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## MEXICAN POLITICAL HISTORIOGRAPHY, 1959-1969

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The building and uses of power still fascinate historians of Mexico. Despite the calls for attention to economic, social, and intellectual history, political history remains our favorite enterprise. We go on telling time here by how a government goes.

My purpose is to discuss the work on Mexican political history since the meeting in Austin in 1958. I do not intend to deliver a bibliographical essay, there already being several such essays with passages on politics.<sup>1</sup> (We need more essays, focussed on specific questions. But those that we now have are good guides.) Nor will I comment on popular history, vulgarization in the French sense, because I think that by now our work is professional, that in intent and in practice professional history is different from popular history, and that our primary obligation in a meeting of professionals is the evaluation of how we have done. Nor will I confine my discussion to the work of younger historians. This would have an interesting point: presumably their work is least familiar in the field and yet of most potential effect there, since they may reshape the lay of the intellectual land during the next generation; we would like to see now what their ideas on political history are. The difficulty is in deciding which historians are young. Most of us juniors and seniors have behaved as if we were still in the 1920's as if we had never heard that we should study several languages, linguistics, sociology, law, statistics, geography, theology, economics, philosophy, ecclesiology, demography, or political science, as if we could not imagine revisions. What I will try in this discussion is simply to determine the character of the professional political history of the last decade, to clarify its main accomplishments and its main problems.

The literature is now considerable. As a body it has grown much larger than it was 10 years ago, when we had little to recommend to scholars in other fields who wanted to read in ours, and even less to recommend to scholars in other sections of our own field, in the

<sup>1</sup> See the articles by Stanley R. Ross and Luis González y González in the *Handbook of Latin American Studies*, Nos. 23, 24, 25, 26, 28, and 30. See also Martín Quirarte, "Historia Política: Siglo XIX", *Historia Mexicana*, xv, 2-3 (October 1965-March 1966), 408-424; and Stanley R. Ross, "Historia Política: La Revolución", *Historia Mexicana*, xv, 2-3 (October 1965-March 1966), 425-433, and "Introduction" and "Additional Readings" in *Is the Mexican Revolution Dead?* (New York, 1966), 3-34, 247-255.

economic or social history of Mexico, who wanted to read about the politics of their periods. By various criteria 60 to 100 books on Mexican political history appeared in the last decade in Mexico, the United States, Great Britain, and France, not to mention eight or 10 documentary collections, 60 to 80 articles in professional journals, and probably 40 or 50 doctoral dissertations. If this is small compared to the work on American or European political history, it is nevertheless a great growth in our field.

The reasons for the surge in the production of political history are, I think, clear. Several major political anniversaries occurred in Mexico in the 1960's—the fiftieth anniversaries of the Maderista, the Zapatista, and the Constitutionalist revolutions, the Sovereign Revolutionary Convention, and the Constitutional Convention, and the hundredth anniversary of the French Intervention—each creating a market for books in the events celebrated, each giving Mexican authors a chance to publish, and Mexican publishers a chance to sell. Also the United States government took a special interest in Mexico during the last decade, in part because of the general American worry over the effects of the Cuban revolution in Latin America, but in particular because of the official desire to show that serious revolutions (Mexico's being the prime example) could take place “democratically”, as the official phrase went, without help from the Russians, it meant. This was good advertising, though bad history, and in the allotment of new American resources for the study of Latin America it enabled us Americans interested in the historical disposition of power in Mexico to seize a good share of the money, jobs, and time for research. (I must insist on our independence from Camelot that our work has been free of official dictation, that our insights and mistakes have been our own, that as historians we have not cared to prove the “preferability” of the Mexican Revolution but only the truth about it as we see it, and that as historians we have hardly propagated the myth of American benevolence here.) But most important for the increased production was the professional work already established in the field during the previous decade, the work that we could depend on, the work out of which our work could grow.<sup>2</sup> Without this basis, without its professional sobriety and integrity, we could not have produced much of value. The Mexican volumes for the anniversaries would have been only merchandise, and the American volumes on the Revolution would have been only propaganda for the Alliance for Progress. Because the field was already

<sup>2</sup> For the previously established work, see Carlos Bosch García, *Guía de instituciones que cultivan la historia de América* (México, 1959), and Robert A. Potash, “Historiography of Mexico since 1821”, *Hispanic American Historical Review*, XL, 3 (August 1960), 383-424.

respectable 10 years ago, we have been able to produce the respectable growth of the last decade.

