Two Decades of Anglophone Historical Writing on Colonial Mexico: Continuity and Change since 1980

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Research on colonial Mexico continues to be one of the most important subfields in the history of Latin America in the United States, but also enjoys considerable attention in Canada and Britain. Over the last twenty-five years or so the approach of English-speaking scholars to colonial Mexican history has changed perceptibly. The rise of cultural history, ethnohistory, and subaltern history, for example, are all fairly obvious trends, even if they were already on the horizon by 1980 or so. On the other hand, more traditional styles of work such as economic history have continued strong. The article traces these changes, both in general terms and by looking at a number of monographs and clusters of works. One conclusion of the essay is that the rise of cultural history, ethnohistory, and the search for “subaltern agency” in more recent historical writings, by de-emphasizing the scalar measurement of Mexican underdevelopment so characteristic of older types of writing, have made for a less grim and monochromatic image of colonial Mexican society.

* I would like to acknowledge that an invitation from Marshall Eakin, at that time chair of the Program Committee of the Conference on Latin American History, to give a paper on almost exactly the same theme as this essay at the CLAH-American Historical Association meetings in San Francisco in 2002 started me thinking about these issues and trends; in the event (to my embarrassment), the paper never materialized, so that this essay constitutes a commitment (belatedly) kept. Matthew O’Hara made some useful comments on an early, partial draft version of this essay. My thanks also go to Karen Lindvall-Larson, Latin American Bibliographer at the University of California, San Diego’s Social Sciences and Humanities (Geisel) Library, for her timely assistance with bibliographical matters.
ya se vieran en el horizonte historiográfico desde 1980. Por supuesto, los trabajos tradicionales, como los que versan sobre historia económica, han seguido fuertes también. Este artículo esboza estos cambios, en términos generales como también a través del examen de varias obras monográficas y grupos de obras. Una conclusión del ensayo es que el ascenso de la historia cultural, la etnohistoria, y la búsqueda de la “agencia subalterna” en las obras históricas más recientes, ha pintado una imagen menos sombría y monocromática de la sociedad mexicana de la época colonial, al desenfatizar la medición del desarrollo mexicano como si existiera en una escala lineal (una característica del estilo de los trabajos más tradicionales).

I think there is little doubt that the Oregon squall will evaporate in big words. American politicians are very good at these. I wish you were in as tranquil a state in your own domestic politics. Mexico has the elements of a great nation, but these seem to be in too disorderly a condition to allow the country any fair prospect of developing its natural resources. Yet I trust the time may come.

—William H. Prescott in a letter to Lucas Alamán, September 28, 1846.¹

Since William H. Prescott casually penned these words to Lucas Alamán in 1846 (the two great historians never met), there has been a tendency for Anglophone writers on Mexican history to treat Mexico as a problem—a problem in economic backwardness, a problem in political instability, a problem in the oscillation between radical reform and stubborn conservatism—in short, as a museum of unmodernity. What can only be seen as a patronizing attitude (tinctured with schadenfreude) has been widespread among U.S. intellectuals but also deeply embedded in the collective perceptions of Americans as a whole.² Condescension has, of

¹. W.H. Prescott to Lucas Alamán, Boston, September 28, 1846; Centro de Estudios de Historia de México Conumex, Archivo Lucas Alamán (Fondo CCLXXXVII), carpeta 17, exp. 1385. Prescott was alluding to the conflict between the United States and Britain over their respective claims to Oregon, settled by President Polk in a treaty of 1846.

². This attitude is not limited to Mexico alone, but extends to all of Latin America and even beyond. Another factor contributing to this posture, along with those discussed below, is the “white man’s burden” attitude probably also shared with British thinking (particularly in the imperial vein) toward non-European areas of the world. There is an enormous literature in several disciplines dealing with this issue, which also lies near the heart of post-colonial studies; for an interesting recent treatment, see David Cannadine, Ornamentalism: How the British Saw Their Empire (Oxford: Oxford University Press, 2001). Furthermore, the very nature of the historian’s craft itself, which is just as likely to dwell on apocalyptic narratives of the human past as triumphantist ones, encourages these biographies of failure.
course, been mixed with genuine sympathy for the political travails of a sister republic, much empathy for the struggles of common people and admiration for Mexican heroes, and an enduring attraction to what, in even the most sophisticated of American eyes, is seen as the cultural exoticism of the country.\(^3\) Even when the purple prose of popular historical narratives is lightened a shade or two, the sheer sweep, grandeur, and tragedy of Mexican history are hard to deny, from the awe-inspiring religious icons of the Mexica to the great political upheavals that have racked the country—the Spanish conquest, the independence struggle, the internecine wars of the early nineteenth century, foreign intervention and national dismemberment, the epic revolution of the early twentieth century, and even the dramatic political reversals of the last decade or so.

The construction of alterity takes many forms, and to some degree Anglophone scholars have invented in their historical writings, albeit unconsciously, a notional (and national) other for their own cultural and political purposes, even though the Anglophone cultural world (one thinks here of the United States, Britain, and Canada) always has been sufficiently different from that of Mexico to require little embroidery to make the point. There is more than a whiff of teleological thinking in this tendency, since in the end it seeks to explain outcomes seen as virtually predetermined: why the United States has been so successful economically, and more recently so dominant on the world stage, while Mexico has often been viewed as the theater for processes of change that were not completed, went awry, produced unintended negative consequences, or were never initiated in the first place. Since the advent of modernization theory and its leftist doppelgänger dependency theory, this case has been put most clearly and powerfully by economic historians of neoclassical bent, although scholars working in a more forthrightly Marxist vein also have made important interventions.\(^4\) There is

3. See, for example, Helen Delpar, *The Enormous Vogue of Things Mexican: Cultural Relations Between the United States and Mexico, 1920–1935* (Tuscaloosa: University of Alabama Press, 1992). These lines are written virtually the same day as Edward Said’s death; the ideas in Said’s *Orientalism* (New York: Pantheon Books, 1978) might well be applied to the establishment of a United States academic specialization on Mexico, to the power relations that have underwritten it and the institutional arrangements that have sustained it, and to the deconstruction of Anglophone writing on Mexico over more than a century. This essay is far too short to attempt such an account.

nothing necessarily sinister about this intellectual process, but it has worked ideologically and historiographically to depress the perception of Mexico’s historical destiny while elevating that of the United States, in particular, turning the former into a laboratory of corruption and failure, and the latter into one of virtue and success. While there has been among some American intellectuals a tendency to see Europe as a hot-house of degeneracy because of its overripe civilization (think of Henry James’s depictions of innocent Americans and corrupt Europeans, for example), Mexican society often has been seen as under-civilized, both archetypes serving roughly the same ends.

It is one of the chief contentions of this essay, however, that as cultural history and the theoretically distinct history of common people (I will refer to this as “subaltern” history throughout this essay not because I particularly like the word, but because it is rhetorically easy to use, and to a lesser degree because it packs a useful density of connotation) have emerged during the last two decades as major sub-genres of Anglophone historical writing on Mexico, they have worked, whether consciously or not, to attenuate the sharpness of the Manichean dyads of success/failure, model/problem. Cultural history has done this by injecting a strong element of relativism into the picture, concomitantly directing the gaze of many scholars away from scalar measurements focused on explicitly or implicitly unilinear concepts of economic, political, and social development, and toward hermeneutic or interpretive inquiry in which societies are studied more in their own terms, less in regard to a grand narrative. Previously thought uninteresting or inaccessible, the newer objects of inquiry include collective (or even individual) mental processes; various sorts of sensibility and systems of meaning (religion, gender, ethnicity); ritual, celebration, and forms of sociability; mechanisms of the social reproduction of knowledge; the construction of group identities, and so forth. There is by now, of course, a heated and fairly well known ongoing scholarly debate as to whether this has been a positive or negative development for Mexican history specifically, and more broadly as to whether the practice of history in this country as a whole

skidded off the road at the linguistic turn and crashed through the epistemological guard rail. Whichever view one takes of the issue, however, it is undeniable that within the last ten to fifteen years, cultural history (and the social history and subaltern history with which it is allied and from which it is in practice often indistinguishable) has come to occupy a prominent place in Anglophone writing on Mexico and on Latin America more generally, staking out relatively new fields for itself (native-language-based ethnohistory, for example, is an obvious success story, within limits explored briefly below) and revisiting others with some new questions (political history, for instance).

Influential and increasingly visible as it is, however, cultural history has not by any means swept all other genres or approaches off the board, nor has it dragged the field into a swamp of postmodernism, as some scholars have professed to fear. Anglophone historians of colonial Mexico are following multiple tracks: they are still doing more traditional forms of social history, neoclassically oriented economic history, prosopography, intellectual history (although very little of it), and so forth, all to very good effect. To take but one example, while it is true that economic history has been less pursued of late among Anglophone historians of the colonial period (although in Mexico it seems to be thriving),

5. The fullest airing of these issues in the Latin Americanist context occurred in the special number of the Hispanic American Historical Review dedicated to “Mexico’s New Culture History ¿Una lucha libre?,” HAHR 79:2 (May, 1999), with essays or critical interventions by myself, William E. French, Mary Kay Vaughan, Stephen Haber, Florencia E. Mallon, Susan Migden Socolow, and Claudio Lomnitz, and a useful brief introduction by Susan Deans-Smith and Gilbert M. Joseph. Alan Knight took up the cudgels in his article, “Subalterns, Signifiers, and Statistics: Perspectives on Mexican Historiography,” Latin American Research Review 37:2 (Spring, 2002): 136–58, in which, after finding that cultural history counts among its efforts more failures than successes, he nonetheless concludes that “we do not have to choose between the pomo funny farm and the positivistic prison. There are plenty of green fields in between” (156). The discussion as to the limits and potential of cultural history vis-à-vis more traditional but no less vital genres finds echo in much recent monographic and historiographical writing; see, for example, Matthew Restall’s useful review essay on native-language-based ethnohistory in Mesoamerica, “A History of the New Philology and the New Philology in History,” Latin American Research Review 38:1 (February, 2003): 113–134. Restall characterizes this scholarship as strongly empiricist rather than unduly given to “political posturing” (126), a passing reference it would be difficult to disassociate from Stephen Haber’s suggestion (“Anything Goes: Mexico’s ’New’ Cultural History,” HAHR, 309–330) that “new” cultural history is the half-life product of dependency theory in decay, a redemptive project distorted by left-wing political sympathies.

6. On ethnohistory, Restall, “The New Philology;” and on politics, see Gilbert M. Joseph, ed., Reclaiming the Political in Latin American History: Essays from the North (Durham: Duke University Press, 2001)—this is what I take to be the project of this interesting anthology of essays, at least in part, although none of the authors deals explicitly with writings on the colonial period.
the growth in sophistication and technical adeptness of what is being done would surely have surprised an earlier generation of practitioners, among them those of us who handled the quantitative aspects of economic history by counting on our fingers and toes. On the other hand, some works that fall well within the rubric of cultural history, or perhaps of social history with a cultural component, are in fact rather traditional in some ways. Take, for instance, native-language-based ethnohistory. Although there is a wide range in the degree of interpretive freedom and the broader claims that ethnohistorians allow themselves, much of what is being done in “the new philology” is distinctly redolent of the old comparative philology of the eighteenth and nineteenth centuries, and it is difficult to imagine any method more solidly empirical than this. To paraphrase Mark Twain’s quip about premature rumors of his death being greatly exaggerated, therefore, hysterical or dismissive denunciations of postmodernism in the field have exaggerated its influence. If by postmodernism one means the denaturalization (but not necessarily the dismantling) by scholars of teleologies and metanarratives such as inevitable colonial maturation, liberal progress, or the development of capitalism; close attention to the subtleties of language and symbol; and methods concomitantly emphasizing local knowledges, thick description, and multiple narratives; then yes, postmodernist influence is rampant in cultural history. But if one means by postmodernism the introduction of an unfetteredly radical epistemological relativism (a la Paul Feyerabend), the denial of external reality, or an indifference to the criticism of evidence on the grounds that all evidence, mediated as it is by inherently unstable language, is simply codified point of view, then the careful reader of recent historical writings on colonial Mexico will find its authors to be more modernist than post-modernist, and a remarkably conservative bunch at that. More useful critical questions about cultural history are: Does it tell us something interesting about colonial society? Does it work as a framework of explanation? Is it susceptible to reasonable evidentiary tests? Does it advance us into realms of thought and behavior inaccessible by other means?

