Commentary on Dissertations

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There is an aura about sessions such as this which suggests analogies to a fraternity initiation. Given that, one of the first questions the discussant must ask himself is where his discussion stands in relation to "Hell Week." All of us, I believe, would agree that "Hell Week" is the responsibility of the pledge trainer, or dissertation adviser in this case. If "Hell Week" is over, is the discussant reduced to saying something like "welcome to the fraternity"? Certainly that is one function of this session, but then does critical discussion become criticism of the student or his adviser? It has been customary to enter a caveat at the outset of these sessions that the present discussion should be construed as "professional criticism," that is, the intellectual umbilical cord between adviser and student has been severed. The new professional stands alone. It should not be forgotton, especially by those who have just presented their masterpieces, or rather doctorpieces, that they are the cream of this year's pledges. A second caveat stems from the fact that we members have only heard the tip of the iceberg. What I have been given to prepare my discussion, and what you have just heard, is the masterplots version of the original. Thus, my discussion may reflect a "fallacy of decomposition," and each of our initiates today must keep that in mind as I fulfill my role in the last stage of their initiation. Don Kemmerer, in his letter to each participant, suggested that a good presentation might serve them well in the job market. This suggests that I am also to act as a quasi-fleshmerchant. As you have heard, each of these gentlemen is possessed of a sound mind and has sunk his teeth into some interesting material. I suggest to you that, although extractions will not be necessary, "look Ma, no cavities" would not be an accurate diagnosis.

Mr. Winsor comes to us as a geographer. I suspect the construction he put on his presentation for our group is different from that of the complete dissertation, but I can only extrapolate from this biased sample. The cavities I see here are mostly the unanswered questions which an economic historian would ask, and I am sure they are different from those asked by geographers. He has convinced me that drainage was both necessary and beneficial and that the events he has singled out for special attention were significant.

Yet I am surprised that as a geographer he limited himself to a single state. The current urban and environmental literature stresses that state boundaries are geographical and historical accidents that should not play a crucial role in analysis. Surely what geographers have called the Central Drift Plain continues into Indiana. The history Mr. Winsor presents to us is true of Indiana as well; the wetness of the prairies near the Kankakee River in Indiana was noted as early as 1837 [21]. The reluctance to adopt drainage and the reasons for it were similar in both states. I wonder what Mr. Winsor's indexes of diffusion would have suggested had he included all the Central Drift Plain.

It appears from his presentation that the indexes were calculated between 1876 and 1884, a period which encompasses the events he explicitly discusses. Certainly the 1850s and 1860s are too early to consider this area. As we are all aware, the initial settlement pattern of both Indiana and Illinois was to the south along the Ohio River Valley and many of these settlers adopted extensive agricultural techniques. It was not until the 1850s that the migration pattern changed and farmers adopted intensive agricultural techniques. Before then the wet prairies could be avoided, and were. Yet this change in the migration patterns may be only part of the story. In a general equilibrium sense, economists would look at such factors as whether increased relative grain prices would bring into production land such as this beyond the extensive margin.

We are told that one of the greatest hindrances to the adoption of drainage was its cost, especially the distance and transport. Mr. Winsor does not tell us what is involved here. Drain tiles were known in Europe and were used in New York and other eastern states prior to 1850 (see [1]). In that year a flatboat cargo of tile was produced in West Virginia, carried to Cincinnati, then transferred to a canal boat, and taken to Indiana [21, p. 382]. Later the railroads entered the shipment process reducing the time of transport, and presumably the breakage, but Mr. Winsor makes reference to high freight rates on tile as a factor inhibiting their adoption. What is clear is that the price of tile was prohibitively high in these years, but was it the result of the freight rates or the distance? Tiles were adopted when the factory moved closer to the consumer, so distance must have been an important element in price, but we are also told that in the late 1870s the railroads reduced their rates on tile and this encouraged adoption. I would argue there are two effects here which Mr. Winsor should identify, and they are presently combined into one.

