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## **Interpreting the Mexican Revolution**

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## Interpreting the Mexican Revolution

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Historians should probably not spend too much time writing or reading historiographical resumés such as this. They can easily become exercises in random name-dropping or contrived polemic (and there are no doubt elements of both in this paper). Carried to excess, they take us away from our main task, researching and writing history. If practiced in moderation, however, they can be useful in allowing one to review the field, to survey the state of the art, in this case in the field of "revolutionary Mexico", which I loosely define as 1876–1940.

I mention contrived polemics. The origin of this paper traces to an invitation kindly extended to me by Eric Van Young, the distinguished colonialist, who was looking for a speaker for a Conference on Latin American History (Mexicanist) session at the American Historical Association Conference, Washington, D.C., December 1987. His invitation was couched in terms a little reminiscent of a boxing promoter putting into the ring his latest find from the barrio de Tepito; roughly speaking, he urged me to come out swinging (I exaggerate slightly—as I may well do elsewhere in this paper). Or, to adopt an apter comparison, he seemed to envisage something akin to Pancho Villa's attack on Hermosillo in November 1915, described by one historian as "a slashing, determined attack in the old Villista style" (Clendenen 1961, p. 214). That may be the model, but Villa's attack, of course, proved a costly failure; so might this. Furthermore, as an Englishman fairly recently arrived in the U.S., anxious not to give offense to the many Mexicanists whose work I knew but whose faces I didn't, I had additional grounds for caution. In the face of this awkward dilemma, I hit upon a safe and congenial strategy (one that comes easy, being graven in the English collective psyche): have a go at the French. That will become evident later.

First, however, I want to sketch a quick outline of "revolutionary" history, as it has perhaps developed through three scholarly generations. Then, I want to look at the

current state of play, in particular the revisionist interpretations that have come thick and fast since, roughly, the 1960s. That gives me the excuse to invoke Agincourt and Waterloo.

You can slice revolutionary historiography any way you choose; but one way, it seems to me, is by generations, roughly as follows. An initial generation of writers were participants/observers; they fought, politicked, or were engaged commentators. These would include Mexican writers/intellectuals of both left (e.g., Jesús Silva Herzog, Isidro Fabela, Andrés Molina Enríquez) and right (Francisco Bulnes, Jorge Vera Estañol), as well as highly influential American commentators, like Frank Tannenbaum and Ernest Gruening (Tannenbaum, in particular, is the *bête noire* of the revisionists; his is the principal scalp that—they like to think—hangs from their belt). Of course, this generation would also include many revolutionary participants themselves, who penned their memoirs, diaries, and apologia. These are numerous and often valuable as sources (e.g., Gabriel Gavira's or Salvarado Alvarado's); but, of course, they are highly partisan and—take the example of Obregón's *Ocho mil kilómetros en campaña* which, on reading, seems at least that long and sometimes more like *ochocientos mil*—they can be quite heavy going. But they, Tannenbaum especially, helped create an image of the Revolution (popular, peasant, agrarian, nationalist), an image that was virtually coeval with the Revolution itself. They were, unavoidably, committed; but by the same token they caught some of the intangible flavor of the revolutionary experience (a point I'll return to).

They also laid the groundwork for generation #2: academic historians, publishing chiefly in the 1950s and 1960s, who adopted a "scholarly", chiefly narrative approach, who usually focused on national elites, governments and major events, but who—even though they were often sympathetic to the Revolution in principle—sought to avoid partisanship: for example, in the U.S., Charles Cumberland, Stanley Ross, Robert Quirk; in Mexico, José C. Valadés, Berta Ulloa, Roberto Blanco Moheno, and the outstanding team which, under the editorship of Daniel Cosío Villegas, worked on the *Historia Moderna de México*. Though their scholarly, "objective", usually narrative approach led them away from grand generalizations—they rarely paused to offer categorizations of the Revolution in its totality—they tended to remain within the paradigm set out by Tannenbaum, which we may term the old orthodoxy. This conceived of the Revolution as popular, agrarian, spontaneous, characterized by powerful peasant participation and a large-scale confrontation of peasants and

landowners (often "feudal latifundistas", as well as by strong sentiments of nationalism, even xenophobia (all of which carried implications for interpretations of the Porfiriato: a regime of authoritarian, xenophile oppression). According to the old orthodoxy, the Revolution—a genuine social revolution—overturned the Porfirian regime and resulted in a new, more radical, nationalist and reformist regime that, despite halts, regressions and betrayals, represented some kind of advance, and that certainly represented a major *departure* from the Porfiriato. Even where this second generation concentrated, as they often did, on the great men and great events, the underlying assumptions were still those of the old orthodoxy.

From about the late 1960s on, a third generation became discernible. These were the baby-boomers of Mexican historiography. They were more numerous, arguably more professional, perhaps more single-minded (and/or narrow-minded). Following the prevailing (global) historiographical trends, they specialized, by topic, even methodology. They ransacked the archives as never before (coincidentally, Mexican archives became much more accessible). And, unlike their predecessors, whom they sometimes scorned, they sought to avoid the old concentration on elites and leaders and to see history from the bottom up; it was time for *los de abajo* to get their deserved attention (it is ironic, in the light of this new approach, that these same historians often concluded that *los de abajo* were so much hapless cannon-fodder).

The most notable feature of this generation is their sheer numbers. As David Bailey observed, in a historiographical article written some ten years ago (and which offers a convenient pre-emptive excuse): "Even the specialists find themselves overtaxed to read—and sometimes to locate—the books articles and dissertations that pour forth, not only in Mexico and in the US but also in a half-dozen other countries" (Bailey 1978, p. 62). This outpouring derived partly from the growth of higher education, hence of PhDs, in the U.S., Mexico, and Europe (Europe now starts to figure in a modest way); perhaps partly it reflected, too, the 1960s vogue for things Latin American, especially revolutions; and it was also a consequence of the inevitable historiographical cycle, whereby, as time goes by and archives open up, once "contemporary" topics recede in time, escape from the irresponsible clutches of journalists and instant commentators, and find sanctuary with serious, source-mining historians. In the ten years 1946–1955, for example, the *Hispanic American Historical Review* published seventeen articles on postcolonial Mexico, of which only two (one a brief note) focused on the Porfiriato or Revolution; during 1956–1975 there were

forty-three articles (suggesting only a modest overall increase in interest in postcolonial Mexico), but of these no fewer than twenty-three dealt with the Porfiriato and Revolution (in the ratio 10:13).