As a body the recent writing on Mexican political history is traditional in style, *which is in part a new development*. I do not mean that it has all come out of the same school, or that it has all gone along the same lines of interpretation, for it has been a literature of diversity, not to say disparity. Nor do I mean that its arguments have not been original, or that its conclusions have not been novel, for in fact it has also been a literature of discovery. But I do mean that as a body it fits the traditional canon of political history. Let me note two of its traditional features. One is the assumption, which is new, that the political history of Mexico is a comprehensible question — comprehensible because, the assumption now is, life in Mexico in no period has been just chaos, irrational and absurd, but has always been a series of patterns, usually obscure but sometimes definite, anyway accessible to our understanding; and a question because life in Mexico, the assumption is, has not been a mere automatic evolution but rather a struggle that has taken surprising turns that require our investigation. (The reason for the new development is still in the dark, but I would guess that it results from the new sense of maturity in contemporary Mexico.) On the assumption that our work is on a comprehensible question we have sorted ourselves out from the popular historians, who alone go on treating periods of the past as chaos.

The other traditional feature of our recent writing that I would note is the assumption, a classical one, which we inherited and have carried on, that politics in Mexico (and elsewhere) is a formal activity — that the study of politics is the study of government, or of institutions and individuals that have directly to do with government. With rare exceptions we have studied power here when it has taken territorial shape and partaken of sovereignty, and rather ignored it in its other, vaguer moments, in families, for instance, or in business, or the Church, which we leave for the anthropologists to study.

Broken down into categories of coverage, the literature is very irregular. During the last decade there were no professional attempts at a grand synthesis of the politics of the whole national epoch, which I take as a sign of wisdom in our profession, a recognition that no one, however ambitious, is yet capable of dominating all the new and old monographs. There were scattered books on specific matters running through the whole epoch, like Turner's on nationalism or García Cantú's on conservatism, but they did not impose a new organization on our section of the field. There were also a few general books, like Bravo Ugarte's or Cumberland's but they did not reorganize our section either.

As for the differing coverage of different political periods within the national epoch, the period getting most coverage was still the Great

Revolution of 1910-20 —especially the years from 1910 to 1915. There was only one professional attempt at a synthesis of the Revolution, that of Valadés, a massive and admirable enterprise but (I think) nevertheless dubious. It was actually more a multi-volume monograph than a synthesis; when synthetic organization did take hold, it was too stiff, and the data got loose again. Valadés's rambling through the Revolution evoked the Revolutionary experience, as his earlier command of the Porfiriato evoked the Porfirian experience. But no more in history than in other arts do the aesthetics of imitation convey conviction.

The normal approach to the revolution was still explicitly monographic —the study of specific phases or movements or men, more or less to the neglect of other issues. As in a blunt fashion this was the approach of the popular historians of the Instituto Nacional de Estudios Históricos, so in a sophisticated fashion it was the approach of professional historians —Mexicans like Amaya C. writing on the Revolutionary Convention or Valadés writing on Madero, and Americans like Blaisdell writing on the Magonistas in Baja California, Clendenen on Villa and the United States, Michael Meyer on Orozco, Quirk on the Revolutionary Convention and on the American Intervention in Veracruz, Sherman and Greenleaf on Huerta, and me on the Zapatistas; in the same sophisticated fashion it was also the approach of Calvert to the Anglo-American involvement in the Revolution, and Katz in his chapters on the German involvement.

