

RECENT WORKS ON COLONIAL MEXICO

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- THE ORIGINS OF MEXICAN NATIONALISM.* By D. A. BRADING. (Cambridge: Center of Latin American Studies, 1985. Pp. 119.)
- PROPHECY AND MYTH IN MEXICAN HISTORY.* By D. A. BRADING. (Cambridge: Center of Latin American Studies, 1984. Pp. 96.)
- MEDIEVAL IBERIAN TRADITION AND THE DEVELOPMENT OF THE MEXICAN HACIENDA.* By WILLIAM SCHELL, JR. Foreign and Comparative Studies, Latin American Series, no. 8. (Syracuse, N.Y.: Maxwell School of Citizenship and Public Affairs, Syracuse University, 1986. Pp. 117. \$11.50.)
- MACHISMO AND CONQUEST: THE CASE OF MEXICO.* By MARVIN GOLDWERT. (Lanham, Md.: University Press of America, 1983. Pp. 96. \$18.25 cloth, \$7.75 paper.)
- COLONIAL BUREAUCRATS AND THE MEXICAN ECONOMY.* By JOHN S. LEIBY. (New York: Peter Lang, 1986. Pp. 252. \$32.00.)
- THE MEXICAN COLONIAL COPPER INDUSTRY.* By ELINORE M. BARRETT. (Albuquerque: University of New Mexico Press, 1987. Pp. 143. \$22.50.)
- IRRIGATION IN THE BAJIO REGION OF COLONIAL MEXICO.* By MICHAEL E. MURPHY. (Boulder, Colo.: Westview Press, 1986. Pp. 226. \$19.95.)
- GUADALAJARA A LA CONSUMACION DE LA INDEPENDENCIA: ESTUDIO DE SU POBLACION SEGUN LOS PADRONES DE 1821-1822.* By RODNEY D. ANDERSON. (Guadalajara: Temática Jalisciense, 1983. Pp. 176.)
- THE WOMEN OF MEXICO CITY, 1790-1857.* By SILVIA MARINA ARROM. (Stanford: Stanford University Press, 1985. Pp. 384. \$42.50.)
- MICHOACAN EN LA NUEVA ESPAÑA DEL SIGLO XVIII: CRECIMIENTO Y DESIGUALDAD EN UNA ECONOMIA COLONIAL.* By CLAUDE MORIN. (Mexico City: Fondo de Cultura Económica, 1979. Pp. 328.)
- RURAL SOCIETY IN COLONIAL MORELOS.* By CHERYL ENGLISH MARTIN. (Albuquerque: University of New Mexico Press, 1985. Pp. 255. \$27.50.)
- ROOTS OF INSURGENCY: MEXICAN REGIONS, 1750-1824.* By BRIAN R. HAMNETT. (Cambridge: Cambridge University Press, 1986. Pp. 276. \$44.50.)

One of the most pronounced trends in colonial Mexican studies in recent years has been the shift away from broadly based research on large political units toward intensive work on specific cities and regions. Judging from the titles under review, this predilection remains alive and well: of the eight books based on original research, seven deal with particular localities or regions. Accompanying this research methodology is a renewed emphasis on social and economic themes. Political concerns are not absent, but only two authors—John Leiby and Brian Hamnett—dwell explicitly on this domain, and even here politics are viewed as embedded in larger economic and social contexts. Many of the authors also share a common focus on the late eighteenth and early nineteenth centuries, perhaps because the richer source materials of the later years are more amenable to social and economic analysis than the more sparse—and less quantitative—documentation for the early colonial period.

Standing apart from the crowd are the two little works by D. A. Brading that deal with the history of ideas. Both consist of short, interpretive essays on well-known writers and ideologues in Mexican history, from Bartolomé de las Casas to José Vasconcelos. Part of a new series of “Latin American Miniatures” published by the Center of Latin American Studies at the University of Cambridge, they are well suited to use in the classroom. *The Origins of Mexican Nationalism*, first published in Spanish in 1973, deals not with twentieth-century nationalism but with its precursor, “creole patriotism,” a kind of proto-nationalism during the era before Independence. Brading traces the emergence of a creole political consciousness in the thought of Juan de Torquemada and Francisco Javier Clavijero and the strengthening of the cult of the Virgin of Guadalupe. The creoles’ search for autonomous origins and their view of themselves as dispossessed heirs fostered the main themes of “Neo-Aztecism, Guadalupanismo, and the repudiation of the conquest” (p. 23). The two central figures in the book are Fray Servando Teresa de Mier and Carlos María de Bustamante, who together devised a nationalist ideology linking the cause of independence (creole rights) to events of the sixteenth century (historical *indigenismo*).