Subaltern history has tended in the same direction as cultural history—one might call it “the disenchantment of structural materialism”—by introducing the notion of agency into the study of common people

7. Several historians offer essays on the economic historiography of Mexico in the colonial and national periods, under the overall editorship of Antonio Ibarra, in a special number dedicated to the field (and to the memory of the late Ruggiero Romano) in Historia Mexicana, no. 207, vol. 52:3 (January-February 2003); of particular interest is Ibarra’s own essay, “A modo representación: la historia económica mexicana de los noventa, una apreciación general,” 613–647. Especially notable of late has been the activity of the Asociación Mexicana de Historia Económica.
within the colonial order. Subaltern history or the study of the condition of post-coloniality are not necessarily the same as cultural history, although they may go together and are often conflated: subaltern history does not necessarily focus on culture, nor need cultural history focus on common people. There is no inherent reason why cultural history should be limited to subaltern groups (or post-colonial societies)—cultural understandings do not atrophy as one’s disposable income rises, although they may become less folkloric. Nor are the construction and imputation of meaning monopolies of proletarians, the socially marginalized, or the decolonized. It is through this mistaken but widely credited association that misapprehensions about cultural history as a redemptive political project may have arisen.8 On the other hand, where forms of popular resistance do not necessarily emerge into the public record (as they do, for example, in cases of deviance, riot, and rebellion), the cultural lens—turned on subaltern social practices with an eye toward deciphering how common people negotiated their lives within the colonial arrangement and understood the world around them—naturally tends to focus on expressive forms such as religious life. The overall effect of the interest in popular agency is to have made subalterns (women in some cases, indigenous people, slaves, the mentally deviant, the poor, the socially marginalized, etc.—most of Mexican colonial society at any given moment, in other words) appear more resilient, and the colonial order more porous, more a matter of negotiation, of “adaptive resistance” (in Steve Stern’s terminology), and even of self-cooptation.9

There are a number of factors responsible for this downward shift in the historian’s gaze. One of them is the demonstration effect of early modern European historiography (i.e., many of us are still looking for our own Menocchio).10 Another is our working our way down the food-chain of sources, expectably, from the prescriptive, normative, institutional to the descriptive, historical, and inferential.8 For an insightful (if at times rather shrill) exploration of the distinction between cultural history and the history of common people, see the book of labor historian Bryan D. Palmer, Descent into Discourse: The Racialization of Language and the Writing of Social History (Philadelphia: Temple University Press, 1990): “Critical theory is no substitute for historical materialism; language is not life” (xiv); and for a defense of cultural history (albeit not an uncritical one), Victoria E. Bonnell and Lynn Hunt, eds., Beyond the Cultural Turn: New Directions in the Study of Society and Culture (Berkeley: University of California Press, 1999).


tional, high-political, and canonical cultural texts (although these are also being read and re-read with different eyes now) to what a scant scholarly generation or so ago was considered archival dross—records of litigation and criminal accusations, for example, or the testimonies of common people. A third is the influence of theoretical and/or comparative scholarship from other disciplines (e.g., political scientist James C. Scott's work on resistance, to cite but one body of work among many). This recent emphasis on subaltern agency does not mean that colonialists are constructing a new White Legend of colonial society, a narrative of self-deluded colonial rulers and crafty colonized natives carrying on life by their own disguised rules alone under the complacent but stupid gaze of their oppressors, but rather that they are painting a more nuanced picture of what formerly looked pretty uniformly grim and monochromatic.

Taking into account some of these changes, the goal of this essay is to map the scholarly geography of Anglophone writing on the history of colonial Mexico over the last two decades or so, years that correspond to the life of Mexican Studies/Estudios Mexicanos. But within this broad rubric there are certain limits. Despite some emphasis on cultural and subaltern history as growing sub-genres, it is not my intention to produce another installment in the continuing discussion over the nature and value of these categories; the essay is therefore meant to be analytical and descriptive, not polemical and prescriptive. The colonial historiography has changed notably since the 1970s or so, a change marked, for example, by a perceptible decline in economic history, the emergence of the concept of political culture, and the application of anthropological conceptual frameworks. In large measure, this change has been due to the impact of the early modern historiography of Europe, as noted above, to which historical writing on colonial Mexico is linked by several substantive affinities in early modern Atlantic World history itself—monarchical structures of government, aristocratic types of social power, a widely prevalent world view strongly religious in tone, a


12. While this review article is not meant to be celebratory of the journal, by the way, it should be noted that Mexican Studies/Estudios Mexicanos has done much to foster the interdisciplinary study of Mexico in this country (although not colonial history in particular), as have a number of academic centers dedicated to Mexican studies, among them those in the University of California system as a whole (the University of California Institute for Mexico and the United States [UC MEXUS], headquartered at the University of California, Riverside), the University of California campuses in San Diego, Los Angeles, and Irvine, the University of Chicago, and the University of Texas-Austin, among others. In the interest of full disclosure I should note that I have for some years served on the UC MEXUS Editorial Board of the journal, and more recently as its chair.
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pre-industrial economic life, the enduring presence of peasantries, and so on—as well as by the influential models of French Annaliste and post-Annaliste writing.\(^{13}\) It even makes a good deal of sense to treat as a single unit the 1750–1850 period, which overlaps the traditional divide between the colonial and national eras while leaving the earlier centuries of colonial rule behind. This is one of the most interesting trends in the reconfiguration of Anglophone historical studies on Mexico, especially where a narrative structure linked to the biography of an institution, or contained by a relatively homogeneous body of documentation, makes the spanning of late colonial and early republican eras possible or even imposes it, as for example in Silvia Arrom’s recent book on the history of the Mexico City Poor House, or Brian Connaughton’s on clerical thinking and political change in Guadalajara, respectively.\(^{14}\) Despite the logic

13. For a recent restatement of some of these trans-Atlantic affinities in the early modern period, see Alan Knight, Mexico, 2 vols. (Cambridge: Cambridge University Press, 2002).

14. Silvia Marina Arrom, Containing the Poor: The Mexico City Poor House, 1774–1871 (Durham: Duke University Press, 2000); and Brian F. Connaughton, Clerical Ideology in a Revolutionary Age: The Guadalajara Church and the Idea of the Mexican Nation (1788–1853), translated by Mark Alan Healey (Calgary: University of Calgary Press and University Press of Colorado, 2003). An older example, dating from the very beginning of the historiographic era I am examining here, would be David A. Bradley’s Haciendas and Ranchos in the Mexican Bajio: Leon, 1700–1860 (Cambridge: Cambridge University Press, 1978). Originally published in Mexico in Spanish, Connaughton’s book has deservedly been made accessible to a broader Anglophone reading public in a new series in Latin American and Caribbean history (which already embraces four monographs on Mexico) initiated by Christon I. Archer of the University of Calgary, the series general editor. [Kudos are also due William Beezley for helping to make the publisher Scholarly Resources an important force in publishing increasingly high-quality monographs and scholarly anthologies on Latin American and Mexican history, some of which touch on the colonial period.] To return to the issue of periodization, another recent example of the overlapping framework, on a smaller scale, at least temporally, is Richard A. Warren’s accomplished study of popular politics, Vagrants and Citizens: Politics and the Masses in Mexico City from Colony to Republic (Wilmington, Del.: Scholarly Resources, Inc., 2001); and at the other end of the spectrum, the hugely ambitious, theoretically sophisticated work of Carlos A. Forment, Democracy in Latin America, 1760–1900, Vol. 1: Civic Selfhood and Public Life in Mexico and Peru (Chicago: University of Chicago Press, 2003). While in the cases of both Warren and Forment colonial political culture is integral to their stories, the colonial period really serves more as a prolegomenon or base-line than as a full partner in their narratives. As a practical matter, a full “Age of Revolution” periodization is most likely to be employed in anthologies of scholarly essays in which almost all the authors write in a monographic mode on either the colonial or the national era and the editor or commentator attempts synoptically to place the temporally bounded essays within the larger framework. For two examples published within the last decade, see Jaime E. Rodríguez O., ed., Mexico in the Age of Democratic Revolutions, 1750–1850 (Boulder: Lynne Rienner Publishers, 1994) and Victor M. Uribé-Urán, ed., State and Society in Spanish America during the Age of Revolution (Wilmington,
of this approach, however, this essay strays relatively little across the colonial/national divide, and then only allusively, since it proved too ambitious to cover in a limited space recent writing about the first two generations or so after the gaining of independence in addition to all of the colonial period. On the other hand, I have found it useful to discuss some works not authored by Anglophone historians, and in some cases only made available relatively recently in English, that have either been widely influential or which exemplify certain trends worth portraying. Examples here would be Juan Pedro Viqueira Albán’s work on Bourbon reformers and stubborn forms of popular street culture in late colonial Mexico City, or Serge Gruzinski’s on religion and the indigenous imaginaire in the wake of the conquest. Needless to say, finally, an essay of this length cannot possibly include everything, and even some interesting themes and key works have necessarily been left aside or only mentioned

15. For a wide-ranging and astute overview of historical writing on Mexico by both Mexicans and foreigners, see Enrique Florescano, Historia de las historias de la nación mexicana (Mexico City: Taurus, 2002), who is more concerned with depicting broad changes in how Mexicans (among them historians) have viewed their own history over time, than with the detailed investigation of changes in approach and methodology, the use of new sources, the emergence of new interpretive models, and so forth. Regarding the period under review here, from about 1980 on, Florescano takes a very dark view of how the professional writing of history has (or has not) developed in Mexico itself: “[E]n el lapso que va de 1980 a fines del siglo pasado [i.e., until 2000] se advierte, en lugar de un ascenso progresivo de los métodos que pusieron en alto el cultivo de Clio, una caída de los niveles establecidos por la historiografía profesional, un deterioro alarmante de las instituciones, fallas en la formación de profesores e investigadores y una perdida del vigor intelectual que animó la fundación de esos centros” (445). He particularly faults Mexican historians for the fragmentation and over-specialization of history as a discipline, and for the incapacity of the field to “ofrecer a la nación una historia de la nación,” which sounds suspiciously like a pitch for a return to historia patria (449). In a sense Florescano is describing precisely what has happened in Anglophone writing on Mexican history, as well, under the influence of the pervasive localization of the discipline and the drawing back from grand narrative projects and strategies, except that he sees European and North American historians as being more reflective and critical regarding changes in the discipline.

in passing. Some readers will surely find certain discussions unduly swollen (that on economic history, for example), others unduly compressed (on ethnohistory), still others strangely absent (the growing Anglophone literature on Mexican independence).17

**Some General Trends in the Anglophone Historical Literature on Colonial Mexico**

To start with (and at some risk, perhaps, of quantifying the obvious), it may prove useful to sketch in some general tendencies in the literature

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17. There are contradictory logics as to whether to include the English-language literature on Mexican independence in an essay such as this one—i.e., independence was the end of the colonial period, or it was the beginning of the national period (it was, of course, both). I have opted to leave aside this literature for lack of space. Modern Anglophone work on the theme can be traced back at least to Hugh M. Hamill, Jr.'s still admirable The Hidalgo Revolt: Prelude to Mexican Independence (Westport: Greenwood Press, 1981; originally published 1966), continued with Timothy E. Anna's The Fall of the Royal Government in Mexico City (Lincoln:University of Nebraska Press, 1978), and saw the publication of two key works in the mid-1980s, Brian R. Hamnett's Roots of Insurgency: Mexican Regions, 1750–1824 (Cambridge: Cambridge University Press, 1986) and John Tutino's large-scale From Insurrection to Revolution in Mexico: Social Bases of Agrarian Violence, 1750–1940 (Princeton: Princeton University Press, 1986). As academic entrepreneurs of the highest efficacy, Jaime E. Rodríguez O. and Christon I. Archer have encouraged this work through the editing of anthologies and authoring of reviewarticles, including Jaime E. Rodríguez O., ed., The Independence of Mexico and the Creation of the New Nation (Los Angeles: University of California Press, 1989), and Rodríguez O., ed., Mexico in the Age of Democratic Revolutions, 1750–1850 (Boulder: Lynne Rienner, 1994); Christon I. Archer, ed., The Wars of Independence in Spanish America (Wilmington, Del.: Scholarly Resources, Inc., 2000), and Archer, ed., The Birth of Modern Mexico, 1780–1824 (Wilmington, Del.: Scholarly Resources, Inc., 2003); and Rodríguez O.'s masterful broad history of Spanish American independence, The Independence of Spanish America (Cambridge: Cambridge University Press, 1998). More recently Anglophone historians of this period have begun to turn in the direction of the social history of the independence struggle, popular politics, and political culture as reflected and shaped by the outcomes of the insurgency against Spain, but this tendency has yet to advance very far; see especially Peter F. Guardino, Peasants, Politics, and the Formation of Mexico's National State: Guerrero, 1800–1857 (Stanford: Stanford University Press, 1996); Richard A. Warren, Vagrants and Citizens: Politics and the Masses in Mexico City from Colony to Republic (Wilmington, Del.: Scholarly Resources, Inc., 2001); and Eric Van Young, The Other Rebellion: Popular Violence, Ideology, and the Mexican Struggle for Independence, 1810–1821 (Stanford: Stanford University Press, 2001).
we are reviewing by resorting to a few statistical indicators about what is being published in English on colonial Mexico in some of the foremost scholarly journals in the field, and about what young scholars are writing in their doctoral dissertations as they train to be professional practitioners of the historian’s craft. A similar but more limited exercise carried out by me in a review article nearly twenty years ago, at just about the time Mexican Studies/Estudios Mexicanos was getting under way, categorized in very broad strokes all the articles on the social and economic history of Mexico and Central America in the period 1750–1850 published between about 1960 and 1985 in the Hispanic American Historical Review (HAHR). This analysis indicated that, counted by quinquennia, there had been a “take-off” in the number of articles published starting around 1970 (representing a doubling over the quinquennium 1960–64, and remaining steady through quinquennium 1980–84). Of greater importance, perhaps, was a notable change in the language of article titles, especially from the late 1960s or so, with pre-1960 key words such as boundary, treaty, party, war, and so forth, giving way increasingly to a more self-consciously social-scientific vocabulary likely to include terms such as socioeconomic, stratification, factors, and elite. I also found that the late eighteenth century was beginning to receive much greater attention than the early nineteenth, although I noted that if the domain had been expanded to include political themes, including the wars of independence, this imbalance would have been redressed to some degree.