This approach, I think, is still the best for us. For though we now have many monographs on the Revolution, we need several more before we can make a good case about it. What, for instance, did the industrial working class do from 1910 to 1920, both the organized and the unorganized workers? Reference to the Casa del Obrero Mundial and the Red Battalions is not enough. We need to know if workers themselves ever tried to seize a factory or a mine and manage it as their own, if their strikes were often political, if the workers of one industry were more political than those of another, and if so why, for economic or ideological or other reasons —all this and much more we need to know, in order to say whether industrial workers made a difference in the disposition of power during the Revolution, or even tried to make a difference. And what about monographs on Carranza's government preconstitutional and constitutional, or the Constitutional Convention, or the Revolutionary governments in certain states like Oaxaca or Sonora or Tamaulipas? The suggestion of these and other new topics for research can plunge us into manic depression —so much to do, too much to do. Even the episode most studied so far, the Sovereign Revolutionary Convention, needs more analysis. The first national assembly of revolutionary characters, stacked with licenciados but still in

its composition the most popular of all the national revolutionary conventions, rowdy and confused, much more deeply soaked in popular hopes and fears than the Constitutional Convention, it was the closest that plain Mexicans came to deciding how they wanted Mexico to be. With the documents that Barrera Flores has collected, we can now get a sense of what they thought their revolution was and what they tried to make it. But the analysis is still there to do.

Saying which of the other periods received most coverage during the last decade is difficult, depending on whether we call a pamphlet a volume. The French Intervention and the Mexican Resistance certainly had much treatment, again wisely (I think) in monographs. But the treatment was not as extensive or as good as it should have been. The publications from the Historical Section of the Mexican Society of Geography and Statistics are, I think, almost all disappointments. More helpful are the collection of essays on the Intervention published by the Instituto Francés de América Latina, the book by Dabbs on the French Army in Mexico, the volumes of French documents that Lilia Díaz collected, the works on Juárez and Zaragoza, and the sets of documents that de la Torre Villar is now having published. But we still need monographs on how the Resistance operated locally, not only in military but also in political terms, how Juárez even in El Paso del Norte and even in a political crisis could retain authority over loyal Republicans throughout the country, how the Republican army redeveloped after its initial collapse, and so on. Most of all we still need a study of the most interesting episode during the Intervention, which is the Mexican Collaboration. No doubt this is a sore spot in Mexican history, which explains why we hesitate to touch it, but the fact is that many honorable Mexicans collaborated with the French and served the Emperor Maximilian—probably in severe tension but nevertheless respectful of a foreign solution to their country's misery. About this episode we still have only the foggiest notions. But we cannot understand Mexican political history before 1863 or after 1867 unless we understand the politics of the years in between. There is one recent piece of exciting writing on the Intervention, a seminal essay that may generate a comprehension of the Resistance and the Collaboration, the essay by Chevalier on the sociology of Liberalism and Conservatism. If Chevalier's earlier piece on *Zapatismo* put agrarian movements in a context explaining Mexico's social history in the 19th century, then this piece on Liberals and Conservatives may put the Intervention in a context explaining Mexico's political history in the 19th century.

The period since around 1920 also had much coverage, in syntheses and in monographs. Most of the work was that of political scientists, economists, and sociologists, not that of political historians. Most of the interest was in how systems, institutions, and agencies function,

not in how they developed. In this work there was a tacit admission that the explanation of a function requires a brief relation of its development, but the purpose was still to satisfy the curiosity of political scientists, economists, and sociologists, not the curiosity of political historians. The work of these specialists in other fields remains important to us, however; some of them showed better historical imagination than we historians did. It honors us to discuss their work together with our own.

The syntheses were impressive. The arguments of Brandenburg, Cline, González Casanova, Padgett, and Scott, with their different emphases on lobbies, mobility, presidentialism, elites, marginality, are now a regular school for political historians. We have here nothing comparable to the arguments on American or European politics. But we do at last have a serious debate going on, informed as never before and in intelligent control as never before. Therefore we may expect more precise and more persuasive syntheses and more pertinent monographs.

