THE ECONOMIC HISTORIOGRAPHY OF TWENTIETH CENTURY MEXICO

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Economic historiography in its contemporary form goes considerably beyond the recording of historical events. What is most appropriate to the discipline is the devclopment of a framework of analysis whithin which a particular set of events may be ordered and employed to shed light on man's material behavior. Economic history according to Schumpeter- comprises one of the three fundamental building bloks of economic science, along with statistics and theory. He considered the interpretation of history to be the acid test of doctrine, each to be in continual interaction with the other, subject to the constraint of statistical verification. It is with this view in mind that the present paper is written. The fact that the Mexican economy since 1900 has passed through a series of internal and external shock waves that have more or less transformed the process of production, distribution, and final demand, along with most of the institutions upon which economic decisions are based, cannot be ignored in any attempt to comment upon the historiography of the period. One wonders whether conventional economic doctrine is competent to analyze so complex a set of political, social, economic, and even psychological relationships, all undergoing continual change.

What does the received body of doctrine on Mexican economic history of the present century provide in terms of the general themes of development economics? How does it serve to unify and hopefully to explain the events of the turmulous decades since 1910? A cursory glance at the bibliography appended to this paper indicates that there is no lack of material dealing with the period. Indeed almost every topic has received some treatment from *ixtle* to industrialization in monographs and essays touching every corner of the nation and every time period. Yet one may look in vain for general theories of twentieth century Mexican economic change or even for a complete analysis of the economic history of a single sector including the most basic units such as agriculture, industry, mining and services. In those cases where some organic unity does appear, more often than not it is purchased at the price of aprioristic periodization, theoretical determinism, or the judicious selection of data to support the writer's initial premises. Where these methods fail there has been the temptation to employ popular phrases in the place of logic in order to bestow an appearance of order on otherwise disjointed fact and opinion. Can this notable lack of analysis in the literature be attributable to the shortcomings of scholarship, the recent nature of the events, lack of adequate statistics and supporting evidence, or weaknesses in economic theory itself?

Such a question takes on even greater meaning when one surveys the body of received doctrine on economic growth and development under conditions of structural change. The models are generally of a beguiling simplicity, if not in their mathematics at least in their economic content. Moreover few if any are capable of incorporating non-economic variables in an endogenous form rather than as stochastic "shocks" to the system. Despite the work of pioneers such as Kuznets, Seers, Ruggles, Goldsmith, Chenery and their students, all of whom are making progress in the identification of common structural characteristics among developing systems as well as the functional relationships between growth and structural change, the work is almost solely on economic variables an the results are yet inconclusive[1] The best models developed to date are confined to technological diffusion within single sectors (industry for Nelson; agriculture for David) or the interaction between two sectors, each of which has a high degree of symmetry, under extremely restrictive conditions (Jorgenson; Fei-Ranis; Johnston, etc.). In addition some scholars such as Mamalakis are busily engaged in the formation of development taxonomies, but these have yet to display the functional cohesion essential to models of growth, much less historical verisimilitude.[2]

It is therefore entirely appropriate to challenge the concept of contemporary historiography presented above as relevant to the analysis of developing countries. Can anything more be done than to catalogue events or to engage in partial analysis of single sectors or markets? This writer is convinced that although conventional economic analysis

[1] Simon Kuznets, *Economic Growth and Structure*, Norton, 1965.
———, *Modern Economic Growth: Rate, Structure, and Spread*, Yale, 1966.
Hollis B. Chenery, "Patterns of Industrial Growth", *American Economic Review*, September, 1960, pp. 624-654.
——— and Lance Taylor, "Development Patterns: Among Countries and Over "Time", Center for International Affairs, Harvard, *Economic Development Report* Nº 102, June 1968 (draft), pp. 1-41.
Dudley Seers, "The Stages of Economic Development of a Primary Producer in the Middle of the Twentieth Century", *The Economic Bulletin of Ghana*, vol. XII, Nº 4, 1963. See also the monumental series of volumes, *Studies in Income and Wealth, Conference on* Research in Income and Wealth. National Bureau of Economic Research. Columbia University Press.

[2] Markos J. Mamalakis, "The Theory of Sectorial Clashes", paper presented at "Class-Sector" Conference, University of Wisconsin at Milwaukee, April 18-19, 1969. This paper and comments by the present writer and others will appear in a forthcoming issue of *Latin American Research Review*.

cannot completely determine the pattern of economic change in a developing system, it can be used to explain a significant portion of economic behavior, leaving a residual to be associated at a later stage with non-economic (or at least non-conventionally economic) factors. Moreover the application of theory to new historical situations will permit its modification and expansion, raising it to a higher order of generality. We may term this historical *-inductive approach* to the development of economic theory. In the Mexican case there is ample scope for the theorizing by induction, since all the best and worst features of both planning and the price system have been present at one time or another during the past half-century. The economy is a mixture of public and private enterprise, artisanry and machine manufacturing, collective and individual farming on both a subsistence and commercial bassi, and domestic and foreign enterprise. Moreover every conceivable type of shock has been experienced by the system from foreign occupation to the massive seasonal emigration of labor in one direction and tourists in the other, to internally and externally induced trade cycles with both surpluses and deficits, deflation and inflation, price stability, capital flight and capital repatriation, agrarian reform, over and undervaluation of the currency, and civil war. The country is an ideal proving ground for social scientific hypothesis-testers, among whom anthropologist, sociologists, and political scientists have figured prominently in recent years. But in the area of economic history caution has taken the better part of courage, perhaps to the relief of more traditional historians, and much of contemporary Mexican development remains an analytical question mark.