More recently I carried out a survey of the 477 substantive articles in the HAHR (excluding other types of pieces, as, for example, published interviews with some of the field’s senior figures) that appeared between 1970 and 2001, embracing not just Mexico or the colonial era, of course, but all of Latin America throughout the region’s history. The object of this exercise was to see what the trend has been with regard to economic

18. I am aware, of course, that a number of caveats are to be kept in mind about such a procedure. Editors of journals, for example, may be following their own biases in what they solicit or accept for publication, rather than reflecting in as neutral a way as possible the prevailing interests in a given field. Moreover, a different (perhaps wider) canvass of professional journals might produce different findings, as might changes in the categories used in identifying articles or dissertations with certain trends. Nonetheless, such a counting exercise is likely to prove valid grosso modo for arriving at broad trends.

history as opposed to other genres. Aggregated by five-year periods, again (and smoothing out certain anomalies), the figures suggest that whereas during the earlier quinquennia the proportion of essays on economic history tended to stand fairly steady at between 30 and 40 percent, beginning around 1990 it had dropped to about half that level (15–20 percent) and has remained there for nearly fifteen years. Reverting just to colonial Mexico yet again, for purposes of the present essay I revisited all the articles published in the HAHR between 1976 and 2003, inclusive. Of the 423 articles that appeared during these nearly three decades, fifty (including long interviews with senior historians in the field) dealt with colonial Mexico. This amounts to about 12 percent of the total (with little variation among the quinquennia 1976–1980, 1981–1985, 1986–1990, 1991–1995, 1996–2000, and the first ten issues of quinquennium 2001–2005), making this perhaps the best represented sub-specialty in the field, hardly surprising given its venerable tradition and the pattern of graduate training in this country. Furthermore, the same trend is noticeable in the analysis regarding the place of economic history within the overall profile of Latin American history present in the historiography of colonial Mexico (at least as represented by what appears in the HAHR), and with roughly the same chronology: a drop-off in frequency of articles on economic history since about 1990. Themes represented with greater regularity in the last dozen years include politics and political culture, forms of religious sensibility, and ethnohistory and indigenous peoples, with an accompanying change in vocabulary. Other journals, other tastes: an analysis of some other major academic venues produces some results that are similar to these, and some that differ. A rapid perusal of the Journal of Latin American Studies issues during the decade of the 1990s reveals that very little was published on colonial Latin America in general, and concomitantly few con-

20. Eric Van Young, “La pareja dispareja: breves comentarios acerca de la relación entre historia económica y cultural,” Historia Mexicana 52 (2003): 831–70. Articles on Mexico were not culled out for separate treatment, but there is no reason to think that the trend would not hold for these, as well. In an article in the same number of the journal, Antonio Ibarra noted that a new wave of economic history was gathering force among Mexican scholars themselves during the same decade, even as economic history seemed to be waning somewhat in the United States; Antonio Ibarra, “A modo de presentación: historia económica mexicana de los noventa,” Historia Mexicana 52 (2003): 613–48.

21. The ten issues (containing thirty-eight articles) published during the years 2001–May, 2003 represent half a quinquennium, during which there would normally be twenty issues of the journal (there was a double issue during these years, combining August and November, 2001); the data were compiled from J-Stor.

22. It is interesting to note that the total number of articles per quinquennium declined steadily from ninety-three in 1981–1985 to sixty-four in 1996–2000, a drop accounted for (as any reader of the journal will attest) by the increasing length of contributions.
tributions on colonial Mexico. This is, of course, a multi-disciplinary jour-
nal (like the Latin American Research Review and Mexican Studies/Estudios Mexicanos itself) in which work on economic history (primarily of the nineteenth and twentieth centuries) remains particularly strong and well represented. On the other hand, historical journals in the United States such as The Americas reflect trends similar to those analyzed for the Hispanic American Historical Review. The U.S. journal in which the cultural history trend (even as opposed to social history in the traditionally understood sense of the term) is most pronounced is the Colonial Latin American Review, established about a dozen years ago, dedicated exclusively to the colonial period, and oriented strongly toward literature, cultural studies, and anthropology.

While acknowledging that the data sets discussed here overlap but were not constructed according to quite the same criteria, it is still possible to suggest some tentative characterizations of the Anglophone historical literature on colonial Mexico over the last two or three decades, set against the historiography on Mexico as a whole. First, beginning some time in the late 1960s, our vocabulary became less traditionally narrative, descriptive, and political, and increasingly sociological and anthropological. About twenty years later it shifted again, into a still more anthropological register this time, with growing overtones of cultural studies, post-colonial studies, gender and women’s studies, and ethno-history. Second, along with this trend in approach, conceptualization, and vocabulary, the late eighteenth century and the early nineteenth (I have elsewhere referred to the period 1750–1850 as “Brading’s Century”) attracted a good deal of attention as opposed to the earlier colonial period and the three decades between the Mexican-American War and the advent of Porfirio Diaz. Lately, this interest has perhaps drifted both forward, into the 1821–1876 period, and backward again into the early colonial era, thus reprising studies of an earlier era (with the seventeenth century still constituting something of a black hole, historiographically speaking).\(^\text{23}\) Finally, economic history ebbed somewhat after 1990 or so, giving way to social and cultural history, at least in the pages of the \textit{HAHR}, although this can be seen in other journals, as well.\(^\text{24}\)

An analysis of the doctoral dissertations on colonial Mexican history


\(^\text{24}\) A highly visible form of scholarly recognition within the field of Latin American history itself, the Bolton-Johnson Prize (formerly the Herbert E. Bolton Memorial Prize) of the Conference on Latin American History, would appear to highlight the centrality of Mexican colonial history generally, and within it the increasing dominance over recent
completed at U.S. universities since the mid-1970s or so bears out these findings quite closely. This analysis warrants at least brief attention, since today’s dissertations are tomorrow’s first scholarly monographs, and the trends to be discerned in theme and approach say a good deal about how historians in the field are being trained and choosing their

years of social and cultural history more specifically. Of the fifty winners or honorable mention award recipients of the prize during the twenty-four years 1980–2003, sixteen have dealt with colonial Mexico. This represents nearly 32 per cent of the total, or almost three times the percentage represented by articles on colonial Mexico published in the *HAHR* during a roughly comparable period of time, and slightly more than the 26 per cent of the prizes (23/88) garnered by writers on colonial Mexico over the life of the prize since 1966. This indicates that historians of colonial Mexico are carrying the palm more frequently in recent years. Of these twenty-three prize-winning works eight have focused on economic history, but only two of these were published after about 1980—Elinor G. K. Melville’s A Plague of Sheep: Environmental Consequences of the Conquest of Mexico (Cambridge: Cambridge University Press, 1994), winner in 1995; and (a work arguably as much social as economic history) Susan Deans-Smith’s Bureaucrats, Planters, and Workers: The Making of the Tobacco Monopoly in Bourbon Mexico (Austin: University of Texas Press, 1992), honorable mention in 1993—and six between 1965 and 1980.

I thank Professor Thomas Holloway, CLAH Executive Secretary, for furnishing this information on short notice.

The information is drawn from a computer search of University Microfilms International’s “Digital Dissertations” database. A search using keywords “colonial” and “Mexico” produced a list of 415 dissertations in several disciplines (with theses in literature being perhaps even more numerous than those in history), the earliest from 1940 (Woodrow W. Borah’s U.C. Berkeley dissertation, “Silk-Raising in Colonial Mexico”), and the most recent from 2003. Of these, I was able to determine that 112 citations pertain to historical treatments, the overwhelming majority of them earning their authors doctorates in history. There is also represented a scattering of theses historical in approach (fourteen), but originating in other disciplines—historical anthropology, historical geography, history of science, economics, U.S. history, and so forth. Since several of these have been influential among historians in the more restricted sense of the term, have become part of the historiographical landscape, or represent notable trends in the field of colonial Mexican studies, I have felt it justified to include them in this discussion. Among these are Jorge Cañizares-Esguerra’s How to Write the History of the New World: Historiographies, Epistemologies, and Identities in the Eighteenth-Century Atlantic World (Stanford: Stanford University Press, 2001) (history of science, University of Wisconsin-Madison, 1995); Laura A. Lewis’ Hall of Mirrors: Power, Witchcraft, and Caste in Colonial Mexico (Durham: Duke University Press, 2003) (anthropology, University of Chicago, 1993); Melville’s A Plague of Sheep (anthropology/history, University of Michigan, 1983); and Herman W. Konrad’s A Jesuit Hacienda in Colonial Mexico: Santa Lucia, 1576–1767 (Stanford: Stanford University Press, 1980) (anthropology, University of Chicago, 1975). The list includes dissertations substantially devoted to the colonial period, even if their subject matter lies partially outside it; works on northern New Spain, including New Mexico, were included, while those on colonial Florida were excluded. For reasons not entirely clear to me (sloppiness? distinct agreements between UMI and different institutions? indexing criteria not captured by my search strategy?) the list appears far from complete (my own 1978 U.C. Berkeley dissertation is omitted, for example), and may have dropped as many as several dozen items. Furthermore, the coverage seems most complete for the years since 1980 or so, spottier for the decades preceding.
initial research subjects. To begin with, as I have noted above, there has been a second shift in vocabulary as indicated by dissertation titles (which presumably reflect the themes and approaches of the articles and books that grow out of the theses). Whereas in the late 1960s one saw the increasing frequency of a more hard-edged social science language, since the late 1980s we are more likely to see words such as identity (identities), culture(s), power, personhood, manhood, gender, sexuality, race/ethnicity, and so forth, along with descriptors or compound forms such as negotiated (negotiating), cultural politics, and social construction. The most common among these are probably “culture” and “identity.” Furthermore, since these terms are common to several disciplines, and since cultural historians have turned more and more to the close exegesis of written (if not necessarily high cultural) texts in their research, if one were to go by the titles alone it would be easy to mistake dissertations in history for those from other fields, especially literature.

As reflected in this newer vocabulary, old themes have been reconfigured into cultural history and new themes introduced. If one aggregates into a group of expansive categories the dissertations produced during the last five years (1999–2003), for example, one sees that of the twenty-six works, the themes of religion/religious sensibility and identity/ethnicity run neck and neck with seven theses each; economic his-

26. It is interesting to note where these dissertations are being produced. Not surprisingly, of the 112 theses on my list, thirteen were done at the University of California, Los Angeles, and a dozen at Tulane University. These two institutions are followed by Columbia University and the University of California, Berkeley, each with seven theses; the University of Wisconsin-Madison and the University of California, San Diego, each with five; and by a group comprising the University of Arizona, the University of Chicago, and the University of New Mexico, each with four. Institutions that have produced two or three dissertations on colonial Mexican history include Emory University, Northern Arizona University, the University of Pennsylvania, Stanford University, Duke University, the University of California, Santa Barbara, Princeton University, the State University of New York-Stony Brook, the University of Minnesota, the University of North Carolina-Chapel Hill, Syracuse University (all in historical geography), the Catholic University of America, and the University of Texas at Austin. Twenty other institutions have each produced one dissertation. This rough breakdown does not include distribution over time, in which there is a good deal of variation for any given institution.

27. For example, Felicia Smith-Kleiner’s 2003 dissertation in literature from New York University, “The Reformation of the Female Body: Gender, Law, and Culture in Colonial New England and New Spain,” might well be taken for a dissertation in history, while Doris M. Namala’s 2002 history thesis from UCLA, “Chimalpahin in his Time: An Analysis of the Writings of a Nahuat Annalist of Seventeenth-Century Mexico Concerning his own Lifetime” or Nora Jaffary’s 2000 history dissertation from Columbia University, “Deviant Orthodoxy: A Social and Cultural History of Ilusos and Alumbrados in Colonial Mexico” might be thought works in literary scholarship. Some of my readers are saying to themselves “Ah! That’s just the problem!,” but the convergence in vocabulary and even approach is undeniably there.
tory boasts four; health/medicine, gender, and institutional/political history each have two; and political culture, one. Extending this categorization back to 1980 or so to embrace a period of about twenty-five years confirms this general tendency. Although it is difficult to squeeze out an unambiguous trend, it is clear enough that economic history, with its ten or so dissertations in the quinquennium 1979–83 and half as many during 1984–88, has declined in overall weight, at least in this country. In the meantime, religion/religious sensibility, with one or two theses in each of the four quinquennia 1979–1998, has picked up momentum in the last few years along with studies of identity/ethnicity, while studies of gender make a strong showing but are perhaps better represented in the field of modern Mexican history. Older, more developed, and still fruitful genres of study such as economic and political history (the latter in a restricted sense of the term) have slipped a notch or two, then, but not yet to the margins of the colonial historiography, where they would nestle with forms now almost completely out of fashion, such as biography and the more traditional history of institutions like religious orders or the organs of colonial government. On the other hand, recent works in a more cultural vein have revisited the records of colonial institutions and turned them to non-institutional uses.