A second crucial element in his analysis is the drainage laws passed by Illinois in 1879. He mentions two laws but apparently has elected to present their combined effects. I am completely mystified about the precise role these laws played. Apparently the principle of natural drainage forced owners of lower property to accept only the natural drainage from higher ground, but not the collected, concentrated drainage generated by tiles. If this is true, the owners of lower ground could force owners of higher ground to refrain from adopting tiles, but the latter group would be the one less likely to adopt tiles. I can see how giving a drainage district the right of eminent domain would improve the routing of drainage to natural water courses and why it might be necessary to exercise this right; however, in this case the unwilling landowners should be the ones on higher ground, and also on a drainage divide, who do not want to give up some of their land to improve a neighbor's drainage. Mr. Winsor needs to make the connection between these laws and the adoption of tile much clearer.

One problem to which Mr. Winsor did not address himself is the relationship between tile manufacture and the size distribution of firms. The necessary raw materials include clay or shale to furnish the base and a fuel for the burning process. Clay was available in the glacial deposits of both Illinois and Indiana. Shale occurred at or near the surface in the southwestern part of Indiana and southeastern part of Illinois. The Illinois coal mines, and some in Indiana, provided one source of fuel, but W. LeRoy Perkins in a 1931 study of the drain tile industry in Indiana reported that 17 of 24 plants in Indiana used West Virginia or Kentucky coal [21, p. 385]. Parenthetically, it should be noted that the largest factory in Indiana, the National Drain Tile Company in Terre Haute, used local coal and sold a large share of its output in the Illinois market. Since the skill level in the production of drain tile is not high, the transport of coal, hence transportation costs, appears to have been a significant share of total cost. This would suggest the average firm would be relatively large, yet Mr. Winsor's analysis suggests a large number of small firms. It is precisely this question of returns to scale that needs to be answered. The preponderance of small firms apparently resulted from the utilization of local resources and from a location near the retail market. Reduced transport costs, as were obtained from the railroads, should have been a greater benefit to larger firms and might have led to some concentration of production. Since the evidence Mr. Winsor offers is in terms of factories, we really know nothing about firms.

Mr. Winsor's summary model is a simple supply and demand structure which is somewhat confused and in need of simplification. Both the wet years of 1875-78 and the 1879 drainage laws can be thought of as shifting the demand for tile rightward as he indicates. It is easier to think of the reduced freight rates as a supply effect; these would have to be viewed as something like a tax paid by consumers to call them demand phenomena. These three effects form the first step in his model and they predict an increase in the equilibrium quantity of tiles. It does not explain why additional tile factories were constructed nor does it explain the reason for the location of the original factories. Given that more factories were constructed, this would shift the supply curve rightward resulting in reduced tile prices, an effect which economists would see as part of the long-run adjustment. This reduced price will cause a movement along the demand curve causing more tile to be adopted. There is no feedback effect from the sequential introduction of the three effects Mr. Winsor lists at the top of his schematic diagram; such a feedback would cause an increased demand for tile, not necessarily additional tile factories. Nonetheless, Mr. Winsor has provided us with some insight into this industry. One conclusion of his work is certainly consistent with that of the earlier work on Indiana; demand and supply must be developed concurrently.

While the subject of drain tiles should not be a totally foreign concept to business historians, it is a relatively obscure one. Thus, it came as somewhat of a surprise when drain tiles played a role in a second dissertation, Mr. Dickman's assessment of James J. Hill's contributions to agricultural development. Mr. Dickman's dissertation represents the result of archival work in the recently available Hill papers, and it should be considered in the same vein as Paul Gates's work on the Illinois Central. As a result of this work, Mr. Dickman describes James J. Hill as a leader in the field; a man whose innovations were copied by others. Mr. Dickman offers three examples to support his view.

There is some reason to question Mr. Dickman's stress on Hill's promotion of product diversification. A shift from wheat monoculture to mixed farming and dairying was not unique to this area. In many areas, wheat production continually shifted toward the frontier as the land was bid away by animal farming due to rising urban demand [11].

