Discussion

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All three of the dissertations assigned to me for comments deal with significant problems and are well written and well researched. I will not attempt the impossible task of examining them in depth in the short time alloted to me. I shall somewhat arbitrarily pick a few points which struck me as merting comment, and I hope the authors will forgive me for concentrating on criticism rather than on these aspects which deserve praise.

There is little one can say of a general nature on studies that are so different in their approaches and in their objectives. The studies range from the straighforward historical study of government policy toward business by Kovaleff, to a heavily quantitative study by Prendergast, to a theoretically oriented historical study by Anderson. The dissertations by Anderson and Pendergast clearly reflect the new orientations in economic history, but they are not entirely successful in their attempts to combine traditional historical with modern analytical skills. In both instances the statistical analysis and the theoretical problem formulations could be improved. Some arm waving about economic theory could be profitably eliminated.

The main thrust of Mr. Kovaleff's dissertation is to show that the Eisenhower administration carried out the most energetic anti-trust policy in American history, in spite of its pro-business sympathies and its extensive support from the business community.

Kovaleff is very successful in demonstrating that this was a sustained and effective policy. The question of the underlying objectives of the vigorous Eisenhower anti-trust policy thus becomes of paramount importance. I am sure that there are some historians, with a penchant for conspiracy theories, who would be inclined to interpret this anti-trust activity as a kind of window dressing, a screen to hide the tremendous, and perhaps growing, concentration of power in the American economy and the emerging military-industrial complex. Mr. Kovaleff has carefully probed into the administration's anti-trust philosophy and concludes that the Eisenhower administration accepted the concept of anti-trust as a means of achieving the country's goal of "economic freedom in a democratic society." In other words, the Eisenhower administration was motivated by a deep faith that the old fashioned remedies, which were shaped in the late 19th and early twentieth centuries, were effective in dealing with the industrial organization problems of the second half of the twentieth century. I believe that Mr. Kovaleff's interpretation of the Eisenhower anti-trust fervor is correct. I wish only that Mr. Kovaleff had explored a bit more in depth the implications of his interpretation. What is the meaning of "economic freedom in a democratic society," and how relevant is enforcement of the anti-trust laws to its achievement? An attempt to answer these questions would put anti-trust policy in the broader context of social and economic policy. The dissertation stresses that administration officials were interested in practical results rather than doctrinal precedents, but they seem nevertheless to have had a legalistic view of the role of anti-trust measures. Kovaleff easily reconciles the Eisenhower anti-trust policy with a pro-business bias. The author might have noted that this reconciliation puts very narrow limits on the Eisenhower concept of economic freedom, especially from the perspective of the 1960's.

Mr. Prendergast's dissertation has two major components. One is a statistical analysis of the performance of fifteen firms over a twenty year period and the other is an historical narrative of the origins and development of ten of these firms. The author emphasizes that he is using a comparative historical approach, which is novel in the study of business firms. I have two reservations about this claim; first, the two components of the dissertation are not really integrated with each other, and second, the dissertation lacks something which could be properly described as a comparative historical framework. The narrative histories contain comparable information but no systematic comparative analysis; the statistical analysis is based on a model which consists of a set of accounting identities, which is unhistorical.

These reservations do not deny the fact that the study yields some highly useful insights into factors associated with the growth and profitability of firms. One of the significant findings is the positive correlation between the firms' rate of growth and their size and age. This is what one might expect. Yet, I am not entirely convinced that this is what the author has demonstrated. At least he has not demonstrated the validity of his conclusion for the industry as a whole. Ihe first problem that arises concerns the biased nature of his sample. Ihe firms are drawn from a larger sample, which the author properly describes as non-random. Ihe bias, which he ignores or considers unimportant, stems from the fact that the sample includes only firms that had achieved annual sales in excess of one hundred million dollars by 1967. Ihe sample systematically eliminates all those firms which did not achieve this level of sales by 1967. When the author correlates size and rate of growth over the twenty-year period, he is thus not paying any attention to all those small and medium size firms that did not grow to the one hundred million sales level. If he had included these firms, his results might look rather different. His conclusions must therefore be qualified to apply only to firms which survived and achieved a large size.

Another problem bothered me a great deal. I don't understand why the author uses only rank correlations; why he does not use ordinary regression and correlation analysis, since his data are cardinal rather than ordinal. By using rank correlations he loses a great deal of information that would be of interest. With rank correlation he cannot show that size and growth are correlated, only that rank of size and rank of growth are correlated, which is something quite different. We certainly would like to know more about the behavior of the relationships between growth and size and age than their rank orders.

The dissertation contains a number of other interesting findings, relating in particular to the profitability of the firms. These findings are of interest mainly from a management point of view. Both the historian and the economist, however, will experience a sense of frustration with the study. The historian can rightly complain that the findings and conclusions are not evaluated in the context of an historical setting. Similarly, the economist will miss the kind of analysis that would show what the findings mean in terms of the economics of the industry. He will wonder how the firms' growth and profitability relate to the industry's competitiveness, conditions of entry, economies of scale, economies of integration, extent of advertising, progressiveness of management, and so on. It would be patently unfair to expect Mr. Prendergast to do all these things in a dissertation. Let us merely encourage him to keep going.

We turn now to Mr. Anderson's study. Its concern is the role of cities in economic growth. Anderson's analysis does not quite measure up to his vision of the city as a unit of production, with its own production function, with its own inputs and outputs, and its own dynamics of growth. In this vision, the economies which are external to the firms located in the city become the internal economies of the urban agglomeration. A city provides economic advantages to individuals as well as to firms, but of course these advantages also have a cost, which individuals and firms must bear.

The author does not pursue this concept of city very far. He devotes most of his attention to one aspect, namely the role of local government expenditure in economic growth. He correctly stresses the large size of these expenditures in relation to those of major industries.

After discussing various kinds of growth theories, the author finds that most of them stress the supply side, but he decides that for his purposes the emphasis should be on the side of demand induced growth. This is certainly a correct decision since local government expenditures of the kind he analyzes represent a bona fide case of derived growth.

Mr. Anderson is much concerned with analyzing the determinants of local government expenditures and with their impact on income and growth of the locality. In simple algebraic terms his expenditure determinants would look as follows:

$$E = F(P, \frac{Y}{P}, \frac{\Delta P}{P}, I)$$

where

E is expenditure

P is population

Y is income

T is technology

The first two variables of the equation, population and per capita income, are used to explain operating expenditure, and the last three variables, per capita income, rate of population growth, and technology are used to explain capital expenditures. In the context of the dissertation, these variables certainly have *a priori* plausibility, but the author does not analyze the relationships. As a consequence, the equation has to remain unspecified (the equation is mine not his). For reasons of his own, the author did not attempt to fit a statistical expenditure curve. We therefore do not know how much is explained by the variables and how important one is in relation to the other. The author hints that there are lagged relationships in the model but we don't know what kind of lags or how important they are.

The intriguing question which runs through the dissertation is never dealt with explicitly. How dynamic is the role of cities in economic growth? The author seems inclined to think that their role is quite dynamic. I would prefer to reserve judgement, but I would be in a better position to make a judgement if the dissertation had explored more the dynamics of decision making on local government expenditure.

I cannot help but feel that the author got sidetracked in his analysis. When he turns to the impact of local expenditures, his only concern is with multiplier - accelerator types of macro relationships. From that point of view it would be hard to argue *a priori* that local government expenditures have more or less of an impact than an equal amount of private expenditures. The concept of government expenditures as an investment into the productive capacity of a city is completely lost sight of. This aspect certainly must be explored more fully before we understand the role of the city in economic growth.