The Emerging Geography of Colonial Mexican History

The general currents sketched above have carved out new features in the Anglophone historiographic landscape of colonial Mexico while leaving some formerly prominent landmarks, if not high and dry, then at least isolated and exposed to erosion. On the other hand, while certain tendencies in the historical literature would appear temporarily exhausted, what were before distinct genres have become “blurred,” to borrow Clifford Geertz’s expression, producing works definitely hybrid in that they are not easily pigeon-holed into exclusive categories such as “institutional history,” “economic history,” “demographic history,” and so forth.

28. Creating these categories involved some compression. For example, when a dissertation has seemed to me to embrace both gender and religious sensibility, I have assigned it either to one or the other rubric depending upon my knowledge of the work in question or my impression of its primary emphasis. The absence of a thesis in ethnohistory from the quinquennium 1999–2003 is an oddity, since this has always been a strongly represented category.

29. This is one of the central points made in the review article of Matthew O’Hara, “Politics and Piety: The Church in Colonial and Nineteenth-Century Mexico,” MSHM 17 (Winter, 2001): 213–31.

Some of these changes have been sudden and would scarcely have been imaginable twenty or twenty-five years ago, while others have been incremental and amount more to the increasing sophistication of familiar genres. The rest of my essay offers some examples of veins that have played out, others that we are still working, and still others only recently discovered, or if previously known, only within the last few years beginning to yield high quality (and in some cases abundant) work.

The Decline of Hacienda Studies

One striking example of how a historiographic field formerly central to the development of Anglophone writing on colonial Mexico has languished is that of hacienda studies, once so numerous that designating this sub-genre as a fairly distinct field of “studies” seems not at all unjustified. The extended scholarly debate in the 1960s, ’70s, and early ’80s over the nature of the Mexican hacienda (against a background of large landed estates elsewhere in Latin America, and in medieval and early modern Europe) and the institution’s place in rural society, often still framed even today in terms of competing “capitalist” and “feudal” models, deeply influenced Mexican historiography in general and that of the colonial period in particular. Nor was this a purely academic debate: there were political stakes involved, especially outside Mexico, in Latin American countries that had yet to experience anything resembling an agrarian revolution. For if the “traditional” rural estate was capitalist (or proto-, quasi-, or even crypto-capitalist) from its inception in the sixteenth century, rather than feudal, then the emergence of a bourgeois

1983), 19–35. There are a number of possible ways of tracing in the colonial historiography the changes I have mentioned: by contrasting pairs of works, by looking at entire sub-literatures, by examining individual scholarly careers, etc. I have opted for a combination of all these, but for the sake of creating several broad categories I have admittedly pushed many works into a historiographical Procrustean bed.

31. A recent synthetic/interpretive treatment of Latin American history affirms the supplanting of “medieval” (i.e., feudal) forms of economic organization by “capitalist” ones only in the late eighteenth century; see Stuart F. Voss, Latin America in the Middle Period, 1750–1929 (Wilmington, Del.: Scholarly Resources, Inc., 2001), xii. Alan Knight, on the other hand, in his Mexico: The Colonial Era (Cambridge: Cambridge University Press, 2002) (especially 72–83, 185–201, but throughout), spends a good many pages discussing this once heated controversy. He concludes that while the notional ideal-typical hacienda may have been commercialized, it was not capitalistic in any meaningful sense of the term, but certainly that forms of landholding were the single most important factor shaping Mexican colonial society. For a review article assessing the state of play in the field of hacienda studies as it stood about 1982 or so, see Eric Van Young, “Mexican Rural History Since Chevalier: The Historiography of the Colonial Hacienda,” Latin American Research Review, 18 (1983): 5–61; and for a slightly later one (playfully eccentric but useful), see William Schell, Jr., Medieval Iberian Tradition and the Development of the Mexican Hacienda (Syracuse: Syracuse University Press, 1986).
order and the ensuing social revolution were not as distant as Marxist thinkers of radical tendency might once have feared. While some studies in English shared in the dependency paradigm so influential from the 1960s, such as Andre Gunder Frank's small but closely reasoned book on early colonial agriculture (which actually looked rather antique even as it became available in English some years after its initial publication), most work by North American or British scholars—books by William Taylor, Charles Harris, David Brading, Herman Konrad, myself, Richard Lindley, and others—followed on the assumptions of neoclassical models. Up until the early 1980s scarcely a year passed without the appearance of one or more such books and a host of articles, but this current of scholarship seems to have shrunk to a trickle during the rest of the decade and then virtually disappeared. Toward the end of this wave one still saw excellent studies being produced, such as Cheryl Martin's study (1985) of the colonial Morelos sugar industry, but Lolita Gutiérrez Brockington's book on the seventeenth-century haciendas marquesanas of the Cortés estate in Tehuantepec was already somewhat isolated when it appeared in 1989, as was Patrick Carroll's interesting 1991 study of free and enslaved blacks (and the forms of rural economic units on which they labored) in colonial Veracruz, while Robert Patch's excellent 1993 work on Yucatán (which, ironically, disputed the narrative of Yucatecan exceptionalism) was really an outlier of this literature.

One is hard pressed to think of major studies in English on this theme that have appeared within the last decade, and younger scholars these days (those doing their doctoral research) seem not to be interested in the topic. When one does see studies of rural economy in more recent years, rather than focusing on the economic workings of rural estates as


such they are quite likely to be cast in terms of environmental history and/or the struggle over resources between powerful land owners and humble people, as with Elinor Melville's work on sheep culture and its environmental consequences in the Mezquital Valley, or Sonya Lipsett-Rivera's on water rights and farming in late colonial Puebla, respectively. While the decline in this sub-field is obvious, the reasons for it are less clear. Among them must surely be counted the advent of other approaches to the history of the colonial countryside, such as the history of rural resistance, peasant uprisings, and political movements. For example, John Tutino's bold and widely influential study of peasant politics over the course of Mexican history, From Insurrection to Revolution in Mexico (1986), dedicates more than half its substantial length and much of its most original archival findings to the late colonial period and Mexican independence. But Tutino's primary objective is to illuminate agrarian pressures and the resulting collective political violence rather than the workings of colonial grain-producing haciendas as such, while his doctoral dissertation, more in the traditional mold of a 1970s regional agrarian study (much like my own, in this respect), remains as yet unpublished. Then, too, although the empirical findings of hacienda studies demonstrated the polymorphousness of the landed estate in colonial Mexico, they tended to keep well within certain parameters, showing less and less variation from one work to the next, so that over the course of a scholarly generation the genre lost the attraction of novelty. Finally, many of the Anglophone (and even Mexican) scholars who made contributions to the historiography of the hacienda followed roughly similar trajectories in their research, moving from the economic history of the colonial and nineteenth-century Mexican countryside, to studies of social movements or protest, and then to cultural or even intellectual history, and thus in some measure abandoning the field (no pun intended) of basic economic history to pursue more elusive themes. To cite but three examples, this has certainly been the case with William Taylor, David Brading, and myself, and, among prominent Mexican scholars, with Enrique Florescano.

Some time in the 1970s or early 1980s, as the reach of the colonial


historiography broadened and deepened, rural history, primarily embracing hacienda studies, began to be subsumed under the rubric of the “new regional history” in the sense that everything in the country outside the Valley of Mexico tended to become known as “regions” or “provinces” (often with a heavily pejorative connotation). The oversaturation (historiographically speaking) of the central parts of the country, the increasing organization and accessibility of provincial archives, and the need to test hypotheses—about the “feudal” nature of the colonial rural estate, for example—all contributed to this outward shift of focus. Although a technical or conceptual definition of what regions were, and how they changed over time, was hardly ever offered, many scholars confidently carved regions out for study, often centering on urban areas. In addition to the hacienda studies mentioned above, which followed in their turn several important books dealing with colonial silver mining published in the 1960s and 1970s (another sub-literature that has virtually dried up), this regionalization of colonial history produced a number of what may be called single-stranded works that plucked out wider contexts for close inspection certain aspects of social or economic life. One fine, widely influential, but hardly unique example of

36. While a personal anecdote hardly constitutes proof of this trend, my own experience in selecting a venue for my dissertation research in rural history demonstrates these tendencies at work. Around 1970 or so I was looking for a relatively uncrowded part of colonial Mexico in which to do my own work, and had considered the Valleys of Puebla and Cuernavaca, when in a casual conversation Enrique Florescano suggested the Guadalajara area because of its rich archival resources and limited modern historiography. My initial ambition was to write a social history of rural society there, but finding I did not have the conceptual tools for such a project I fell back on the economic history of the city and its hinterland, and of the great estates there, producing the sort of single-stranded regional history discussed below. My own doctoral students have since produced much more sophisticated work on rural society both in Mexico and elsewhere. Another influence in this outward pressure in rural, regional, or provincial history was the hugely important work of François Chevalier, although much of what he had written in fact dealt with the Mexican north. In a recent revision of his own work in the light of more recent studies of rural history, Chevalier himself has acknowledged that his study of the colonial hacienda offered a somewhat abstract, generalized model that needed to be confirmed or disconfirmed by regional case studies; see François Chevalier, La formación de los latifundios en México. Haciendas y sociedad en los siglos XVI, XVII y XVIII (Mexico City: Fondo de Cultura Económica, 1999), 33 (where the sub-heading “Caos regionales” must surely be a misprint for “casos regionales”).

37. For a discussion of how regions might usefully be defined, specifically drawing upon the colonial economic historiography, see Eric Van Young, “Doing Regional History: Methodological and Theoretical Considerations,” Conference of Latin Americanist Geographers Yearbook, 1994 (vol. 20), 21–34; and Eric Van Young, “Introduction: Are Regions Good to Think?,” in Eric Van Young, ed., Mexico’s Regions: Comparative History and Development (La Jolla, California: Center for U.S.-Mexican Studies, University of California, San Diego, 1992), 1–36, and several of the other essays in that volume.
such a study is historical anthropologist John Chance’s 1978 work on colonial Oaxaca, which emphasized race relations, the fluidity of racial ascription over time, and the increasing weight acquired by wealth and class status in a society previously thought to be dominated by the rigid *sistema de castas*. Other representatives of the genre, dating from slightly later than Chance’s book, are historical geographer Michael Swann’s study of settlement and demographic patterns in colonial Durango, Evelyn Hu-DeHart’s ambitious but still basically structuralist study of Yaqui-Spanish interaction up until Mexican independence, and, more recently still, Rik Hoekstra’s study of the Valley of Puebla in the late sixteenth and early seventeenth centuries.

Works that two decades ago, or even a decade, might have taken the form of single-stranded histories—of political structures, for example, or elite groups, or of racial ascription practices, such as explored by Chance—have now mutated instead into more complex narratives of localities through time, although work in the older style is still very much being done as well. French-style *histoire totale* models were certainly of some belated and ongoing influence here, as was the totalizing micro-history of the sort promoted in Mexico by the late Luis González. Probably also influential—even where authors might shrug their shoulders at the suggestion, if not actively disavow it—has been the overt influence of anthropology in a particularly Geertzian, “local knowledge” register, in contrast, for example, to the broader political-economy sort of model characteristic of Eric Wolf’s late work. This approach emphasizes the interrelatedness of social practices and cultural meanings, the inextricability of major strands of such practices and meanings from their context (the “thick description” method is surely about explaining why such extraction or abstraction is not a good idea, and has always been in the repertoire of good historians), and the need to study these processes in concrete localities, the physical settings where people live. Recent well executed examples (both published within the last decade) of this localized, intensified total history of communities, albeit in what can still


be described as a structuralist mode, are Frank de la Teja’s study of San Antonio de Béxar and Leslie Offutt’s of Saltillo, both in the eighteenth century. Yet another is Cheryl Martin’s fine study on eighteenth-century Chihuahua, Governance and Society in Colonial Mexico (1996). Given the breadth of her study within a limited venue, what Martin seems in fact to intend by “governance” is perhaps better conveyed by a more expansive term, “such as political culture,” which embraces power relationships in many spheres of life and the way these dispose people to establish and live with structures of authority and legitimacy, in this case specifically the relationships among ethnicity, social hierarchy, and political subordination. Martin weaves together not only forms of livelihood, including mining, ranching, and labor relations, but also the symbolic presence of the frontier, elements of public ritual practice, and ideas about honor, race, and gender. In doing so she creates a multi-stranded cultural history and is able to make suggestive comparisons about how Chihuahua was similar to, and different from, society in central Mexico, the template upon which many of its essential social categories were modeled. This concern with culture is characteristic of many recent studies of provincial society, and shows up in the post-independence period as well. Martin’s own trajectory as a scholar in fact reflects the sort of shift suggested here, from her earlier study on sugar production and labor systems in the Morelos sugar zone, to a more complex study of a diminutive frontier social order: they are both works on local communities, but one is single-stranded and economic in emphasis, the second multi-stranded and largely social and cultural.

