Commentary on Dissertations

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Dissertation sessions are valuable parts of our meetings, offering newcomers to the discipline a chance to present the results of their research and allowing those of us who have been around for awhile the opportunity to hear about new and interesting work. At the same time these sessions are unfair. Young scholars who have spent at least a year, and usually much longer, in preparing a dissertation are asked to present the results of all their work in 10 minutes, or, as the editor of our *Proceedings* puts it, in five double-spaced, typewritten pages -- with ample margins. Then, this brief presentation is subjected to an even briefer critique by a discussant who cannot possibly be an expert in all the areas covered by the dissertations but who is required to act as if he were.

As the organizer of this year's dissertation session I am especially sensitive to these problems, yet I remain convinced that these sessions are useful. The five dissertations briefly summarized here range from 9th century Egyptian agriculture and manufacturing to 20th century American movies; they come from three academic departments -- history, economics, and communications. If they are not united by a single theme, they do have two things in common: all are interesting and imaginative research efforts; and together they show that business history is alive and well.

Edward Bubnys's dissertation is a study of social mobility in Chicago in the late 19th century. He drew samples from the 1870 and the 1900 censuses and gathered data on family wealth (for 1870 only; the 1900 census did not contain this information), occupation of the family breadwinner, and education of the male children. Dividing his sample population by ethnic origin allowed Bubnys to compare the performance of various ethnic groups during each census year and between census years. He assigned various occupations to five categories ranging from upper-white-collar to unskilled-blue-collar, giving each category a number ranging from +2 through 0 to -2. This allowed him to construct an "occupational index" for each ethnic group in his two sample years.

Some of Bubnys's findings are not surprising (which is not a criticism, for having hard data to replace general impressions is valuable): wealth was sharply skewed in 1870 and the native-born were the largest wealth holders and dominated the high-status jobs in both years. More unexpected were figures showing that average family wealth in Chicago in 1870 was a remarkably high $5,000 and that Chicago had a greater proportion of wealth holders in its population than did cities elsewhere.

Although it contains useful information, Bubnys's analysis suffers from a number of problems. One problem, common to all such statistical studies of social mobility, stems from the high incidence of geographic mobility; as many as half the people sampled in one census year are gone in the next. This problem is compounded in Bubnys's study because he samples by age cohorts and does not try to follow the same population. Therefore, his data refer to the status of the ethnic population in general in Chicago and not the changing status of particular people. Complicating matters even further are the great increase in the city's population, the massive influx of immigrants, and the changing nature of the economy and, therefore, of the job structure. Finally, the occupational index may be misleading despite its numerical precision. Assigning many jobs to a particular status category is difficult enough for any single year, but using the same categories over time ignores the fact that the nature of particular jobs often changes because of changes in technology and business organization.

To be sure, Bubnys does not explicitly claim that his study measures the extent of social mobility between 1870 and 1900; rather he seeks to relate wealth, occupation, and education to ethnic origins at two different periods. Nevertheless, such comparisons are sometimes implicit in his discussion, and, indeed, comparisons over time are really the more interesting and significant problems to consider. Additional work on the census would, of course, help, but the work entailed in following the same population from 1870 through 1880 to 1900 probably would not provide an adequate payoff in results given the extent of geographic mobility. Work in more "traditional" sources would be more useful, providing information about the nature of the jobs and the perceptions of the people. For example, white-collar workers might have been better able to afford to send their children to high school and beyond and forgo earnings while children were in school, as Bubnys suggests. But it is entirely possible that the cost of schooling was not as important as differences in the attitudes towards particular jobs. For the native-born, a high-status, white-collar job requiring formal school preparation might have been important, while an unskilled immigrant might have considered a skilled trade requiring an extended apprenticeship to be important. In both cases there would be costs and forgone income, but only in the former would the cost of formal schooling appear be the determining factor because the census would list apprentices as working.

I doubt that many members of the Business History Conference are specialists in 9th century Egyptian business practices. Nevertheless, I think those who would read Gladys M. Frantz's fascinating combination of historical detective work and sophisticated economic analysis, will agree with me that her dissertation is an important study.

The scarcity of data prevented a statistical or quantitative study. Indeed, information is not even ample enough to provide many of the details required merely to tell the story. The Arabic histories are mainly concerned with imperial relations and the activities of key leaders and say very little about the day-to-day activities of agriculturalists and people involved in trade and manufacturing. To fill in some of the gaps, Frantz turned to the papyrus documents, many of which have not been edited and published or, for that matter, even read by modern scholars.

Still the data remain sparse, fragmentary, and in certain key respects -- population figures, for example -- nonexistent. Here is where Frantz employed economic theory (in a general, descriptive way) and some good old-fashioned historical detective work. She found that the evidence points to a prosperous agriculture, adequate food at low prices, and little grain export, suggesting that there was an agricultural surplus other than grain available to appropriate and an opportunity for investment of that surplus. She found the appropriators were an elite of government officials and land contractors and the investment was linen production for export.

