REMARKS BY THE DISCUSSANT

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These papers are all fascinating on one account or another, but I don't mind saying that the job of discussing them has very nearly driven me stir-crazy. Just pause a moment and do some figuring with me: let us assume that each of these splendid young bucks has put in an 8-hour day 25 days a month over two ten-month periods to produce the studies from which these papers are drawn. That figures out to 24,000 hours of work reported here in 36 pages. Thus each page represents no less than 666 2/3 hours of concentrated thinking. And I'm supposed to discuss them all in 15 minutes. I should have signed on as a magician instead of an historian!

However that may be, what do these six presentations have in common, if anything? Or, if not, is there any common measure that can be used for purposes of comparison? Well, presumably each of these theses is in the field of business history. But if this is so then we are propelled promptly back to what I call the basic question of our guild, namely: "What Is Business History?" And that basic question, by implication, poses others such as: What is the difference between economic and business history, if any? And if there isn't, then is there any difference or lack of compatibility between business history and economic analysis? Does business history include F-ratios and constructs? Are these tools of the new economy that should be brought duty-free into the domains of the business historian? Should the garden-variety mortal, used to communicating to unadorned English be able to understand this? "These factors determining the rate of growth of potential, or full employment, output are also shown to be relatively more important than the rate of growth of aggregate demand in determining the amount of retardation that takes place in the rate of growth of total output."

Well, now, despite my not-so-oblique digs at the deductive thinkers amongst us, I am not prepared to say, axiomatically and with the authority of white hairs on a bald pate, that any papers read today are not business history. In other words, I have no pat answer as to what business history is, although I had always thought on a more or less pragmatic basis that it has to do with discovering and interpreting the evolution of business policy and practice over a period of time. Now, such policy and practice may be that of a single individual, a firm or some other distinguishable group, or a whole sector of the business community. Put otherwise, I believe business history deals with the causes, nature, impact, and relationships of business decisions. And these decisions may be made by one or more entrepreneurs, by the public in a welfare state situation, or by government acting for the people. What these decisions have in common is a distinguishable and measurable effect on the business life of the area and era under consideration. I should perhaps add that to me, at least, business history cannot be divorced from the human element; that is, somewhere along the line the stuff of business history emerges from one or more human minds; it it not wholly made up of unseen inanimate forces or hypothetical forces which exist only in the imagination.

Now, if we look at these six papers in this light, what do we find? Mr. Parks has concerned himself with a group of state-sponsored enterprises, and has explained why sectional rivalry in Michigan, aggravated by the financial problems peculiar to that day, made it politically impossible to act with economic rationality. In other words, the decision-making process was not allowed to proceed under anything approaching normal conditions. Consequently the entire program-- in so far as it was conducted by the state-- was replaced when enough citizens became convinced that private enterprise could do a much better job. In the course of his research Mr. Parks has uncovered valuable data on population patterns, but he highlights, correctly

I think, what apparently was the reason in those early days for Michigan's preference for railroads over canals; the railroads, even in 1837, represented the low-cost mode, and such experience as there was in actual construction bore out this conclusion. Incidentally, Mr. Parks observes--and I quote: "The canal system suggested by Robert Fogel illustrates the limits of absurdity which can be reached by a high level of abstraction and minimal familiarity with environment."

This study, then, is a straight-forward account based on meticulous inductive research designed to illuminate the development of a particular group of enterprises over some 20 years. It is to be hoped that other serious scholars will undertake parallel studies on a state-wide basis so that we may have more grounds for comparison and, perhaps, regional generalizations.

Mr. Olmstead does a service in stressing the tremendous importance of mutual savings banks in the ante-bellum years, and particularly in New York City; the wonder is that there has been so little attention paid to them aside from the classic study by Davis and Payne. At any rate, Mr. Olmstead proposes to delve into the records of eight of New York City's oldest mutuals; what interests him--and us--are (1) the changing nature and operation of the banks as they grew, (2) their investment portfolios, and (3) the nature of their depositors. Today Mr. Olmstead has limited his detailed remarks to this last factor--the types of depositors. This subject has special value because it permits a more accurate appraisal of the banks' impact on increasing the supply of capital available. Here again, then, is a piece of inductive research, carried out for a specific place and period, and holding out the promise of significant findings.