Economic History

Many works in colonial economic history are as much respecters of place as the multi-stranded regional or local history just mentioned, which itself found one of its important sources in economic studies of the hacienda. To embrace effectively all the variables and production factors in play in a given economic activity, sector, or locality, economic historians typically delimit the venues of their work while often making broader claims for the processes they describe as characteristic of a larger class of phenomena. There are some exceptions to this in the form of large synoptic works that are nonetheless based partially on strong archival re-
search, and which attempt to paint a picture of the economy of New Spain as a whole, typically in the late colonial period. The two most obvious examples of recent years are Richard Garner's ambitious study of economic growth in the eighteenth century (1993) and Arij Ouweneel's very interesting, quirky, and unfortunately little-cited study of central Mexico during roughly the same era (1996). Although both works are heavily quantitative, neither is obtrusively econometric, Garner's more interested in deploying mathematical models of explanation, Ouweneel's in interpreting his data in the light of ecological and demographic frameworks derived from European agrarian history and theories developed to make sense of it. Both authors find that the silver-based late colonial economy of New Spain was in some ways running to stay in place and was poised by 1800 (if not earlier) to undergo a crisis, a point of view a good deal different from that prevailing twenty-five years ago, and one that expresses the darker view of the late colonial period generally held nowadays.43 Most "big" histories of colonial Mexico, indeed, or histories of the country more generally, tend to be economic and political in emphasis even where their central theme is not economic history per se. One would have thought, for example, that the debate as to whether colonial Mexico was a feudal or capitalist society had lost much of its heat by now (the discussion as it stood in the early 1990s is glossed well in the English-language preface to Semo's History of Capitalism in Mexico), but Alan Knight's confident and highly readable recent synoptic history of Mexico, cast firmly in a political economy vein, dusts it off to good effect and concludes that although the late colonial economy displayed a high degree of commercialization, it was a long way from being capitalist both in its modes of production and social relations.44

43. Richard L. Garner, with Spiro E. Stefanou, Economic Growth and Change in Bourbon Mexico (Gainesville: University Press of Florida, 1993); Arij Ouweneel, Shadows Over Anahuac, An Ecological Interpretation of Crisis and Development in Central Mexico, 1730–1800 (Albuquerque: University of New Mexico Press, 1996); and for a closely reasoned defense of late colonial Mexico as prosperous and economically dynamic, Jaime E. Rodríguez O., Down from Colonialism: Mexico's Nineteenth-Century Crisis (Distinguished Faculty Lecture, University of California, Irvine, 1980). For a discussion of these issues see various chapters in Eric Van Young, La crisis del orden colonial: estructura agraria y rebeliones populares en la Nueva España, 1750–1821 (Mexico City: Alianza Editorial, 1992), mentioned here by virtue of its Anglophone authorship. Also worthy of note is the scholarly anthology by Nils Jacobsen and Hans-Jürgen Puhle, eds., The Economies of Mexico and Peru during the Late Colonial Period, 1760–1810 (Berlin: Colloquium Verlag, 1986), although the individual essays are monographic rather than comparative.

44. Knight, Mexico: The Colonial Era, xvi, and note 32 above. In addition to Knight, see for example Brian Hamnett, A Concise History of Mexico (Cambridge: Cambridge University Press, 1999); but for exceptions, some of the essays in Claudio Lomnitz-Adler, Deep
While not numerous, narrower monographic works in economic history have spanned a broad range of approaches and have made for some of the most interesting contributions to the colonial historiography since the early 1980s, even while they take into account the subtleties of place. Anthropologist Ross Hassig's elegant 1985 study of exchange and transportation systems in the Valley of Mexico in the wake of the Spanish conquest knits together considerations of the nature of the Aztec and Spanish colonial states, native population decline, and reconfigured market networks to conclude that the sixteenth century actually saw a substantial involution in productivity and organization, a sort of conquest-induced entropy. Nor have the genre-bending and -crossing potentialities of labor history (is it economic, social, political, cultural, or all of these?) gone unrealized, as in the somewhat celebratory study of mining labor and protest in Real del Monte by Doris Ladd (1989), for example, or the detailed and eloquent work of Susan Deans-Smith on the colonial tobacco monopoly and manufactory in the late colonial period. Economic sectors, activities, or regional economies earlier treated from a strictly institutional point of view have come in for revisionist attention that uses econometric techniques to explain their internal dynamics, the logic of their operation, and their macroeconomic context, albeit in some cases with a reconfigured attention to institutions in the mode inspired by the work of Douglas North. One may detect here, at least in part, a turn inward and downward analogous to that taking place with cultural history. Two outstanding examples are Richard Salvucci's 1987 work on textile mills throughout the colonial period, and Jeremy Baskes's recent study (2000) of the cochineal industry in late colonial Oaxaca. The
more technically mathematical of the two monographs, Salvucci’s book
concludes that the obrajes were capitalist enterprises, of a sort, albeit
existing within a context of imperfect markets and resource allocation,
coerced labor, and (after independence) a hostile international envi-
ronment in which they simply could not compete, and that they there-
fore failed to form the basis for a lasting capitalist transformation. And
there could be no more telling indication of what the sophisticated mode
of economic history has brought to the field over the last couple of
decades than the contrast between Jeremy Baskes’ book and Brian Ham-
nett’s foundational and still valuable 1971 study of the Oaxacan cochineal
trade.47 While both works are heavily institutional in their frameworks
(that is, they emphasize the effects of ongoing, sometimes codified eco-
nomic and political arrangements such as forms of state regulation),
Baskes’ study is much more deeply immersed in the details of how the
cochineal trade actually worked, and less with the prescriptive and nor-
mative aspects of institutions as such. Granted, the objects of the two
studies were different, as clearly expressed or implied in their titles: Ham-
nett’s was to look at the notorious repartimiento and the cochineal in-
dustry in the context of the Bourbon Reforms, while Baskes wanted to
look at the micro-economics of production as mediating relations among
Spanish officials, entrepreneurs, and native people. In a highly revisionist
move, Baskes has turned the arrangement of the repartimiento on its
head, demonstrating that coercion of native producers was minimal and
difficult, that the institution proved a relatively efficient way of allocat-
ing scarce and expensive capital, that apparently extortionate price
markups and rates of interest on loans to native producers constituted
risk premiums for Spanish traders in a volatile economic environment,
and so forth.

Allied to economic history because of their strongly materialist-
empiricist interests and methods are works in historical demography,
huahua), the colonial silver-mining sector seems virtually to have dropped out of the
picture for Anglophone historians since the path-breaking studies of scholars such as

historical geography, and environmental history. These have followed with remarkable fidelity the channels carved out early on by the work of the Berkeley historical demographers (Lesley B. Simpson, Woodrow W. Borah, Sherburne F. Cook) and historical geographers (Carl Sauer, Sherburne Cook). The 1970s saw publication of Cook’s and Borah’s Essays in Population History, its last volume appearing only in 1980, and, in the year following, Jack Licate’s little-cited but valuable study of the historical geography of the sixteenth-century Valley of Puebla. A decade later followed the demographic studies of Northwestern New Spain by Daniel Reff (1991), and a long probative exercise on the Cook-Borah disease-induced catastrophe model for the Valley of Mexico by historical geographer Thomas Whitmore (1992). The latter work, in particular, brought the technical capacity of computers to bear upon the perennial discussion of post-conquest native population collapse in manipulating large data-sets and for statistical simulation. Whitmore provided substantial vindication for the more apocalyptic views of the Berkeley school of historical demographers that Old World diseases induced an indigenous population drop of something like 90 per cent over the course of the first century or so after the Europeans’ arrival. Environmental history looked to be coming on strong with the publication of Elinor Melville’s 1994 study of the Mezquital Valley, and with Michael C. Meyer’s work of a decade earlier on water in what he called the Hispanic Southwest (the old Boltonian Borderlands), but the field has stalled for reasons that are not clear—perhaps because of the intensive site-specific field work required to carry it out, or because of the expertise in ecology and biology needed to do it convincingly. Melville’s compelling study found stimulation in the earlier work of Sherburne Cook and concluded that the environmental degradation of the Mezquital obvious as early as the later sixteenth century was produced by Spanish over-grazing of sheep rather than by a late pre-conquest human population surge (the third in a series showing a periodicity of about 400 years), as Cook had postulated. Reff’s work on northwestern New Spain produced analogous conclusions with regard to native population decline. While Meyer’s work is heavily legal and institutional in emphasis, Melville’s can without too much effort be assimilated at least partially to a cultural model in which the invading Europeans made choices not always dictated by an obvious rationality. As for the demographic work, it is worth noting that it has transcended materialist history (Charles Gibson drew extensively upon the Cook-Borah-Simpson studies, after all, in his 1964 Aztecs Under Spanish Rule, although he did not have the entire corpus of work.

48. Works in these categories are bundled together here for lack of space, although they are a very heterogeneous group.
available to him when he wrote) and made its way into almost all scholarship on native peoples and ethnohistory, even the most avowedly cultural of it. James Lockhart’s now well known stage-model of central Mexican native language change, for example, can be mapped onto the familiar demographic curve for the Valley of Mexico quite neatly, and even onto Lesley B. Simpson’s still earlier monograph on land use in central Mexico in the sixteenth century. Still more recently, and in a more explicitly cultural register, we have the superb hybrid 1996 work—is it art history, cartographic history, historical geography, cultural history?—of Barbara Mundy on native spatiotemporal representations as exemplified in the seventy or so maps submitted with the relaciones geográficas of the late sixteenth century mandated by the Spanish metropolitan authorities.49

Elites, Commoners, Race, Class, and Gender

Among the strongest works in colonial history published through the first decade or more of the period under review here were those lying at the intersection of economic and social history, corresponding to the study of elite groups and elite individuals, much of it focusing on the colonial capital, and some of it overlapping temporally in interesting ways the late colonial and early national periods. While this had always been a prominent tradition in the colonial historiography, in my 1985 inventory of An-

Anglophone historical writing on Mexico and Central America in the “Age of Revolution,” I was able to point to the “socialization of elite studies” characteristic of the period, of which the avatar was David Brading’s 1971 Merchants and Miners. Overlapping with the wave of hacienda studies, and drawing its impulse from much the same sources—early modern European models (e.g., Lawrence Stone’s work), the use of notary, intestate, and litigation records, and the localization of colonial social power in groups of merchants, miners, and large landowners—, their studies appeared regularly from the late 1970s through the early 1990s. Nestled among fine works such as Doris Ladd’s study of the Mexican nobility (1976), Charles Nunn’s on foreigners in Mexico City in the early Bourbon period (1979), Linda Arnold’s interesting book on bureaucrats in Mexico City in the late eighteenth and early nineteenth centuries (1988), Robert Himmerich y Valencia’s prosopography of sixteenth-century encomenderos (1991), and Jackie Booker’s study of Veracruz merchants in the late colonial-early national period (1993), two of the most interesting and influential of these books have been Louisa S. Hoberman’s on seventeenth-century merchants in the capital (1991), and John Kicza’s on colonial entrepreneurs in late colonial Mexico City (1983). Hoberman’s book has the great virtue of dealing centrally with the seventeenth century, still very much a historiographical black hole, and of looking at actual capital flows in commerce and at the relationship of the colonial state to merchants’ dealings, as well as with the social history of the group. Kicza’s book has proved foundational and of enduring interest, since along with earlier work by Brading he sketched out patterns of career mobility, marriage alliance, and strategies for the preservation of family wealth among elite merchants, entrepreneurs, and landowners.

50. Van Young, “Recent Anglophone Scholarship,” 733.
Perhaps these historians of colonial elites did their work too well, since this rich vein of study seems to have played itself out by the early 1990s, much as hacienda studies had done a decade earlier, to be succeeded by the downward drift of colonial historians’ gaze to the popular classes, in keeping with what has happened in other national historiographies. One of the more widely influential and floridly culturalist examples of social history in this genre is Juan Pedro Viqueira Albán’s engrossing 1999 study of popular recreations, social control, and Bourbon modernization efforts in Mexico City, a book that glitters with a distinctly Gallic élan despite its birth as a thesis in Mexico. Viqueira Albán’s work raises one of the major methodological difficulties faced by historians of subaltern groups (and culture, especially popular culture), which is the thinness of the sources on groups that typically do not enter the written record except in general descriptive statements, or when they bump up against the state and thus generate criminal documentation. What we have yet to see, at least in the form of monographs, are studies that take up where Viqueira Albán has left off, either in expanding on his account, deepening it, or extending it outside Mexico City, the venue for most of the works just mentioned. Yet what Viqueira Albán has done, along with the authors of the colonial chapters (and the work as a whole in a conceptual way) in the influential anthology on public ritual in Mexico edited by William Beezley, Cheryl Martin, and William French, is to describe the teeming ritual and celebratory life of the lower classes in the colonial period, to construe it as resistance to the hegemonic claims and modernization projects of the colonial regime, and in the process to lighten colonial life a shade or two and complicate it by introducing notions of subaltern agency. Considered as a whole, this is one of the two most notable new trends in Anglophone historical writing on colonial Mexico over the last fifteen years or so, the other being the emergence of ethnohistory, which overlaps subaltern history in obvious ways.