Let us examine the literature on the subject. Appendix B, "A Selective Bibliography of Mexican Economic History (Post 1900)", includes several hundred books, articles, and monographs as well as a substantial collection of basic source material on the period. Clearly scholars have been active in the field, and much raw material remains with which to analyze national, regional and/or sectorial relationships. Yet of the mountain of manuscripts which have appeared on the post 1900 economy, few are truly historical in scope, few employ more than the most elementary economic analysis, and still fewer attempt to relate their special topic to broader dimensions of economic and social change except in the most casual and intuitive manner. For example the term "Revolution" appears frequently in association with a host of topics from agriculture to manufacturing, yet with little specificity. If such facile slogans could provide a catalyst for history, this profession would be both richer and wiser. But, alas, terms such as this evoke more emotion than understanding and sell more books than arguments. What is needed is clarification of the postulational basis of such concepts as "Revolution", decomposing them into their institutional and behavioral roots, such that the social structural changes which they presently conceal rather than reveal may be brought to light. Once this is done, it then becomes possible to disaggregate the events which have taken place during the period of abrupt transition covered by the term and to relate these changes to their economic, social, and political determinants.

The same holds for periodizations of history, often taken from the misleading temporal specificity of political events. The abuse of periodization permits scholars to skip lightly over and even ignore important transitional mechanisms whereby one set of institutions and behavioral relations merges into another. It is of little help to those interested in investigating the mechanism of development for one to impose aprioristic time periods on the sequence of events. Instead one should attempt to uncover the synapses of change and, if necessary, induce periods from the data itself, with allowance for those relationships *which do not change markedly as well as those which do.* More light is often shed on the process of historical evolution by comparing sectors which continue as before with those which do not, than by forcing all observations into the same framework. Thus the continuity of production in the Mexican mining and petroleum industries through most of the period of armed conflict and political rivalry from 1910 to 1920 not only provides important evidence on the nature and intensity of the Revolution but also illustrates a general principal: that periods of major social and political transition tend to depend upon the maintenance of stable and substantional revenue flows, preferably from an export industry.

For Mexico it is beguiling to begin and end time periods with the dates of political administrations. The justification is that new brooms sweep clean and that the executive policies of a highly centralized political system are both the creatures of the current regime and of fundamental importance to the economic process. The dates 1910 and 1940 tend to be taken uncritically as watersheds not only of political but also economic change. Similarly the years 1934 and 1946 marking the beginning of the eras of Cárdenas and Alemán often appear as historical benchmarks. While this writer is as guilty as any of employing such shorthand methods for the classification of economic events, it should be note that the Mexican economy from 1909 to 1912 did not exhibit any notable variation in the pattern of development, nor did it from 1939 to 1941. Just as the rudder turns the vessel only by degrees, so the most basic intention to change policy will often anticipate the results by months and years. The "revealed preference" of planners is subject to a considerable lag. It is perhaps unfortunate, therefore, that most of the literature on modern Mexican economic history begins either with 1910 or 1940 while that of earlier periods tends to end with 1910. Little effort is made to describe or analyze the process by which one period of rapid economic growth such as the Porfiriato merges into a following period of social upheaval and economic disorder, or how a period of relative *laissez faire* such as the twenties might logically be followed by a decade of major institutional reform.

In addition to the aforementioned lack of theory and inattention to transitional detail, much of the literature of the period makes only minimum of use of available statistics and tends to accept the data which are presented with critical abandon. Highly questionable figures appear again and again, in a pyramiding of research, each additional level of which is built upon an initial foundation of statistical sand. The result is a product of scholarship having the same qualities of functionless endurance as the great pyramids themselves. Mexican economic statistics are no different from those of most developing countries. Though they rarely rise above repute and generally demand a considerable degree of cross checking for consistency, the figures can be profitably employed provided the scholar knows something of their sources and methods. Such information has not always been readily available to historians, since Mexican economic statistics have tended to be used as instruments of political prestige and power, rather than as the neutral sources of impartial information which they are supposed to be. Economists have been tempted to assume the role abandoned by court magicians and astrologers who, with their arcane paraphernalia and incantations, prophesied triumph or disaster. Thus economists have tended to keep their data in locked drawers to be drawn forth only at kingly command. Fortunately this situation is now beginning to change as *data power* is shifting to *analysis power* in the higher councils of state. Hopefully this will mark a trend toward a more thorough and critical application of statistics in subsequent writings on the period.