In his study, Mr. Becker takes the wholesale jobbers of drugs and hardware and examines the impact on them of the fantastically growing industrialism of the 1870-1900 period. During that time the character of the trade was basically and fundamentally altered: American products replaced English imports, and the markets expanded throughout the nation. Basing his work on the trade press and association proceedings, Mr. Becker reaches the conclusion that the trade under scrutiny was characterized throughout by decreasing profitability, and he goes carefully to work to explain why. Overproduction, increasing costs, and vigorous intra-trade competition were only the more important reasons. The answer to this decreasing profitability seemed to lie in the formation of trade associations by wholesalers to stabilize prices and, not unimportantly, to make systematic studies of the industries. It seems to me that Mr. Becker's paper is a model of lucidity and that his findings are and will be useful for any student of the industrial revolution.

So far as I was concerned, Mr. Puth's paper on Negro life insurance firms opened for me an entirely new vista. Certainly I never realized that only in the field of life insurance has there been extensive large-scale activity by Negroes. Mr. Puth makes clear why: after about 1880 national firms simply would not insure Negroes partly because of their higher mortality rate, and partly because of plain discrimination. Thus Negro companies inevitably were formed to fill the gap; they have been particularly active in offering industrial insurance. Among their special problems has been the difficulty of attracting sufficient capital, but this now seems solved. More basic was the relatively rapid post-World War II growth of Negro firms and, since 1950, the much slower growth as a result of the fact that national-market firms are now increasingly competing for the Negro market. Mr. Puth outlines some measures that one Negro company--the Supreme Life--and others have taken to reduce costs. But he feels that whereas the Negro buyer of insurance has been benefited, the outlook for Negro firms is less encouraging. He concludes with the observation that it would be ironic indeed if Negro firms were to decline precisely because of improvement of the income and of the longevity of Negroes! Here, then, is a refreshingly new subject, clearly presented, and significant not only intrinsically but for the contrasting light it provides on the insurance business as a whole.

As suggested earlier, I was, frankly, puzzled by a part of Mr. Nicholls' paper on Indianapolis. But perhaps I shouldn't be because he gives fair warning on his opening page that he proposes to--and I think does--omit "even the most entertaining historical details . . . to emphasize the empirical findings." Well, as nearly as I can figure, Lösch has developed a rigidly deductive theory to account for the spatial nature and location of economic activity. What Lösch expected to find and what Mr. Nicholls has found out about Indianapolis in 1880 confirms what, to my untutored mind, you would expect to find on the basis of common sense. Mr. Nicholls has studied 124 industries in 75 townships, and classified these industries into 15 "ubiquitous", 51 "unique", and 58 "intermediate," according to the frequency with which they appear. As I do believe one might expect, such enterprises as lumber-sawing, flour and gristmilling, and blacksmithing might be and were in virtually all townships, while the more specialized undertakings were only in the larger towns and, of course, Indianapolis.

It is indeed fascinating and enlightening to be guided by Mr. Nicholls through the museum of statistical permutations, and one can hardly agrue with the four tentative hierarchial levels he spells out for the Indianapolis region. But when he turns to conduct what he calls "formal tests," I simply lose contact. Now let me be explicit: I am certainly not the untimate authority on what business history is or should be, but I would register an earnest plea that statistical analysis--a sort of economist's esperanto--should be translated for us ordinary mortals. I simply don't know what it means to say: "In six instances, the F-rations were 'unambiguously' significant at a 95 percent confidence level." Hence with as much grace as I can muster in this embarrassing situation, I leave comment on this paper to someone far better equipped than I am.

Mr. Neal's piece on the "Growth, Stability, and Financial Innovation in the American Economy, 1897-1914" is, I feel, more straight-forward, but again it strikes me more as a specialized analysis of certain given economic phenomena than a history of trends, decisions, or changes in the economy. To start with Mr. Neal says he wants to explain the "conjuncture" of "a retardation in the rate of growth of aggregate output after 1907, a fall in the ratio of gross capital formation to gross national product after 1907, and a surge in external financing of gross property additions by firms in the manufacturing and railroad sectors followed by a sharp shift after 1907 to internal financing." He must consider too five business cycles, the crest of the merger movement, and the flood of new products reaching the markets.

I have read and re-read this paper; I have underlined it. But is beyond me, and it is only as an act of faith that I want to believe that Mr. Neal's conclusion, "reached from these successive levels of hypothesis testing is that the concept of financial innovation provides an extremely useful tool for understanding the vagaries of the American economy in this one period of time." This may well be so, even though for the life of me I don't see why. And I'm frank to say that this may be counted a black mark against me and not against the aspiring author of the piece!

This much certainly can be said of all these papers: they represent vigorous work, warming enthusiasm, and the new sharp cutting edge of genuine intellectual curiosity. I congratulate you, one and all. Now, whether all these papers can properly be considered business history is less clear; I hope that those of you who will now make individual comments will enlighten me and all of us on this point.