In keeping with this decentering of established modes of historical inquiry, and the turn of many historians toward the social margins for their subjects, the conceptual cluster of class, race, and gender that has so transformed the landscape of U.S. historical writing more generally has also had considerable influence on the Anglophone historiography on colonial Mexico. Aside from the profusion of ethnohistorical works

(given separate treatment below), however, the trend has not progressed as far as one might have thought. Of this trinity, class has tended to recede from the picture somewhat since it appeared so prominently in the title of John Chance’s book on Oaxaca a quarter-century ago, although it still remains a strong ghostly presence at the banquet, mostly in the form of the idea of racial or ethnic drift in relation to economic position.54 The issue of race itself as a determinant of social being appears at its starkest in the case of African slaves and the fate of Afro-mestizo populations. In relative terms, colonial Mexico was not a major slavocratic society, which probably explains why Colin Palmer’s 1976 study of blacks in the mid-colonial period was succeeded by only a very few major works on African slavery and free people of color during the last two decades or so, including Gerald Cardoso’s unconvincing comparative work on Veracruz and Pernambuco up to 1680 (1983), and Patrick Carroll’s deeply documented study on colonial Veracruz (1991). Apart from these focused works, where there were slaves (virtually everywhere in New Spain) or free laborers of African background, they are mentioned in passing, sometimes in considerable detail (e.g., in Cheryl Martin’s 1985 work on colonial Morelos), but always as part of a larger picture. More recent works on African-origin populations in the colonial period have been concerned less with slavery than with the apparently more fashionable issues of ethnic identity and consciousness, ethnic blending, social mobility, and so forth. These include Ben Vinson’s finely realized study of free-colored militia units, centering primarily on the eighteenth century, and Herman Bennett’s book on an emerging Afro-Creole consciousness in the mid-colonial era.55 For the late seventeenth and early

54. On the eruption of the social margins into the center of historians’ concerns in United States history writing (which has an obvious parallel in the emergence of postcolonial studies), see Joyce Appleby, Lynn Hunt, and Margaret Jacob, Telling the Truth About History (New York:Norton, 1994). It is telling about the state of play in the debate about race and class in colonial Mexico — did race determine class position, or vice versa; and of what did casta position consist? — that the terms of the debate seem to have been set by about the early 1980s. For example, a recent brief account of the issue by Ben Vinson, III, Bearing Arms for His Majesty: The Free-Colored Militia in Colonial Mexico (Stanford: Stanford University Press, 2001), 4 and note 9, p. 240, mostly cites historical works authored before the mid-1980s or so. For a prolonged discussion of why class analysis, and models of revolution based upon it, do not work well in explaining the advent and processes of the Mexican independence movement because of the prominence of ethnic conflict, see Van Young, The Other Rebellion, especially 1–39, 495–523.

eighteenth centuries we have for the sistema de castas more broadly the suggestive work of R. Douglas Cope on Mexico City, which also emphasizes the fluidity of racial categorizations and the considerable degree of agency of common people in turning the caste system to their own purposes, and in forming patronage alliances with their white “superiors” that allowed them to temper the grosser disadvantages of ethnic ascription.56

The third member of the trinity is gender, and although works on women, gender, and the family can be treated as separate historiographical categories, they are deeply interrelated and are combined here for purposes of brevity. The most widely known pioneering work in this field was the book on women in Mexico City by Silvia Arrom (1985), preceded by the articles and dissertations of other scholars, most notably Asunción Lavrin, and included here by virtue of its being at least half dedicated to the colonial era. Relying upon a variety of strongly institutional sources, Arrom tempered the dichotomous view of colonial women as either considerably autonomous, well-off widows in control of substantial property or submissive wives within a heavily patriarchal social order, to show difference across life-cycle, class, and ethnic group. Although women’s status changed for the better in some respects with the advent of Mexican nationhood, a cult of the domestic sphere and something resembling the “republican motherhood” so characteristic of the newly independent United States came to subvert the promises of nationhood and liberalism. Less sanguine still in its view of women’s freedom to exercise choices about their lives—in this case the selection of marriage partners—in the late colonial period, was Patricia Seed’s book of 1988, which took the discussion further into the realm of cultural history with its discussion of racial ideas, concepts of honor, and the theology of marriage, as did Richard Boyer’s interesting study (1995) of colonial bigamy, family formation, and forms of community social control. While none of these three major path-breaking books bore particularly


Press, 2003); and see also Andrew B. Fisher, “Worlds in Flux, Identities in Motion: A History of the Tierra Caliente of Guerrero, Mexico, 1521–1821” (Ph.D. dissertation, University of California, San Diego, 2002), for a thorough study of the problematics of ethnic identity in the Balsas River Depression of colonial Mexico, including the self-reinvention of free people of color as Indians.

on systems of gender relations, sexuality, women, or the family among indigenous groups, the 1997 anthology Indian Women of Early Mexico has gone a notable distance in opening up questions of rural and native women’s participation in community and political life, as actors within the family, and as property-holders. Laura Lewis’s recent Hall of Mirrors (2003) has taken us even further along this path, not only demonstrating the hybridity of theme we might expect from a historical anthropologist, but also a turn toward the multi-stranded approach—religion, practice (albeit heterodox), ethnic identity, and gender, all rendered through “thick description”—that relates the work to the local total histories mentioned above. Finally, Steve Stern’s extremely thoughtful and wide-ranging book on gender relations in the late colonial period, the system of what he refers to as “gender rights,” and the projection of ideas about gender into the political realm in the post-colonial era, makes an admirable contribution to this burgeoning literature. Stern delves more deeply than most other authors who have ventured into this field into the actual gender system in which the women whose lives and deaths he evokes were enmeshed. He demonstrates how male notions of honor, claims upon women for sexual, child-rearing, and domestic services, and the inclination of women to protect themselves if those claims crossed accepted boundaries into excessive male violence, were mediated by family relationships, public opinion, and women’s social networks. From the methodological point of view, Stern provides in his book a fine object lesson in how to read often ambiguous criminal records (in this case, of violence against women) to maximum effect. It is worthy of note, however, that Stern’s analysis is framed more in terms of a subalternist than a cultural history approach, making it clearer than in some other studies what the difference is between the two frameworks.57

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Resistance and Rebellion

To return to studies of popular culture and resistance per se exemplified by the work of Viqueira Albán, it is but a short step narratively (if a longer one theoretically) from descriptions of street and ceremonial life to the studies of resistance by common people that, construed in their broadest meaning, can encompass everything from ethnohistory, to works on the native imaginary and collective memory, to religious practice, and certainly the background and processes of Mexican independence. Such an analytic move does make of resistance much too plastic a category, and therefore a dull instrument, so that the first three categories of work mentioned, each broad enough in itself, will come in for separate treatment below. There has been a small but interesting literature by Anglophone historians over the last two decades, however, on the institutional integuments of colonial life. These have followed less the pattern of earlier works on the formal structures of church and state that formed much of the basis of colonial Mexican studies, and have focused more on the legal arrangements of social control within a context of persistent and invidious ethnic distinction, broad social inequality, and recurrent stress between metropolis and colony. A pioneering work in this respect was the 1979 book of William Taylor on drinking, homicide, and rebellion in the later colonial period, in which he demonstrated through a comparative approach the social embeddedness and normative limits of both drinking behaviors and homicide among village Mexicans, as well as the “bargaining by riot” that went on when an always outnumbered colonial officialdom often gave ground to villagers’ political and economic grievances in seeking to suppress local uprisings. The means by which state-indigenous interaction was kept within juridical boundaries, and by which Indians at the same time learned to manipulate the colonial system to defend their interests, were detailed in Woodrow Borah’s magisterial 1983 study of the General Indian Court (a line of research he claimed to have inherited from Lesley B. Simpson). Two pivotal studies of the colonial legal system (both emphasizing culture) appeared in 1995, one on the Mexican north by Charles Cutter, in which he demonstrated the existence of a prevailing legal culture much less arbitrary and crude than we had thought, the other by Susan Kellogg on the adaptation of central Mexican indigenous people to Euro-
pean legal norms and practices in the two centuries after the conquest. Cognate to Taylor's work on village deviance and homicide, and to an earlier, strongly institutional study by Colin MacLachlan (1974) of the famous rural constabulary, the Acordada, was the book on crime and the system of criminal procedure in Mexico City in the eighteenth century by Gabriel Haslip-Viera (1999), in which the author specifically disavowed the idea of street crime as resistance and assimilated it more to a safety-valve model, seeing it therefore as a mechanism for the maintenance of social order. Finally, Silvia Arrom (2000) has traced in great detail, in the fashion of a European social and institutional historian, the fate of a failed Bourbon experiment in social welfare (and, it goes without saying, social control) in the form of the Mexico City Poor House, demonstrating how the stubborn culture of private charity and begging—i.e., the informal practices and values organizing these activities—circumvented state efforts to corral the capital's poor and after the coming of independence turned the institution itself into an increasingly impecunious orphanage.58

Full-court colonial resistance movements, primarily in the form of native uprisings, have attracted a good deal of attention from English-speaking historians in the last fifteen years or so, and are likely to continue to do so because of their dramatic nature, the often abundant documentation generated by them, and the window they open onto indigenous life and European-indigenous relations not only during periods of armed confrontation, but also during “normal” times. In a classificatory exercise such as this, the difficulty arises of separating works on resistance from those in ethnohistory, and the division is ultimately somewhat arbitrary, particularly outside the central areas of Spanish colonial domination. The genealogy of many such studies can probably be traced, whether consciously or not, to this country's experience with a

stubborn peasant war of national liberation in Vietnam, as well as to a long tradition of studies of popular political disturbances and revolutions in American and Western European historiography, and in the historiography of Mexico, both colonial and modern. Because this body of historical literature is relatively cohesive, often addressing the same issues and working with or against a body of common theoretical and comparative works, many introductions to the books in the field cover the literature well (e.g., Schroeder, 1998 and Deeds, 2003), so I will not belabor it here.59 Studies of this nature include portraits of quite prolonged conquest and frontier situations, such as those in the Sierra Zapoteca of Oaxaca by John Chance (1989), the Maya area of southeastern Yucatan up to about 1700 by Grant Jones (1989), the more compressed history of the early sixteenth-century conquest of the Tarascans by J. Benedict Warren (1985), and, more recently, colonial Nueva Vizcaya (embracing parts of modern-day Sinaloa, Durango, and Chihuahua) over some 250 years by Susan Deeds (2003), in which stress between indigenous groups and European invaders was endemic over hundreds of years, and the best that could be hoped for on the part of the colonists was a sort of armed and uneasy truce.60

Works on Indian resistance movements as such (the N here is admittedly pretty small) have tended to bifurcate into northern and southern branches because of the location of the episodes themselves and the patterns of colonial acculturation and exploitation that brought them forth. But it is interesting to note that the northern studies of resistance often are embedded in broader ethnohistorical treatments, such as those of Susan Deeds (2003), Evelyn Hu-DeHart (1981), and Cynthia Radding (1997), since no northern indigenous groups generated the sort of large-scale movements seen in the Mexican southeast. An exception to this is Roberto Salmeron’s 1991 book on Indian revolts in northern New Spain, a relatively short treatment of a large subject in which half the work is devoted to a survey of frontier development and half to the actual uprisings themselves. For the central regions of the country we

59. For a somewhat more extended discussion of the theoretical and comparative aspects of resistance, rebellion, and insurrection among rural people, see the Introduction (1–36) of Van Young, The Other Rebellion.

have the highly suggestive essays of Serge Gruzinski, in 1989’s *Man-Gods in the Mexican Highlands*, which uses official inquiries into idolatry cases to study the careers of four indigenous messianic figures spanning more than two centuries. Although Gruzinski himself might well characterize these episodes as instances of the assertion of Indian power based upon the recurrent theme of the Mesoamerican man-god, rather than as cases of resistance to the colonial order, it is undeniable that absent aggressive European evangelization and the subordination of indigenous people, these messianic figures could hardly have arisen. For the southeast we have two absorbing studies of indigenous resistance movements, Kevin Gosner’s on the Tzeltal Rebellion of the early eighteenth century (1992), and more recently Robert Patch’s on several eighteenth-century Maya revolts in Yucatán and Guatemala (2002). The Mexican southeast (with essays by Ronald Spores, Kevin Gosner, and Robert Patch) also dominates the five monographic essays (the remaining two are by Susan Deeds on Nueva Vizcaya and Christon Archer on Lake Mezcalá, in modern Jalisco) in the excellent anthology on native rebellion edited by Susan Schroeder (1998), which includes an acute short introduction by her and a thoughtful afterword by Murdo MacLeod. While there is a certain amount of hand-wringing by the authors of these fine studies (e.g., the essay by Gosner in Schroeder, 51–52) about the need to emphasize cultural aspects more strongly—in this case, for example, the actual theology and cosmology of native rebellious ideology—, most of them (with the exception of Gruzinski) work within a materialist framework, giving actual belief systems relatively short shrift. Aside from personal predilection and theoretical inclination on the part of the authors, the reasons for this are not far to seek, chief among them the problem of sources. By contrast, Paul Vanderwood’s recent study (1998) of the Tomochic episode in Chihuahua in the early 1890s (admittedly lying nearly a century beyond our period) offers a convincing reconstruction of the ideology of religiously inspired millenarian rebels while stinting neither the social, political, nor economic developments providing the immediate backdrop of the episode.61