Further, Hill imported pedigreed bulls to act as cooperative herd sires in an attempt to diversify production and distributed these bulls at no charge to farmers along the line of his railroad. Mr. Dickman refers to this as an ingenious device, presaging modern practice. What initially appears to be significant is the importation of bulls but a quick glance at several standard sources suggests that this technique was consistent with long-standing practices of selective breeding (for example, [28, 9, 23, and 14]). This cannot be the ingenious device but the free distribution of them could be.

Irrigation and drainage, examples of Hill's interest in both produce diversification and soil conservation, are not new techniques either and, as discussed by Mr. Winsor, the railroads of Illinois were persuaded it was in their interest to help promote the drain tile industry. Hill's recognition of the need for drainage and irrigation shows he was a careful student of history. His railroad's involvement in these pursuits, as well as the soil testing and extension service, may well be unique but Mr. Dickman did not address himself to that in his short remarks. In fact he added a footnote which works to minimize the impact of this point by saying that, in any event, Hill's program was the first of its kind in Minnesota.

Mr. Dickman's third example stresses the link between the railroad and the formation of the United States Reclamation Service. Many speakers to this group have expressed their opinion that more work is needed of this type which develops the link between private industry and the government. This is an excellent find, and one for which his archival investment returned a handsome dividend.

Yet the totality of Mr. Dickman's paper left me with a hollow feeling. Where was the hidden cavity in this polished piece? Who was Hill's Hercules? Was there one? This speculation led nowhere. The cavity is simply the lack of perspective. Hill was important, but how important? Hill was unique, but how unique? Outside the US Reclamation Service connection, I simply am not confident Mr. Dickman has made his case. In the end, I find myself commenting on this effort what James Baughman concluded of Albro Martin's *Enterprise Denied,* "To this member of the audience his evidence still remains circumstantial and inferential... but one must admire the diligence and skill with which [he] builds his case" [3].

We next turn to Mr. Pratt's careful case study of the impact of refining growth of the Gulf Coast from Corpus Christi to New Orleans. Mr. Pratt is no stranger to this group; many of you will recall the paper he presented at the Wilmington meetings [22]. That paper was a case study of the political involvements of Standard Oil. On that occasion he attempted to integrate political factors into a theory of the growth of a firm. In his present case study, Mr. Pratt attempts to integrate these factors into a theory of the growth of a region. In both cases the same set of books form the economic core of his conceptual model. The earlier paper owed a relatively larger debt to the ideas of Edith Penrose and Alfred Chandler, but the current study owes a larger debt to Robert Averitt. Although Averitt acknowledges his intellectual debt to Penrose and Chandler, they are not easily comparable. Both Penrose and Chandler addressed themselves to forces within the firm. Pratt's earlier paper adds the political dimension, but does not change the focus. On the other hand, Averitt is concerned with a more aggregated problem; the stress is on industrial structure as opposed to an industry, much less a firm. He is operating in that ill-defined world where microeconomics and macroeconomics merge. Mr. Pratt's case study is an application of Averitt's ideas to the Gulf Coast.

The central ideas of his study come from concepts in the economic development literature. In that literature, a "dual economy" usually refers to a capital-using industry occurring beside subsistence agriculture; this is the Fei and Ranis definition. In Averitt the distinction is between the center economy with small, competitive industries. The second idea from the development literature is that of linkage effects, usually attributed to Albert Hirschman. These are the two main tools of Mr. Pratt's analysis, both from development, and both fraught with ambiguities.

Mr. Pratt's case study fits neatly into Averitt's model, but it does not test Averitt. The weakest chapter appears to be that on the refinery and the environment. In his synopsis, Mr. Pratt states that "it is clear that the sustained expansion of refining has resulted in the parallel growth of pollutants." He has made an honest attempt to establish these elusive linkages. Yet like many others, he begins to search for reasons why pollution and waste problems were not attacked earlier. His answers provide only part of the story. It cannot be overemphasized that, when pollution is considered in the context of economic growth, it too is a compound growth rate problem. One important feature of these problems is that the size of the increment increases absolutely over time. Thus very small absolute increases initially can become gigantic, if the system continues to grow at the same rate. A comparison of American industry in 1976 with what it was in 1940 provides an example of the care that must be taken in the kind of backward extrapolations Mr. Pratt is attempting. It also helps to explain why the public's perception of pollution is so recent. In my opinion, a comparison of social costs and benefits with private costs and benefits, at several benchmark time periods, would be a more satisfying approach to evaluating the environmental impact of the refineries in a context of growth.