What we need, therefore, and what this paper calls for, is a fresh approach to the writing of contemporary Mexican economic history. Scholars must be prepared to forget whatever prejudices they have formed on the basis of insufficient evidence, in order to view the whole period in a new light. The existing literature must be called ruthlessly to separate the bits and pieces of hard analysis based on firm factual and analytical foundations from the rest. To these few building blocks it will be necessary to add great chunks of new research which require renewed attention to basic statistical detail, patient archival research in Mexico and abroad, interviewing where necessary to fill remaining gaps, and always subject to anaytical rigor. While this may seem an extreme objetive it is essential if the present state of mediocrity in the field is to end. Fortunately there is evidence that a generation of new scholars, well-trained in technical economics and econometric analysis, is already beginning to work on pre-19th century and post 1940 material. Hopefully this research will extend to the intervening years for which much raw data is now waiting to be analyzed.

In the process a whole new set of basic economic statistics will have to be generated, both at the national and regional level, a process which will require substantial institutional support. The Department of Economic Studies of the Bank of Mexico has already financed a considerable body of research on the pre-1940 economy, much of which is now beginning to appear in the form of aggregate indicators of economic activity (382) (388). These series need to be complemented with data on distributive shares, the composition of final demand, savings and investment. Foreign trade data similar to these generated by El Colegio de Mexico's research project on the Porfiriato, under the direction of Daniel Cosío Villegas, need to be extended to the period from 1910 to 1940. Moreover existing times series require considerable cross checking for accuracy both of level and trend, and especially those before 1950. The frequent revision of the national accounts by the Bank of Mexico, while praiseworthy, tends to wreak havoc with scholarship, and this writer has found his own efforts frustrated continually by the need to generate analysis capable of hitting a moving target. A glance at Appendix A ("A Brief History of National Account Estimation in Mexico") will suffice to illustrate the problems that can arise for quantitative historians dealing with Mexico. The estimates of national income and product for selected years vary from ten to fifty percent depending on the date of their derivation, the data then available, and the assumptions employed in their use. In adition the composition of output has changed notably from revision to revision of GDP, as has the rate of growth of such basic aggregates as agriculture, industry, services, and gross investment.

Despite these difficulties enough information now exists to make considerable progress in such broad areas of analysis as: a) the economic determinants of demographic change; b) proximate sources of productivity growth by sector and region; c) the effect of policy decisions on the pattern of resource allocation and growth; d) the effect of structural changes in output on income distribution, the formation of markets, and the pattern of final demand; e) trade and factor movements in relation to the internal structure of production and distribution; f) the regional pattern of economic growth as it has been influenced by public policy, migration, and innovations in transport. It is not that topics such as these have failed to receive attention until now, but rather that they have been placed low in the order of research priorities in favor of a largely descriptive treatment of individual sectors, institutions, and policy problems. While a postponement of attention to the broader issues of economic history may have been justified in the past by shortcomings in data and technique, these difficulties are being gradually overcome. It is hoped that the improved quality and accessibility of both statistics and technique will soon lead to a higher level of analytical research.

In addition to the pure economics of Mexican historiography, which as we have seen leaves much to be desired, there is a problem which we might term the *social scientific identification problem.* This problem has to do with the difficulty of analyzing economic relationships which are affected by the interaction of non-economic factors. The problem is especially complex when non-economic "inputs" are themselves at least partial functions of economic events in the past. In the broadest sense the history of developing countries involves a multidimensional interaction of social, political, economic, and even psychological variables. Thus to simulate, or "explain" the pattern of economic behavior using economic variables alone will tend to misspecify relationships and lead to erroneous conclusions. The more a social system is subject to structural change, the greater the likely error from one-dimensional economic analysis. This means that for countries such as Mexico which have experienced fundamental changes in their social structure in the course of economic development one cannot expect to explain the process of change with accuracy using a solely economic framework of analysis. Rather what is needed is an analytical model which is broad enough to accomodate non-economic factors in the transition process, yet narrow enough to permit the statement and testing of hypotheses. An example will help to illustrate this point.

The economic disturbances in Mexico in the late twenties and early thirties brought about import by the world depression, led to a shortage of liquidity, falling prices and wages, high unemployment, and general social unrest. This was compounded by the internal political and religious struggle, of which the Cristero Rebellion and the assassination of President-elect Obregón were but symptoms. It has been argued that deteriorating economic conditions brought about a growing lack of confidence in the *laissez faire* policies advocated by Calles and the hand-picked administrations from 1928-1934. Dissatisfaction with the state of the economy as it presently was run reawakened interest in more radical goals of the Revolution as expressed in the Constitution of 1917. This led to a groundswell of support for Cárdenas, a man backed by a coalition of progressive state governors, supported by General Calles, who had already proved his zeal for agrarian reform and labor organization as governor of Michoacán. The changing economic circumstances of the time, according to this view, laid the groundwork for political change.