Religion and Evangelization

The sharpening realization on the part of historians of native resistance movements that a broad range of collective behaviors were strongly religious in character, even where the actual ideation of such movements and their leaders may lie just beyond our reach, has been paralleled by a larger trend in the literature. Although Robert Ricard (1966) offered his account of the evangelization of Mexican native peoples in the 1930s, it has recently become clear that religion cannot be isolated in the colonial project, but informed most areas of life, particularly the relations between conquerors and conquered. Much of the middle ground (to use Richard White’s apt phrase) between native peoples and Europeans on which contact, mixing, adaptation, and the genesis of new cultural understandings and practices took place was delimited by the porous borders of religious sensibility, and the increasing focus on this theme has within the period since the early 1980s generated a great many studies on religious institutions, religious belief, and evangelization. Since most of the objects of the church’s evangelization effort were Indians, and since colonial native society became infused in virtually every corner with Catholic belief, practice, and idioms, it is extremely difficult to separate the strands of the history of colonial religious institutions, native conversion, changes in indigenous ways of thinking, and ethnohistory. The easiest works to categorize here would be those keeping most closely to what I have been calling institutional lines, in this case the formal structures of the Church, its normative practices, the career patterns of its agents, and so forth. The first decade of our review period, in fact, saw the publication of two fundamental works in this genre, although they became increasingly rare thereafter: John Schwaller’s 1985 study of Church finances in the three generations after the conquest, and his extremely valuable work on the ecclesiastical hierarchy and career patterns two years later. While individual biographies of famous churchmen were fairly common earlier on (e.g., Lillian E. Fisher’s 1955 work on Manuel Abad y Queipo, or Fintan B. Warren’s on Vasco de Quiroga of 1963), if not as common as those of high secular officials, they faded out of the picture after the early 1980s, with the possible exception of Stafford Poole’s book on the sixteenth-century archbishop Pedro Moya de Contreras (1987). Nearly a decade later, in close proximity to each other, were published works of a strongly institutional character by David Bradford: Stanford University Press, 1998). The books by Gosner and Patch can profitably be read along with those of Terry Rugeley on the Yucatec Maya, Yucatan’s Maya Peasantry and the Origins of the Caste War (Austin: University of Texas Press, 1996) and Of Wonders and Wise Men: Religion and Popular Cultures in Southeast Mexico, 1800–1876 (Austin: University of Texas Press, 2001).
ing (1994) on the diocese of Michoacán in the second half of the eighteenth century, and William Taylor’s magnificent work on secular parish priests in the eighteenth century (1996). Yet both books far transcended institutional studies, especially Taylor’s. Brading’s book not only built upon several decades of research on the Bajío region, forming what the author himself characterized as the third in a trilogy on the area (after Miners and Merchants and Haciendas and Ranchos), but also provided a full profile of the western Church, including in its treatment the effects of the Bourbon Reforms, the regular orders, and the episcopal hierarchy. In some ways echoing Schwaller’s earlier research on Church career patterns, Taylor’s study delved profoundly into issues of popular piety, the volatile priest-parishioner relations that formed the main point of contact in many areas between indigenous villagers and the structures of colonial society, issues of social control in the countryside, and the background of the clerical participation in the independence movement. Difficult to categorize readily, but partaking partly of the institutional history of the late colonial Church (policy and prescription, especially in the area of burial reform), partly of the history of ideas (the advent of Jansenist influences in Mexican Catholic theology and religious practice), and partly of the history of the late Bourbon modernization project (public hygiene and buen policía), is Pamela Voekel’s bold and suggestive book in the cultural vein, Alone Before God (2002). While some parts of this work hold up to scrutiny less well than others, it does suggest an imaginative and intriguing link between the advent of liberalism in the early republican decades, and what Voekel sees in the later eighteenth century as a turn away from Baroque piety, and towards the interiorization and individualization of religious belief among late colonial Mexican elite groups under the impact of Bourbon rationality and Jansenist discourse. A cousin of Viqueira Albán’s work on popular cultural forms during the same period, and related more closely to studies of the history of early modern European piety and modernization than to most of what has been done for colonial Spanish America, this book could not be more different from the work of Ricard and Taylor, although they inhabit the same world of religious discourse and practice. Insightful as the book is, it also exemplifies the ways in which good cultural history is sometimes less well realized as social history.62

My brief account of the outward and downward move of William Taylor's work from Church institutional structures to priest-parishioner relations brings us immediately into contact with the evangelization process and popular (some would prefer the term "local") religion, most of it among indigenous people. In one corner of this terrain we have the growth in the last decade of a small cottage industry on the cult of the Virgin of Guadalupe, given a special piquancy by controversies surrounding the canonization of Juan Diego by the Pope in Mexico City in 2002. First to appear (preceded by a decade by Jacques Lafaye's 1976 work on Guadalupe and Mexican national consciousness) was Stafford Poole's (1995) eloquent and authoritative account of the devotion up to nearly the end of the colonial era, which dated its initial propagation from the mid-seventeenth century and located it socially at first among Mexican creoles rather than Indians. This was followed some years later (1998) by a careful edition and translation of Luis Lasso de la Vega's 1649 Nahuatl account of the apparition, the Huey tlamahuiçoltica (also known by its first words, Nican mopuhua), authored by Lisa Sousa, Poole, and James Lockhart. David Brading's dense and fascinating history of the devotion appeared in 2001, a tour de force of intellectual genealogy, inter-textual detective work, and sensitive readings of Marian theology. Brading agreed on many points with Poole's treatment, but emphasized Nahuatl accounts less and creole accounts more, carrying the complex story of a burgeoning national devotion into the late twentieth century. As his book on the diocese of Michoacán forms the third work in a trilogy on the Bajío (although the three studies were executed from different points of view and each stands on its own vis a vis a dis-
crete sub-literature), so Brading’s *Mexican Phoenix* forms part of a trilogy on creole patriotism and Mexican national consciousness with his *Origins of Mexican Nationalism* (1985) and *The First America* (1991), on the whole perhaps the most influential body of work on colonial Mexico by any Anglophone scholar other than James Lockhart. It is worth noting, however, that concerned as they are with textual exegesis, questions of authenticity, and Marian theology, among other questions, neither Poole’s study nor Brading’s pays much attention to the popular reception of the Guadalupe cult, or the ideological uses to which Marian devotion was put by common people in an ethnically stratified society. With the exception of a few works discussed below, in fact, this descent into the affective and ideational world of colonial popular religion has not been successfully achieved, primarily due to the same problem we have with resistance movements and ideologies—the lack of sufficient sources to get us there.63

So closely imbricated were the processes of conquest, evangelization, and the contacts between native people and Europeans, that a large, innovative, and often speculative historical literature, lying at the intersection of studies of religiosity and ethnohistory, has developed within little more than a decade. The relative recency of these works, together with the inherently elusive nature of their subject matter, provides a strong hint that they fall almost exclusively into the cultural history reg-

ister. Among the earliest of them was Louise Burkhart’s important study (1989) of the categorical slippages between Christian message and Nahua understanding in the early decades following the conquest, followed by the same author’s works on Nahua religious drama (1996) and her contribution to the pre-history of the Guadalupe devotion (2001). Another avatar of this wave of studies was Inga Clendinnen’s captivating 1987 book on the conquest of the Yucatecan Maya, a prolonged episode during which the sheer nervousness and violence of the Spanish invasion itself provide the backdrop for a brilliant treatment by Clendinnen of the projection by Europeans of their fears onto the natives, the latter’s evasion and resilience, and the invaders’ savage reaction to what they regarded as a betrayal when native converts reverted to their old ways. For all its virtues, based as it is substantially on published European sources or on native accounts filtered through the Europeans or influenced by them, Clendinnen’s book exemplifies some of the problems with this sort of historical undertaking in the absence of a relatively large and substantially unmediated corpus of indigenous testimonies. Nearby Chiapas provides the scene for Amos Megged’s study (1996) of Franciscan and Dominican efforts to evangelize the Tzeltal Indians under the sign of the Counter-Reformation and Tridentine reforms between about 1550 and 1680, touching upon the now familiar but almost infinitely varied themes of the translatability of Christian doctrine, native recalcitrance and backsliding, and the adaptation of colonialist institutions (e.g., the religious confraternity) to native purposes. These slippages along cultural faultlines and the unanticipated consequences of introducing Christian belief during evangelization furnish the raw material for two fascinating studies, the first by Fernando Cervantes (1994) on diabolism among the natives of New Spain, and the second by Pete Sigal (2000) on changes in Yucatecan Maya thinking about sexuality. Both bear the hallmarks of the most intense sort of cultural history that we have yet to see: density of argument, allusion, and theory; penetration into realms of native consciousness and collective psychology in a somewhat speculative vein; close attention to high theology, inter-textuality, and forms of representation; and a notable tendency, because of the nature of the documentation, to emphasize the thinking of elite groups. Cervantes’s study, in particular, weaves together elements of European theological disputation (e.g., the Franciscan critique of Thomism), native belief systems, European representations of native culture, and the practices of early colonial evangelization. He finds that the central Mexican natives adopted a belief in the Devil, which the friars had introduced in order to claim Indians for the Christian universe, to preserve monistic notions of the creator-destroyer divinities in their own traditional belief systems. Later in the colonial period the Devil was reduced in European theology to a
sort of trickster figure, but the natives were stuck with him well beyond the end of colonialism. Finally, two works on evangelization in early colonial Michoacán published in 2000 demonstrate the variety of approaches in the field even at this late date. One of them, Bernardino Vérastique's *Michoacán and Eden*, is a sort of earnest, conventional throwback centering on the life of Bishop Vasco de Quiroga. The second, James Krippner-Martínez's *Rereading the Conquest*, is a much bolder and more ambitious work. Based almost exclusively on the critical exegesis of four major early colonial texts, the book is devoted to deconstructing the way in which the history of early colonial Michoacán has been remembered in history writing, from the death of the Tarascan cazonci at the hands of Nuño de Guzmán to the virtual canonization of Vasco de Quiroga by writers over the last two centuries or so. Interesting as it is, it is a form of cultural history that indicates how very different the genre can be from subaltern history, and that more closely resembles intellectual history with an attitude.64

The text-centered nature of Krippner-Martínez' work leads our attention quite naturally to a special little cache of studies that are almost entirely text-centered but broader in approach, and which all deal with what we may call, following Serge Gruzinski, the "colonization of the native imaginary" (my own preferred translation of the original French term imaginaire would be something like "cultural template"). All these works share an interest in the way Christianity was received and adapted by Mesoamerican native peoples, and thus devote a good deal of energy to the evangelization process, but they also embrace the reconfiguration of indigenous mythologies, forms of non-religious representation, the encroachment of European practices of reading and writing, native historical memory, and so forth. And although they typically deploy very eclectic sorts of evidence, from European religious treatises and histor-

ical accounts, historical chronicles in alphabetic renderings by native authors in either their native tongues or Spanish, and native codices, to Inquisition testimony, maps, and (some) other types of archival evidence, they are very abstracted from mundane social practices and realities, and are meant to be so, since they work on the assumption that the mental changes are antecedent to the social instantiation of them. These authors make a convincing case that native ways of remembering, thinking, and even perceiving were colonized just as profoundly as native lands, labor relations, political structures, ritual behaviors, and social arrangements.

The earliest of these works, and the least affected by the theoretical considerations of the linguistic turn, was Enrique Florescano’s very suggestive *Memory, Myth, and Time in Mexico* (1994, originally published in Spanish in 1987), which the author has followed with a number of other book-length studies concerning the symbology of Mexican nationalism, native mythological figures, and the system of Nahua pre-Columbian religious belief. Beginning with Nahua concepts of time and space, and always keeping at the center of his vision the practices of native historical remembering as shaped by the Aztec, colonial, and national states, Florescano takes the story of native concepts of the past through the colonial period to the independence era and the emergence of a Mexican patriotism cobbled together from elements of indigenous mythology, European linear histories, and burgeoning nationalist imagining. There followed in 1993 (originally published in French in 1988) Serge Gruzinski’s *The Conquest of Mexico*, a quite intellectually stunning work which in emphasizing discontinuity in native forms of collective memory, the devaluation of native oral and pictorial modes of representation, and the aggressive propagation of European Christianity to supplant indigenous belief systems, presented a much less sanguine picture of native cultural forms under colonial rule. Published in France just two years after the original publication of *The Conquest of Mexico*, Gruzinski’s *Images at War* (2001, published in French in 1990) is a more modest, less substantiated, yet more speculative work that nonetheless packs a powerful interpretive punch and continues in many ways where the previous book left off. Here Gruzinski traces the history of the implantation of images—primarily religious ones, including church frescoes, pictures of La Guadalupana, illustrations in the famous títulos primordiales, religious sculptures, etc.—by the Europeans into indigenous culture, arriving along the way at the cult(ure) of the Baroque image where his discussion converges interestingly with that of David Brading in his book on the Virgin of Guadalupe. A playful if not entirely convincing coda takes the story through the twentieth century, through the flickering images of Televisa and Ridley Scott’s 1982 film *Blade Runner*, where the apocalyptic icons of an ever more dystopic Los Angeles dom-
inate a landscape of post-modern cultural hybridity. And just a few years beyond the 1990 book by Gruzinski we saw the publication of Walter Mignolo’s dense and very impressive *Darker Side of the Renaissance* (1995), in which the importation and imposition of European writing systems on native cultures in Spanish America (Mexico is the central case examined), and the control they came to exert over forms of representation—religious, historical, even cartographic—take center stage still more explicitly than in Florescano’s or Gruzinski’s books. It is not clear if we may expect to see more such large-scale interpretive works from other authors in the near future (although Jorge Cañizares-Esguerra has recently given us one). In some ways recent books like those of Pamela Voekel and James Krippner-Martínez, although each very original in its own right, are probative explorations of the lines of inquiry in cultural and intellectual history laid out by the works of Brading (in *The First America*), Florescano, Gruzinski, and Mignolo in the early 1990s.65