We are clearly dealing with growth, since Mr. Pratt gives as one reason for the failure to recognize the environment danger "the societal faith in sustained growth as the essential economic priority." One person who has such a belief in the efficacy of growth is Robert Averitt, "The U.S. economy's most prominent post-war deficiency, and its greatest hope for abundance, lies in the areas of full employment and growth" [2]. Averitt saw his book as the microeconomics of the "new economics." Yet consider the message. When the book appeared in 1968 the "new" economics had been defeated by the "old" politics, to borrow Arthur Okun's phrase. Today, through post-Vietnam (and post-Watergate) eyes, Averitt's distinction between the center and periphery economies appears to be competitive with the ideas of a leading sector stressed by Walter Rostow in his "non-communist manifesto" and the ideas popularized by John Kenneth Galbraith for the last generation. In relation to the economic development literature, Averitt's center economy is a less well-developed theoretical idea than the economic base of Homer Hoyt or the economic pole of Francois Perroux, both of which have had a controversial reception [8].

The critical cavity of Mr. Pratt's dissertation is that he adopted the Averitt model without question.[1] Consequently, Averitt's conclusions become Pratt's conclusions; Averitt's weaknesses become Pratt's weaknesses. I am disappointed that Mr. Pratt did not attempt, in some sense, to test Averitt's model. At its heart, Averitt's growth scenario is export-led, unbalanced growth, the same scenario which emerges from all the theories mentioned above. In the end, that is really all Mr. Pratt has documented economically for the Gulf Coast region.

Mr. Collins turns our attention to macroeconomics. He is concerned with how three business organizations, the US Chamber of Commerce, the Committee for Economic Development, and the National Association of Manufacturers, contributed to the definition of modern American political economy. His study makes three points. First, there was a fiscal revolution in America over the years 1929-64, but this point is little more than a reiteration of the title of Herbert Stein's book [24]. Mr. Collins, however, disagrees with Stein at several junctures. Second, the definition of what constituted Keynesianism changed over these years. Though the changes he describes did occur, it is too simplistic to describe the principal change as a distinction between the secular stagnationists and the new economics. In particular, I believe he has failed to capture the correct spirit of Milton Friedman's familiar quotation. Third, the three business organizations played a critical role in the new economics of the 1960s. Stein also documents a role for each of the three organizations but he left lots of room for amplification of their role.

Prior to Keynes, the general acceptance of laissez-faire doctrines meant that economists were not inclined toward interference in the macroeconomy. As the depression worsened, it became more difficult for economists and businessmen to argue that laissez-faire would lead to recovery. Although the Roosevelt administration showed a willingness to deviate from laissez-faire, its actions, outside the 1933 decision to devalue the dollar, bore little resemblance to formal economics, either laissez-faire or Keynesian. Walter Heller hailed the publication of Keynes's theory in 1936 as "a spectacular rescue of economics from the wilderness of classical equilibrium which had assumed away the critical issues of employment and income and their determinants" [12] (see also [27]). Yet Roosevelt was simply not impressed with Keynes or his economics, as Mr. Collins notes. According to Mr. Collins, however, FDR's decision in April 1938, to engage in deficit financing "represented the President's first acceptance of fiscal policy as a legitimate tool for economic stabilization." Stein's interpretation is somewhat different:

> The big fiscal decision made in Washington between the publication of the *General Theory* and [World War II] was the decision to embark upon a spending program in the spring of 1938. The assimulation of the *General Theory* into American thinking and the formation of a Keynesian school came too late to be of much influence in that decision. Of course, there was in this period a considerable group of "spenders" in Washington.... Their ideas were of pre-Keynesian origin and did not respond quickly, if ever, to what was new in the *General Theory* [24, p. 165] (see also [19]).