The march of time, however, had other consequences, I would suggest. First, it distanced historians from their subject; while this may have made for greater objectivity, it did not necessarily make for greater comprehension, empathy, or understanding (*verstehen*, in the classic Weberian sense). A feel for context—for the intangible moods, sentiments and assumptions of historical actors—gave way to a more positivistic reliance on facts, especially hard facts, if possible quantifiable facts. Also, the late 1960s and after were years when the Mexican political system and "economic miracle" seemed to falter (looking back from today's perspective we may wonder what the fuss was all about). But it is not surprising that those who, as practicing historians, working in post-1968 Mexico, had known only the Peace of the PRI, the "economic miracle", Tlatelolco (1968), and ritual official invocations of the Revolution which seemed to fly in the face of Mexican reality, should question what the Revolution was all about and feel the urge to debunk and demystify it.

Thus, from the late 1960s on, new, detailed monographs—books, theses, articles—flooded the market. Given their sheer number, it is obvious that they did not—could not—follow a common pattern. Some tried new methodologies: quantitative history (John Coatsworth, Peter Smith, James Wilkie); oral history (Ann Craig, Arturo Warman, Eugenia Meyer and their respective teams, who sought to recover *agrarista* memories; Wilkie, again, who recorded the well-processed recollections of high-up political veterans). In most cases, however, oral history was a means to get at "history from below", and it reflected that prevalent trend, by no means confined to *Mexican* historiography in the last twenty years. But the most common device or approach for such history was regional or local. Thus, aside from their archival originality or critical stance *vis-à-vis* the official revolution, the new history of the post-1960s was above all characterised by a local or regional focus. Such a focus, of course, was not new; there was a venerable tradition of histories-of-the-*patria-chica* (some of them very useful); but now "professional" historians—Mexican and foreign—acquired their own, adoptive *patrias chicas* (and sometimes their own adoptive local chauvinism to go with them). The list is long and I will merely list some of the better ones; several, of course, combine biographical with local/regional studies: Héctor Aguilar Camín, Dudley Ankerson, Thomas Benjamin, Ann Craig, Romana

Falcón (twice), Heather Fowler Salamini, Paul Friedrich, Ian Jacobs, Gilbert Joseph, Carlos Martínez Assad, Jean Meyer (a special case), Enrique Montalvo, Francisco Paoli, Frans Schryer, Stuart Voss, Arturo Warman, Mark Wasserman, Allen Wells, John Womack; the several contributors to David Brading's *Caudillo and Peasant* (notably Raymond Buve); and to Benjamin and McNellie's *Other Mexicos* (e.g., David LaFrance, William Langston, Daniela Spenser). Of course, the doyen of local history—of *microhistoria*—was and is Luis González; and there are several flourishing centers in Mexico which generate valuable local studies: the Instituto Mora; the Jiquilpan Centro de Estudios de la Revolución Mexicana; the Colegio de Michoacán; the Colegio de Jalisco. Recent examples of sophisticated regional symposia are Víctor Raúl Martínez Vásquez's *La revolución en Oaxaca* and Martínez Assad's two volume *La revolución en las regiones*.

So the most obvious and indisputable feature of post-1960s revolutionary historiography has been its geographical or spatial disaggregation. The national historian—the norm of the earlier period—has been supplanted by the regional and local historian. And there can be no doubt that this represented evolutionary progress. There, have, of course, been countervailing attempts at aggregation—at synthetic studies, either national and comprehensive in approach, or at least national in their treatment of specific themes. Studies of the revolution's foreign relations are one species, which I will pass over (they are something of a breed apart, though it is worth noting that the outstanding foreign relations study of this period—Friedrich Katz's *Secret War*—also sheds a lot of light on domestic politics and social movements).

Given the plethora of recent monographs, and the greater accessibility of archives, strictly domestic themes—such as labor or the peasantry—are less easily synthesized. We have some good studies of labor (by Rodney Anderson, Joe Ashby, Barry Carr, John Hart, Ramón Ruiz, as well as the valuable series edited by Pablo González Casanova), but they necessarily focus on major industries (textiles) and national confederations (COM, CROM, CTM). Mexican labor history is still a long way behind its European and U.S. counterparts in breaking the tyranny of acronyms and seeking to reconstruct the real "lives of labor" or the "making of the Mexican working class" (rare examples are García Díaz's brief but cogent study of Santa Rosa and Leif Adleson's work on the Tampico labor movement).

The agrarian sector (peasant, hacienda, rancho) has been better served—partly through the regional and local studies already mentioned, as well as a number of

valuable hacienda studies (by Marijosé Amerlinck de Bontempo, Marco Bellingeri, Juan Felipe Leal, Simon Miller, Heriberto Moreno García, Herbert Nickel, Manuel Plana, Beatriz Rojas, María Vargas Lobsinger)—and more ambitious syntheses are now starting to appear: by John Tutino, Friedrich Katz, and John Coatsworth. David Brading, Ian Jacobs, Frans Schryer and others have rescued the *ranchero* from oblivion (I think we can now stop lamenting his historiographical neglect); but, if we know a good deal more about agrarian change in central and north Mexico, *México Sur* remains—withstanding excellent work by Gilbert Joseph, Allen Wells, Thomas Benjamin and others - something of an agrarian *tierra incognita*, especially outside Yucatán.