Once in office Cárdenas invoked new policies, different in degree if not in kind, leading to wholesale reform of the land tenure system. This had been tried earlier by Portes Gil, but his term of office had been short-lived. The greater degree of economic policy space created by the depression was taken advantage of by Cárdenas from 1934 to 1938 and he acted boldly to alter the pattern of asset ownership. However he was limited as to the extent of actual income distribution which could be achieved, since income itself was depressed during the thirties through depression, pessimism regarding the treatment of private property, as well as general uncertainty about the future. Investment tended to dry up, along with tax revenues, and government expenditures were limited as the government attempted to avoid inflation and devaluation of the currency. In this case political action was both impelled and constrained by the level and growth of economic activity. At the same time the pattern of economic activity began to reflect the asset redistributive policies of Cárdenas. Thus the process of economic history of the period involved an interaction between economic and political events, not subject to any simple apriori model of material or political determinism.

To adequately interpret such political economic symbiosis calls for a modern Beard or Turner. Economic events should be characterized by the way in which they are interwoven into the fabric of social change as a simultaneously interacting recursive system. This is quite different from the tendency among Mexican writers dealing with the post-revolutionary period to impose aprioristic doctrines on events, sweeping all contravening evidence under the rug. It is time to break the chains of simple dialectical materialism and open the profession in Mexico to an objetive analysis of the process of social interaction. This will lead to a true dialectic in which economic factors are seen to be both causes and effects of political, social, and attitudinal change. If the revolution can be ascribed in part to failures of the preceding economic production and distribution process, so it can be explained by the capacity of the same production process to provide material support for fundamental social and political change. Similarly changes in the economic process cannot be wholly attributed to prior economic events, but rather to the way in which society in response to these events, acted to change the nature of the economy and its direction of growth. Under the circumstances it is important to free the analysis of the period as much as possible form ex post doctrines of historical inevitability.

One way to overcome the natural tendency to regard observations as the inevitable consequence of their antecedents is to focus on the synapses of change mentioned earlier. It is useful to speculate on what might have happened had slight variations taken place at those transaction points. Such speculation, which would be idle without at least some simple analytical model, becomes possible once social theory is introduced into historiography. The "new economic history" is an example of the use of such models to predict what "might have been" under slightly different conditions, as a way of shedding light on what did in fact occur. This is more than the idle amusement of armchair brigadiers refighting Borodino with factors of production instead of troops and resource-allocation rules rather than military strategy. Counterfactual analysis can do much to clarify causal relationships in the historical process. By determining the limits of economic behavior under given conditions of factor endowments, supply and demand relationships, and public policy, subject to exogenous disturbances, it is possible to determine "residuals" in the pattern of change which cannot be accounted for by economic factors alone. For example, by estimating production functions for which assumptions are made about input-output relationships, one can discover the portion of observed changes in production which the analysis would not predict. An effort can then be made to account for this discrepancy in the analysis by respecifying the model, or by adding new factors which had preciously been ignored.

The entire growth process of an economy or sector is subject to this type of analysis. It is possible to estimate how change would have occured under conditions assumed to characterize the period under consideration. The extent to which actual events departed from the predictions of the model show up the strength of the initial assumptions, specification of equations, or degree of underdetermination of the model (more unknowns than equations). Among the missing elements it is quite possible that social and political factors will prove significant. The socially-conscious economic historian, by seeking to improve the explanatory power of the analysis, may be hoped to uncover important new dimensions of social interaction.

This method of using theory to shed light on the influence of non-traditional factors in economic change was experimented with by the writer in the case of Mexican agricultural development under circumstances of radical land tenure change (94). Agricultural production functions were estimated independently for the five census zones of the country by decade from 1930 to 1960, and regional output was estimated back to 1900. The results showed significant differences in inputs, outputs, and productivity both by regional and decade. These disparities when associated with the differing regional and temporal pattern of tenure change, infrastructure investment, and rural population growth, do much to clarify the relationship between traditional and non-traditional factors in the development of this key sector of the economy. In addition the analysis lays the groundwork for subsequent research on the causes of change underlying the proximate sources of production and productivity growth measured here. Hopefully a literature will develop on rural technology, marketing, cropping, tenure conditions, incentives, migration, and institutional change, building on and modifying the results of these highly aggregative regional models.

A similar investigation at a higher level of aggregation was undertaken to uncover some of the possible consequences of social and political changes associated with the Revolution and subsequent Reform, during the years 1910 to 1940. [3] A simple model was employed to estimate the expansion of value added in seven principal production sectors for the period 1910 to 1940, under a range of assumptions about productivity and population growth in the absence of Revolution. The population growth assumptions were crucial, in that they supposed that those deaths during the decade 1910-20 attributable to the disturbances of the Revolution and its aftermath did not, in fact, occur. Non-agricultural output was projected on a commodity by commodity basis under various productivity assumptions, all of which were constrained from outperforming representative Latin American countries over the same period. Agricultural production for export was estimated on the same basis, while that for home consumption was related to the hypothetical rates of population growth assuming that actual per capita productivity trends would have held despite the considerably higher rate of population growth from 1910 to 1920.