Today many are inclined to call the spenders "Keynesians," so the difference in emphasis is not all that crucial to Mr. Collins's story. What is important is that he has changed the date at which Keynes's theory became acceptable and in doing so has put much less emphasis on the role of World War II.

Mr. Collins ignores the role of the war in his short paper. It is an unfortunate omission which puts too much emphasis on the 1938 response to the "Roosevelt recession." Consider E. Cary Brown's reappraisal of fiscal policy of the 1930s.

> ....it took the massive expenditures forced on the nation by the second World War to realize the full potentialities of fiscal policy. Until then, the record fails to show its effective use as a recovery measure. Indeed the general expansionary policy seems stronger in the early part than in the later part of the decade [4, p. 869].

Since Stein gives the same emphasis to these events, Mr. Collins needs to explain his emphasis on 1938.

Mr. Collins then takes his story to the Employment Act of 1946, but only briefly. Later he reminds us that this bill began its legislative life as a "Full Employment Act" and remarks that organized business played a significant role in exercising the word "full." His brief stop in the Eisenhower administration is to make the point that "the lessons of Keynesian economics had been internalized"; they accepted "the passive side of the New Economics." There is some danger of misinterpreting this remark. It would be incorrect to assume that the passage of the Employment Act introduced a new American fiscal policy; the act merely confirmed a policy that existed throughout the war. The act was, in part, an affirmation that the government would do everything to prevent another depression and most knowledgeable sources were predicting a postwar depression. Furthermore, the Council of Economic Advisers created by the act was not very influential during its early years. Both Truman and Eisenhower held conservative economic philosophies and neither was receptive to abstract ideas. The Truman chairmen of the council, E. G. Nourse and Leon Keyserling, were more concerned with reforming the structure of the economy than in using fiscal policy to stabilize output and employment. Eisenhower's chairmen, Arthur Burns and Raymond Saulnier, were not devoted Keynesians but they did recognize "that the economy would not stabilize itself, that the government could and should act to reduce instability.... and that the fiscal instrument was one of the most powerful the government could use for that purpose" [24, p. 294]. All four were aware that there were factors other than fiscal policy which influenced the performance of the economy. Yet Mr. Collins essentially "tells it like it was." The postwar problem the Truman administration unexpectedly faced was inflation, not depression. The Eisenhower administration was one in which the council played a much more active role, but their use of fiscal policy was described as a "fire-fighting strategy." As Arthur Okun viewed it, "because the deliberate application of fiscal policy was widely viewed as an emergency measure, there were considerable inhibitions about initiating a policy program" [20, p. 37]. This is what Mr. Collins has called "the passive side of the New Economics."

To Mr. Collins, the tax cut of 1964 was the culmination of the Keynesian revolution; to Stein, it was the culmination of the fiscal revolution. The difference is significant. The use of a discretionary fiscal policy to prevent an inflation must be viewed as a triumph of an activist approach as opposed to the passive approach typified by the earlier administrations and by Kennedy's prior to 1962. According to Walter Heller, this shift was made possible "by steady advances in fact gathering, forecasting techniques, and business practice" [20]. It should be noted that there is no mention of a distinction between Keynesians and non-Keynesians because by 1960 those earlier distinctions had been muddled. All parties had come to accept the notion that changes in both fiscal and monetary variables could affect GNP.