In 1794 Fray Servando linked the Quetzalcóatl and Guadalupe myths by claiming that Quetzalcóatl was the Apostle St. Thomas and that the Indians had worshiped Mary at Tepeyac before the conquest (Servando was not the first to make this claim). The friar was sentenced to European exile for his views, but Brading shows that he nevertheless gave Mexico a Christian foundation and history while denying the justness of the conquest and the monarchy’s right to govern. Bustamante, the chief historian of the insurgency, is presented as a promoter of historical *indigenismo* and the last of the creole patriots.

Brading argues that nationalist thinking fell into abeyance during the age of liberalism because it remained more creole than Mexican and was thus tied to a colonial and indigenous past that the liberals rejected. But how is the popularity of nineteenth-century liberalism to be explained? Brading regards it as an ideology that was inappropriate for the problems of postcolonial Mexico, but he ascribes its success as a political doctrine to the fact that it had little competition. No form of agrarian socialism existed to challenge it, and the Bustamante-Mier version of nationalism lacked a theory of society and failed to offer any practical remedies. Brading believes that radical indigenismo never developed because of elite fears aroused by the caste war in Yucatán. *The Origins of Mexican Nationalism* concludes with an examination of the concept of *patria* put forth by Ignacio Ramírez and Ignacio Manuel Altamirano during the Reforma. The reader interested in the revolutionary period of the twentieth century must turn to Brading's other volume.

Prophecy and Myth in Mexican History starts earlier and ends later than the first book, concentrating on Bartolomé de las Casas, Simón Bolívar, Andrés Molina Enríquez, and José Vasconcelos. It is also more explicitly comparative, invoking Europe and other parts of Spanish America in order to elucidate Mexican concerns. Las Casas is portrayed as "profoundly Augustinian" (p. 27), and when viewed from the standpoint of the *conquistadores*, as an architect of royal absolutism who disdained the conquerors as the equivalents of a gang of criminals. Reviewing Jacques Lafaye's *Quetzalcóatl y Guadalupe*, Brading disputes Lafaye's claim that creole patriotism embodied in Guadalupanismo was the progenitor of Mexican nationalism. What unites the two phases of national consciousness in New Spain and Mexico—before and after the liberal interregnum—was not the cult of the Virgin of Guadalupe but a continuous preoccupation with the Mexican past.

Simón Bolívar's doctrine of "classical republicanism" is compared with the Mexican version of creole patriotism to demonstrate that in both Venezuela and Mexico, ideology determined the context in which independence was attained. The indigenous and religious character of the Mexican insurgency led by clerics is placed in sharp relief against Bolívar's secular philosophy, which was unconcerned with the Indian past. Finally, Brading discusses the ideology of nationalism at the time of the Revolution through the eyes of Molina Enríquez and Vasconcelos. Here the ideas develop dialectically, simultaneously reaffirming and repudiating the liberalism of the Reforma. Molina Enríquez, a social Darwinist, was both a liberal and a radical revisionist who praised Benito Juárez and Porfirio Díaz yet attacked their agrarian and economic policies. He was the prophet of the Revolution and the Partido Revolucionario Institucional (PRI), convinced as he was of the necessity of an

interventionist state. Vasconcelos, in contrast, was the first Mexican Romantic. He shared Molina's faith in the Mestizo as the embodiment of Mexico's future but carried it to a nearly messianic extreme in his vision of *La raza cósmica*.

Both Brading's books contain penetrating observations on important thinkers, but *The Origins of Mexican Nationalism* is superior in its greater coherence. *Prophecy and Myth* is actually a mélange of three lectures, two book reviews (the other review is of Jean Meyer's *La cristiada*), and brief interludes on Churrigueresque architecture and nineteenth-century banditry. Those interested in the entire sweep of Mexican intellectual history will want to look at both books. It is a pity that they were not issued as a single volume, minus the interludes and reviews.

