Cynthia Taft Morris is Distinguished Economist in Residence at American University; she retired from her position as the Charles N. Clark Professor of Economics at Smith College in 1998. She has worked at the American University of Beirut, the Agency for International Development, the Economic Commission of Europe in Geneva, and the Mutual Security Agency in Paris. She is founder of the Economic History Seminar and past president of the Economic History Association (1993-94). Mike Haupert conducted this interview between August 2006 and March 2007 via email and concluded with a wonderful visit with Cynthia at her home in Washington, DC, in May of 2007.

Let's start with background. I know that yours is not the traditional road to economic history.

True. My personal life dominated every decision I made between 1949 and 1965. In 1949, I was accepted for graduate work at the top colleges that took women and at LSE. I picked LSE because (1) I was sick and tired of the conventional sequence of classes followed by exams and LSE had neither regular exams nor regular classes but instead employed the British tutorial system; (2) I wanted to go back to Europe where I had made many friends the summers of 1947 and 1948; (3) Edna MacMahon, my most beloved teacher from my undergraduate days at Vassar, whose passions were the detailed economic history of Dutchess County and comparative systems, wanted me to go there; and (4) above all, I wanted "Adventure" with a capital A!

My love of history was supported by my LSE tutor, Henry Phelps Brown, who had me write essays every week on subjects of my choice (he made suggestions) and who had a lively seminar that I attended. My thesis on the impact of nationalization on collective bargaining in the electricity industry was way more thorough than required for a Master's degree. After I got my Master's, I rejected the idea of returning to the U.S. to further my career and decided instead to go to Paris without a job. From a career standpoint the move to Paris looked stupid, since I was told that in six months I could have turned my thesis into a Ph.D. in labor economics.

But once again, my sense of adventure won out. In London, I had met Bill Gausman, a former trade union official, who then moved to what became the Mutual Security Agency of the Marshall Plan. I persuaded him to hire me, hoping that in spite of my college reputation as a radical, I could pass the FBI security check. I did. He was the only person actually offering me a job that could support me. I was really interested in his desire to find out whether the Marshall Plan had raised the living standards of workers. I thoroughly enjoyed the work, the travel to five countries that it financed, and the insights it gave into the differences among institutions in these countries.

My move from Paris to Yale was the consequence of the persistence of Lloyd Reynolds, who wanted a research assistant with a background in labor economics and European language skills. He went to Henry Phelps Brown in search of one. My guess is that I was probably the only one who fit the description. I was fluent in French and German, spoke rudimentary Italian and had my LSE degree. I was not ready to go back to the U.S., but when he offered to wait six months for me to make the move, I agreed.

I took my first real theory courses as a graduate student at Yale with William Fellner. John Sawyer taught economic history at Yale in those days. I had Henry Bruton for statistics and James Tobin for econometrics. At the time I was very involved with Lloyd Reynolds's book.

I had not arrived at Yale seeking a Ph.D., but I guess the ambiance shifted my ambitions back to academia. When I passed my comprehensives at Yale in 1955, I seemed at first to have no options since Yale did not permit women to corrupt their young men by teaching them. I was getting married that fall and my husband-to-be was thinking of going into the Harvard Graduate Russian Studies program. Finally, with two glasses of sherry under my belt, I telephoned Arthur Smithies, Chair of the economics department at Harvard, and persuaded him to hire me as a teaching fellow. I suspect I was helped by the fact that Lloyd Reynolds had made me co-author of *Evolution of Wage Structure,* the book on which I was working for him. I also got the impression that Smithies liked the idea of forcing Radcliffe to give me the paper appointment that would permit him to hire me. I wanted to be an assistant teacher to John Dunlop, but he turned me down on the ground that his field was not suitable for a woman. In the end I taught introductory economics in Lamont Library, the only place with easy access for someone on crutches. I wasn't allowed to teach women because the deed of the building forbad it.

My personal life continued to dominate my decisions and the haphazard way in which I continued to acquire and develop my interest in economic history as well as economic development. My husband had decided to go into the Foreign Service, where he remained for 10 years, then moved to the Space Agency's international relations section for another 10 years before retiring and becoming an Episcopal priest.