Finally, it is no exaggeration to say that the study of church-state conflict has been revolutionized by the work of Jean Meyer, who has been ably seconded by other scholars (Alicia Olivera Sedano, David Bailey). That, in fact, is a theme that very readily translates into national history; as I shall suggest, the power of Meyer's revisionist study of the Cristiada (a movement that, for all its importance, was quite limited in time and space) has enabled it, like some errant giant planet, to exert an influence far beyond its legitimate orbit.

This brings us to the nub of the question: the interpretations of the Revolution which these recent studies have stimulated. First, it should be noted that the conversion of local/regional studies into more general, higher level syntheses is not an easy task. Confronted with such studies, the historian, as reader or writer, has several options. One is to avoid such syntheses altogether; to see the Revolution as an irreducibly complex patchwork, defying generalization (I will come back to this, the patchwork approach, in a moment). A second option is limited, qualified comparison and contrast. This can produce useful *aperçus* and observations, but no genuine structured synthesis. A third option consists of generalizing a particular case, boldly asserting—or tacitly implying—its typicality for all (or most) of Mexico. Arguably, this is what Jean Meyer has done (although with great power and panache): the Center-West experience of the Cristiada informs his entire interpretation of the Callista revolutionary state and, by further extension, of the entire Revolution (and, even, of Mexican history since the conquest) (Meyer 1973). Of course, this process of ever-radiating generalization does not mean that Meyer ignores contrasts or baldly asserts the outright typicality of the Cristiada, but the effect is sufficiently strong, I think, to justify calling Meyer's Revolution a Revolution seen through Cristero stained glass,

and, in consequence, heavily colored. A fourth, final and contrasting option consists of denying the typicality of any given case; of asserting not its typicality but its uniqueness; and thus, by implication, generalizing (negatively) about the rest of the Revolution. The clearest and commonest case of this involves Zapatismo, which is often categorised as the sole genuine peasant/*agrarista* revolutionary movement amid the otherwise meaningless chaos and caudillismo of 1910–1920 (Ruiz 1980, p. 200).

Thus, as historians begin to synthesize the mass of new, chiefly local/regional studies, they begin, explicitly or implicitly, to generate interpretations of the Revolution as a whole. Some have gone the whole hog, attempting genuine syntheses: François-Xavier Guerra, John Hart, Jean Meyer, Ramon Ruiz, Hans Werner Tobler and myself. Allow me here a brief digression. Several of these synthesisers are European (a Frenchman, a Spaniard based in France, a Swiss and an Englishman). Why should this be? I attribute it partly to a set of very practical considerations: Europeans have less access to primary sources, hence are driven to synthesis (Guerra used no archives, Tobler a limited selection); they also operate in an academic world where Mexican history is a pretty obscure and exotic *rinconcito*, hence they are driven to teach, and write, at a higher level of generality virtually to justify their academic existence; conversely, they feel less inhibited by the scholarly proximity of "rivals". Where the North American Mexicanist may sometimes feel a sense of scholarly claustrophobia, the occupational neurosis of the European Mexicanist is more likely agoraphobia; the sense of roaming in a vast empty terrain, occasionally meeting long-lost colleagues rather as Stanley met Livingstone in the wilds of the Congo.

But, as I have suggested, general interpretations are also built up piecemeal and incrementally, like coral reefs, as well as deliberately and architectonically, like skyscrapers; they depend therefore a good deal on works that are not synthetic, and on historians who are not committed synthesizers. Romana Falcón, for example, whom I would see as an able and significant contributor to the revisionist view, has written three regional studies as well as several substantial articles. Other local/regional historians venture their generalizations, in the manner(s) suggested. It all adds up. What does it add up to? Again, you can slice the melon all different ways. However, I would choose four broad areas from which interpretative conclusions—notably those of revisionist stamp—can be extracted and discussed. The first concerns the homogeneity/ heterogeneity of the Revolution; a question that must precede any attempt at further generalization. The second concerns the character of the 1910–1920 Revolution



itself. The third and fourth, closely related, concern the Porfiriato, and the post-1920 revolutionary settlement.

The accumulation of recent research has clearly displayed the complexity and heterogeneity of the Revolution. "Many Mexicos", "Other Mexicos", are the phrases on all lips nowadays. But this is a pretty modest gain. The supposed orthodoxy—of a monolithic popular revolution, seamless and uniform—is a complete myth (or, if you like, a giant straw man). Its only proponents are PRIista ideologues and orators and I doubt we want to waste time arguing with them. Tannenbaum, the "orthodox" standard-bearer and revisionists' sacrificial victim, was quite explicit about this, as he was bound to be, given his intimate knowledge of revolutionary Mexico. Indeed, the heterogeneity of the Revolution was central to his argument. The Revolution, he wrote, "has not been a national revolution in the sense that all of the country participated in the same movement and at the same time. It has been local, regional, sometimes almost by counties". It was not monolithic; nor was it unidirectional. "So rapid and varied have been the cross currents that have come to the surface in the Revolution that it is most difficult to discover any direction in the movement" (Tannenbaum 1966, pp. 121, 147).

To say, therefore, that the Revolution as a whole was not monolithic, that there were "many revolutions", just as there were "many Mexicos", is not to say anything very profound or original. Certainly it is not a rebuttal of Tannenbaum. It is, at best, a ritual exorcism of official ideology. In scholarly terms, it is a good beginning but a banal conclusion. The key question is, where next? We can, of course, stop at that point, and rest content with the patchwork revolution—as many revolutions as regions, localities, movements, even individuals. The Revolution becomes a bewildering collage of atomistic events and peculiarities. Oral history (especially if pursued as a mainstream, rather than an ancillary, methodology) can lead in this direction. The recollections of individuals—the moreso when they are stripped, as they should be, of ex post facto rationalisations—often seem to suggest a random array of motives, ranging from personal grudges to dislike of one's mother-in-law, motivations that are not easy to generalize. Oral accounts of revolutionary participation are often similarly episodic and ostensibly meaningless (Warman 1976, pp. 104-105). Likewise, images of the Revolution derived from literary sources, even from the incomparable Azuela, are chaotic and patternless. The masses become ignorant cannon-fodder, the leaders cynical contenders for power, and the Revolution itself is metaphorized as a grim, game-

playing deity (Rutherford 1971). Interestingly, this vision of the aimless, amoral revolution comes across strongly in foreign diplomatic sources, as well as oral and literary accounts. There is, in consequence, no shortage of sources—oral, literary, diplomatic—which can be utilized to support the notion of an aimless, formless revolution.

