COMMENTARY

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INTRODUCTION: THE FLORESCANO BIBLIOGRAPHY

Enrique Florescano's contribution consists of two separate sections, bibliography and observations on the state of the discipline of history as it has been and is currently practiced in Mexico, and recommendations on how it ought to be practiced. Students of Mexican history are already in his debt for his insight, commitment and enthusiasm for the possibilities of the renovated and enlarged discipline of history and for 72 pages of bibliography. One need hardly add that he speaks with the authority of his recently published *Precios del maíz y crisis agrícolas en México* (1969).

His analytical bibliography lists major publications and articles on Mexico's economic and social history sweeping through more than four centuries from pre-conquest times to the end of the Porfiriato. Not only is the bibliography logically analytical and frequently annotated, it lists published materials on the new foci of the discipline, e.g., historical demography, economic history in particular agrarian history, price fluctuations, changing technology and agricultural crises. It is in no sense a criticism to note that bibliographical distribution among the three periods covered, preconquest, colonial and independence periods, is uneven. For Florescano has had to follow the distribution of historians' interests and consequently the colonial period occupies roughly sixty per cent of the total bibliography by pages. It should also be noted that the bibliography has no separate section devoted to the period of Insurgency, 1810-1821. This is a selective not a comprehensive bibliography, for published primary source are omitted; it is sparely but judiciously annotated. One suspects that Florescano will not be able to prevent its early publication in the Proceedings of this Reunion dispite his stated intention to withhold its publication until a later date.

OBSERVATIONS ON MEXICAN HISTORIOGRAPHY

Florescano has introduced the bibliography with a widely ranging review of the way in which the history of Mexico has been written and with suggestion for new themes and new methodology. By turn, he emphasizes the petrification of political history in Mexico, the isolation of practitioners of the discipline both from colleagues and from the potential advantages of other disciplines; he indicates the utility of economic history especially the quantitative over the purely descriptive; and he pinpoints the type of investigation potentially most fruitful at the macro— or national level, at the regional and local level, and by economic sectors; and he concludes with a plea for the integration of economic and socio-political factors to create a rounded history or in his eloquent phrase, "historia del hombre todo".

Although Florescano is impatient to see the new quantitative economic history incorporated into the research, reflections and writings of Mexican historians, he recognizes that one need not await decades of monographic accumulation for the appearance of works of synthesis. He recognizes too the danger that the narrowly focused monograph on one aspect of economic history may well leave the traditionally oriented political historians unimpressed. So he suggests that economic historians present their findings within a larger framework and that they, along with other types of historians, consider the heuristic synthesis, the synthesis based on insight derived from the monographic fragment, from the research probe in depth which has often opened up wider horizons. Above all, he offers no unilateral road to the understanding of an era via one variety of economic or any other history. There are, he indicates, many approaches to economic history.

A CRITIQUE

To fault so comprehensive and judicious an introduction to a substantial bibliography is not easy. Nor is there any reason to do so. What follows is not criticism but views that have been stimulated by his underlying emphasis and reliance upon the ambitious, comprehensive quantitative techniques of what may be termed the French school of "total" history.

Florescano's strongest criticism is directed against the persistent tradition of political history among Mexican historians despite the appearance elsewhere of new historical modes of thought and techniques of analysis, notably in Western Europe and the United States. His impatience is understandable; yet equally understandable is the fact that national historiographical trends reflect the predominant preoccupations of every epoch. No doubt the variety and utility of new analytical tools affect the mode of analysis; what precedes the choice of approach and technique, however, is primarily the desire to achieve explanations of the past more illuminating of the present than the accepted, that is, the traditional. But more baldly, pressures and interests determine the historians' interests and methodologies as historiographical trends of the twentieth century in the United States, France and Mexico indicate.

In the historiographical development of the United States interest in economic factors obviously antedates Charles Beard's *An Economic Interpretation of the Constitution of the United States* (1911). Beard was not the first in the United States to observe and document the relationship of economic interest to political structure, function and groups. What must be recalled is that the *Economic Interpretation* appeared precisely at that moment in US history when powerful monopolistic and oligopolistic business organizations had aroused the opposition of broad segments of the people of the US, when business organizations seemed to control effectively access to the political system and when the people of the US wondered whether the constitution was indeed a God-given instrument to permit the flowering of liberty for God's chosen people in the Promised Land. Not inappropriately, Beard and his Columbia University colleague, Robinson, were later to become the leading protagonists of integrated history rather than history as chronicle of past politics, what became the "new history" in the US.