Milton Friedman's renowned quotation is the groundwork for Mr. Collins's second point, that the definition of Keynesianism had changed over the years. The problem is that one never knows whether to take the term Keynesianism in the context of the theory or the policy. The first time Mr. Collins makes this point he explicitly says policy; the second time, he says Keynesianism. What Friedman claimed he meant by his statement is that all economists begin the theory from the Hicks-Hansen IS-LM approach, and then go their separate directions with respect to policy.[2] Several economists have denied the premise is true, and some refer to the progenitors of the IS-LM system as "bastard Keynesians." Alvin Hansen is the most famous stagnationist but the epithet would indicate the definition of Keynesianism changed before the 1930s ended.[3] I cannot accept Mr. Collins's implication that Friedman was drawing a distinction between the secular stagnationists and the New Economics; too much happened within the profession between the time each of these schools was prominent.

Certainly Mr. Collins is correct that the first American Keynesians were stagnationists, and their policy conclusions favored an increase in public expenditures. Their conclusions agreed with the aforementioned "spenders" even if their theory did not. The changes Mr. Collins discusses came to pass, but is the end product Keynesian? This conundrum is the legacy of Friedman's quotation. Both theory and policy have changed over the years. Even if theory had been stagnant, changed economic circumstances would have led to changed policy conclusions. If Keynesianism refers only to policy, we may be closing our historical eyes to significant changes.

Mr. Collins attempts to resolve part of this problem by looking at the role of three business organizations. The role he ascribes to the CED is identical to that of Stein. Both scholars accord significant positions to Beardsley Ruml and Thomas B. McCabe. Mr. Collins provides more information on the Chamber of Commerce and NAM, at least in relation to more recent history. One wishes, however, that Mr. Collins had dwelt more on the differences between himself and Stein.

The theme is present in Stein that organized business adapted to the Keynesian revolution, but Mr. Collins makes it explicit. One only hopes that in his dissertation Mr. Collins did document the contributions of the Chamber of Commerce of the US, the CED, and the NAM. The major cavity is simply that from his short piece it is difficult to find much that Mr. Collins has added to Stein. Apparently his emphasis on the role of the business community would cause him to disagree with Stein's assertion that "a large part of the distance between 1932 and 1964 had been traveled by 1933" (see [24, p 54]).

Our last presenter is Mr. Sass, who discussed the intellectual origins and proceedings of what became the Entrepreneurial Research Center. It was not clear to me what Mr. Sass hoped to accomplish in his short presentation. The facts of the center and the personalities involved are well known to this group (for example, see [26]). I think it best to assume Mr. Sass was writing intellectual history, and in this connection I have but one comment to make. Where is Karl Marx? The German historical school? Surely both are lurking just behind many of Mr. Sass's comments about such things as the search for a unifying theme to research and the use of theory in economic history.

If this is not an intellectual history, then it becomes a methodological treatise. Yet here is the great cavity of Mr. Sass's dissertation. Though he has a good grasp on the methodolog of entrepreneurial history, he shows little understanding of the methodology of the new economic history. What begins in the spirit of intellectual history ends as a lament for the old economic history. Whether this be intellectual history or methodology, I cannot accept Mr. Sass's conclusions.

Entrepreneurial history and its cousin business history clearly are not in decline as he alleges. The growth rate may not have been as rapid as that of the new economic history, but it is positive and respectably large. Only the center is gone. In the past few years there have been several studies which are clearly entrepreneurial history. Jonathan Hughes's *The Vital Few* [13], Paul Uselding's study of Henry Burden [25], and my own study of Ellis Sylvester Chesbrough [5] come to mind immediately,[4] as well as the papers of Donald Paterson, Ralph Gray, and Stephen Salsbury from our Wilmington meetings [16]. I should also mention the papers John Harris, Paul Uselding, and Ralph Andreano presented to this group at our Oberlin meetings.[5]

Mr. Sass offers three reasons why he believes entrepreneurial history declined. One is that "entrepreneurial research was so foreign to traditional economic historians that it was pursued only sporadically outside the research center." Given the schism between "entrepreneurial" and "traditional," the people I have listed would probably call themselves "traditional." Further, many of them are clearly members of that heretical band of traditionalists called "new." When the center closed and its members left Harvard, the techniques diffused, but not until then. Mr. Sass needs to cast his net within Harvard as well as without.