### Ethnohistory

Finally, there is little question that the works in ethnohistory produced at a quite steady pace over the last two decades comprise the single largest and most coherent sub-genre in the Anglophone historiography of colonial Mexico. Since the Nahuatl-, Maya-, and Mixtec-based works, in particular, share such a distinct identity, they have been glossed a good deal already in methodological and review articles (most recently by Matthew Restall), and I will not linger over them here as much as their importance warrants.66 Although much recent work in ethnohistory re-


lies on native-language sources, not all of it does for the simple reason that native-language sources may be missing, as with the northern indigenous peoples who never produced alphabetic writing in their own tongues. Certainly a kinship exists between ethnohistory in a general sense and the subaltern history to which it may be assimilated by its focus on colonial indigenous people (subalterns ipso facto), even if their methods are distinct. Its relationship to works at the further reaches of cultural history is a bit more tenuous, again on the grounds of method, since in purely substantive terms its findings about native peoples after the European conquest lend themselves readily to appropriation by scholars studying the slippages of meaning and understanding within colonial society. Some of ethnohistory’s best recent works and most adept practitioners, on the one hand, are quite resolutely structuralist in their approach, while on the other hand, those ethnohistorians who work in the well established native-language-based tradition keep within remarkably conservative boundaries. Impressive as it is, much of the native-language-based investigation tends to be linguistically self-referential and therefore ultimately descriptive, relying upon the accumulation of details about linguistic analysis and change to provide some insight into native society and thinking. One limiting condition here is the sources, a problem with social and cultural history in general noted here throughout, and which in the case of native-language-based ethnohistory has now been pushed down a level into native society. Here the documentary sources are still largely (although by no means exclusively) the written records of native elites, as they have tended to be in the social and cultural history of colonial society more widely, which depends upon documents for the most part generated by the colonial state, by the literate, and by the superordinate social sphere. Nonetheless, ethnohistory as a whole has magnificently redressed the imbalance in our reconstruction of colonial Mexican society, a foreshortened vision conditioned by our having seen indigenous people as the objects rather than the subjects of their own history.

Two extremely significant works, published within a year of each other, can stand as the modern progenitors of colonial Mexican ethnohistory. These are Edward H. Spicer’s Cycles of Conquest (1963) and Charles Gibson’s The Aztecs Under Spanish Rule (1964), focusing respectively on what we might call the Greater Mexican North, the Amer-
ican Southwest, or the Borderlands (depending upon one's point of view), and the Valley of Mexico, or Nahu-speaking lands. As with studies of native resistance movements, then, ethnohistory has tended broadly to bifurcate into two branches: a northern branch, much less developed in quantitative terms but with distinguished exemplars of the genre, and a southern branch (well, really central, with southeastern spurs) with a great many more works, dominated by the native-language-based studies (in Nahuatl, Zapotec/Mixtec, and the Maya group) strongly associated with James Lockhart, his collaborators, and his students. The northern ethnohistories tend for obvious reasons to blur into studies of the colonial mission regime, the social conditions peculiar to the long history of the Spanish frontier (about which David Weber has written so compellingly), the complex history of Mexican/United States interaction, and the harsh ecology of the northern Mexican steppe-lands, mountain ranges, and river valleys. An environmental history or ecological approach makes a good deal of sense in this area of the world, since the environment obtrudes itself so obviously into cultural formations and patterns of historical change, while ethnohistorians of the more congenial central and southeastern Mexican lands rarely mention it at all. Two recent distinguished books on this northern macro-region are Cynthia Radding's *Wandering Peoples* (1997) and Susan Deeds's *Defiance and Deference* (2003), preceded and accompanied by a host of interesting works by Mexican scholars and by other Anglophone investigators (e.g., Thomas Sheridan’s *Empire of Sand* [1999]). Radding’s work on the Sonoran highland indigenous groups in the late colonial and early national periods—their interactions with missionaries, the changes in their economic life, their coping strategies and ethnic identities in the face of relentless pressure from ranchers, miners, settlers, and military intrusions—takes a strongly ecological perspective but is too sophisticated to be reductive. Focusing on several indigenous groups further south and southeast, and on the comparative history of their survival through the colonial era, Deeds’ book is cognizant of the environmental constraints within which Indian cultures and economies developed and interacted with the encroaching colonists, but if skeptical of some of the claims of cultural history, is nonetheless more tilted toward the cultural history of the native peoples she studies.67

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At least in terms of the number of monographs produced by Anglophone scholars, the central/southern branch in colonial ethnohistory has been the most impressively productive, and arguably the most methodologically innovative, because of its heavy reliance on native-language documents and the linguistic skills required to use them. Many of the authors of these works have found themselves in implicit or explicit dialog with Charles Gibson’s foundational work on central Mexican Nahua speakers, and even their titles reflect this (e.g., Farriss [1984] and Lockhart [1992]). The first major monographic study in this genre took some time to materialize after the publication of Gibson’s Aztecs, and then dealt with Yucatán rather than the Nahua lands. Following upon the heels of a host of anthropologists, archaeologists, and other sorts of scholars who had already studied the Maya (e.g., Victoria Bricker’s 1981 The Indian Christ, the Indian King), Nancy Farriss’s Maya Society Under Colonial Rule (1984) realized a Gibsonian study for the Yucatec Maya some two decades after the publication of Gibson’s monumental work. Structuralist as most of the work is (Farriss acknowledges [x] as her inspirations Gibson, of course, but also Eric Wolf, little cited now by ethnohistorians of cultural inclination, an indication that our understanding of what “culture” means has swung in a different direction), however, Farriss devoted several chapters to native religious belief and cosmology under the impact of the Spanish presence, something one sees little of in Gibson’s work. A dozen years further on, and we have Matthew Restall’s (1997) impressive native-language-based study of the Yucatec Maya with “culture” in the title and a good deal of the text devoted not only to gender, sexuality, and religious thinking, but also nearly a hundred pages to language phenomena. Even more insistent on the primacy of language is Kevin Terraciano’s recent prize-winning volume on the colonial Mixtecs of Oaxaca. After devoting nearly a third of its ample length explicitly to Mixtec writing and language, Terraciano takes the reader through a series of chapters encompassing community structure, social relations, forms of native rulership, patterns of land use, ownership, and conflict, religious thinking, and ideas about ethnicity. Nearly all of this is viewed through the lens of native terminologies and concepts as they survived into the colonial period and were progressively modified by the introduction of Spanish language and lifeways. With meticulous scholarship, unimpeachable expertise in the native-language materials, and an uninflected Gibsonian voice that itself helps to establish his authority, Terraciano’s work is about as far from the indeterminacies and sometimes
over-heated readings of the linguistic turn as any materialist-structuralist history.68

The most obvious flowering of this sub-genre of ethnohistory is represented by a half-dozen or so monographs, published for the most part during the last fifteen years, dealing with the Nahua lands of central Mexico. Although some of these works nominally extend past the middle or so of the seventeenth century to embrace the late colonial period, they tend to concentrate on the first 150 years of colonial life, corresponding to the first two stages of native language transformation (1519–ca. 1550, 1550–1650) famously posited for central Mexico by James Lockhart, which the authors seem to find richest in linguistic transformation and most important for acculturation processes. Within a few years of Farriss’s book on the Maya was published the first of these works, S. L. Cline’s 1986 study on Culhuacán in the last two decades of the sixteenth century, based substantially on native testaments. This was preceded two years earlier by the documentary collection edited by Cline and Miguel León-Portilla, *The Testaments of Culhuacán,* and followed after some years by her transcription, translation, and analysis of an early post-conquest Nahuatl-language census, *The Book of Tributes.* There followed in rapid succession Susan Schroeder’s deep analysis of the Chalco and Amecameca chronicler Chimalpahin (1991) and Robert Haskett’s (1991) study of surviving and adapted native forms of governance in the Cuer-
navaca area, especially for the later colonial period, and a few years later Rebecca Horn’s eminently Gibsonian study of early colonial political and economic structures in Coyoacán (1997). It is a tribute to these and other works by Nahuatlato historians (and to their maitre James Lockhart) that they are not cookie-cutter copies of each other, but rather move toward different goals through similar approaches. Cline’s book focuses on matters of native piety, family, and gender, for example, and Schroeder’s on a detailed reconstruction of the political structure of Chalco through a fascinating exegesis of the thinking of a sophisticated native chronicler and micro-patriot. Robert Haskett’s study concentrates almost entirely on indigenous political structures within what survived of an important pre-conquest city-state to the south of what became Mexico City, and Rebecca Horn’s book looks at the traditional themes of taxation, labor systems, land-holding, Spanish-owned haciendas, and market relationships—traditional in the sense that she seems more concerned to “establish the objective state of things” than native mind-sets as revealed in political thinking, piety, or ideas about gender.69 These fine studies all share a documentary base in native-language sources (supplemented by the same sorts of Spanish-language official documents that non-ethnohistorians rely upon), an intense interest in the native political form of the altepetl, and a fundamental belief that native agency within the colonial regime can be demonstrated through their common approach.70

In the meantime, during the 1980s James Lockhart had been producing with collaborators a series of volumes of transcribed Nahuatl documents with commentaries, and also essays of his own, as he would continue to do during the rest of the 1990s and beyond, after the publication in 1992 of his major work to date, the magnificent Nahuas After the

69. The quoted phrase is drawn from Susan Schroeder, Chimalpahin and the Kingdoms of Chalco (Tucson: University of Arizona Press, 1991), 112, in which she contrasts her own approach to the Chalco chronicler’s work with the more materialist frameworks of other scholars.

Conquest. The structure of this book seems almost to provide a pretext for a long, technically adept treatment of culture change as manifest through the developmental stages of colonial Nahuatl language. Beginning with a dense chapter on Nahua political organization and terminology centering on the *altepetl* (roughly, a native ethnic city-state) and its fate through the mid-colonial period, Lockhart moves downward to household organization, then outward to social differentiation, economic life, and religious practice, and finally turns to language phenomena, which occupy nearly half the text. Lockhart has gone to some pains to indicate that his own work and that of many of his students, aside from language change, focuses centrally on the altepetl and the modular-rotational forms of native political organization following from it, as well as upon the interplay of cultural change and considerable continuity in central Mexican native society under Spanish domination, and also that these themes were at the core of Gibson’s *Aztecs Under Spanish Rule*, absent only the term altepetl itself. While there are undeniable affinities between the work of Lockhart and Gibson on these points, the language-based approach of the former and the heavily materialist approach of the latter—on labor systems, land-holding, farming, and so forth—could hardly be more different. Gibson did set much of the agenda for colonial Mexican ethnohistorians and other scholars, it is true, directing their attention toward the lives of indigenous people, but it is hardly surprising that these chapters on Valley of Mexico economic (and to some extent political) life, rather than references to language or his work on Aztec political entities, seem to have been the most influential in the broader field. And there is also something of irony in the fact that the most intensely language-centered project to have arisen in the Anglophone historiography on colonial Mexico during the last quarter-century seems hardly to have taken the linguistic turn at all in the sense in which cultural history is ordinarily conceived today (and sometimes shrilly criticized), but instead hangs in an empiricist space somewhere between economic determinism and the speculative imputation of changes in native mental templates from religious or political practices imposed by the conquerors.


Conclusion

The way Anglophone historians approach the history of colonial Mexico since 1980 or so has changed, but the changes have been incremental, not sudden; this is a social science and humanistic endeavor, after all, and not quantum mechanics. Some formerly strong currents have dried up or gone into a historiographical hiatus (hacienda studies, biography); others, even though they were already in evidence by the early 1980s, have come on very strong (ethnohistory, resistance studies); and the field as a whole has perceptibly tipped away from structuralist-materialist approaches, and toward cultural ones. The turn in the direction of cultural history cannot be explained exclusively by reference to internal developments in the area of Mexican studies or history; that is, to discoveries of new sorts of documentation, the arrival of new methods on the scene, the internal logic of the work being done, the influence of a single towering scholarly figure or school. If it could be so explained, Mexican historians of colonial Mexico would be following the same trend, and with a few exceptions they are not. In fact, one worrisome trend lies in the increasing divergence of the two national historiographies—the Mexican still committed to fairly traditional (although still compelling) questions, methods, and materialist paradigms, while at least a part of the American scholarship is apparently flying off into the empyrean. It is in the United States community of Mexicanist scholars where the great bulk of this Anglophone work has been and will continue to be done, and it is the intellectual environment of North American scholarship—the impact of cultural anthropology upon history, the emergence of post-colonial studies, the influence of European historiographical models, the linguistic turn in general—that has shaped the product of our labors. A judgment as to whether these are salutary or noxious influences is partly a matter of taste, and partly a matter of the explanatory power that the new styles bring to the questions they are seeking to answer, and the very nature of those questions.