To go down this path—to embrace the random and the individual and to deny form or pattern to the Revolution—seems to me a counsel of scholarly despair, even if it can be decked out in a spurious display of academic sophistication (i.e., with disparaging talk of metahistorical theories or Procrustean beds—the familiar quibbles of a pernicky empiricism). Here, the parallel with the French Revolution is illuminating.

One school of French revolutionary historiography has assiduously pursued more and more research about less and less, devoting pages to bloodcurdling incidents of the White Terror in Nîmes, or to the suicide of pregnant chambermaids in Paris. Mexican revolutionary historiography has not gone that far (the output is less and the archives, especially the police archives, are less copious and informative); but the researching-away of the Revolution in France offers, I think, a sobering lesson for those engaged in the study of Mexico's Revolution (and maybe other revolutions too). More research and publication do not necessarily mean better; archival work alone does not unlock historical secrets; and historiography does not inevitably and progressively advance from generation to generation. Indeed, some of today's revisionists could not hold a candle to Tannenbaum or Gruening. And—although this is a question of personal inclination—the reality of the Revolution cannot be established simply by narrating individual experiences and accounts. There are historical patterns of which the historical subjects themselves are unaware; or, to put the argument differently, historians, like other social scientists (be they economists, sociologists, or psychologists) must incorporate latent as well as manifest motives/functions into their analysis. The Cristeros rose in arms to defend the faith—by their own account; but that does not exhaust the list of explanatory factors underlying the Cristiada. Some Villistas resolved to *ir a la bola* to escape their oppressive mothers-in-law; but we should hesitate to make oppressive mothers-in-law a generic cause of revolution.

Once we go beyond individual, episodic narration, we begin to generalize. What are the generalizations that have emerged from the plethora of studies of the last twenty or so years? When David Bailey wrote his resumé of revolutionary historiography some ten years ago, he discerned a revisionism that was "exciting and perplexing" but in which he saw no coherent drift: "the only common ground left is the acknowledge-

ment that there is less agreement today about the nature and meaning of the revolution than at any time since scholars first turned their attention to it more than fifty years ago" (Bailey 1978, p. 63). Bailey thus saw revisionism as a kind of inchoate antithesis, a negative repudiation of the old which did not yet constitute a positive alternative. I am not sure if such a positive alternative is now clearly established, nor if we can begin to talk of the now not-so-new revisionism consolidating itself into an orthodoxy (that is arguable). But we can certainly discern common interpretative features within the corpus of revisionist scholarship and, even if this does not yet comprise the mainstream (perhaps there is no mainstream), it represents a powerful current, not just a multitude of random whirls and eddies. This current includes a number of the local/regional studies already mentioned, and three, perhaps four, of the major syntheses we have to hand: Guerra, Meyer, Ruiz, and perhaps Tobler. Interestingly, the strongest current rival—at the level of grand synthesis—is a Marxist one, represented in the main by highly schematic studies, usually devoid of original archival evidence: Adolfo Gilly's *Revolución Interrumpida*, Anatol Shulgovski's *México en la encrucijada de su historia* (John Hart's recent *Revolutionary Mexico*, belonging to the same theoretical camp, does, in contrast, embody a wealth of archival data, supportive of the traditional view of a popular, agrarian revolution; it also purports to support the—also somewhat traditional—view of a xenophobic, anti-imperialist revolution, which I believe is a different, and much more questionable, thesis).

The interpretative impact of revisionism can be seen in the domain of the Revolution, the Porfiriato, and the postrevolutionary phase. I will begin (in defiance of chronological sequence) with the Revolution, first, because I know it best and, second, because I think it makes analytical sense to start there. The essence of the revisionist interpretation is to de-emphasize and at times deny the popular and agrarian character of the Revolution. It does *not* de-emphasize a feature of the old orthodoxy that I *would* de-emphasize: namely, the popular nationalism or xenophobia that allegedly underwrote the revolution, which I consider highly exaggerated and which Hart's new synthesis particularly stresses (Knight 1987). However—and this is the central point—according to the revisionist interpretation, the peasantry played a limited, dependent role; middle-class and landed elites called the tune. Zapatismo (whose popular and agrarian character no-one can deny) is the exception that proves the rule. In fact, a quick litmus test of revisionism is often provided by a writer's treatment of Zapatismo: Is it a powerful example of a more widespread phenomenon

(the old orthodoxy) or a unique aberration (revisionist)? Incidentally, this means that (superficial) studies of the Revolution which, in discussing peasant rebellion, concentrate heavily or solely on Zapatismo, indirectly lend aid and comfort to the revisionist cause, for they advance no evidence to suggest that Zapatismo was *typical* rather than *unique*. In similar fashion, I would add, analyses that go to the other extreme and appropriate widely divergent movements—such as Villismo or the Cristiada—for a catch-all agrarianism also succor revisionism, by offering it cheap and easy shots. Such sweeping assertions of an indiscriminate agrarianism, common among radical writers and rarely supported by good evidence, resemble the old Flowery Wars of pre-Columbian Mexico: rather futile undertakings that only supply the revisionist Aztecs with their necessary quota of sacrificial victims (an example would be John Tutino's otherwise excellent synthesis of Mexican agrarian history, which shows signs of jumping the rails as it leaves the nineteenth century and enters the twentieth). There are other hallmarks of revisionism to look out for: an emphasis on the role of landlords and caciques as the key revolutionary actors/leaders; a denial of the significance of the Maderista revolution (a mere storm in a teacup); and a contrived effort to rehabilitate Huerta (on the grounds that, if he was cast as a villain by the Revolution, he can't be all bad; this is one of the simplest forms of revisionism, since it involves a mere inversion of the old Manichaeian orthodoxy of the Revolution itself).