His second reason is that after 1950 the center lacked the resources to draw graduate students. On this point let me note parenthetically that it would be interesting to read a business history on the business of business and entrepreneurial history and, in particular, how business and entrepreneurial historians are recruited. Can it be true that an academic subdiscipline must offer competitive monetary bribes to attract students? Whatever the case, the fact remains that economic history graduate students at Harvard sought alternative avenues than entrepreneurial history.

Mr. Sass's third reason is that the link between entrepreneurial history and sociology was weak, as are most interdisciplinary ties. As discussed by Harold Williamson, the diversity of approaches collected in the center "had the effect of broadening the concept of the entrepreneur well beyond the heroic figure set forth by Schumpeter and his classic model" [26, p. 9]. It also made the study of entrepreneurship so broad that a single general theory of the entrepreneur was impossible. Thus one weakness of entrepreneurial history was that it was as diverse as the other specialties in economic history. Without a single conceptual model, it could never become a "focus" for all work in economic history. The problem was not that the link to sociology was weak but rather that interdisciplinary studies are only as strong as the weakest discipline. Since the Schumpeterian tradition has proved stronger than the Weber-Parsons tradition, a case can be made that it was not the link, but sociology itself that was weak. There is still no generally accepted theoretical core in sociology as there is in economics and, unlike economists, sociologists often give the impression of not knowing where they disagree. This central theoretical core is one of the advantages of economics and the new economic history. In sum, entrepreneuria history did not decline but like the old economic history, it was transformed.

Mr. Sass appears to believe that the new economic history is exclusively econometric, and I believe this is a fundamental misunderstanding in his analysis. The 1920s and early 1930s saw the publication of the superb collection of topical economic histories produced under the aegis of the Carnegie Institution. Two decades later the mainstream of the profession reworked those books on a chronological basis and produced the equally valuable Holt, Rinehart and Winston series. These two massive series represented the collected wisdom of the profession. Three avenues were open to enterprising new scholars. First, they could continue to mine the available stock in the traditional manner for new insights from a permutation of the facts. Second, they could mine archival sources for new information, and I would say this is where business and entrepreneurial history made its contribution during the time period Mr. Sass investigates. Third, they could return to the data sources and, through many painstaking hours, do the calculations required to generate new data sources. This, more than econometrics, is the new economic history.

There is a second element to the new economic history which must be explicitly mentioned. Throughout a long methodological literature which emerged with the new economic history one point was stressed, the need to properly specify what is being done. In part, this is a result of the econometrics but in larger part it is the result of two important factors. The first is that new economic historians utilized the computer for data manipulation and provided new evidence concerning old questions. They felt a need to explain the processes they used to generate this new data. The second is that there has been a move among most social science to the methodology of the natural sciences and, particularly, to hypothesis testing. This trend has meant that all work in economic history, whether new or old, is expected to specify its underlying hypotheses, whether they will be tested econometrically heuristically, or not at all.

In actuality there is nothing new about the new economic history. Simon Kuznets and the founders of the NBER all could lay claim to the title of new economic historian. It is more a question of method than of technique. Mr. Sass is correct when he states that the new economic history became ascendant in the latter half of the 1950s but he is wrong when he claims the new economic history was the historical application of econometrics. It is the converse which is true; the historical application of econometrics is part of the new economic history. Econometrics is but one technique. By focusing on that technique, Mr. Sass has lost sight of the method.

As noted, specification is an integral part of the method. It is an outgrowth of the use of the computer, the godfather of the new economic history. The computer enabled economic historians to tackle problems which were not practical in its absence. Thus among the first work attempted by new economic historians was a thorough search of the manuscript censuses, but the initial questions they asked were generated by the old economic history. In this sense the computer breathed new life into the discipline. The possibilities for new research opened by the computer but still guided by the concerns of the old economic history were immense. This electronic gadget was the greatest threat to entrepreneurial history, and probably business history as well. Yet the computer was not the only significant development of the postwar years.