During the first five years of this 23 year period we spent a year in Geneva and two years in Beirut. In both places, I picked up what I could in the way of interesting work. I don't remember much about what I did to be worth my pay at the Electricity Power Committee. When my husband was assigned to a conventional Foreign Service tour in Lebanon, I was determined to end up with a regular teaching position in the economics department at American University of Beirut and took advantage of the fact that a member of that department was at Princeton. I went to see him and persuaded him to consider me seriously. He said he thought I could be a visiting assistant professor and I was offered a position.

I was persistent. Men had amazing ways of blocking women from good jobs. The women of my generation did not have women role models. When we met with the kind of stupidities I have recounted here, we thought: "What a stupid man. I'll go find an intelligent man to help me out." If we were determined enough, we found one and kept going. It does make for some amusing stories now!

Women are still underrepresented in economics. Do you see any progress? Are there good role models out there?

I went as a teaching fellow to Harvard in 1955. There was a fairly big change in the next five years. Yale started taking women as TAs. Probably by 1960 there were no more John Dunlop's saying frankly that a certain field of economics was inappropriate for women to study. Some of those men are now very apologetic about what they said and did back then. I was once told that I had to pick either family or career, but that a woman could not have both.

In spite of the fact that economics is not particularly good at advancing women, there is a considerable supply of role models. All should do what Marty Olney did to get grad students into the EHA. It is a long run approach. You need money to give them scholarships and make studying economics attractive. She got a tremendous increase in grad students in EHA in general. She wasn't necessarily focusing on diversity, but she is the model.

How do you think economic history can appeal to more women?

Extend the spectrum of courses and topics economic history covers in order to attract the interest of minorities and women. It will take a lot of money and effort in the long run to attract, mentor, and get them jobs. I hope someone comes along who will put the effort into getting this job done. There are no rewards for doing so at the moment. Nobody will promote you for this. It must be institutional. We need to widen the types of subjects we're willing to admire to interest some of these students and allow for them to be promoted when they study them.

What kind of impact did your stays in Geneva and Beirut have on your career?

My stay in Beirut afforded me my first opportunity to teach history of economic thought. There were few books in the AUB library so I ordered a whole series of classics from Blackwell--Adam Smith, Jevons, Wicksteed, Marshall, and John Stuart Mill, among others. I read them all through with huge pleasure. These men fully appreciated the importance of institutions and institutional change. I can honestly say that they greatly increased my fascination with the historical, institutional context of economic change. I had six months to devote to preparation of my class in history of economic thought, a luxury made possible by a wonderful retired Nubian chef who did all our cooking during this period.

When did you finally return to the U.S.?

In 1962 I returned from the Middle East. I was working at the Brookings Institution while waiting for AID clearance to work there. This is when I first met Irma Adelman. We worked for Hollis Chenery, a Harvard professor known for his work on structural change and development policy. Irma had an idea for a book ultimately published as *Society, Politics, and Economic Development* (hereafter *SPED*) in 1967. She had nerve, and she billed most of the computer work for the book to Chenery's budget—much to his dismay. He got a thank you in the book for it, but no reimbursement for his budget.

SPED focused on underdeveloped countries. We thought we'd apply the same model to economic history.

In order to conduct the historical research we used Chenery's development funds and obtained our NSF grant. I wrote the grants, which in those days were quite generous. NSF financed a copy machine, generous salary, and a full set of the *Cambridge Economic History of Europe*. The first grant was used to finish up *SPED*. The next two in principle were still for *SPED*, but we used them to start work on economic history.

I needed an academic connection to get an NSF grant. Since I lived a few blocks from American University (AU) and did not wish to take a full-time teaching job because I had a very young child, I decided I would try to get a paper appointment as an Associate Professor of Research. That way I could be paid by the NSF to carry on my own research. The economics department at AU, while sympathetic, said they had not the power to do such a thing. I went to the Provost of AU. In the most critical bit of persuasion in my career, I convinced him that he had nothing to lose and quite a bit to gain in overhead for the University. I told him that he need not pay me anything if I failed to get the grant. In fact, I got three grants in a row and several years later, a fourth.

Tell us about your collaboration with Irma Adelman.

I was the "get your hands dirty detail person" and Irma was the theoretical, cliometric, "avoid getting your hands dirty with the details person." That's one reason why our partnership worked so well: we each exploited our comparative advantage.