I can only give quick illustrative examples of the revisionist approach. "The bulk of the peasantry . . . was not much involved in the revolution of 1910–20", Eric Hobsbawm has written, on the basis of a (rapid?) reading of Jean Meyer (Hobsbawm 1973, p. 10). This is no doubt an exaggerated reading, but it is not altogether unwarranted or surprising, since Meyer stresses the scope and spontaneity of popular mobilization during the Cristiada, while de-emphasizing the same for the Revolution (Meyer 1985: 3, p. 23; 1973, p. 104). Following his American namesake (Michael C. Meyer), (Jean) Meyer also has to put in a pitch for Huerta, since—though he strives unconvincingly to deny and de-emphasize it—he has to accept a degree of Catholic connivance with the Huerta coup and regime. The final escape is to assert Huerta's broad popularity: "It is true that eminent members of the PCN [Partido Católico Nacional] . . . participated in Huerta's government, but who was not a Huerta supporter?" (Meyer 1976, p. 11; 1985: II, pp. 64–67; 1973 pp. 48–49). As for the landlord captains and controllers of the Revolution, they figure prominently in Falcón's work, thus as key items in a argument specifically directed against Tannenbaum (figurehead of the old

orthodoxy) and latter-day Marxists, like Gilly, both of whom err, Falcón says, by stressing the autonomous role of popular, peasant forces; Falcón, in contrast, points to "the decisive participation of the middle classes and the well-to-do sectors (*sectores acomodados*) in the leadership of the [Maderista] revolt'; these groups initiated the rebellion, which "found a popular echo", and they were able to maintain their hegemony over their johnny-come-lately popular followers (Falcón 1979, pp. 198-199). Ramón Ruiz, similarly, sees the Maderistas as "not peasants but small-time entrepreneurs and others eager to improve their station in life, mature heads of families, rancheros, the owners of lands often left behind in the care of their sons" (Ruiz 1980, p. 214; and see Meyer, 1973, p. 107). This sounds pretty doubtful from a generational as well as a socioeconomic perspective: the armed revolution was the work of the young, not the middle-aged. Note, too, the recurrent emphasis on rising expectations and upward mobility, which is evident in several recent studies (e.g., Vanderwood, 1981, 1988). And it is not just a question of the Maderista revolution, for Ruiz' citations—and his overall methodology—collapse together successive waves of rebellion under much the same rubric. Revolutionary leadership—whether in 1910, 1913 or 1915—is seen to be middle class or better: "of the rebel luminaries . . . only a handful had come from the rural villages"; "shopkeepers and merchants, almost always from towns in the provinces, from the start participated in the rebellion"; so, too, did landlords. Ruiz mentions about a dozen, who are included among his "profile of rebels"; several of these are classic bandwagon-hoppers, whose "revolutionary" credentials depend either on belated and opportunistic switches, designed to protect their property, or fullsome overtures to the victorious Carranza, written from exile in New York (Ruiz 1980, pp. 216, 230, 234-235).

Of course there were some landlord-revolutionaries. Some were pioneer leaders of popular forces (especially serrano forces); as such they were members of the popular movement, sharing with peasant followers a common antipathy to Porfirian centralization and "progress". Their participation does not negate the popular, local, "traditional" character of the Revolution. Some, alternatively, were die-hard liberals or frustrated "outs". But not that many. They certainly did not supply the chief motive force behind the armed revolution; on the contrary, most hacendados resisted it, sticking with Díaz until Díaz's goose was cooked, then switching to Reyes, De la Barra, Huerta—the successive inheritors of the Porfirian mantle. Ultimately, some hacendados became tardy tactical converts to the Revolution, especially as it entered its conservative phase

after 1915, and as Obregón struck deals right and left in 1920. None of these groups was sufficiently numerous, powerful, or, above all, precocious, to set their stamp on the armed popular revolution of 1910-1915, which was of very different character. Serrano pioneers aside, landlord "revolutionaries" made their biggest contribution to *counterrevolution*, locally or nationally (Garciadiego Dantan 1981; Benjamin 1981; Martínez Vazquez 1985; Ruiz Cervantes 1986). One point to stress is that, given the swift changes in political climate during this decade, as well as the marked regional variations of the Revolution, it is highly misleading to assemble lists of landlord "rebels" who hailed from different times and places and often obeyed different motives. Imagine the conceptual chaos that would ensue if the French Revolution were similarly homogenized and lumped, so that early aristocratic reformers rubbed shoulders with Breton backwoodsmen and Bonapartist parvenus!

Conversely, peasant participation and autonomy were much greater than the revisionists—influenced, perhaps, by the long peace of the PRI, the apparent relative quiescence of the peasantry since the 1940s, and the prevalence of a controlling caciquismo—are nowadays prepared to admit. I know of no way of settling this argument except by trading examples (I am not at all sure I know how to quantify the examples). How many peasant rebels make a peasant *revolution*, how many landlords to refute it in favour of an elite-controlled *rebellion*? Maybe John Coatsworth, who has shown great ingenuity trying to calibrate rather intractable rural protest movements, could help out. I will say that my own research threw up plenty of peasant movements (which does not mean, of course, movements in all cases led by peasants); and other researchers seem to be discovering more—even in regions, like Chihuahua, where the classic peasant/agrarian syndrome has often been regarded as weak (Alonso 1988; Koreck 1988; Nugent 1988b). Such research reinforces the notion of a popular peasant revolution not only by virtue of simple head-counting, but also by analysing the modes, continuities and discourse of peasant protest. It is thus possible to argue—in opposition to those who would prefer a more individualist, "rational-actor" model—that peasant communities displayed certain distinct, shared values, perhaps rooted in a "moral economy", which facilitated prolonged resistance to political and economic threats (Knight 1986: I, pp. 150-170). Tannenbaum did not talk about "moral economy"; but his notion of a defensive, popular and agrarian revolution is readily assimilable to a moral economy thesis.