Concurrent with the computer, mathematical economics developed rapidly. Mathematical economists developed both macroeconomic and microeconomic models which gave to the subject the air of a natural science. Econometrics developed to estimate the behavioral parameters of these models and to forecast the future. Economic historians trained in economics departments were quick to see the usefulness of these techniques to the study of many longstanding questions. It was not so much that "tortuous interdisciplinary ties were avoided," as Mr. Sass maintains, as that they were not necessary to handle the questions answerable by these techniques. Further, Mr. Sass alleges "the sociological research of entrepreneurial historians was viewed with increasing scorn by other members of these economic departments." He offers no documentation for this point and, although I will agree that economists and sociologists often do not hold the opposite discipline in high esteem, I can only say from my own experience that I believe his statement to be an exaggeration. One final exaggeration is Mr. Sass's comment that "the contributions of the new economic history flowed readily into the decision-making process, especially that concerning growth." While the new economic history is often concerned with growth and development, so was the old. The relationship between economic history and growth policy is unclear at best. Certainly the results cannot be considered a success.

It is fair to say that the new economic history dealt entrepreneurial history a death blow? No. As I discussed earlier, it is still very much alive and well and even being practiced by new economic historians. Why then did it fall behind the new economic history? As I have tried to suggest, part of it was internal, entrepreneurial historians failed to develop an operational theoretical model, and part was external, the computer was not as practicable a research tool for entrepreneurial historians as it was for new economic historians. As Hal Williamson noted, that group of economic historians associated with the Entrepreneurial Research Center should take note that conscientious scholars could not "afford to ignore the roles that entrepreneurs, however defined, have played at all levels in business and economic activity from the individual firm to the entire economy" [26, p. 9]. This perspective does not appear in Mr. Sass's presentation and so I have chosen to characterize his conclusion as a lamentation.

In conclusion, the methodology of the new economic history offers something to all economic historians regardless of their persuasion. That is, by being explicit about hypotheses and methods, by completely specifying what is being done at each step, others can more easily evaluate and appreciate an author's work, and the author gains a clearer perspective of his own project. One general comment about all the papers you have heard today is that they would have benefited from the use of this methodology. I do not mean to say that the elements were uniformly lacking; they were not. To take but one example, Mr. Pratt, whose dissertation comes closest to this method, was explicit about the model he used, but he really did not enumerate the specific questions in which he was most interested. What is missing in these dissertations is perspective, and that is a cavity which can be filled by further professional growth, maturity, and a lot of reading. It is a cavity which will never be filled because, like the microeconomist's long run, it is a will-o'-the-wisp we chase but never catch. In spite of the impression I may have given from my comments, all our participants have demonstrated an auspicious start. So as I say "welcome to the profession," I also say "welcome to the chase." The fraternity expects you to participate; the interfraternity council insists on it.

NOTES

1. It should be noted that some of the same problems are discussed in [7]. Two recent books [18 and 17] might provide ideas for alternative hypotheses.

2. In the introduction to *Dollars and Deficits* [10, p. 15], Friedman says, "We all use the Keynesian language and apparatus; none of us any longer accepts the initial Keynesian conclusions." This is also discussed in [29, pp. 9-10].

3. There is reason to believe Keynes himself changed his mind about some of the elements in the *General Theory*. See [15] and his replies to articles about his consumption function in the *Quarterly Journal of Economics* during 1938.

4. Henry Burden was interesting to Paul Uselding because his experience explained much about how technology was transmitted internationally. Chesbrough was interesting to me because his experience explained much about how urban engineering practices developed. Neither of us saw a need to investigate socio-psycho-political variables or to attempt econometric tests to establish the significance of these entrepreneurs. In this, the technique we used was consistent with that used at the center, but the motivation for the questions we asked, and the questions themselves,

were different.

5. These papers appear in [6]. All three articles make use of statistical techniques. In particular, Ralph Andreano explicitly tested the Horatio Alger legend, as it was formulated at the center, and found it wanting. In large part the difference between Andreano and the center was a difference of specification. Although the articles in this note and in the preceding note do not explain the changes in entrepreneurial history, they do illustrate them.

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