Also interpretive in nature is the essay by William Schell, Jr., *Medieval Iberian Tradition and the Development of the Mexican Hacienda*. This iconoclastic portrait of the hacienda rejects the feudalism-capitalism dichotomy that underpins much of the current literature in favor of a sociocultural interpretation stressing the diffusion and persistence of a medieval Iberian trait complex in the New World. The key cultural elements are those surrounding the highland Iberian *domus*, or house and family, with its premise that land must be passed on intact to a single heir, whose siblings are consequently encouraged to be mobile and seek their livelihood in other pursuits. In Schell's view, the Mexican hacienda is a New World *domus*. Because investment in land during the formative colonial years was socially motivated (although the profit motive was not totally absent), the medieval Iberian system that emphasized maintaining a landed core of wealth could endure. In both Spain and colonial Mexico, "wealth did not originate in land, but was absorbed and purified by it" (p. 19). Haciendas were not so much landed estates as family fortunes with origins in commerce and mining that needed to be "legitimized." In spite of late colonial efforts at "rationalization," real change did not occur until the 1870s, when railroad transportation destroyed the regional context in which haciendas had existed, thus opening them up to national and world market forces.

Although Schell's argument has its clever aspects, I find it on the whole unconvincing. The weakest point is its extreme diffusionism, particularly the overdrawn comparison of Aztec and Spanish patterns of land tenure and the suggestion that they had common roots stemming from hypothetical pre-Columbian transoceanic contacts. Another problem is that in replacing the feudal-capitalist dichotomy with a "medieval-rationalist" one, Schell comes close to reinventing the traditional-modern continuum with all its problems. Ultimately, his analysis seems too culture-bound and provides few diagnostic criteria for comparing

the hacienda with landed estates in other agrarian societies. The “medieval Iberian tradition” is certainly not irrelevant to understanding the Mexican hacienda, but I am not convinced that it tells the whole story.

Another new look at an old problem is Marvin Goldwert’s slim volume *Machismo and Conquest: The Case of Mexico*. An explicit attempt to blend “Freudian insights with historical synthesis,” the essay begins with the oft-repeated observation that Mexican machismo is rooted in the conquest experience and the underlying male insecurities that derive from it. According to this view, the modern Mexican mestizo, “haunted” by the passive, feminine Indian role in the conquest (as opposed to the active, masculine Spanish role), attempts to assert his masculinity through the conquest of women and power. This much is familiar, but Goldwert goes far beyond the usual scheme in applying Freud’s concept of *kairos*, “that crucial occurrence early in a civilization’s history which shapes all ensuing developments” (p. vii). Just as early traumas shape the life histories of individuals, the traumatic event of the Spanish conquest is presented as determining aspects of Mexican history as it lives on in the “mass unconscious” (p. viii).

Freud’s historical formula of history-as-neurosis has four stages: early trauma, defense (childhood neurosis), latency (a period of undisturbed development), and full-blown neurosis (a partial return of the repressed material at puberty or later). In introducing this scheme, Goldwert implies that he is concerned with diagnosing “the aetiology of social illness” brought on by the “rape” of the Spanish conquest (p. 8). The four-stage scheme is applied to Mexican history from the conquest to the present in terms of four macho behavior complexes: religion versus power (or son versus father), male versus female, dictatorship versus separatism, and individual charisma (with Cortés as the prototype). The historical stages differ slightly, but the early traumatic stage is equated uniformly with the colonial period, the second stage of childhood neurosis corresponds to the tumultuous years of the nineteenth century, the third stage of latency generally runs from the 1870s up to or through the Revolution, and the fourth stage brings us up to the present.

Overall, the neurotic cycle forms a closed order. The only way out of it, according to Goldwert, would be for a “liberationist” clergy to join forces with the women’s movement. Such a unified reformist thrust “could modify machismo and reverse the tides of kairoitic time. Out of Freud’s closed order . . . Mexicans and Spanish Americans could move towards a brighter and more open-ended future” (p. 61). Those who subscribe to Freud’s belief that the stages of neurosis in individuals can be applied to the study of whole societies may find this essay of interest. Others (like me) who find the analogy of dubious value for social

analysis are not likely to be convinced otherwise by *Machismo and Conquest*.

Colonial Bureaucrats and the Mexican Economy by John Leiby is an administrative study of the Bourbon bureaucracy in New Spain between 1763 and 1821. The first part of the book discusses rather enthusiastically the Bourbon economic reforms, the role of the bureaucrats in their implementation, and the economic expansion that followed. The second part analyzes the growth of the patrimonial state, giving special attention to the regime of the second Conde de Revillagigedo (1789–1794). Separate chapters are devoted to influential Spanish economists, transportation networks and public works, agricultural production, industrial production, commerce, governmental reforms, financial management, the Revillagigedo regime, insurgency and independence, and modern patrimonialism. These topics set an enormous agenda for such a short book, and while the author has done some archival research of his own, I found little in *Colonial Bureaucrats* that is new. The scope of the inquiry strikes me as unwieldy; indeed, a book or more could be written on each of these chapter topics. Inevitably, some chapters are superficial and others simply summarize current knowledge, but the book is nevertheless useful as a basic text or reference work.