From the Revolution itself, we can turn back to the Porfiriato. If the Revolution was less a broad, popular, agrarian insurrection than a series of controlled, factional battles for power, it follows that the old *leyenda negra* of the Porfiriato stands in need of revision too. And it is the Porfiriato which has become the focus of the most systematic piece of revisionism, that of François-Xavier Guerra (Guerra 1985). But Guerra isn't wholly innovative. Cosío Villegas' massive political history (criticized by some for its elitist approach) offered a newly rounded, qualified and even sympathetic picture of the old regime some years ago (Cosío Villegas 1970, 1972). Jean Meyer, skeptical of the Revolution's claim to represent the oppressed masses, also questioned the fact of Porfirian oppression: "popular discontent alone", he wrote, "did not make this revolution. Had it even increased prior to 1910? That isn't clear". Rather than a revolution provoked by mounting socio-economic grievances, Meyer discerned one born of "modernization", of "rapid progress," of the "diffusion of a *prise de conscience* . . . at the level of the middle and upper sectors of society," in short, he concluded, "it is the revolution of rising expectations, dear to American sociologists." Meanwhile, "for the majority of the people, life remained hard, but also palpably less tough, more easy, and longer too. There was no war, no hunger, no plague". (Meyer 1973, pp. 23, 47, 103).

So, the Revolution ensued less because the poor were getting poorer (or because peasants were being dispossessed and villages were being ground under which is rather different from simple pauperization) but rather because "modernisation" generated new social groups, new demands, new expectations. Here we have the mid-twentieth-century U.S. sociology of Edward Shils (complete with its emphasis on *declassé* intellectuals) applied to Porfirian Mexico. And where Meyer subscribed to the thesis in somewhat discursive, even elliptical, terms, Guerra has, more recently, produced a massive, forthright work of historical certitude. Guerra's two volumes are long, detailed, and impressive. They contain a wealth of valuable evidence concerning the Porfiriato, the opposition and (rather belatedly and briefly) the early (1910—1911) Revolution. All of that evidence, it should be noted, is derived from secondary sources; Guerra appears not to have set foot inside an archive, certainly outside Europe. Nevertheless, he comes up with some interesting and convincing arguments: his Namierite dissection of Porfirian elites and factions and of the Porfirian *cursus honorum* is excellent (it out—Peter Smiths Peter Smith); his evaluation of the anti-Porfirian political opposition, especially Maderismo, is one of the best studies we have

of that often misunderstood phenomenon. But the whole analysis is yoked to a tendentious and stultifying theory (which is in turn based on several tacit assumptions). On the one hand, Guerra believes in the politics of faction and clientelism, which is fine—it makes possible his acute analysis of Porfirian politics. On the other, he believes in the power of ideas, especially the "modern" ideology rooted in the Enlightenment which, according to his somewhat eschatological view, is locked in permanent combat with a rival set of ideas/principles, those of tradition. What we have here are the old antitheses of modernization theory (sacred/secular, folk/urban, *gemeinschaft/ gesellschaft*), which become analytical open sesame to unlock the secrets of Mexican history from the Bourbons to the Sonorans. What we don't have—because what Guerra leaves out is as significant as what he puts in—is any analysis of class: Guerra's Mexicans are organized either in clientelist factions or in ideological movements, thus by bosses or intellectuals. Autonomous peasant movements have little place in such a scheme. Furthermore, obsessed with the *political*, Guerra completely misses a component of "modernising" ideology that should be central to any such study: I refer—for want of a better word—to the "developmentalism" that pervaded liberal, Porfirian and revolutionary thought, and which stressed the need to educate, moralize, and—both literally and metaphorically—clean up the dirty and degenerate Mexican people. This is a theme which González Navarro noted in his pioneering *Vida Social*, which other scholars—such as Mary Kay Vaughan and William Beezley—have begun to explore, and which I think is deserving of much greater attention, especially by those fond of discourse analysis (Knight 1986: 2, pp. 499–503; Vaughan 1982; Beezley 1987).

Thus, Guerra's scheme is strangely self-limiting and traditional; for all its bold revisionism, it operates within a familiar, given and essentially political context, within which it chooses to invert common assumptions (modernization and progress are questioned, even condemned; tradition is exalted). It is also a global scheme; the Third World, for Guerra, is a kind of Arcadia of self-contained, rustic, "holistic" communities, often still "tribal", and wedded to traditional faiths and mores (in all this, "tradition" and "modernity" provide the key antitheses, though they are never properly explained). Then, meddlesome reformers/intellectuals/liberals enter Arcadia. For Mexico, the process of degeneration starts with the Bourbons (centralizing, anticlerical statists: Jean Meyer, of course, has little time for the Bourbons either; nor does that other doyen of the pro-Catholic, or antianticlerical, school, David Brading).



Degeneration accelerates during the nineteenth century, with the freemasons playing a pervasively subversive role; the Díaz regime represents a kind of final holding operation, a muted conflict between modernizers and traditionalists, with the president himself acting a benign, paternalist role, protective—in Hapsburg style—of threatened traditional communities (the peasants, we are told, look to Díaz as they did to the kings of Spain: Guerra 1985: I, p. 51). Alas, with the revolution the meddlesome, modernizing elites—more deraciné intellectuals, liberals, and freemasons, all possessed of a dogmatic, universal, individualist philosophy and some even tainted with Protestantism—come into their own. Tradition is now trashed. 'Holistic' communities - villages and haciendas (the hacienda community, for Guerra, has a distinctly benign, paternalist character)—are shredded by these upstart reformers and state-builders. Guerra's book stops in 1911; perhaps his computer was now engorged (with its 7,838 *individus et collectivités* and 30,540 *modules différents*, making a total of 150,000 *données*); or, more likely, its master simply could not contemplate the final apocalypse of old, Catholic, traditionalist Mexico and the triumph of secular centralizing liberalism. Anyway, that apocalypse had already found its chronicler.