The resulting estimates of gross domestic product (GDP) for the years 1925, 1930, and 1940 were subjected to consistency cheks in terms of domestic savings, foreign exchange, and labor and implicit productivity requirements necessary to achieve the projected levels of GDP by 1940. The two most optomistic projections indicated that the actual level of GDP in 1940 might have been achieved as early as 1925 had the losses of the Revolution not occured, and that GDP in 1940 might actually have been 36 to 61% above observed levels under alternative conditions. The two low projections, on the other hand, were considerably less favorable, the lowest placing GDP at only 12% above the observed level. All of these estimates were extremely sensitive to the assumed rate of population growth. Thus while projected GDP was shown to be as much as 61% above the actual 1940 levels, *per capita* product under the most optomistic alternative assumptions only reached 12% above observed levels of per capita GDP in that year. The range of per capita estimates was considerably narrowed, and the lowest figures were actually 2% *below* observed levels in 1940. What is evident from the results is that in the absence of Revolution Mexico might well have possessed a considerably larger population and corres-

[3] Reynolds, Clark W. *The Mexican Economy: Twentieth Century Structure and Growth* (94), Appendix B. See also, "Ideology and Economic Development in Mexico", Food Research Institute Discussion Paper N° 69-1 (Revised). April, 1969.

pondingly greater absolute income, but even under the most optomistic assumptions it would not have enjoyed a much greater rate of growth in *per capita* income than did, in fact, occur during the thirty years after 1910.

This type of analysis is by no means intended to substitute for the hard factual research of traditional economic historiography. At best a complement to such investigation, it can nevertheless prove helpful in clarifying basic questions of interpretation such as alternative views institutional change. It enables one to uncover weaknesses in discriptive on the economic consequence of revolutionary political, social, and analysis, by forcing the scholar to determine an internally consistent set of relationships and to be explicit about the assumptions involved. Since any interpretation of history involves implicit if not explicit model-building, this approach simply allows the theorist to test out his hypotheses in terms of the best evidence available. Where evidence essential to the analysis is missing, it points to the direction in which statistical research should proceed. Where the model reveals the outcome to be extremely sensitive to certain relationships, one may then go to a more detailed institutional investigation of these relationships. For example in the case of the model above, the obvious next step is to investigate the limits between economic, social, political and demographic change for the period 1910-20 in order to isolate the underlying causes of massive loss of life during those years. To what extent were the causes associated with the Revolution, and to what extent were they randomly induced? Was the influenza epidemic of that period directly or indirectly attributable to the socio-economic disturbances of the time? If not, then the "opportunity cost" of the Revolution as measured above may be based upward by the assumption of population growth "as usual" in the absence of Revolution.

Similar work should be done on the market versus non-market influences on output and productivity over the period 1910 to 1940, including a clarification of what actually did occur in production, distribution, and final demand. For example in the case of agriculture the research mentioned above permitted the writer to engage in a major revision of the gross agricultural production index for the years 1910 to 1930. The figures turn out to be quite different from those in even the most recent Banco de México series (382), owing to the inclusion of a much wider sample of commodities in the index. The preponderance of traditional crops such as *maíz* and *frijol* in the earlier indexes bias them downward substantially (139) (262) (31) (382), since these commodities behaved far worse than cash crops during the period. The use of the new series on production, along with the regional disaggregation, permits a new interpretation of the consequences of armed conflict, population dislocation, agrarian reform, and world prices on the agricultural sector. For example production in the Northwestern region moved forward rapidly from 1910 to 1930, while that of the Center and South Pacific lagged far behind.

In addition to more quantitative exploration of aggregate and sectorial production, productivity, and distributional characteristics during the twentieth century, and particularly for the statistical and analytical "dark age" from 1910 to 1940, there is the need for critical investigation of key decision-making units and their historical evolution. Mexican economic activity has been profoundly influenced by the activities of such entities as the Banco de México, Nacional Financiera, Mexican Social Security Institute (IMSS), Banco de Comercio Exterior, CONASUPO, and private banking chains such as the Banco de Comercio. The major work and most conspicuous success to date in the area of institutional historiography has been on the development of financial intermediation. A descriptive literature on the history of banking and financial intermediation appears in the works of Bett (12), Shelton (119), Anderson (48), Goldsmith (49), Blair (119), and Aubey (3). This is supported by the somewhat more analytical studies of the relationship between growth and financial intermediation by Bennett (10), Campos Andapia (18) (333), Brothers and Solís (14), and Koehler (341). Piecing together this material, one begins to see a fairly clear picture of the broad lines of institutional change in the financial sector during the twentieth century. The decade from 1910 to 1920, during which repeated currency crises and bank failures severely upset the monetary system, is treated with the kind of critical insight that comes from close association with some of the institutions involved, by Kemmerer (60).

The inflationary years of the forties and early fifties are subjected to alternatively monetarist and structuralist approaches so ubiquitous to Latin American economics of the period in the works of Siegel (106), Navarrete (78) (354), Mueller (346), Brothers (155), and Solís (298). The transition from inflation to price stability between the mid-fifties and mid-sixties is skillfully handled by Brothers and Solís (14) and Koehler (341), with Koehler adding insights from a disequilibrium income-determination model (fixed interest rates, credit rationing, and restrictions on foreign investment) to the more conventional quantity-theoretical foundations of his predecessors. The twenties and thirties have yet to receive adequate attention by monetary historians. The earlier decade is of interest because of the association between liquidity shortages, falling prices and their possible impact on growth. The latter period combines expansion of liquidity in the form of paper pesos with a growing government deficit, both of which lead to moderate inflation. The affect of the silver agreements with the U. S. during the early thirties, their eventual expiration, and capital flight associated with the oil expropriation of 1938 deserve to be analyzed in more detail, especially in relation to income stability and growth.