We ended up writing books because no journal would publish our stuff. Every leading journal turned us down, but we just kept moving forward. The first time we got an article published was in the *JEH Tasks* issue after it was presented at the 1982 EHA meetings. I insisted our reply be published with Jeff Williamson's discussion because it was an important exchange. Jeff was a good friend. We have always gotten along well. His arguments were very familiar so it was easy to reply to them.

Applying our ideas about institutions and development to history was a far bigger job than we thought it would be because we had to rethink to fit the historical context.

For example?

Consider the measurement of the literacy rate. Our original definitions would have put too many nations in the same high category of literacy in the late 19th century. In developing categories for our historical work, I fell in love with economic history. Irma didn't fall in love with the history as I did.

I found the country histories fascinating. I was forced to do so much reading. I had to learn Dutch to read the good stuff on the Netherlands. I also had to pick up some Italian and Spanish to read some of that stuff.

It was so much more work than I thought it would be. I wouldn't try it again. I wouldn't have touched the project with a ten foot pole knowing what I know now about how much work it was. I told Irma I wouldn't finish it without a fourth NSF grant.

This is how I finally came into contact with the community of economic historians. The final grant, which made it possible to complete *Comparative Patterns of Economic Development,* took four tries before the NSF finally came through. After the third failed economic history grant application, NSF suggested that I give more papers to economic historians in order to get a few more favorable approvals on grant applications. Jonathan Hughes told us that we would have much more trouble getting our work accepted by economic historians than we had had getting it accepted by development economists. That is why our preferred title for *Comparative Patterns* was *Where Angels Fear to Tread*.

Until my second child was in her early teens, I taught, supervised a large number of dissertations, and did research, rarely leaving DC. I did not give papers at conferences; that was Irma's job. I worked a great deal at home. That was my particular compromise between home and job. So my first direct involvement in the world of economic historians was to go to a workshop at Princeton around 1978. There I met Gavin Wright, Bill Parker, and Claudia Goldin, among others. Later I began the Washington Economic History Seminar, persuading Bill Parker and Claudia Goldin to come at their own expense. Mike Haines came while at a DC conference. After that I persuaded AU to give us about $1000 a year. I put visitors up in my home and cooked delicious meals before the monthly meetings. For that first seminar I cooked *coq au vin*, using Julia Child's recipe, for 27 people!

When did you start teaching economic history courses?

At AU; in 1965 there was a critical and long-lasting shortage of college teachers as the baby boomers entered college, so I was able to bargain for a half-time teaching load of, eventually, mainly graduate courses. The AU economics department had a strong institutional slant and a policy of noninterference in the content of courses. American economic history was a required course in the Ph.D. program. Along with the University of Massachusetts-Amherst, the AU economics department was one of the first in the U.S. to introduce a program in radical political economy. Because UMass gave financial aid only to minorities and women, starting about 1970, AU got a brilliant batch of white males wishing to study radical economics. To these were added excellent students from various government agencies and a few very talented and diverse women students tied down in Washington by family obligations. As a European-type social democrat, I was part of the AU department's "maverick middle."

I was tremendously stimulated by teaching classes where students questioned everything I said. For the political economy students I developed courses with unusual texts such as Graaf's *Theoretical Welfare Economics* and Dobb's *Welfare Economics and the Economics of Socialism*. I ended up chair of several dissertations written by Marxist students who chose to subject themselves to criticism, hard work, and high standards. It is difficult to exaggerate how much these students influenced me, introducing me to unfamiliar perspectives and diverse subject matters. For example, I read Marx's analyses where he characterized capitalism in ways that only applied to a minute part of the economy in which he lived. He maintained that large capitalists create their own demand; minimally true in his day, more widely applicable under today's high-powered international marketing of products.

What lured you to Smith?

I was a pure affirmative action hire at Smith. They had five large departments with no women in senior positions. Jill Conway, the Smith president, told the economics department they could hire a senior professor as long is it was a woman. They had trouble finding someone because there were not many women economics professors with research records in my generation, and even fewer who wanted to relocate to Northampton.

