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COMMENTARY

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Although the terms, political history or political historiography, feature in the title of this session and both papers, I am relatively disturbed that neither author has bothered to clearly define his own meaning of political history. Professor Perry avoids the problem completely; Professor Womack often refers to “power”, “study of government”, and “historical disposition of power in Mexico”, without clearly trying to show *where political history might end and economic and social history might begin*. Although both authors might believe an attempt to define these terms either impossible or counterproductive in relation to space and time, the lack of clear definition limited the overall coherence of both these papers.

My own approach would have been to confront the problem by dividing Mexican political history into two blocs, the Marxist or materialist, and the traditional. The historian following the materialist approach seeks to establish the relationship between political history and the social and economic interests of its participants. He would argue the impossibility of understanding the political activities of a power oriented group without a thorough understanding of their economic interests. (Professor Womack shows perception of the importance of this interrelationship in his brilliant study of the Morelos aristocracy on the eve of the Revolution and in his brief allusion to the importance of the connections between formal power and social power.) The traditionalist on the other hand usually sees the political process occurring in a more pluralistic context often independent of social and economic motivations; thus the leaders or the parties act more from a desire for power or for purely personal reasons rather than to gain, or protect, vested economic or social interests. This latter approach has often led historians of Mexico to view the De la Huerta rebellion, the Escobar rebellion, or the Calles-Cárdenas split purely in terms of the political ambitions of the individual participants without trying to relate their activities to contemporary social and economic developments. This methodology limits the validity of some of the important North American and Mexican work on the Mexican Revolution; such as the general Studies by Parkes, Valadés, Taracena, and Tucker and the political monographs by Meyer, Blaisdell, Ashby, Dulles, and numerous Mexican historians.

Although most historians of Mexico might desire to establish a closer identification of economic and social power with the political they face staggering problems in doing so. In Mexico where most important politicians describe themselves as “Revolutionaries” the records of financial holdings of public figures are not open to public scrutiny. This leaves us with the problem of trying to use the meagerest of evidence to explain the economic motivation and class bias of political leaders. Such superficial evidence as a villa in the Lomas de Chapultepec or a ranch in Michoacán

do not prove corruption or even great wealth. Furthermore, many Mexican leaders have certainly acquired their wealth legitimately after leaving office; there would be no justice in citing possessions often obtained after political power was lost. Yet recently both Womack and Cockroft have shown the results of imaginatively applying available economic information to political developments.

Professor Womack has pointed out the vast number of articles, books, dissertations, and documentary collections on Mexican political history which have appeared in the last decade, yet he chooses only to discuss a handful, mainly the better known books and one article. Obviously, he could not have read all of these but I would have been interested to have seen more of an attempt to bring some of the better articles and dissertations into his general discussion. Most important, I came to realize my own ignorance of the volume of this work and was struck by the urgent professional need for a working bibliography of Mexican history, including purely economic, social and foreign policy studies as well as history.

Both Perry and Womack call for a reexamination of the current periodization used in Mexican history and try to suggest refinements. Both also discuss the need for more studies comparing Mexican developments to those in other societies. However, neither paper convinces me that we presently have the depth of quality monographs that might allow us to attempt such refinements or comparisons. A case in point is the period after 1920 of which we know so little. Professor Perry suggests that "the oligarchic forces which characterized Porfirismo lasted until 1939". Given our knowledge of recent Mexican history such a suggestion seems premature. We have much to do before such suggestions can be proved or seriously discussed. Most of our reperiodizations or comparisons would have to be based on general works which, despite their high quality, still rest upon a bedrock of insufficient monographic material.

Perhaps the setting of priorities for future research is the most urgent task confronting us. Whatever our approach to history, whatever our goals in studying Mexico, we should try to define the most urgent tasks to be completed in the next decade. Perhaps we might stop studying Woodrow Wilson's foreign policy and the oil controversy and get down to studying Mexico's perplexing internal problems and institutional growth. Such priorities coming out of a meeting such as ours might help to avoid the duplication and wasted effort that has characterized some of our work. Professor Perry has capably suggested some possibilities for the Porfirian period. Professor Womack has concentrated on the 1910-1920 period in which he has done most of his work. Rather than duplicate or even discuss these suggestions, I will try to point out some important gaps in our knowledge of Mexican history concentrating on the 1920 to 1940 period; the decades most neglected by those currently interested in Mexican political history.