Despite all of the institutional material on financial activities of the modern period, one looks in vain for a work comparable in depth and scope to the admirable historiography of Potash in his now-classic study of the nineteenth century Banco de Avío. What is lacking in these studies, from the viewpoint of the historian, is a fully fleshed contextual framework within which the institution finds its place. There is too little attention to the character and motivation of entrepreneurship involved, the alternatives facing the enterprise, its role in the broader cultural and political history of the time. One would hope that increased analytical sophistication would not drive out painstaking attention to institutional detail. Yet this seems to be happening in work on the twentieth century. Hopefully the theoretical and statistical rapacity of the modern economic historian can be combined with renewed enthusiasm for archival research. Until then the best that can be expected are well-sculpted systems of bone, nerves, and sinews, all of which distinguish anatomy from design and laboratory specimens from statuary.

More work is also needed on the history of policy-making than one now finds in the literature. The dimensions of policy space within which government decision makers operate are broadly defined and subject to considerable change over time. Historiography can be used to clarify these relationships and reveal their implications for economic activity. The transmission of information on market signals to decision makers, and the reverse flow of commands, are both subject to a greater or lesser degree of "noise" in the political system, if one may borrow from the terminology of communications theory. Socio-economic historians such as Vernon and Wionczek (118) (119) (126) (322) skillfully interpret the subtle interplay of social-psychological and economic factors underlying the pattern of public policy in Mexico. As such they set forth a pattern of research which might well be followed by other students of contemporary Latin American history.

Wionczek in his thoughtful analysis of the nationalization of electric power, and more recently that of the sulphur industry (126) comes to grips with problems which beset all developing countries forced to balance the requirements of foreign capital and technique against a mandate of national autonomy and maximum relative shares from domestic resources. While one might have hoped for more statistics and conventional economic analysis in his two important studies, including a fuller statement of sales, costs, profits, and returns to the domestic economy over time, as a basis for evaluating the purely economic content of the decisions both of industry and the government, it is perhaps fair to say that Wionczek offers the most carefully researched and conventionally historical scholarship of the modern period. Vernon on the other hand is at his best when dealing with the broad sweep of events and the atmosphere in which they take place (118). His insights, such as those concerning the role-playing of *políticos* versus *técnicos*, are often revealing, though he is content to paint history with a broad brush. One is left with a highly impressionistic set of interpretations of events, so much so that as hypotheses they are difficult if not impossible to test. There are few statistics employed in his analysis, and those which are (such as his now obsolete series on gross investment) lead to a quite erroneous view of the mechanism of income determination and stabilization in the postwar period. (See, for example, the detailed critique of the Vernon model by Koehler [341], as well as that of the writer [94]).

In calling for more detailed research on the individual subperiods of the modern era, one may point to the pathbreaking precedent of the Cosío Villegas series on the Porfiriato, and particularly the brilliant analyses of the development of industry, foreign trade, and the general economy prior to 1910 by Fernando Rosenzweig (280) (281) (282). This work provides not only an uncovering of hitherto buried factural evidence on the period, including structural evidence on the evolution from artisanry to machine manufacturing in such basic activities as textiles, but a sophisticated interpretation of the interaction between production and employment and its influence on the wage level and the rate of urbanization.[4] What Rosenzweig did for the pre-Revolutionary period must now be done for the successive years, starting perhaps with the manufacturing sector. Mosk's classic o the subject (75), while still a vital source of institutional detail for the period of the forties, suffers from a reliance on production data which are now obsolete and tends to neglect the years before 1940 during which much capacity was installed which came into full production only after the beginning of World War II. For more recent years there is a growing literature on the process of import substitution and the use of commercial policy to promote industrialization, owing to the recent research fad in this area (see Izquierdo [119], Bueno [332], Bacha [144], Strassman [112] [208], King [339], Maneschi and Reynolds [343] and Reynolds [359]). However little of this material covers the prewar period, and most of it deals with the fifties and onward. (A longer study on industrialization by King has come to the attention of the

[4] In addition an analytical reworking of the census data on employment by sector for the period 1895 to 1950 has recently been prepared by Keesing (338), qualifying some of the earlier series in El Colegio de Mexico's sourcebook on the Porfiriato (396). This important study will shortly appear in the *Journal of Economic History*.

author but is still in draft form and was not available for examination at the time of this writing.)