I was not geographically bound after my divorce in 1983, so I could take the Smith job. I never really went though a normal job search in my life. I talked my way into a job or the opportunity presented itself, as at Smith.

It all worked out nicely for me. I got a nice pay bump. I went from a 2/3rds load at American to a 4/5ths load at Smith. I had been half time at AU until my son went away to college. AU had treated me like gold but paid me like dirt. At Smith I did two courses per semester, the same load I had at AU. I was hired with tenure and a chair-level salary, and ultimately I was given a chair. I continued to return to DC for the Washington Area economic history seminar series.

I lucked out at Smith. The economics department was almost as diverse as at AU and I once again fell in the maverick middle. The undergraduate students were fun and bright, though I missed my radical AU graduate students. I was thrilled when I was allowed to organize a new undergraduate course in European Economic History, which fascinated me. I moved back to D.C. when I retired at age 70 in 1998.

You do not have a conventional economic history background. How did that affect your approach to the new economic history in its infancy?

The Clio *Newsletter* article on Gavin's experience inside the flowering of the new economic history is a beautifully done interview of a leading figure in that history. My experience in economic history is outside that flowering. Yet my work is a product of the intellectual developments of the same period of time in which the new economic history developed. Two broad themes marked my thinking when I first came into contact with economic historians in the 1970s: a keen interest in institutional change and a passion for economic equity and social justice. The former was a result of my training and my work with Irma. The latter I probably absorbed from a Christian socialist pacifist mother and a father immersed in municipal politics. He was active in the ousting of city bosses and establishing good government as well as running the municipal relief organization during the Great Depression.

There is another theme I should not omit. I have always been intrigued by problems of measurement, with an emphasis different from that in the new economic history. It is true that I was active in conventional "measurement" when working for the labor information section of the Marshall Plan. My main interest has been in the weakness of the conceptual link between the rules for forming the many indexes we use to study economic history (national income, unemployment rates, etc.) and the theoretical concepts we wish them to represent.

Please expand on this.

The data we have are determined by specific historical situations and institutions. Then we have theoretical concepts. When we put the data into a theoretical model they don't always fit. For example, we measure the concept of productive capacity using GDP. But what do we produce what? For whom? There are so many ways to answer. How we choose to produce or distribute our income influences measured GDP. Even looking at changes in GDP we have to be careful. GDP in a rich country consists of much different items than a poor country. We don't necessarily learn about the capacity of a country to produce for the citizens of its own country. If we don't question measurement, we can miss the story about why income distribution varies across countries. There are institutions that make it either easier or harder to allocate income evenly.

I decided early on that I would not try to introduce my preferred political philosophy, which has its roots in European democratic socialism. The overwhelming current philosophy of economists and economic historians favors the institutions of capitalism. I started, therefore, with the idea that I would keep focused on the question: "What kind of capitalist institutions favor a widespread distribution of the benefits of capitalism?" That was the explicit focus of my EHA presidential address in the fall of 1994.

As an aside, I have to tell you the humorous side of the actual address. Five minutes into the talk, a group of beer drinkers on the other side of a very thin partition started singing beer songs so loudly as to drown me out. I took a deep breath and started belting out the rest of my talk at the top of my lungs in a good imitation of a second alto in a Bach choir (I had sung in one for quite a few years). At the end, I got the only standing ovation I have had in my life! But I doubt the cheering was for my approach to economic history.

How can capitalist systems spread out their benefits?

What capitalism needs to distribute income more evenly is a highly productive agricultural sector. Plus, when GDP numbers are published they should be accompanied by reports on median income as well, not just average income, because it tells such a different story.

I'm strongly influenced by a European vision of democracy with a focus on eliminating poverty. I'm really concerned with the element of extreme poverty. Once people have the essentials their tastes change. I'm in line with Jeffery Sachs on ending that extreme poverty. But he's unrealistic about what can be done. He imagines it's simpler than it really is. Assuming that rich countries could simply reallocate their surplus to poor countries is a bit naive.

How do the contributions you and Irma made address this topic?

Irma and I wrote two books on the subject, one focusing on noneconomic influences on economic growth and one on social equity and economic growth, before we turned to comparative economic growth in the 19th century. After that, I wrote a long essay examining in detail how well one set of our historical statistical results matched what actually went on historically. The essay took far too much time for me to consider examining others of our results in the same detail. I wish I could have, probably because I cannot get rid of a certain degree of skepticism about the validity of most cliometric work, including ours. I did discover that even with the variety of our indicators, there were important omitted influences.