I lack the skills and the knowledge necessary to evaluate Frantz's work directly. Instead I want to consider the general historical context of her discussion and how her work may be of value to those of us interested in Western history and in the general problem of early capitalist development.

Frantz begins her dissertation by noting that historians dealing with the rise of western capitalism generally find a kind of linear progression because they seek those factors in early Western history that provided the impetus for the evolution of modern institutions. Those who study nonwestern societies tend to seek flaws or problems that prevented or diverted the development of modern institutions. Frantz's research, however, discovered a thriving 9th century Egyptian economy providing an agricultural surplus that was invested in profitable manufacturing and trade. The problem remains, however: Why did this prosperity disappear?

Answering this question goes beyond what Frantz attempted in her dissertation, and I do not mean to criticize her for not doing more. What she has done was clearly arduous enough. But she suggests in a brief epilogue that alien invasions, the replacement of the elite, and the shift to new crops -- mainly from flax to sugar -- are factors in the change. This is more descriptive than analytical and does not explain why a prosperous arrangement would be scrapped in the course of replacing one elite with another.

Perhaps Frantz's continuing work in the area will provide some of the answers. This in turn may aid some of the rest of us concerned with Western economic development to see by way of contrast and comparison why perhaps similar shocks did not have the same effects on Western areas. Her work might also give additional insights to those studying other nonwestern areas. Some Indian historians, for example, have argued that India was on the brink of modernization when the westerners arrived and disrupted the economy by subordinating it to the imperial countries' economies. Perhaps the alien invasion of Egypt and the disruption of its prosperous linen industry may be explained in similar terms.

Douglas Gomery's dissertation is concerned with technological innovation -- the addition of sound to movies. But it is far more than that. Dealing with the movie industry is perforce a study of the entire mass entertainment business because vaudeville, Tin Pan Alley, recording, and the radio were intimately connected with the movie industry. It is also a study in business consolidation because of the combinations in the entertainment business, in the manufacture of technology, and in the distribution of the product. Finally, it is a study in entrepreneurship because of the manner in which some film businessmen saw the possibilities for sound and quickly exploited them, while others were far more conservative. Gomery has written a massive study that in some ways seems to have come out of the movies itself. He has literally a star-studded cast of entertainers, Hollywood moguls, and industrial tycoons.

Although Gomery traces a significant and rapid change in the entertainment industry, he insists that his story is not one of revolutionary change but of gradual planned evolution. This is no semantic quibble but stems from Gomery's use of a rather formidable theoretical apparatus which he insists gives coherence and originality to his study. It is in this area that I have my doubts.

Gomery insists that his work is guided by three economic theories. The first is profit maximization which he takes as an assumption, arguing that businessmen in the movie industry were interested in making money from their operations, and therefore sociological, artistic, and psychological explanations for their behavior may be discarded. I have no trouble accepting profit maximization when used in this way, but I question whether it says very much. In any event, it seems inappropriate to *assume* profit maximization, since that automatically precludes the testing and evaluation of the theory.

The second theory is that of technological innovation. The theory, Gomery says, suggests three stages: innovation; making the invention practical; and diffusion. To this, Gomery adds a fourth stage (and a third theory), oligopolistic competition. The problem is that Gomery uses these theories only in the most general and descriptive way. Indeed, he does not have (or, at least does not use) those data necessary to apply and test his theories --

data such as profits, losses, expenses, and other day-to-day business records. The theories, therefore, seem to be afterthoughts rather than the basis for the research and the evaluation of the data.

I would suggest that a far more valuable theory, or organizing principle, would be that employed by Alfred D. Chandler, Jr., in his recent book, *The Visible Hand*. Indeed, it appears to me that such an approach is in fact implicit in Gomery's work. He describes efforts to consolidate to diminish competition and to integrate vertically to ensure control of actors, musicians, and other entertainers (the "raw material" of the industry) and to ensure and control the wide distribution of the finished product. These are the central themes in the study and they are the same themes which Chandler and others have so successfully explored in other industries.

J. W. Lozier's dissertation is a broad and diverse study of manufacturing and related business practices in southern Massachusetts in the half-century after 1811. He considers the evolution of the manufacture of textiles, textile machinery, locomotives, and nails and other hardware items, considering in the process such questions as innovations in technology and design, the sources of capital, the recruitment of labor, the organization of production and distribution, and the evolution of management practices. The study is immense and detailed, containing a mountain of material of great value to students of early American economic development.

The major problem is that the dissertation lacks a central organizing focus. Lozier raises a number of topics or themes but the relationship among them is at best tenuous, and the evidence presented, extensive as it is, often does not fully convince the reader.

One such theme which dominates the first part of the study concerns the technological adaptation of Taunton manufacturers to the absence of fast flowing streams necessary to provide adequate water power. In Lowell, adequate power sources allowed for the creation of the well-known integrated factory system under central management, but in Taunton the smaller and more sluggish streams made the large factory impossible and, Lozier maintains, prevented centralized management. In order for the less efficient southern Massachusetts manufacturers to be able to compete, businessmen had to innovate, producing textile machinery that ran faster with less power. The smaller, innovative establishments at Taunton, Lozier argues, were more typical of early American manufacturing.