Although Guerra's schema is global, it is also (like so much globally pitched modernisation theory) highly Eurocentric. Mexico is an extension of Roman and medieval Europe. Cicero and the Merovingian villa offer models that Mexico has faithfully followed; Mexico is a "prolongation" of Europe (Guerra 1985: I, p. 128). More specifically, Guerra, a Spaniard working in France, uses Spain and France as models. Spain (he must mean Aragón) provides the model of a traditional society built upon *pactisme*; the (presumed) decline in communal fiesta expenditures of an eighteenth-century Indian pueblo is demonstrated by analogy with contemporary Seville (Guerra 1985: I, p. 231); and—in a moment of some evidential desperation - Guerra seeks to demonstrate the pervasive importance of freemasonry in nineteenth-century Mexican politics by pointing out that 39 per cent of the delegates to the Spanish Constituent Congress of 1931 were freemasons (Guerra 1985: I, p. 420). Q.E.D.

But the key model is France. Guerra repeatedly cites Augustin Cochin's study of the free-thinking societies of eighteenth-century France, which Cochin saw as the intellectual and social solvents of the ancien régime. Guerra lifts Cochin's thesis and applies it, lock-stock-and-barrel, to Mexico. He sees himself performing, for Mexico's old regime and revolution, the same counterrevolutionary historiographical role that Cochin and his latter-day disciples/discoverers (notably François Furet) have sought to

perform for French revolutionary studies: that is, to banish the old class—or, as it is often called, "social"—interpretation of the Revolution and to replace it with a thesis that stresses the central role of intellectual elites—free-thinkers, freemasons, liberal reformers—who railroad an elitist, secularizing project through a fundamentally hostile, traditional society (Furet 1981). The Revolution thus becomes a *political*, rather than a *social*, event; an event devoid of clear class significance (hence it cannot be "bourgeois"); but involving the subversion of tradition in favor of modernity, the substitution of old "holistic" bonds/allegiances by new, cerebral, ideological and atomistic attachments. And this—in the immortal words of Sellars and Yeatman—is decidedly a Bad Thing. It is hardly coincidental that Guerra relies heavily on Francisco Bulnes for his Mexican observations, just as he relies on Cochin—as well as Pierre Chaunu, Roland Mousnier, and Louis Dumont—for his ideological inspiration. Bulnes, the maverick Porfirian conservative, is the darling of the revisionists: both Ramón Ruiz and Jean Meyer rely a good deal on his questionable authority. Thus, the laments of the displaced Porfirian elite, their diatribes against revolutionary upstarts, meddlers, and opportunists, form an important part of Guerra's scheme; and they harmonize with Cochin's own refrains, which in turn reflected his status as the scion of a conservative, aristocratic family, steeped in "Catholic traditionalism and . . . hostility toward the republican regime" (Furet, 1981, 163).

In short, Guerra harnesses a mass of evidence, culled from secondary sources and coupled with a high-tech methodology, in order to impose upon the Porfiriato and Revolution a Euro-(and moreso) Francocentric scheme, which sees the Revolution as the ultimate political triumph of modernizing elites, enemies of tradition, Catholicism, and the healthy, holistic communities of old Mexico. It damns the Revolution (as Cochin did), it gilds the old regime, and it banishes class.

You may say that the Revolution was all these things, and, to some extent, it was. But there is a need for balance. Vulgar modernization theory is no advance on vulgar Marxism. The main criticism of Guerra must be that his picture is dogmatic and one-sided, that it romanticizes the old regime, and that it imposes a narrow, political, classless, and Francocentric interpretation on what was a complex, class-based, social revolution, above all during the years 1910-1915. It also leads to a basic misconception concerning the Revolution's outcome, thus concerning the labels or categories we may ultimately attach to it. Guerra's analysis, though it halts in 1911, fits neatly with the prevailing revisionist trend that emphasizes (1) the *political* - and often elitist -

character of the revolution and (2) the basic continuities that linked it to the past, making it more "neo-Porfirian" than genuinely revolutionary. For Guerra and others, Vanderwood suggests, "the revolution . . . is considered simply another blip, although a rather large one . . . in the continuum of Mexican history" (Vanderwood 1987, 432). Ramon Ruiz is at pains to point out that it was not a true social revolution at all, just a "great rebellion" or a mere "mutiny" (Ruiz 1980, pp. 4–8; 1988, p. 228). It "updated" capitalism, but in no sense transformed society. Its chief effects were political (here Tocqueville is ritually invoked): it served to create a powerful, centralized state; thus it consummated the work of the Bourbons, of the liberal reformers of the nineteenth century, of Díaz himself. Revisionist studies of the postrevolutionary era thus focus heavily on the rise and rise of the state (I have elsewhere termed this approach *statolatry*), which they see as above society, "relatively autonomous," and even "Bonapartist" (another French interpolation); indeed, they see the state as mixing and kneading the inert dough of civil society much as it pleases. Not only does this dovetail neatly with French revolutionary revisionism, it also draws inspiration from recent "statist" theories of revolution, notably Skocpol's, which lumps together "bourgeois" and "socialist" revolutions under a common rubric as state-building movements, highly conditioned and determined by state actors, rivalries, and divisions (and, therefore, relatively autonomous of domestic social pressures). It even wins Marxist converts, aficionados of the state-oriented theories that have become fashionable in recent years. Arnaldo Córdova's analysis—which denies the fact of social revolution and sees instead the rise of a *sociedad de masas* and a controlling Bonapartist state—is a case in point (Córdova 1973).

For want of time (and because I have addressed this point elsewhere in print: Knight 1985) I will not go into the revisionist interpretation of postrevolutionary Mexico in any detail. But there are at least three fundamental objections. First, there are theoretical objections to such relatively autonomous states. They fit awkwardly within both Marxist and the liberal pluralist theory. Often, the relatively autonomous or Bonapartist state appears as a *Deus ex machina*: it is called upon when all other explanatory factors fail, or seem to fail. It is also an unmoved mover; it acts but is not acted upon. Second, it is empirically questionable. The power of the Mexican state was, in my judgment, much less than commonly imagined for the period 1910–1940 (again, there is no agreed criteria for the measurement of "state power", though I think if we could come up with some such criteria we would avoid a lot of fruitless debate).