Which of your contributions do you consider most important?

It has become completely accepted that you can't study differences in economic performance among countries without considering a wide variety of non-economic variables. That wasn't accepted when we started. That is what we have had influence on. The historical study groups countries into common characteristics and periods and develops a factor analysis explanation of the interrelationships. We provided guidance to the types of variables that get excluded in such studies and which omitted influences are most important.

What is the factor analysis approach that you use and why is it controversial?

Most quantitative work in economic history adopts a theoretical approach. It starts with reasonably precise hypotheses and conducts test of the hypotheses using available quantitative indicators and models guided by economic theory. Such work does not "prove" hypotheses because of omitted influences and imperfect indicators, but it can reduce the level of uncertainty about common hypotheses. *Comparative Patterns* takes a more empirical, less theory-driven approach. It is an explicit search for hypotheses that might help explain patterns of intercorrelated influences on economic growth. Most of the variables are classification systems that rank countries using a wide range of theories about social, political, and economic institutional change in industrializing and developing countries.

The contrast between the theoretical and empirical approaches is not as great as might at first appear. It is rare that the theoretical model used in a published article is exactly the same as the initial model chosen before the research begins. Almost all research that starts with a theoretical model involves a back-and-forth between early empirical findings and the precise formulation of models. An empirical approach that does not have a good theoretical basis for the choice and construction of indicators as well as an appropriate statistical model and explicit use of theory in final interpretations is likely to produce "garbage in—garbage out."

Factor analysis groups things that are closely correlated. One of the useful things we found when writing *SPED* was that you could take out straight economic variables and use social and socio-economic variables instead and you'd get just as high a correlation with per capita income for the group as you would have if you used only economic variables. This was one of the results in our comparison of income among countries in our contemporary growth study.

Regarding your first book, *The Evolution of Wage Structure*, with Lloyd Reynolds, what role did you play in the collaboration?

The Evolution of Wage Structure was Lloyd's book. I really wanted to stay in Europe. He needed a labor economist good in languages, and I was that person. I was desirable for my skills but I enjoyed my work in Europe. I love working in archives with original data. I learned a lot about cultures. One had to be careful in asking governments for data in Greece and low-income countries; they viewed it as a discourtesy on their part if they could not help you. If you pushed too hard they would make up something to please you.

The book focuses on the role of unions in helping lower skilled workers climb the income ladder. Do you view the decreasing role of unions today as a factor in the flattening of the growth of income for the lower-skilled sector of the U.S. economy?

I'm an extremely strong supporter of trade unions. Lots of evidence suggests wages are increased when trade unions are active. A recent student of mine looked at the wages of welfare moms who went into the job market. She found the strongest correlate of higher wages was whether the profession they chose was unionized. I think the best solution for the problem of wages of lower income is stronger unions. But there are more problems than just unions that explain low wages. Immigration, as it was in the 1870s, is a factor in decreasing wages, though the effects tend to be localized. Globalization has had a big impact on wages in this country. Some states are more affected than others and it is very difficult to disentangle the cause of the inequality. Steady, fast economic growth will lead to increased inequality. The major beneficiaries are those who are most productive. That usually isn't the poorest workers. It can take decades for the earnings of the poorest to increase when the economy in general improves. For example, in the 1990s there was virtually no gain for the lowest earners. Trade unions do better in good than bad times. The other thing is that the U.S. has a strong culture of the attainability of the middle class. Middle class is not associated strongly with unions. People don't consider themselves middle class if they are unionized.

In *Economic Growth and Social Equity in Developing Countries* and in your 1995 EHA presidential address, you focus on the "trickle down" of economic growth to

the lowest earners. Has progress been made in the past decade in our understanding of how to achieve this "trickle down"? How about in our efforts to move in that direction as a society through our political programs?