Lozier presents a great deal of evidence to support his argument, including a detailed discussion of the imaginative technological changes introduced to save power and speed production. Yet the evidence is not fully convincing. The Taunton textile manufacturers, it appears, did not really compete with Lowell. Rather they produced finer grades of cloth (in contrast to Lowell's concentration on the coarser grades) and, therefore, really competed with the English producers. In this competition it seems that they were less than successful. Also, while it is clear that inadequate power sources dictated the building of smaller shops and prevented putting all operations under a single roof as in Lowell, it is less clear why several shops could not come under centralized management and control. Indeed, the role played by Mills and Company -- considered in another context later in the dissertation -- seems to have been to provide that management control.

Another theme in Lozier's study is that of entrepreneurship, the ability of Taunton manufacturers to seek out the workers and mechanics with required skills, to pick up capital from a variety of sources, to make necessary changes in technology, design, and distribution in order to succeed under less than perfect conditions. Again the evidence of these efforts is presented in great detail, but, again, the argument is not fully convincing. There is much in the story he tells of failure, of poor judgment, and mistakes.

I do not wish to leave my discussion of this dissertation on a negative note. It tells us a great deal about early manufacturing enterprise in New England and for that reason is valuable. What it lacks is the editor's blue pencil to reduce the detail to manageable proportions and a tighter conceptual scheme to hold it all together.

Robert A. McGuire's dissertation is a study in econometric history, or, as it is sometimes called, clinometrics. Like so many works in this genre it exhibits a prodigious effort in the collection and manipulation of data. McGuire took 11 major crops and 6 kinds of livestock, collected annual data, 1866-1909, for income, price, and yield for each crop and numbers, and value per head on farms for each kind of livestock for each of the 48 states -- generating in the process some 1,800 time-series. He then used those data to construct a coefficient of random variability for income, price, and yield by crops and regions; this coefficient, as its name suggests, is designed to measure the random as opposed to systematic variability. Finally, he used the information generated to see to what extent it helped solve two historical problems: the reasons for the corn-cotton mix in the post-Civil-War South and the causes of late 19th century farmer unrest.

The corn-cotton mix controversy is a very small part of larger questions concerning the course of Southern economic development after the Civil War. Of late, this smaller part of the larger questions has claimed the attention of a number of cliometricians. The debate runs along the following lines. If the proportions of corn and cotton grown by Southern farmers after the Civil War were not economically rational, that is, more specifically, if Southern farmers grew too much cotton relative to corn (as measured by the prices and costs of each), thereby getting lower returns for the cotton (because of overproduction) and paying higher costs (because they had to buy the corn they could have grown), then the historian must seek causes beyond simple reaction to the market place to explain their actions. If, on the other hand, the farmers grew corn and cotton in a proportion that the market would dictate and if they adjusted the mix to market signals, then they were behaving in a rational manner, and there is no need to seek further explanations for their behavior.

McGuire's contribution to this controversy is to attempt to discover if the farmers' choice of crops was dependent upon the ralative uncertainty they would face in cotton and in corn. The greater the uncertainty the greater the unlikelihood they would concentrate on a particular crop. He presents his results very tentatively, but generally they tend to support the idea that farmers were not gambling on an uncertain crop when they grew cotton and corn in the proportions they did.

The problem here is not with McGuire's work as such but in the way in which the question has been formulated by some, a formulation McGuire accepts. The difficulty is that when the question is formulated so narrowly it fails to ask the bigger, and to my mind, more important question: Why was the choice of economic activity of so large a proportion of the population limited to labor-intensive, small-farm agriculture? In a word, what forced most Southerners into the narrow choice of growing cotton or corn? Data on prices, output, and variability by themselves cannot answer the larger question which is a political and social as well as an economic question.

The second historical problem considered by McGuire is the matter of farmer unrest. He seeks to determine to what degree this unrest was a function of random economic uncertainty. Again, McGuire is very tentative in his conclusions, but the general thrust of his argument is that uncertainty and the unrest were related. Despite the rhetoric that accompanied the unrest (which implies that farmers knew what was bothering them and what had to be done about it), McGuire's findings suggest that the farmers were in fact reacting to the uncertainties they faced. This is useful information potentially adding another dimension to interpretations of the farm protest movement. But finding correlations between uncertainty and unrest does not mean finding a cause and effect relationship.

This leads me to a general problem with the cliometric approach as it is sometimes used, a problem that cliometricians share with others who adopt an exclusively quantitative method. Finding that certain kinds of behavior are correlated with any given set of conditions does not mean that the conditions motivate the behavior; or, to put the matter more generally, motivation cannot be determined by results. To make that link requires other kinds of information.

Admittedly my comments on McGuire's work have been unfair; I have criticized him for not doing something other than what he did do. The value of his effort is that he has provided historians with a mass of new and potentially useful data. What he and others make of these data remains to be seen, but McGuire has made a significant contribution in providing them.