The monographs, without anniversaries to concentrate them, varied widely in focus and in quality. They ranged from González Navarro's thorough and thoughtful study of agrarian organizations and Wionczek's fine study of economic nationalism down to Millon's indefensible hagiography of Lombardo Toledano, from Bazant's careful chapters on the foreign debt and Lorenzo Meyer's excellent book on the struggle over oil down to Dulles's credulous chronicle. Most of the monographs were sound productions but not strong enough to change the shape of the field —Ashby's book on Cárdenas and the CTM, Brothers's and Solís's on official financing, Cancian's on Indian villagers, Cronon's on Ambassador Daniels, Lieuwen's on militarism, Moore's on official financial institutions, Olivera Sedano's on the Cristeros, Orive Alba's on irrigation, Ruiz's on education, Schmitt's on the Communist movement, and Shafer's on planning, to mention a few stout examples, and Ezcurdia's book on the PRI, Kling's on Monterrey lobbies, and Morton's on female suffrage, to mention a few thin examples. In this middling range I think the most interesting effort was Wilkie's, on federal budgets and expenditures. The book had a tremendous impact; it is important for the study of Mexican political history. That the analysis had faults —the easy reliance on official statistics (without research, difficult but not impossible, into the economic conditions during the period), the mechanical identification of ideology with spending (without taking politics into account), the brief glance at inflation (without making allowances for its different effects on different items of budgeting and spending), the practical supposition of Mexico's fiscal autonomy (without locating the country in shifting international currents) —this does not lessen the book's importance. Simply to note that it will move us into serious research

on Mexico's central government is to indicate the strength of its contribution to our field.

The monographs we still need on this period are numerous. Topics obviously waiting for treatment for 20 years remain virgin. At least we can wonder where the histories are of the Labor party, the Vasconcelista movement, the PNR-PRM-PRI, and the several federal elections, and where the biographies are of all the ranking politicians. The plea that entry into private archives of the period is difficult, often out of the question, does not convince me. I wonder then why we have not consulted archives we could have entered, or even public records like those that political historians of other countries have put to good use.

Attracting less coverage during the last decade was the Porfiriato, and that again in monographs. For this period the professional work was mainly in three volumes on Mexico's international politics, the two magnificent productions of Cosío Villegas on Porfirian foreign relations with Central America and with the United States, and the rich study by Katz on the German relations with Mexico. Here we have major refinements in our knowledge. But the recent treatments of Porfirian domestic politics were few and more puzzling than enlightening. Bernstein, despite his promise, hardly helped us to understand the politics behind Porfirian mining legislation. Cockcroft, although he provided much interesting information about the opposition centered in San Luis Potosí, hardly explained why the dictator tolerated its appearance or how young sycophants of his could have joined it. Gutiérrez Santos, absorbed in the defeats of the Porfirian army, hardly outlined its construction. Lemus, who reanimated the animated Bulnes, hardly deciphered his odd political career. Niemeyer, concentrating on Bernardo Reyes as the public man, hardly revealed to us the *presidenciable*.

Again we need many more monographs —especially on institutions, both formal (Congress, the *jefatura política*, the Law School, the Judiciary, the rural police, the army, the press, the rural school, the state governorship and legislature, the Ministries), and informal (the científico clique, the Porfirista circles, the Jockey Club, the groups in opposition, the foreign colonies), to mention only a few examples. These studies I think we need more than biographies, which are liable in this period, I think, to distort our image of how the structure of power developed. Now that Díaz's archive is open, it is incumbent upon us to do justice to the man and the age we have named for him.