In much the same light one may view the wide vogue of price and wage history in the post-World War I years of inflation terminated by the deflation of the Great Depression. Earl Hamilton, as Pierre Vilar has reminded us, was stimulated by profit inflation in the US between 1916-1919 and by the phenomenon of post-war inflation in Western Europe. These elements and —one may presume— the upsurge of socialism in once capitalist Russia led him to examine the origins of capitalism in Western Europe and to ascribe over-riding importance to price inflation and to profits developing from the lag of wages behind prices. To be sure, Hamilton was not the first historian to focus upon the sixteenth-century origins of capitalism or upon prices. Nor was he unique in the field at the time. His article on "American Treasure and the Rise of Capitalism, 1500-1700" appeared in 1929; the carefully elaborated monographs on Simiand (1932), Labrousse (1933) and Hamilton (1934) rapidly followed. Without streching the point unduly, these historians utilized a sectoral analysis to cast light upon capital formation in the long-term economic growth and development under capitalism.

The clearest example of methodology at the service of new foci of interest, and not the reverse, is offered by the French school of historiography. It may be facetious, but it is clearly beyond dispute that in this case quantity alone is not responsible for qualitative change. What characterizes the group which includes Braudel, Goubert, Baehrel, Meuvret, Leroy Ladurie and —in Iberian and Ibero-American studies— Chaunu and Vilar is their emphasis upon quantification in handling economic and social data and, in the second place, their attempt to view the past through many analytical prisms to achieve "total" history. They have been distinguished disciples of distinguished master —Lefebvre, Bloch, Febvre. More to our point, the historians who have focused upon French problems have structured their analyses around the problems of pre-industrial agrarian societies at the regional level in response to such nagging questions of their time as (1) the relationship between enduring agrarian structures and the timing and rate of French industrialization; (2) the factors responsible for long-term French demographic patterns, i.e., the role of the epidemic disease, subsistence and property size and ownership in demographic fluctuations; and (3) not to exhaust the catalogue, the role of market forces —supply, demand, prices— in agrarian production. In other words, this school, has furnished outstanding examples of aggregative regional studies, underscoring socio-economic factors, utilizing quantitative as well as descriptive data. Integrative at the regional level, these studies still await a new national synthesis of the character supplied almost forty years ago by Bloch. Admirable models of the historians' craft, the French studies have certain limitations in the Latin American context. First, their regionalist focus, and, second, the fact that with the exception of Lefebvre's *Les paysans du nord pendant la revolution* the French essays in "total" history simply omit the great watershed in French development, the Revolution.

Even de much studied French Revolution is a *terra incognita* from the viewpoint of long-term agrarian studies.

THE CASE OF MEXICO

It appears to be the burden of Florescano's argument that Mexico has lagged in the development of economic and social history and that where such studies have appeared since about 1940 the methodology employed indicates "los viejos hábitos, los métodos gastados, y la incapacidad creativa de la historia tradicional". By this he seems to mean that economic and social history is still descriptive rather than quantitative and that it lacks any new conceptual apparatus.

These phenomena are easily explained. In the first place, no one questions the high quality and volume of Mexican historiography of the nineteenth century from Bustamante and Alamán to Hernández y Dávalos, Paso y Troncoso and Genaro García. Their focus was properly that great single block of Mexican history and its inevitable sequel, the colony and the insurgency and its aftermath. That they emphasized politics and personalities reflects an age which saw in political structure and function the resolution of conflicts confined largely to the economic and social elite. On the other hand, at the end of the Porfiriato when the splendor of the age was already tarnished and dissent became more widespread and dangerous, there appeared the classic work of Molina Enríquez which economic and social historians may still consult with profit. The Mexican Revolution diverted and inevitably wasted energy and talent; it is only since the end of large-scale agrarian reform, the acceleration of industrial growth and the unbroken succession of governments that new cadres of historians have appeared. One might hypothesize that those talents which under other circumstances might have been channeled to economic and social history, have instead been absorbed by government and by the discipline of political science and especially economics. Clark Reynold's bibliography suggests that in Mexico the interest in economics has been sustained and substantial.

The end of the most recent, most profound and most popular of Mexico's historical cycles, the Revolution, offers historians an opportunity to employ a sophisticated and interdisciplinary methodology to illuminate Mexico's past. The opportunity, however, should not be limited to the Revolution; rather, we should examine all the great cycles of Mexico's past, conquest in the sixteenth century, rapid growth and change at the end of the eighteenth which culminated in anti-colonial warfare, then the Reforma and its sequel. It is not enough, for example, to formulate a balance sheet of the Revolution. Historians must sort out the unique and incidental from the perennial, the persistent, the enduring; in other words, now is the time to emphasize the structures, the *supervivencias*, the hard substrata of Mexico's history. This was the challenge before Lucas Alamán and later Molina Enríquez and each met it in his own way, through his own ideological prism. It is our challenge to examine an even longer timespan than theirs, to pinpoint the structures and to place them intelligibly within the context of the long-term or secular movements.