In the sprinkling of things that I have read I'm not impressed with our progress at explaining differences in the equity in wage structure. There are always some people interested but I haven't seen any studies that really explain why. There is no neat model with which to study inequality. I think Irma and I took a step in the right direction and our exchange with Jeff Williamson in the JEH makes that quite clear. One of Jeff's major criticisms of us is that we don't have anything new to say about influences on poverty. On the other hand he implies that economic growth and industrialization lead to decreases in poverty. Actually, this is only true of western Europe, not most of the rest of the world in the late 19^{th} century (the period he focuses on). So I think our work makes it very clear in many different places the conditions necessary for economic growth to spread widely: increased production in agriculture or domestic sectors with increasing wages so you get favorable demand for the products in domestic markets. Otherwise the benefits will end up going abroad.

That statement needs further qualifications for the last decade. Patterns of globalization over the last ten years are quite striking; meaning generalizations made a decade ago need to be reevaluated. Our work in *SPED* indicated that political influences are extremely important in explaining distribution consequences. If you have those who already have a great many assets in position of political power, the chance of widespread distribution of benefits of economic growth will be much less.

Nothing will happen if the government is corrupt. Take Nigeria or Congo for example. Under their current governments wages have decreased and infrastructure has diminished. Nothing good has happened. In low income countries in particular, an aggressive and reasonably non-corrupt government is a major prerequisite.

You have some interesting dedications in your books.

The press didn't like our dedications. The first one was "to our mothers who bore us and our husbands who don't." In the third book it was "to our children who bore with us and husbands who didn't."

What about Cliometrics?

One of the major characteristics is the use of neoclassical modeling. I really like Gavin Wright's work, which fits this model. A lot of them concentrate on neoclassical theory of property rights. Concentrating on the neoclassical theory excludes a wide range of very important influences. How many countries can we cite that grow due to improved property rights only? Not that many. Lots of growth and change is motivated by ethnic identity, pure nationalism, etc. Things omitted from the neoclassical model. The weakness of neoclassical economics is that it is so narrow. It excludes many explanatory variables.

It must have some redeeming features…

Its strengths are that it brings a great deal of clarity to the model. It is precise but not accurate. Our work concentrates on the things the neoclassical model leaves out.

When I reread our conclusions in *SPED* they seem strange to me now. They don't fit our current line of thinking. Our conclusions are really that development occurs for very different reasons in different places. Improved tax systems, leadership, and sound financial institutions are highly correlated with economic growth in a place like Chile for example. But those same things will be ineffective in underdeveloped African nations because the institutions necessary to make a new tax regime effective, for example, aren't present.

What makes economic history *Cliometrics*?

The theory and statistical rigor, but that doesn't necessarily apply to our (Irma's and my) early work. The earlier economic historians were buried in data and not much interested in theory. I think the quantitative work is the interesting part. Some of the early practitioners of Clio were not very quantitative. Some of Douglass North's research, for example, *The Rise of the Western World* (coauthored with Robert Thomas) was not very quantitative. It's more like the older economic history.

So what does one require on the quantitative side to call it Clio?

In respect to theory, most everyone uses neoclassical models. That is Clio's strength and weakness at the same time. It allows for the formalization with good tools. It's not that you exclude all else, but the typical Clio approach is market-focused. Early Clio started as counterfactual modeling (think Fogel and railroads). It's gone a long way since then. If you don't include those who don't use the quantitative approach then you'd have to exclude Douglass North, so you can't do that. But can you include mavericks like Irma and me?

What do you recall of your first Clio meeting?

I had tried to get on Clio programs but never got accepted until D. McCloskey accepted a paper at the 1978 meetings in Madison. He was the first economic historian to accept our work for presentation at a conference. He didn't agree with it, but thought it was worth discussing. It came about the same time as our last NSF grant. I was excited for the opportunity to give the paper and was pleased with the reaction. I did not expect it to be liked. There were lots of questions. It was well worth it. I don't get to many Clio meetings. I didn't make another one until the 1995 sessions at the ASSA meetings, and then World Clio when it was in Montreal in 2000.

As a relative late-comer to economic history you became president of EHA rather quickly.

I was EHA president in 1995. Lois Carr came before me and she pushed for me. I think it's possible that because I was a woman that pushed me ahead in the queue. I think Gavin Wright should have been president before me. I had done lots of development economics in the 1960s before changing to economic history. You can't really explain my presidency without considering that Irma and I had established a reputation in the development field. Our book was big and fat and not everyone had read it. If they had, they might have changed their minds about me!