Conversely, the power and autonomy of civil society were greater. Civil society may have been a mess, but it was not a disaggregated, anomic, classless mess. Repeatedly, the state was buffeted by conflicts that welled up within society; and the state's attempts to curtail or channel conflicts were often unsuccessful. In successive decades both Callismo and Cardenismo rose, flourished, then wilted. The battle against the church in the 1920s produced a stalemate, the battle for socialist education in the 1930s was lost. This was hardly the record of a mighty Leviathan. Furthermore, the changes taking place in civil society were profound and fully deserving of the "social revolutionary" label. Not because the new revolutionary elites invariably willed such changes; often they did not. Official agrarian reform—the key example—came slowly and grudgingly.

Hence some scholars suggest that the 1920s were basically "neo-Porfirian" in terms not only of continued capitalist development but also of continued hacienda hegemony (there is something of a contradiction here). In fact, capitalist development was profoundly affected by agrarian change, and change that was often not legislated, but initiated at the grassroots, first with the violent popular upheaval of 1910–1915, then with the long, arduous, process of agrarista organization, lobbying, politicking, and fighting. This was not a process begun and controlled by the state, nor was it a superficial process. Long before Cárdenas accelerated the process of formal land distribution, the hacienda had come under severe, in many cases debilitating, pressure; it confronted newly mobilized peasant antagonists; and the landlord class had in consequence lost the social and political hegemony that it had arguably enjoyed during the Porfiriato.

This was something Tannenbaum, Gruening, and others—first-hand observers of the scene—fully appreciated; it is something that today's historians, remote from the time, overfond of statistical certainties (another contradiction), and familiar with the more quiescent, minority peasantry of modern Mexico, sometimes have difficulty in conceiving. Many, therefore, stress the top-down, contrived, manipulative character of agrarismo. They see the ejido as an alien form foisted on happy bucolic communities; the ejido represents another imposition by reformist elites—Bourbon, liberal, revolutionary—who seek to "modernize" a traditional rural sector that is, in some sense, at peace with itself. Secular education, too, appears as a statist steamroller flattening a hitherto happy, Godfearing peasantry (Becker 1988). So far as the rural sector is concerned, conflict comes from without rather than from within; and it is

political rather than class conflict. Again, therefore, revisionism stresses the role of elites (which is, of course, better documented); it plays down popular mobilization and tends to ignore grassroots shifts in mentality and organization. Yet such shifts, indicative of a real transformation in the sociopolitical climate, are hallmarks of the revolution—of any revolution—even if they cannot be quantified and do not leap at the reader from printed sources.

Such shifts are also likely to be violent, chaotic, and downright nasty. Property and class relations do not change according to neat, consensual agreement. Thus, the revolutionary process, especially the agrarista process, is littered with accounts of conflict, violence, resistance and factionalism. Contrary to some versions, this does not mean that agrarismo was a mere vehicle for elite control, that the peasants were manipulated clients, or that the ejido was an alien imposition (Knight 1988). As Paul Friedrich has brilliantly shown, Machiavellian politics, caciquismo, agrarianism, and violence flourished together and—we may go further—it would have been remarkable if they had not. The destruction of the old order in rural Mexico could hardly proceed according to the principles of Western European social democracy. The ugly face of agrarismo was inseparable from its progressive, transforming face, and there is little point in moralizing about it, or damning the agrarista cause for its deviations from some ahistorical norm. "The revolution is the revolution," as Luis Cabrera said; and, if this sententious phrase means anything, it means that we should take the Revolution as it was, put value judgments to one side, and try to tell what happened, why, and what its significance was. We should not lament lost Arcadias or try to tell the Revolution where it went wrong.

Finally, revisionism grossly homogenizes Mexican history, even as it often asserts Mexico's infinite geographical variety. Guerra squeezes the long nineteenth century into a pretty crude dichotomy (such that the *yorkinos* of the 1820s have to cohabit with the Magonistas of the 1900s: both are carriers of modernity). Statolaters yoke postrevolutionary history to the inexorable onward march of the state. Important conjunctures are therefore neglected: 1910, when, to almost universal surprise, the old regime spectacularly collapsed; 1915, when the triumph of Carrancismo on the battlefield ensured that state-building would promptly resume and that the popular movement would have to reckon with the revolutionary state and its newly incumbent norteño elites; 1934–1935, when a new, radical project was forced upon the political agenda (not least, by popular pressures); and, finally, 1938–1940, when that project

faltered, when its enemies rallied, and when the tide turned in favor of a rival agenda (the one which, in pretty tattered form, still remains on the table today).

To conclude: we know a lot more about the Revolution than we did twenty years ago; our sources and methodologies have greatly diversified; in particular, our knowledge of the Revolution's regional variations and embodiments has grown apace, and with it (though to a lesser extent) our understanding of certain key themes—peasants and caudillos, workers and caciques. This has not necessarily improved our general grasp of the Revolution in its totality. Case studies are not easily integrated into general syntheses; they may actually impede synthesis. Syntheses also demand theoretical underpinnings and, to my mind, these have often been inadequate, or downright misconceived. And a good deal of revisionism has been revisionism for its own sake, simplistic inversions of the old orthodoxy or bold rebuttals of a caricatured orthodoxy. To my mind, the orthodoxy—the work of perceptive observers and participants—had and still has a lot to recommend it. It is flawed in important respects (it exaggerates the nationalist/xenophobic content of the Revolution, it often sees "Indians" where some would prefer *campesinos* ), but its basic vision of a popular agrarian revolution, which overthrew the old regime and contributed to a series of decisive changes in Mexican society—changes which, perhaps, constitute elements of a "bourgeois" revolution—remains valid. It deserves careful qualification rather than outright rejection. As we of the third historiographical generation look back on those who went before, we owe them, perhaps, rather more Confucian respect and rather less Oedipal resentment.

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