour and how economic behaviour changes, not only in response to shocks to the system but in response to the constraints that are imposed on the system by policies.

I know you're still researching actively. Could you tell me what you're doing now?

Well, as usual I want to do something different, and I have decided to write a biography. I'm not sure that I know exactly the shape it'll take. I just know what kind of material I want to embody. I want to write a biography of John Neville Keynes, who was the father of Maynard Keynes. John Neville lived from 1852 to 1949. He came up to Cambridge as a student in the early 1870s, and became a Fellow of Pembroke. Later he became Registrar of the University. So his career fell in what was a revolutionary phase in the history of the University. There were terrific changes in his time in the nature

of the University, which was almost a clerical enclave at the beginning of the story.

John Neville came from a dissenting family at a point of time when it was possible for dissenters to take degrees and eventually to become members of its governing body. He was an economist and a logician. He took the moral sciences tripos, which was a combination of moral philosophy, psychology, logic and political economy. It was a broad area of study, and he wrote a famous treatise on *The Scope and Method of Political Economy*, published the same year as Marshall's *Principles*. He was closely associated with both Marshall and Henry Sidgwick.

A few years ago I was invited to write an introduction to an Italian edition of *Scope and Method*, which represented the final word on methodology of economics until the 1930s, and I found myself skimming through his diaries to analyse his relations with his colleagues, for example, Marshall, Foxwell, Sidgwick and Pigou. Many people have skimmed through these diaries with aims which are not associated with Neville Keynes as much as with his son Maynard Keynes, or with Marshall and others with whom he associated. Having read some of these references to the diaries, I decided that they had misinterpreted John Neville Keynes, and that I would let him speak for himself by basing my biography largely on his diaries. So this for me is a new sphere of writing, but it's an interesting challenge.

The modern way would be to take a serious training course before doing this.

I'm too old for serious training courses, so I'm just going to learn the hard way! This is really a chance for John Neville Keynes to vindicate himself (or to be vindicated), and an attempt to bring out the different kind of environment and the different kinds of forces acting on academics in the 19th century.

That prompts a final question. Cambridge University must have changed a lot in the many years you have been associated with it. On balance, have those changes been for the good?

When I first came to Cambridge in 1950, the typical undergraduate was not a schoolboy but an ex-serviceman, and in that period it was really very exciting talking with these young men because they were not blank sheets to be written on. They were people who already had questions pouring out of them. Of course the servicemen went, and

one came to the schoolboys and schoolgirls again. I think it is still true that the undergraduate in Cambridge gets much more attention and personal tuition than is true of most other universities, and that the research student is relatively neglected.

So by the 1980s a return to your first love, which was research, seemed the order of the day.

That's right.

Phyllis Deane, thank you very much.

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SE Asia Report (continued from page 16)

late 19th century than in the late 20th century, since governments have increasingly intervened to ensure food supplies. What are the prospects for improving market integration via international trade organizations? Next, Latham recapped his arguments from the day before, urging scholars to pay more attention to the role of staple crops other than rice. He also extolled the data-collecting proclivities of European colonial governments, citing the modernizing force of colonialism. A Chinese historian pointed out that China had kept outstanding price data for each prefecture in the 18th century and suggested that economic historians truly interested in the long run do more work on the pre-1870 period. Yasuba then discussed several misconceptions about Japanese economic history. For example, he reminded us that Japan dropped its rice tariffs for Korea and Taiwan in 1920 and re-emphasized the fact that Japan had been an importer of cotton manufactures until the late 19th century. Comparative advantage can shift with development, and economic historians should think more about how this

happens. He also noted that "cultural endowments" are actually quite malleable, citing the development of a Thai work ethic in recent decades as an example.

Williamson brought the proceedings to a close by announcing that similar "Third World Cliometrics" conferences are already in the works. The next in the series will be organized by Williamson and Sevet Pamuk (Bosphorus University) and will be held in Istanbul in the spring of 1998 under the title "Long-Run Economic Change in the Mediterranean Basin." Additional conferences are planned for South Asia and Latin America. A central goal of these conferences is to stimulate local scholarly interest in the economic history of the Third World and to grab the attention of policy-makers who, after all, should be far better informed than they (and we?) are about the economic evolution of their societies.

While the supply lasts, copies of the Honolulu conference papers can be obtained by e-mail request to Williamson at the following address: jwilliam@kuznets.fas.harvard.edu

Health Report *(continued from page 12)*

highland populations, but that differences in average age at death might not show the same change.