Even skimpier was the recent coverage of what I would call the period of the Bourbon Republic, from 1821 to 1854. But the coverage, in a synthesis and in a few monographs, was high in quality. (Why this is so, and whether we should devise a motto —the less the better —I cannot say. But I would guess that we should not flaunt a poverty of aspiration.) The synthesis was Hale's work on Mora and the age

he called the Age of Liberalism. It is, I think, another important book for the study of Mexican political history, nicely organizing its period. Though its main argument on Liberalism was not original, Tena Ramírez having already advanced a similar thesis, and though the implication was wrong that Mora cast as much weight in political action as in political thought, the book did give an analysis of Liberalism that explained its appeal and its frustrations—not only in Mora's time but in the times after him. If disagreements will inevitably persist about the period, at least we now have terms in which to debate them. Another attempt at synthesis, that of Reyes Heróles, did not (I think) succeed, falling between mere exposition of Liberal positions and mere assertion of Liberal cogency. The monographs have been among the best in the field—Costeloc's neat study of the Church as a financial agency, Potash's superb book on the Banco de Avío, and Reyes Heróles's sharp essays on Mora, Otero, and Zavala. Again our cry is for more monographs—on the masonic clubs, the cathedral chapters, the seminaries, the officer corps, the institutos, the familial connections among Liberals and Conservatives and between Liberals and Conservatives, the politics of strategic states, and so on. Our hope is only that the high standards now established for this period do not collapse in the new research and writing.

On the Reforma and the Restored Republic, after the flurry of anniversary volumes in the 1950's the coverage during the last decade was scanty. The publication of documents (like the Comonfort papers) is a valuable service we all appreciate, but it is only the commencement of our task. With all the primary and secondary materials that have been available for the study of these periods, we should have several new syntheses and monographs to discuss. But we have only a few, like Fuentes Mares's, "welcome additions", as we say in reviews, but no great shakes in the field. Most embarrassing is that we do not even know what the political continuity was between the Reforma and the Restored Republic. The legend is that the first climaxed in the second, but without the monographs we still cannot say *en cristiano* what the climax amounted to in terms of power.

Almost all our recent work has painfully conspicuous shortcomings. It has suffered from our traditional assumption from the classics that politics is governance. This formalism is snobbery, and it has impaired our professional vision. We know that manifold relations of power have existed in Mexico besides those between the government and the governed, that formal politics is only one kind of politics, and that politics is only one kind of social action, yet generally we have persisted in researching and writing as if we could understand formal power without understanding social power. Few among us resemble the model described in the preface to a recent book on 19th-century European politics, the scholar who proceeds "not as a political scientist with a



penchant for history but as a social historian interested in perceiving how change in governmental and political institutions affects and expresses social change".<sup>3</sup> Because we have remained traditionalists in defining Mexican politics, we have missed seeing it as a theme in Mexican social history. Our work has also suffered from our acceptance of the established periods, 1821 to 1854, 1854 to 1876 to 1910, and so on (not to raise questions about how long the Revolution lasted). This rigidity has cramped us badly. We know that the history of power does not start and stop like tenure in office, yet generally we have hesitated to listen again to the past's political rhythms. Because we have heard the same old beat, we have danced the same old steps. The worst shortcoming that our work has suffered, I think, has been our astonishing failure to try a comparative method. We know that for two generations historians working on European countries have taken for granted that they should make comparisons, yet generally we have gone on as if we could rightly interpret Mexican political history without keeping in mind that Mexico is in Latin America. The result is that we have often misconstrued local or national developments of power in only local or national terms, ignorant of the fact that they were local or national versions of continental developments requiring interpretation in continental terms. Because we have studied only Mexico, we have learned less about Mexico.

All this production during the last decade is nevertheless a substantial accomplishment. Given the inherent difficulties of the field, the still relatively few men regularly researching and writing on political history, and the still relatively scarce resources to subsidize them, we have consolidated the professional temper of our work and dutifully extended its application through layer upon layer of rhetoric to all the recognized periods of power in the Republic's history. Our shortcomings, I think, are not very worrisome. Once we are aware of them, we can correct them. And in time, I imagine, we and the historians we train may move—not in legions, because no one would support so sizable an army of scholars here, but at least as the point-men for squads—to study power in Mexico in its social dimensions, to define new periods for telling time in the country's political history, and to place the country in the continental context where it belongs. Then we could conceive of questions we cannot dream of now. At the moment we can only record the limited but fairly solid and promising advances that we have made.