As to historiography of other sectors of the economy, agriculture alone would deserve a paper in itself. Fully one quarter to one-third of the references in the bibliography deal directly or indirectly with this sector, yet, as has been mentioned above, the quantitative analysis of agricultural development in Mexico remains fragmentary and relatively unrelated to the general development process. Studies such as those by Flores (44), and Moisés de la Peña (32), introduce important issues of social and political development into the agrarian model. As such they are written in the best tradition of post-Revolutionary Mexican historiography, but in such efforts there is a far greater need for factual support and analysis than even a purely economic study would require if the reader is to accept the conclusions on the basis of reason rather than faith. Moreover the process of intersectoral flows of labor, capital, and intermediate goods and services between the agricultural and non-agricultural sectors has yet to be analyzed in detail, despite the crucial nature of such flows to the development of agriculture itself. Rural income distribution studies do not abound, though some material is available in the works of Navarrete (79), Singer (367), Solís (368), Sturmthal (302), Reynolds (94), and others. One of the critical issues facing contemporary policy-makers is the eventual disposition of the institution of the *ejido*. The history of this institution occupies a good share of the literature on Mexican agriculture, from which we should not omit the classical offerings of the Northamericans Simpson (109), Seinor (102), and Whetten (123), as well as the important earlier work on land tenure by McBride (68). Fernández y Fernández has competently dealt with the dilemma of this curiously Mexican institution as it relates to the technological and sociological exigencies of the modern era (43) (180) (181) (185) (186) (187) (188), while Eckstein has employed econometric analysis to determine the productivity of a sample of agricultural units by size and tenure class (37). This research sheds much light on the historical impact of agrarian reform on production, since it combines the statistical analysis with case studies. The first volume, which ends with the 1950 census data, is brought up to date in a more recent study for CIDA (38) in which agriculture is more explicitly related to the overall process of Mexican development.

Considerable insight into broader issues of social change may often be obtained by looking in detail at a given region or project. Project analysis in Mexico has bee relatively popular in the thesis mills of American universities, owing to the lure of the land and the romance of its history and institutions. Two studies of river basin development written under such circumstances deserve mention, those of Barkin on the Tepalcatepec Region (328) and Kink (339) (to be published jointly in Spanish by Siglo XXI. To these should be added the work by Poleman on the Papaloapan Project (89). While these works represent useful applications of comparative cost analysis, they tend to be confined to the strictly economic dimensions of the subject and as such may not be said to qualify as economic history in the broadest sense. Indeed the impact of major expenditures on rural infraestructure in hitherto impoverished regions has an effect on the pattern of society, culture, and economic institutions far beyond those which are readily amenable to statistical measurement. What is called for if this impact is to be adequately interpreted, is *regional* rather than project historiography, in which the broad social characteristics of the region are examined in detail before, during, and after the advent of the project. While such an undertaking may require far more time and attention than a conventional Ph. D. dissertation in economic development, it is essential if we are to fully appraise the impact of a major exogenous economic shock on the economic life of the community.

Perhaps at this point it might be appropriate to note that the weaknesses of contemporary economic historiography, of which the work on Mexico is but a sympton, reflect the institutional contraints of the modern university system. In this system students are required to "do a thesis" in a circumscribed period of time, with limited resources. If the student is to research a foreign area, as is often the case for those who explore the curiosities of Mexican history, still more time is required in simply becoming acquainted with the lay of the land. It is not surprising, then, that the product of this type of scholarship is so frequently 40% hypothesis, 40% technique, and only 20% historical detail. What may be required if the products of scholarship are to be improved is a change in the conditions of research. Perhaps the period for writing of a Ph. D. dissertation in economic history should be extended to take into consideration the special requirements of the field. Perhaps more funds should be allocated to post-doctoral research in this area. Perhaps local institutions should support the kind of work on the post-1910 period that has been so impressively undertaken for the Porfiriato by El Colegio de México. Indeed there is no reason why Mexican students, with their rising degree of analytical sophistication, should not be incouraged to explore the neglected areas of contemporary economic history with the most powerful techniques at their disposal and with the requisite amount of time for careful attention to institutional detail.

A few other exemplary cases of historical research deserve mention. The work of Wilkie on oral history of the period promises to fill important gaps in the history of decision-making (124), while his comparison of the policy statements with actual expenditures of Post Revolutionary administrations throws considerable light on the revealed preferences of Mexican presidents (124). His indexes of social change represent a highly experimental and controversial application of quantitative indicators of welfare at the state, regional, and national level. While these indexes are arbitrary in their choice of indicators (relying on census reporting of social characteristics), weighting, and mixture of stocks and flows, they are indicative of the process of social change and offer something beyond the characteristic series on *per capita* income alone. Alas, they do not substitute for the kind of detailed welfare analysis which one might wish in attempting to interpret the social impact of economic change over the period. Here again one is happy to have at least a skeleton of information but will be more content once there is flesh upon the bones. What will perhaps prove to be Wilkie's most important work on contemporary Mexican history is yet to appear, that dealing with Cárdenas' governorship of Michoacán and later succession to the presidency. In this work the logical next step is taken, following the formulation of broad social-economic indicators, and that is to provide the institutional detail within which they take on meaning.