The final Saturday session focused on "Native Americans of Eastern North America." Clark Larsen led off with "A Biohistory of Health and Nutrition in the Georgia Bight", focusing on the two major events influencing Native American health in the region: adoption of maize agriculture and contact with Europeans, with particular emphasis on the establishment of Catholic missions by the Spanish. Most of the 1,413 skeletons studied dated from after 700 AD, the earliest being from before 1150 AD and the latest from approximately 1700. He found little evidence of trauma, with other trends similar to those found among other Eastern Woodland populations: a decline in workload leading up to contact, followed by an increase, consistent with the written record of labor coerced from natives of Spanish Florida at the time. Larsen stated his view that the health index does not fully capture the information available from these samples.

Kiple commented on the rise in population after the adoption of maize and the spread of infectious disease in the native compounds at the Spanish mission. Milner provided a thoughtful analysis of the tradeoff between precision and accuracy. The more precise we want to make our generalizations, usually the less accurate they are. We need to understand, he said, the accuracy of the health index, and its sensitivity to variations in health conditions. We are making a big leap from the anthropological custom of analyzing small samples from a particular area only in the context of that small region and time. Here, the aggregation of skeletons into what we call samples may generate as yet unknown dangers for our generalizations. He concluded with a plea for greater understanding of the local community context of these skeletons before generalizing. Discussion ensued on the feasibility of generalization, where the alternative, as Gutmann noted, lack of comparability, leaves us able only to describe and not to explain.

The final paper of the day was presented by Paul Sciulli (Ohio State) on "Native Americans in Eastern North America", focusing on the Great Lakes region. He acknowledged the oft-mentioned issue of sampling in presenting his data. Since they come from the equivalent of neighborhoods or economic units, and not from biological populations, they are not really a slice in time, but rather a "scoop" with some characteristics that are unique

and some that are typical. Most of the Great Lakes Native American samples show relatively tall people with relatively low levels of stress. The greatest variations are found in dental pathologies, with higher frequencies of caries and evidence of anemia in maize agriculturalists. In the Ohio Valley it seems that the transition to maize was not complete upon contact. Walker commented that, in this sample as in so many other New World samples, rising incidence of pathologies could be dated to the 500-1100 AD period, which, if tied to other samples, might indicate climatic change at the time. Filled with deep thoughts about the value of small community study and the desire to generalize about larger human groups, the conferees retired to a sumptuous dinner at the Refectory, one of Columbus's finest restaurants.

On Sunday morning the last session, on "Native Americans in Western North America", was convened, with Debra Martin presenting "The American Southwest: Living at the Edge of Existence." She directly attacked the problem of the representativeness of her samples by noting that the architecturally spectacular sites in Arizona, which were the first to be studied, are not likely to have been representative of most Native Americans of the Southwest. Abandonment of these sites was probably gradual, deriving from an ideology that favored moving around rather than from a sudden depopulation due to disease or famine. Skeletal samples from this region show evidence of a wide range of stresses, including a high parasitic burden. The health index of the several Southwest populations was among the lowest in the entire project. Co-author Dan Reff (Ohio State) emphasized the importance of cycles of epidemic disease in post-contact New Spain, especially of smallpox.

Phillip Walker next examined "Health, Nutrition, and Demographic Change in Native California." Since many California Indians practiced cremation, the sites considered in his paper provide two of the very few skeletal samples of California Native Americans. He finds that evidence of infectious disease among populations of the Channel Islands (across from present day Los Angeles) increased with distance from the mainland, perhaps due to difficulties in obtaining clean, fresh water. Evidence of arthritis in women before about 1250 AD was associated with the arduous work of digging up plants, which formed the bulk of their diet, in contrast to men, whose diet was more diversified, including fish. In the later populations, however, men also showed signs of arthritis. Walker emphasized possible effects of climatic change, linking evidence of declining average temperature and

increasing temperature variation between 1100 and 1200 AD with a decline in stature in both the Channel Islands and the Sacramento Delta (Northern California) samples. Demographic disaster awaited contact, especially after the late 18th century. In discussion, Kiple noted that among the Channel Island populations, disruption occurred c. 1200 as with other populations, but without an equivalent transition to maize agriculture. This might support Walker's emphasis on climatic change as a source of the disruption. Milner observed that what seemed to be a relatively high level of violence-induced trauma does not differ much from that found in bellicose societies in the present day. It may have been due to the Channel Islanders' search for food in ever more distant locations.