One task of immediate importance would be a close examination of the many evidences of social protest, armed or otherwise, which occurred far more often after 1867 than we once realized; such a study might begin with the movement around Manuel Lozada in Nayarit and include the Yaqui revolt in Sonora, the Caste Wars in Yucatán, various agrarian

revolts, the Liberal party, social catholicism, and Maderismo. With such a study we could then better judge Professor Chevalier's contention that the Zapata uprising in Morelos "was an explosion in a critical zone of the deep social disorder whose more noteworthy manifestations had been banditry . . . and above all the most uninterrupted succession of peasant and indigenous insurrections based essentially on agrarian issues". If Chevalier's thesis depicting the Porfiriato as a time of dynamic tension proves correct, we will no longer see the 1876-1910 period as a necessary respite connecting two violent anarchical eras. Perhaps we might then look elsewhere than Madero and the events of 1910 for the Revolution's starting point. Already Cockcroft, Wilkie and Womack have clearly shown Madero as a social conservative concerned mainly with political reform. Could not the real Revolution have begun with Lozada in the 1860's or Zapata in 1912?

The political historians with an interest in computers can have a vital role to play in making the Revolution more comprehensible to us. The important post-Revolutionary positions, national, regional, and local, can be isolated and those officials who held them can be listed. A team of researchers could then compile short biographies of at least some of those men as to birthplace, background, and career during and following the Revolution. The biographical information could be fed into a computer and the results would surely help to correlate the relationship between changes in the executive with individual political mobility, the regional basis of important power groups and the importance or lack of importance of the age differential in various governments. Perhaps this study would show us who really has ruled twentieth century Mexico. Professor Wilkie has tested the ideology of the Revolutionary governments by examining the federal budgets, but nobody has tested changes in personnel. I expect that the results might show us that Mexican politics particularly between 1920 and 1933 were more than a game of musical chairs.

Womack's excellent historical analysis of recent works of the period between 1910 and 1920 needs little elaboration. Yet there are some important possibilities which he has not mentioned. A study of the movement around Pancho Villa similar to Womack's study of Zapata would be extremely instructive. Particularly valuable would be a survey of Chihuahua on the eve of the Revolution with a special emphasis on the expansion of the vast estates of the Terrazas family. Another important contribution might be a study of Carrancismo between 1914 and 1920; at the present we know little about the "First Chief", his ideas, his relationship with his followers, and the true configurations of the power groupings which he apparently cleverly manipulated. Finally, one might cover Múgica and Blanco's attempt to carry out agrarian reform in Tamaulipas; Múgica's experience there might have colored his future career and the advice he later gave to Lázaro Cárdenas. Many other important political problems (in the Revolution's first decade) have not been studied but the gaps in the next twenty years really define the depths of our lack of knowledge of revolutionary Mexico.

At present we lack even one good political monograph on the 1920-1940 period. The best work, Lyle Brown's study of Lázaro Cárdenas and his manipulation of power, still remains unpublished. Both authors bemoan

the lack of political biographies of prominent Mexican leaders. Although such studies appear non-existent, what seems more urgent is not to write biographies but to identify if possible the political movements of the 1920's and decide how, if at all, they differed from one another. Once these movements have been clearly defined, they must then be studied as to social composition, objectives, and reasons for success or failure. Surveys such as these will help us to better explain the differences, if there are any, between the movements represented by Carranza, Obregón, Calles, De la Huerta, Escobar, Vasconcelos and Cárdenas. We must also decide whether the struggle for power between 1921 and 1933 was purely political and personal as Dulles implies or whether the often bloody disputes of these years signified a deep ideological or social split in the so-called Revolutionary family. Such a deduction might clarify the enigmatic role played by Plutarco Elías Calles who I believe showed a greater ideological consistency throughout his long career than many of his critics have yet suggested.

The role of the church in the 1920's and 1930's also remains largely unresolved. After an intensive study of the many polemics of these years one can not easily decide whether the bishops and their lay supporters really believed that the government threatened the church's existence or whether they hoped to use the constitution's anti-clerical laws as a pretext for a conflict which would cause a North American intervention and a counter-revolution. Another possibility often suggested is that Calles and Obregón frequently used the danger of church opposition to the government as an excuse for curtailing social reform. An examination of church leadership during these years might help us to more fully understand the relationship between social background and clerical resistance to the Revolution; it might also suggest that an important turnover in clerical leadership helped account for the church-state detente arranged under the government of Lázaro Cárdenas. The Cristero rebellion has been the subject of several monographs but we still know little of the socio-economic background of the Cristeros themselves. We could in addition formulate a study comparing Cristero leadership and ideology with that of the Sinarquistas to better understand the variety of Catholic opposition to the Revolution.

Alvaro Obregón's death in 1928 ushered in one of the most confusing periods in Mexico's political history. The only coverage of these years is in a few memoirs and the narrations by Taracena, Valadés and Dulles. These inform us about day by day events but shed little light on the realities of power. They do not help us to understand the extent of Plutarco Elías Calles' rule over Mexico or anything about his relationship with CROM, Portes Gil, Ortiz Rubio and Rodríguez. More important, no historian economic, or otherwise, has fully evaluated the full effect of the World Depression upon Mexico. Without such analysis how can we entirely understand the economic factors limiting political and social action and the desire for change which clearly influenced the rise of Cárdenas to the presidency. Finally, what can be concluded from Cárdenas' nomination in 1933? Does this nomination show us that Calles power has been over-estimated or that he made a wrong calculation in thinking that Cárdenas would be a malleable puppet?