We must avoid the congenital tendency of historians to examine discrete elements and periods of the past, to confine our conclusions to the boundaries of one era. For example, we will continue to have an imperfect view of the society and economy that Spaniards erected in Mexico by the last quarter of the sixteenth century unless we review the pre-conquest cultures of Central Mexico, their society and economy and in particular the stresses and strains, the frictions and fissures that most certainly had developed in the fifteenth century when Central Mexico may have had a population upwards of 20 million. Put another way, historians must reflect upon the implications of the painstaking reconstruction of Mexico's historical demography by Simpson, Cook and Borah. In a similar vein, we have perhaps permitted the Insurgency to obscure our understanding of Mexico's great export cycle at the end of the eighteenth century and its relationship to demographic growth, increase in imports as well exports, and changes in land ownership, land use and output. To the economist of today, this pattern suggests a sort of archetype of enclave economy not so much in terms of linkages to the host or domestic economy as in the export of silver and the outflow of interest and profit. The Soviet historian, Alperovich, who has read his Humboldt carefully, has reminded us that Humboldt went to the core of Mexico's mining economy and its colonial function within Spanish imperialist structures when he wrote that "La Nueva España ... proporciona a la hacienda real dos veces más ingresos que la India británica con una población cinco veces mayor al erario". An equally careful reading of Pierre Vilar's *La Catalogne dans l'Espagne moderne* indicates the key role of the Mexican colony to what was perhaps Spain's most dynamic regional economy at the end of the eighteenth century, that is, Mexico's role as importer of Catalonian wines, brandies, textiles and paper, and as exporter of silver.

No doubt the Insurgency affected mining operations, particularly in the form of labor shortages and inadequate maintenance and pumping operations. Yet the production peak came perhaps a decade before the outbreak of revolution and there are reports of this period which affirm the apparently inexorable rise of cost levels reducing the profitability of most Mexican mines. A long-term examination of the mining sector, sweeping from the late eighteenth to the late nineteenth centuries may lead to the conclusion that there was in fact a mining crisis antedating 1810 and that the causes of collapse or contraction in the period 1820-1880 were less political and more technological.

The trajectory of the Mexican Revolution and the revindications demanded and sometimes achieved have lead historians often to view the movement as a revolution by peasants. No economic historian can deny that it was a peasant revolution; but it was this and something more. No doubt the grievances of Mexican peasants in 1910 go back in part to the land policies and practices of federal, state and local governments from 1857 onward. However, we cannot overlook that land concentration, rural unemployment and under-employment and just plain rural misery were clearly recognized and criticized at the end of the eighteenth century and thereafter in the nineteenth. Yet it seems to me that in our examination of the origins of the peasant contribution to the Revolution, we overlook the fact that the development of the Mexican hacienda in the late nineteenth century was closely linked to the revival of the Mexican mining economy after about 1880 in much the fashion that in the eighteenth century a developing mining sector had its direct linkages to the hacienda. When the economic historian views the Mexican economy under the Porfiriato he cannot help concluding that in terms of expansion of output, sources of investment flows, imports of technology and the creation of necessary infra-structure via railroads, the growth of the mining and petroleum sectors of the Mexican economy is another classic example of an export economy of an enclave type. Thus we may view the Mexican Revolution as the first of the twentieth-century upheavals in the neocolonial or Third World to uproot and export economy which, among other effects, exaggerated and exacerbated the secular agrarian problems of Mexico. At the risk of oversimplification, mining has been to Mexico at least until 1930 what sugar has been to Cuba in recent times.