The final paper of the conference was on "Mortality and Skeletal Pathology in Equestrian Nomads and Village Farmers of the Great Plains", presented by Doug Owsley and Sheila Ryan Johansson. After reviewing the history of modern sampling of Plains Indian burial sites, Owsley distinguished between hunter-gatherer societies and settled farming societies on the Plains. European influence had some positive effects before the mid 18th century (for example, by making horses available to nomadic tribes). With the expansion of hunting ranges, acquisition and consumption of bison meat increased. Territorial expansion also led to conflict. Forced movement to reservations resulted in an increase in certain pathologies, for example, bone lesions indicative of anemia. On the reservations, nomads appear to have fared worse than farmers, suffering more infections and more trauma. In particular, the nomads seem to have experienced increases in facial trauma (e.g., broken noses from domestic fights). Johansson observed that the Plains Indians exemplified populations for whom the "whopper assumption" of stationarity creates more problems than

it solves; it should be replaced by an assumption of moderate growth. Reff commented that meat-corn trade between hunter-gatherers and agriculturalists might lead to a similar diet for both groups, making bone analysis problematic. Milner again cautioned against making the leap from bone lesions to disease prevalence without further information. Walker discussed the importance for nomadic societies of moving away from their waste sites, and suggested present-day central Asian nomads as possible comparison groups.

It is not possible here to sum up a project undertaken by five dozen scholars from various disciplines, and covering several millennia on two continents. A few tentative findings can be advanced, however. Abundant evidence indicates the difficult circumstances of life in pre-Columbian America. In particular, some of the lowest health/quality-of-life measures were scored by cultures that created the monuments recognizable to modern-day readers of *National Geographic*. For the historic period, synthesis of the bone record and the written record yields insights unavailable from either source alone. Notably, life expectancies are now much longer than those of a diverse collection of past populations who experienced a wide variety of diets, disease environments, and work regimes.

For the future, project leaders Steckel and Rose plan to edit and publish a two-volume work containing the revised conference papers, due in early 1997. Volume I will contain relatively short (25 page) essays intended for an audience of advanced undergraduates. Volume II will be an extended technical appendix designed for scholars interested in the research methodology, nuances of interpretation, qualifications and extensions. Finally, tentative plans to extend the project to Europe, the Mediterranean and the Middle East are already under way.

Eh.Net Report (continued from page 17)

A short lecture by H-Holocaust list editor Jim Mott (H-Net) and a productive discussion on list editing facilitated by Mitch resulted in decisions to refocus many Eh.Net lists. Several participants left the conference with plans for refining their editorial roles and with ideas for novel approaches to increase quality and quantity of subscriber activity.

Specifically, EH.Res will promote full-scale discussion of research topics directly related to economic history. Rosenbloom and new editor Jonathan Bean (Southern Illinois) will initiate and moderate a series of discussions and organized "seminars" on specific research issues. EH.Res also will be the primary outlet for book reviews and article reviews; reviews of interest to specific lists will be cross-posted. The list continues as a forum for requests for research information.

EH.News remains the distribution list for information useful to all economic historians: Calls for Papers, conference programs, website addresses, and announcements of new publications.

EH.Disc continues as a forum for extended discussions

and freewheeling debate about issues related to economic history, but not necessarily tied in to research – a relatively informal place for economic historians to address topics of interest. The list will not be moderated, but Joseph Ferrie (Northwestern) and an editorial board will monitor discussion.

John Murray (Toledo) will be the third editor of EH.Teach, joining Mitch and Whaples.

Databases has become active again, now that its editor, Don Schaefer has returned to the US.

EH.Student editors Lynne Kiesling (William and Mary) and Kyle Kauffman (Wellesley) have arranged for a number of American Economic History instructors to initiate and coordinate discussions among students in their courses. The editors invite all economic history professors and students to join.

In addition to volunteering to write book reviews, submitting abstracts and registering their data sets, readers are welcome to join EH.Net as list editors, members of editorial boards, or project assistants. Please send an e-mail message to EH.Net Director Sam Williamson: sam@cs.muohio.edu

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CALL FOR PAPERS

World Congress of Cliometrics

Munich July 10 - 13, 1997

The Third World Congress of Cliometrics will be held July 10 – 13, 1997 in Munich, Germany. All members of sponsoring organizations are invited to submit proposals for papers or requests for attendance. Knick Harley, Timothy Hatton, and Gianni Toniolo will serve as Selection Committee. Authors interested in presenting papers should send four copies of a two-to-five page proposal by post or e-mail by **October 1, 1996** to

The World Congress Secretary
Laws Hall Box 42
Miami University
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Oxford OH 45056 USA
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At least one author must be a member of one of the sponsoring organizations: the Center for Economic History and Theory at Moscow State University, the Canadian Conference in Economic History, The Cliometric Society, the Economic History Society of Australia and New Zealand, the European Historical Economics Society, and the Japanese Quantitative Economic History Group.

Relevant Dates:

Deadline for proposals:	October 1, 1996
Authors notified of acceptance:	December 1, 1996
Requests for invitations due:	March 1, 1997
Completed papers due:	April 1, 1997
Conference books mailed:	May 15, 1997

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