Imbedded in this accomplishment are problems much more dangerous for us than simple shortcomings in conception and method. They are philosophical problems of history, which, unless we understand them, will impede our work and sap our confidence that we can do

<sup>3</sup> Eugene N. and Pauline R. Anderson, *Political Institutions and Social Change in Continental Europe in the Nineteenth Century* (Berkeley, 1967), vii.

better. Mexican political historiography as a whole is now in a state of analysis like that of medieval European historiography two generations ago, when Marc Bloch wrote that "analysis" can only be transformed into "synthesis" if it has had the latter in view from the beginning and has been deliberately designed to serve that purpose".<sup>4</sup> Unless we understand the philosophical problems of designing historical analysis for historical synthesis and of turning analysis into synthesis, we will soon find ourselves in the most frustrating debates.

The problems express themselves in certain contradictory mistakes that we often make. One is to treat episodes happening at one moment in Mexico's past as no more than preparation for other episodes happening at a later moment in the country's past, as if we meant that the first episodes had happened so that the second could happen. This is the mistake of "precursorism", the Latin American counterpart of the notion now current in the United States that all history is the history of modernization, both mistakes being variants of what I learned from my teachers to call "the Whig interpretation of history". I am not arguing generally that history is not in the great chain of being, "the seamless web", as we are fond of saying when we cannot explain how a change happened. Nor am I arguing in particular that some men did not try and fail at ventures that other men later tried and succeeded at. Granting that time is coherent and that men do carry on heritages, I am arguing that the past has a right to our professional respect—that as professionals we are under the obligation of seeing how men made history as they really and bravely made it, without knowing beforehand how it would turn out.

Let me cite a couple examples, to show that "precursorism" infects our explanations of not just one period but the whole epoch. (I will not cite the most blatant example, the official argument that Mexican governments represent the Revolution of 1910, as if Madero had revolted to the cry—"On November 20, 1910, let us become the precursors of the PRI!" Take our ideas on Liberals and Conservatives in the 19th century, from the 1820's through the 1860's. Without professionally argued evidence we still write as if the gomezfaristas of 1833 were embryonic puros of 1856, who were themselves homuncular juaristas of 1865, and as if Lucas Alamán were the natural father of Juan Nepomuceno Almonte. We should at least wonder whether the developments within the parties amounted only to crystalizations of already established patterns, and whether the conflict between the parties was the same in the 1860's as in the 1850's, or in the 1830's. This is not necessarily to imply that partisan traditions were weak, or that new generations developed new disputes. But maybe the terms of Mexican

<sup>4</sup> Marc Bloch, *Land and Work in Medieval Europe, Selected Papers* (New York, 1969), 72.

political development did change profoundly from 1821 to 1867. Actually we do not know.

Take for another example our ideas on the opposition to Díaz from around 1900 to around 1910, in particular Cockcroft's recent book on the Liberal clubs. Here is the word itself —the growing opposition among young intellectuals in the 1890's and early 1900's was the action of "precursors", whose cause the Maderistas took as their own, which cause the Zapatistas and the Constitutionalist took as their own, so that is finally flowered in the Constitution of 1917. I doubt that this conveys much sense of what really happened. I am not denying the courage of those who spoke out against dictatorship and even organized clubs to act on their feelings, nor am I denying their claim on us to remember them in respect. I am also not denying that in the Liberal clubs many young men learned to think programmatically about free politics, social welfare, and national pride, nor am I denying that individuals who were Liberals in 1901 or 1906 were later Maderistas and Constitutionalist. What I do deny is that the connection of the facts is easy, that it is like a flow of water from one spring through one channel to the sea. We cannot imagine Ricardo Flores Magón and his cohorts saying, "Let's oppose Don Porfirio so that we can get new articles 3, 27, and 123 in a new constitution in 1917". We cannot even imagine them saying, "Let's oppose Don Porfirio so that what will happen from 1910 to 1920 will happen". But this is what treating the Liberals as "precursors" boils down to. To treat the past as a series of precursory events is to lose the sense of the past as it was for the people who lived it —a series of difficult presents, one difficult present after another.