One final example of the need to study long-term trends rather than discrete phenomena may be drawn from agrarian history and agrarian historiography since 1910. No one need be reminded of the vast literature of Article 27, the hesitant and often contradictory policies of the 1920's and in the 1930's massive land redistribution and the increase in the number of ejidos, in the overwhelming majority individually operated ejidos. A situation that appeared well on the way to solution by 1940 hardly seems so by the 1960's. Now the ejido program, we are informed, is in crisis and we are overwhelmed with statistics on rural poverty, rural unemployment and underemployment, rural illiteracy. To crown the calvary of agrarian reform, there are hard quantitative data to prove that in 1960 over 3.3 million rural Mexicans or 53% of the rural population were landless and that there were as many haciendas as in 1877. The quantitative and qualitative changes induced by the Mexican Revolution in the countryside cannot be denied. Yet the historian must be permitted his moment of cynicism when he dips into the literature on the contemporary rural scene of Mexico and encounters references to *grandes terratenientes*, *grandes propietarios*, *burguesía rural-comercial* and countless references to *campesinos sin tierras*. Since historians are tolerated by society for their possible moments of insight and not their cynicism, I suggest that a long-term perspective of Mexico's agrarian structures and agrarian fluctuations, a view sweeping back to the late sixteenth century, leads inescapably to the conclusion (or is it hypothesis?) that in 1969 Mexico's rural conditions are the product of more than four centuries of capitalistic development in agriculture, of the sometimes slow, sometimes rapid but always inexorable expansion of private enterprise into the Mexican countryside. In fact, this is what Clark Reynolds argues by suggesting that we look at the basic, unchanging economic and social patterns which persisted from the Porfiriato to the era of Cárdenas, at least. When he urges that we determine "the limits of economic behavior under certain conditions", I deduce that he proposes that we review the way in which structures and interests associated with them blocked significant economic change.

THE DIRECTION OF FUTURE STUDY

What the papers of Florescano and Reynolds share are a common insistence upon quantitative verification, periodization reflecting economic trends, and upon a conceptual framework. Both stress the need for methodological innovation; yet neither one is specific about defining a conceptual framework. Reynolds argues against what he calls "simple dialectical materialism" and for an examination of what he describes as a "multidimensional interaction of social, political, economic and even psychological variables". It is not clear, however, whether this conception deals with dialectics or interplay. What is encouraging is that an economist and a historian agree that we need a framework incorporating both economic and non-economic factors.

Such a framework must meet a number of criteria to have the widest applicability. First, it must grow inductively from the historical pattern of Mexico; and, second, it must permit relevant comparison with the growth patterns of other Latin American nations, of the so-called Third World and ultimately of Western Europe and the United States. In other words, only into a very broad framework can we fit logically and correctly small bits oldstone to form a large and coherent mosaic. That mosaic will never adequately be achieved by indiscriminate borrowing of reference framework and methodologies fashioned for different realities.

As at other times, so today social scientists find that the needs of their era shape the questions they ask of the past. The questions in turn grow out of distinctively different national historical patterns. In the United States, racism, poverty and imperialism have led to one syndrome of questions; another syndrome emerges in England where the promise of what was once the world's foremost industrial nation seems blighted. In Mexico, the shortcomings of the Revolution receive more attention than the gains perhaps because all revolutions fail in some degree. If I interpret accurately the uneasiness of historians looking at Latin America's past through the prism of western Europe's history, and of economists perplexed by Latin America's inability to close the gap between underdevelopment and development, that uneasiness may be located in the growing perception that Latin American conditions have been and still are different from those of Western Europe, that they may be characterized as typically colonial or neo-colonial. This explains the current vogue of the concept of dependence in Latin America's political, social and economic literature. For dependence seems to provide a general concept of backgroundness. I would refine that general concept slightly and propose that economic historians move toward an examination of the origins, patterns and stages of capitalism in dependent areas to arrive at a definition of colonial or peripheral capitalism.

To the weaknesses inseparable from capitalism developing in a colonial economy may be traced the failure of industrial capitalism to take deep root in Catalonia in the eighteenth century. Likewise, the inadequacy of data and indiscriminate application of a frame of reference developed for an area outside of Latin America may explain the confusing interpretation of the Mexican Insurgency. To Silvio Zavala the movement "esbozó . . . la revolución burguesa en un país señorial" and to M. S. Alperovich it created "condiciones muy favorables para el desarrollo de las relaciones económicas capitalistas y para la incorporación de México al sistema económico mundial" and was "en esencia una revolución burguesa anti-colonial" with "un carácter antifeudal". Later Alperovich argues that at independence, Mexico had solved "una de las tareas de la revolución burguesa", that the Plan de Iguala's guarantees blocked "una serie de transformaciones de carácter antifeudal" and that independence "no condujo a una transformación radical de la estructura económico-social de México". * Zavala and Alperovich are not the only historians to appear confused by the Mexican insurgency, by the wars of independence or by the Mexican Revolution. The confusion probably arises from the fact that in Mexico as in Latin America a variety of stages of capitalism have long co-existed symbiotically. Could it be otherwise in a continent conquered by Iberian entrepreneurs leaving a metropolitan economy already colonialized and who perpetuated in America relations of dependence?

* In Ricardo Levene, ed., *Historia de América* (Buenos Aires, 1940), VII, p. 10; M. S. Alperovich, *Historia de la independencia de México (1810-1824)*. (México, 1967), pp. 277, 279, 281.