Leiby's main point is that the Bourbon reforms, especially the economic reforms, led to the growth of a patrimonial state. One wishes that he had elaborated more on this theme. Although Leiby states in the preface that "Late Bourbon Mexico was patrimonial in the sense that the Bourbon monarchs considered New Spain as an inherited patrimony and its subjects children of the Spanish Empire" (p. x), he says little else about the concept of patrimonialism and the considerable literature on the subject. It is unclear whether Leiby conceives of the differences between the Hapsburg and Bourbon regimes as being of degree or kind. At the beginning, he asserts that the Bourbon reforms constituted "a decided advance—an advance toward patrimonialism" (p. x). At the end, however, he notes that a patrimonial state had been present from the outset of Spanish rule and that the Bourbons were elaborating on structures already in place (p. 204). Leiby also conveys a troubling sense of the inevitability of patrimonialism, which tends to weaken the concept's explanatory powers. After the chaotic years of the mid-nineteenth century, patrimonialism reappears with the Porfiriato, and it is "only natural" that today's postrevolutionary state is patrimonial as well (p. 196). Significant continuities exist, to be sure, but a satisfactory account of them will require a more sophisticated kind of analysis than is offered in *Colonial Bureaucrats and the Mexican Economy*.

Turning now to the local and regional studies, Elinore Barrett's *The Mexican Colonial Copper Industry* provides new data on this little-

known industry. Her study focuses on mining in south-central Michoacán, the principal source of copper before and after the Spanish conquest. Although mines were opened elsewhere in the late eighteenth century, Michoacán never lost its preeminence in copper production. While Barrett makes clear that colonial copper miners were not an especially wealthy group, copper was the most widely employed utilitarian metal in Mexico (and Spanish America generally), being used in armaments, coins, sugar and brandy making, silver mining, and in the manufacture of a variety of household articles. Guilds of coppersmiths operated in Mexico City and Puebla; Santa Clara, Michoacán, was the principal smelting center and Pátzcuaro the main locus of copper entrepreneurship.

Barrett's book is in many respects a study in administration because the colonial copper industry was regulated by the Crown. Production at the principal mine at the Real de Inguarán in Michoacán was monopolized by a royal concessionaire between 1613 and 1787, and distribution was controlled by the treasury from 1780 to 1809. The industry was plagued by problems throughout most of the seventeenth and eighteenth centuries, and not until the very end of the colonial period did the supply begin to meet the demand. Copper brought perennially low returns, miners had little capital to invest, and credit from suppliers was scarce. Because deposits were worked in a piecemeal, destructive manner, the frequent cutoffs, cave-ins, and flooding made much ore inaccessible. In Michoacán, mining and smelting were carried out ninety kilometers apart, creating high transportation costs. Labor, supplied by *repartimiento* drafts through the seventeenth century and wage earners in the eighteenth, was always in short supply. The low price of copper kept wage levels down (at the same level as agriculture), and the location of the mines in the hot lowlands of Michoacán simply exacerbated the problem.

The Mexican copper industry is a model case of how regulation in the form of a Crown monopoly could be self-defeating. When the government's need for copper for its armaments factories in Spain rose precipitously in the 1780s, the official price of copper was raised and a monopoly on its distribution instituted. But the supply did not measurably increase until a second price hike in 1799, and in 1809, the government abandoned its regulation altogether and free trade was initiated. Those interested in copper production, distribution, and regulation will find Barrett's book indispensable, but it rarely strays from this narrow focus (although it includes an added chapter on tin). The study contains little information on the copper manufacturing process and its technology, guild organization, careers of miners, the social relations of production, the social and economic impact of the industry on towns

like Santa Clara and Pátzcuaro, and similar topics. *The Mexican Colonial Copper Industry* is an important first step, but many facets of the industry still await investigation.