Another mistake we often make, contradictory to "precursorism" but born of the same philosophical problems, is to treat one period as radically different from periods before and after it, as if the codes of living and understanding were entirely different from one period to the next. This is the mistake of "age-ism". Certainly "times do change" in Mexico, as elsewhere. Certainly there have been stages and periods in Mexican political history, and logically therefore differences between them. And certainly the political alterations from one period to the next have been great. But it is not certain that the essential patterns of one age are thoroughly different from those of another age, that time cracks when one age ends and another age begins.

Let me again cite a couple examples, again to show the variety of the mistake. Take the 1920's. They were the first years of the new revolutionary age, the new nationalist state, the new political organization of the masses. But were not the men then politically active in the new republic all products matured in the Porfiriato? Consciously or unconsciously did they not revive many old habits of political thought and action, bred into them during the Porfiriato? In this perspective,

despite official declarations of socialism, is the official encouragement of capitalism so bizarre? I am not arguing that Mexico in 1926 had not changed from 1906, but only that the change was a matter of elaboration as well as a matter of revolution, that the nature of the change was not simple but complicated and subtle.

Take the Porfiriato for a more familiar example. Supposedly in 1876 a radical change occurred in Mexico, separating the Age of the Republicans from the Age of Don Porfirio and bringing in the *científico* theory and practice of politics. Certainly the Mexico of 1892 was quite different from the Mexico of 1872, not least in the prevailing political attitudes and procedures, in particular because of the *científicos'* rise into national authority. But did Mexico not become what it was in 1892 with the full participation of many old republicans, who made the country into a place where the *científicos* could rise into authority? If the change was radical, how do we explain Carlos Pacheco, heroic republican soldier, rich porfirista minister, patron of science in agriculture and industry—all before the *científico* entry into national authority? Was not the Republic a premonition of the Porfiriato?

These contradictory mistakes of “precursorism” and “age-ism” derive from another mistake we often make—which is to mistake the explanations of social science for the explanations of historiography. Both social scientists and historians study processes, a modern concern with movement distinct from the ancient concern with great deeds. But the process the social scientist studies is operational, whereas the process the historian studies is after all chronological. Linguistically the work of explaining an operation is distinct from the work of explaining a chronology. The essence of social scientific explanations is timelessness, its essential categories being regularity and universality. Its paradigm is a law. The essence of historical explanation is time, its essential categories being endurance and change. Its paradigm is a narrative. As we have to our credit learned the tricks of social science, we have to our confusion tried to treat history as social science in time—as social science factored through time. This is a gross mistake, to confuse categories, to confound explanatory laws and explanatory narratives. Time is the condition of history, not a factor in it, which is an image leaking into history from mathematics and eroding our sense of what we professionally should be doing. The mistakes in our language reveal the problems of our field. Time in itself does not, as we often write, make a political difficulty easier or harder. Time only tells—who did make the difficulty easier or harder.

It is beyond me here to go into the dialectics of induction and deduction, the dispute between methodological individualists and methodological socialists, or the contention between the idealist proponents of “understanding” and the materialist proponents of “explanation”; on this I will only recommend a recent book by Arthur C. Danto

analyzing the philosophy of history.<sup>5</sup> But let me cite one example of how the mistake of confusing social science with historiography has affected our work. The example is an essay that I think is exciting and seminal but loaded with a problem, Chevalier's essay on Liberalism and Conservatism. What we as political historians want is an essay in political history —the definition of the historical subject, a discovery of its origin, an account of its development, and a conclusion about it at a moment manifestly crucial to it, with maybe an epilogue suggesting its subsequent fate. What we have instead from Chevalier, as he subtitles it, is "An Essay in Political Sociology and Geography". In fine French style it broaches the political history for a illuminating sketch of the social origins of both parties, their geographical distribution around the country, and the local clans into which they organized for action. But it does not continue into political history, to trace the parties' developments, to explain their chronologies. What we as historians miss is the narrative —of how the escoceses became the centralists, the mochos, the greens, and the monarchists, if they did, and how the yorkinos became the federalists, the puros, the reds, and the republicans, if they did. This precisely it is the historian's obligation to produce. But this precisely it ruptures the social scientist to produce. The instinct to tell a story to explain what happened is, I think, the mark of the profession we now belong to —the legitimate ground of its vitality after two and a half millenia, the inexhaustible source of its insights. The search for factors to balance an equation is a response to another calling. Until we clarify the claims on us, we will hardly get out of the debate whether time is continuous or discontinuous and into the stories where time is our element.