All too often it is to historians, rather than economists, that credit must be given for the broad-based research on which economic analysis must depend. In this respect Howard Cline's two volumes dealing with the pre and post-1950 periods respectively are invaluable sources of information on the panorama of change within which economic events play only a partial, if often crucial, role (24) (25). The bibliographical Appendix to the first volume is particularly helpful in bringing up to date (1953) the material in the Mexican section of *The Economic Literature of Latin America* (2 vols., 1935-36). Similarly Bernstein's massive volume on the history of Mexican mining provides much legal institutional and historical detail on the evolution of this sector which first led and then followed the general development process (11). One of his most provocative suggestions, worth testing in detail, is that U.S. ownership of the mining sector increased during the twenties. This is supported by Sherwell's data on the falling Mexican share of value added (105). Unfortunately the mining sector, despite its importance, has yet to be subjected to a thorough economic analysis. Except for some valuable estimates in the work of Navarrete on returns to Mexico from the extractive industries over time the wealth of statistics on mining remains largely unexplored. The petroleum sector has a relative large literature, especially in terms of the circumstances surrounding the expropriation in 1938, including a classic analysis of the history prepared at the time by Jesús Silva Herzog. The memoirs of U.S. Ambassador Josephus Daniels and Cronin's highly readable *Josephus Daniels in Mexico* shed considerable light on the economic as well as political issues. A valuable collection of statistics and an insider's report on the industry from 1938 to 1960 appears in ex-PEMEX director Antonio Bermúdez' volume *The Mexican National Petroleum Industry* (Stanford, 1963). Nevertheless much remains to be done to analyze the economic causes and consequences of the petroleum seizure, a subject which could prove to be a scholarly bonanza.

Time and space do not permit attention to the fundamental contributions to scholarship of the national and international agencies whose concern has been to discover the economic structure on which economic and financial policy must depend. The annual surveys of the UN Economic Commission for Latin America, beginning with the essay on Mexico in the 1949 survey (430) are important sources of both data and interpretation of the economic growth process. Similarly the ECLA study of external disequilibrium, its causes and consequences, prepared in the mid-fifties by a group of extremely insightful political-economists including Víctor Urquidi, Juán Noyola, Celso Furtado, and Osvaldo Sunkel, all of whom were associated with the Mexico City office of ECLA at the time, proved highly prophetic in its projections of the balance of trade and constitutes one of the first attempts at model-building to simulate the process of Mexican economic development (431). The World Bank study of the structure of the Mexican economy from 1939 to 1950 and its implications for capital —absorptive capacity is another landmark, including some of the first estimates of the economic and functional distribution of income for that decade (55) (85). More recently the Banco de México has vastly improved the quality and availability of its economic statistics. Input-output tables have been prepared for 1950 and 1960. Time series on the level and distribution of income by activity and factor shares for the period 1950-1967 have just been released, on a provisional basis, and deserve careful attention (379). Earlier series used as a basis for policy making were prepared by the Banco de México and the Secretaría de Hacienda in a joint volume for the period 1939 to the mid-sixties (387). Economists Víctor Urquidi, Leopoldo Solís, Ernesto Fernández Hurtado, statistician Rubén Gleason, and numerous others have cooperated to bring about an outpouring of data which cannot help but revolutionize the interpretation of modern Mexican economic history. The profession owes a vote of thanks to the untiring efforts of these economic pioneers who have labored to bring order from chaos, both in terms of data and their interpretation.

In summary the ground has only been scratched in this field. There is need for considerably more attention to such basic issues as the economic impact of roads and railroads, technological innovations in construction and earth-moving as they have contributed to a unification of national markets, the regional pattern of internal trade and its relationship to natural and induced processes of sectorial expansion, the role of mining in the initial expansion of the modern period and the opportunity cost of policy-induced neglect of this sector in recent years, the economic impact of the bracero program and the unbalanced growth of the frontier economy along the U.S. border-its benefits and costs, the economic history of the cattle industry from the Porfiriato to the present —a virtually neglected sector, and that of the service sector which has absorbed most of the increase in the urban labor force since the Revolution, rather than industry. Foreign investment needs considerably more attention than it has yet received, the interesting interpretations of Ceceña (22), Wionczek (126), Brandenberg (153) (13), and others notwithstanding. Fertile ground still exists for work on the henequen industry and its impact on the export economy of Yucatán, the history of the sugar industry (including an investigation of its amazingly rapid recorvery in the twenties), cotton, and textiles, all of which, by reflecting in a single sector the varying currents of economic, political, and social change, could provide vast amounts of new evidence for interpretation of the period. In short much remains to be done. The areas of analytical wilderness which remain are much wider than the present patches of plowed ground. Hopefully those who wish to clear the new land will be pioneers in social development analysis as defined above, rather than purely institutional or theoretical in focus. Disaggregation is likely to prove more helpful than aggregate analysis, in the future, provided that the broader set of social-economic relations are not forgotten in the process. Hopefully the methodology developed in connection with Mexican historiography will be applied elsewhere in those cases where the process of economic history involves broad interactions of regions, social classes, and production sectors subject to periods of profound political shocks. For one who feels that U.S. economic history leaves much to be desired in its dealings with times of major social-political-and institutional change, such as the Civil War, the Great Depression, both World Wars, and the postwar assimilation of racial and population explosions, Mexico may set a pattern of investigation. In all of this it should not be forgotten that economic historiography is the handmaiden of theory and policy. While events never exactly repeat themselves, their components often recombine in similar ways, allowing the insights drawn from earlier periods to be applied at great social saving.