I got to know lots of folks through our DC seminar. I ran it or helped run it for seven years, had about fifty seminars in that time, and no repeat visitors. So in fairly short order I got to know lots of economic historians. And they couldn't really forget me. They ate and slept in my house and we treated them well. I'm not certain my election as president was based on my economic history work as much as the fact that I was very well known in economic development already. That made a big difference.

What is your philosophy of teaching?

The personal recipe I have followed is that I care deeply for every student I have who makes an effort, no matter how poor the student is. I am very demanding and give few A's. I will, however, give time to every student who is making an effort. I make clear from the beginning that they should feel very free to take a different class if they don't want to work hard. I grade the short essays they write at least once a week in most of my classes (they can choose their own question but it must be relevant to the readings) and make specific suggestions about how they can raise their grade. (They can revise a paper once). None of this is of any use unless the professor provides the most critical ingredient: a passion for the subject. Without passion for one's subject matter all the rest is of little use!

What is the most promising direction for the next wave of economic historians interested in the question of economic growth?

They should concentrate more on the distribution of the benefits of economic growth and less on straight rates of growth and GDP per capita. It's very difficult, as no clear cut theory exists for studying income distribution. It's not an attractive field as a result. Data are hard to come by as well. Studies of income distribution have been greatly undervalued in economic history. Part of the problem is that not enough basic archival research is being done to uncover basic data needed for such a study. John Wallis is a good example of someone willing to do the kind of basic data work that is needed. The disadvantage of the neoclassical market focus is ignoring the hands-on dirty work of digging in the archives. It is more prestigious to do fancy modeling and statistical work. One of the difficulties is that the data digging is not likely to lead to promotion. Students need a two prong approach in which they can pick a problem that will show off their proficiency at quantitative work and be sure at same time to develop interest in basic data that are lacking that they'd like to know more about. Young professors do have to consider promotion, so it is a practical issue to practice what is currently rewarded. I'm careful to help students to get ahead so they need to the modeling, but the basic data need to be gathered as well. The grubby data is hard, necessary, but fascinating. I've always enjoyed working with basic data. That is why I liked the Marshall Plan job so much.

What can senior economists do to help?

Senior economic historians should promote the importance of basic data and show the way. They control promotion. They could make it happen through effort and example.

Finally, what role does the study of economic history play in the education of an economist, and what future do you think the field has?

It is a terrible thing to eliminate the required economic history course for graduate students. It is the study of change in institutions. A lot of work in economic history doesn't pay much attention to that, but over time institutions change, so economic history is important because it studies those changes over time.

Where will they learn to consider changing institutions if not in economic history? Development economics does this, but usually focuses on a special group of countries. Economic historians are more and more interested in those areas where data are abundant. The difficulty is that the available economic history readings focusing on changing institutions are pretty sparse. I was delighted to learn that Yale still requires economic history, but universities all over are eliminating economic history positions right and left. I taught intermediate price theory when I first began – that's what paid the freight. I had to ask to teach economic history at AU. I was never hired as an economic historian, I changed into one. Unfortunately, that is too often the case today.

Thank you for your time Cynthia.

Mike, your questions got me to think about a lot of things I hadn't considered for a long time. It has been a lot of fun reminiscing. When I retired I was feeling worn out, so I haven't published since then. When you contacted me for this interview I considered it as my last official duty, and it is one I enjoyed very much.

Selected Publications of Cynthia Taft Morris

Books

Evolution of Wage Structure. New Haven: Yale University Press, 1956. (Co-author: L. G. Reynolds)
Society, Politics, and Economic Development. Baltimore: The Johns Hopkins Press, 1967. (Co-author: Irma Adelman)
Economic Growth and Social Equity in Developing Countries. Stanford University Press, 1973. (Co-author: Irma Adelman)
Comparative Patterns of Economic Development: 1850-1914. Baltimore: Johns Hopkins University Press, 1988 (Co-author: Irma Adelman)

Articles

"Some Neglected Aspects of Sixteenth-Century Economic Thought," *Explorations in Entrepreneurial History*, IX, No. 3 (1957)

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