Cárdenas' political period is somewhat more completely covered in several dissertations and it will be shortly covered by a book of essays by Brown, Wilkie, and Michaels. However, today we have little easily obtainable information of the political struggle of the 1934-1940 era. Books by Millon and Ashby cover labor-government relations but both lack profound analysis. Lieuwen's chapter on civil-military relations adds little to the earlier work of Virginia Prewett. We still do not understand the economic and social implications of the Calles-Cárdenas split, the reason for the quixotic Cedillo rebellion, the forces around Almazán and Ávila Camacho, and the 1931 *Shift of the Revolution to the right*. We know Cárdenas expropriated much of Mexico's land, but we have little idea of who owned the land and how they obtained it during earlier administrations. With so much unanswered in what is probably the Revolution's pivotal period how can we even begin to speak of historical synthesis?

Local political history, above all, has been the most sadly neglected. What little that does exist has been the result of local historians trying to glorify their home towns or states and in the mostly superficial volumes stemming from the Instituto Nacional de Estudios Históricos de la Revolución. Although we lack the resources, human and financial, to study every state or region throughout the Revolution, we can research particular problems in order to better comprehend certain important problems already touched on in this discussion. Michoacán, from 1921 to 1933 is the scene of events and personalities whose influence spread far beyond the states borders. Pascual Ortiz Rubio, Francisco Múgica, Lázaro Cárdenas, Benigno Serratos, Gildardo Magaña all interact within the state. Múgica's struggle with Ortiz Rubio explains much about the eclipse of the former and the latter's rise to the presidency; it also helps us understand the methods by which Álvaro Obregón so successfully ruled Mexico. In 1926 Lázaro Cárdenas as governor of Michoacán began a series of reforms that in many ways anticipated the national changes he initiated in mid 1930. He also acted as a lodestone for many young radicals seeking refuge from the government's increasing conservatism after 1929. Calles later replaced Cárdenas with the more conservative Serratos raising important questions about both Cárdenas-Calles relations prior to 1933 and Cárdenas' eventual rise to the presidency.

Another example of a state which served as a stage for events and personalities that far transcended its overall political importance is Yucatán. Both Salvador Alvarado and Felipe Carillo Puerto carried out important reforms supposedly benefiting the states' impoverished Maya Indians; yet the latter had to wait for the visit of Cárdenas to Yucatán in 1937 before they actually received their lands. A study of this area would enlighten us to the interesting careers of both Carillo Puerto and Alvarado but more significant it would show how a small group of wealthy landowners worked to successfully thwart social justice despite articulate local radical leadership.

A study of Tabasco under Tomás Garrido Canabal also might deepen our understanding of Mexico's revolutionary process. The controversial caudillo apparently neglected or abused agrarian reform while he terrorized the Catholics. He entered the government under Cárdenas but appeared to remain loyal to Calles thus losing his baliwick in 1935. A history of

his rule would divulge the relationship between agrarian reform and anti-clericalism as well as the diversity of those around Calles. Garrido's relationship with Archbishop Pascual Díaz also raises questions about the Cristero rebellion and the churchman who negotiated the truce.

Finally, a study of San Luis Potosí after the revolution would be of the utmost importance: First we could use Professor Cockcroft's study to compare the states social and economic structure before and after the Revolution. Second, we then could understand the rise and fall of Saturnino Cedillo whose history has meaning for Caudillismo and its role in the Revolution, agrarianism, Cárdenas' rise to the Presidency, and the church-state conflict. Third, by tracing developments in the state we would need to examine the roles played by Aurelio Manrique and Graciano Sánchez in their attempt to promulgate agrarian reform and political democracy in their home state. These suggestions of the inherent possibilities in the state and regional history are hardly exhaustive. Any of these studies could for example be related to Dr. Wilkie's poverty index to help us explain the lag in the economic development of certain states. It is certain however that if we are ever to understand power, both formal and social, in Mexico, we must know more about what was going on outside Mexico City.

I agree with Professor Womack that we historians of Mexico have made great progress in the past ten years. An impressive number of monographs has appeared along with articles, document collections, and dissertations. Yet too much still remains to be done. Perhaps it is now time for our better scholars to stop compiling overviews of the modern period and concentrate on less impressive but more needed in-depth studies of specific situations. Perhaps we must also stop allowing graduate students to persist in studying United States —Mexican relations and direct them towards Mexico's neglected internal history. Finally, many of us must stop narrating the struggle for power in México as solely a personalistic rivalry between ambitious politicians and begin to investigate the social, economic, and ideological factors which underly these rivalries. Yet here too we must avoid the temptation of attributing all political action to selfish economic motives. After all, man's actions are usually complex and traditional narrative must not be replaced by Marxist polemic.