These mistakes all derive, I think, from a problem of meaning in Mexican political historiography that still afflicts us. It is not that we merely disagree about what it means to say, for instance, that Lucas Alamán was conservative, nor that we merely disagree about what it means to say, for another instance, that Cárdenas was an agent of reform. It is also not that we grandly disagree on whether the meaning of all Mexican political history is a meaning we cheer or deplore. For light on these problems of meaning, which I cannot now give, I can only recommend close reading of the essays in Gardiner's book on theories of history.<sup>6</sup> Our problem of meaning is rather immediate, in that generally we have not yet recognized what we want to mean in our section of the field. If the paradigm of a historical explanation is a narrative, a story meaningful not only because it hangs together, from the beginning through the middle to the end, but also because it has a role in a big story, a transcending creation, then our problem is that

<sup>5</sup> Arthur C. Danto, *Analytical Philosophy of History*. Cambridge, 1968.

<sup>6</sup> Patrick Gardiner, ed., *Theories of History*. Glencoe, 1959.

we often do not know what the stories are that we want professionally to tell.

We have an excuse for why we do not know what we want to mean, for why we do not know what stories we want to tell. It is that we feel we do not know for sure what the big stories are that our stories would fit into. This is because the biggest story for us, the story of the Great Revolution of 1910-20, is not only a story of the result of the politics of the last century, which is political history, our professional concern, but also a story of the origin of the politics of the present century, and by implication a judgment of the present government, which is political criticism, not our professional concern. The classic exercise of scholarship is to make a hypothesis to explain why the big story turned out as it did, then tentatively fill in the data, then mark the incredible leaps in the explanation, then re-make the hypothesis. But in Mexican political historiography the exercise is still suspect, because the very declaration that the big story has or has not turned out for us to make hypotheses about rings not like scholarship but like politics. In Mexico the present still seeps back into the past, and the past up into the present, like blood through a bandage. Let me cite a final example. I would argue now that after all the popular strain and sacrifice the meaning of the Great Revolution is that it issued in the regime prevailing since the 1940's, which itself issued in the government that massacred the citizens in Tlatelolco Plaza in October 1968. My position, I would admit, has political implications. Suppose a colleague argued instead that the meaning of the Great Revolution is that it issued in a workers' resort in Oaxtepec. His position, I would insist, is implicitly political too. But suppose that another colleague argued instead that the meaning of the Great Revolution is still at issue because the Revolution itself is still at issue, that neither the griefs of Tlatelolco nor the delights of Oaxtepec are warrant for drawing conclusions, that the meaning of the Revolution will come clear only when the Revolution triumphs. His position, I would insist, is no less political. The history of power that we try to make sense of depends inevitably on a criticism of power that we try to steer away from.

But there is a resolution of this problem of meaning, and of the derivative mistakes and confusion. It lies not in logic or in time, but, I think, in a new trust in the profession we have made. It is to argue our cases about the past regardless of their political import in the present, on the faith that our profession can stand our contentions and prejudices and even blend them into moments of the truth. Now that we are professionals, we can count on each other to do responsible work in professional terms without worrying about its import in other terms. We can count on each other to take our explanations, our big

stories and our little stories, with regard only to their quality, without suspicion of their politics —as citizens of a democracy take each other's opinion "for what it's worth" without suspicion of treason. We can count on each other to write history, not briefs of indictment or defense. Trusting in our profession, we may improve our practice.