Irrigation in the Bajío Region of Colonial Mexico by Michael Murphy is another contribution to the history of technology and use of natural resources. Murphy gives detailed histories of colonial water systems in Celaya, Salvatierra, Valle de Santiago, and Querétaro, and he also analyzes irrigated agriculture, hydraulic technology, and water law. Irrigation in the Bajío turns out to have been limited almost entirely to wheat-producing estates and was “employed to avoid fungal infestation and other climatic hazards of summer months rather than to raise the productivity of agricultural haciendas” (p. 137). The major irrigation systems were established in the sixteenth and early seventeenth centuries, with more deterioration than improvement occurring in subsequent years.

In light of all the theorizing about the relation of irrigation to sociopolitical organization, some readers may be surprised to find that in this case the linkages between the two appear to be minimal. Murphy shows that irrigation did not create high labor demands, nor did it require much in the form of a “responsible authority.” Only in the Valle de Santiago was there a water judge with important functions, and “[n]owhere—not even in the Valle de Santiago—did the practice of irrigation vitally affect social or political structures” (p. 199). Even in Querétaro, which had an elaborate water system for the city and surrounding gardens and farms, irrigation evolved in the seventeenth century with no central planning or administration. In general, irrigation in the Bajío was built and controlled by the landowning elite with sporadic intervention by the central government. Municipal authorities, chronically short of funds, played only a minor role.

My only quibble with Murphy’s study concerns his assertion that the case of colonial Querétaro calls into question the “hydraulic hypothesis” about the central role of irrigation in the rise of states and city-states. I do not think it does, for there is a significant difference between colonial Querétaro, with its dominant elite, and pristine cases where urban and state structures evolved for the first time in minimally stratified settings. Murphy’s study is a useful reminder, however, of the importance of the overall context of irrigation works and the use to which they are put. One may surmise that the social and political linkages of a public irrigation system employed to increase production would be quite different from those surrounding the private systems described in this volume. I recommend *Irrigation in the Bajío Region* to those interested in haciendas, agricultural technology, and water law, but readers looking for social or political history will have to look elsewhere.

Two of the books under review are directly concerned with the urban social structures of Guadalajara and Mexico City in the early nineteenth century. In *Guadalajara a la consumación de la Independencia*, Rodney Anderson dissects a set of censuses from 1821–22, which he claims are “without doubt the most complete censuses that have been made in any Latin American city at the beginning of the nineteenth century” (p. 10). Taking a 10 percent sample (procedures are not discussed), Anderson groups the data from the city’s twenty-four *cuarteles* into seven artificial districts for analytical purposes. Separate chapters are devoted to population (although no age pyramids are provided), migration (which came from all social classes), the family (with qualified support for Laslett’s hypothesis that the nuclear family predates the industrial revolution), occupations, and social stratification (the relationship between race and class).

Students of colonial Mexican cities cannot afford to ignore this book because it provides valuable data that can be compared with other studies of Mexico City, Guanajuato, Oaxaca, and Durango. Analytically, however, Anderson’s book leaves several fundamental questions unanswered. For example, occupations are sorted into six status categories, but without any clear discussion of the criteria employed. Another methodological problem concerns disparities in data available for the twenty-four different *cuarteles*; these disparities are acknowledged but not fully explained. Anderson ponders the question of whether Guadalajara’s mode of production was capitalist or “artisanal” in 1821, only to conclude that the answer “is not very clear” (p. 156). In a similar fashion, he discusses the recent debate over the relative weight of racial and class factors in the stratification systems of colonial Mexican cities yet is reluctant to interpret the Guadalajara case, claiming that the data are not good enough. Anderson’s reluctance to take even tentative stands on such issues means that the book has no real conclusions. This result is unfortunate because *Guadalajara a la consumación de la Independencia* contains some strong sections, especially the two chapters on the family. As a case study, however, it demonstrates the difficulties involved in analyzing a census without other kinds of supporting documentation to aid in interpreting it. The Guadalajara census of 1821–22 may indeed be an extraordinarily rich source, but its full potential has not yet been tapped.

A much more satisfying study in many respects is Silvia Marina Arrom’s *The Women of Mexico City, 1790–1857*, which ranks as a major contribution to colonial Mexican social history. Utilizing city censuses of 1811 and 1848 and a wide variety of other materials, this book is brimming with insights into the role of women in Mexico City in the first half of the nineteenth century. Data on women’s actual behavior are effectively marshaled to counter the old stereotype of colonial women

“as passive, powerless beings, absorbed in familiar duties, confined to the home, and totally subordinated to men” (p. 1). The dates of the study are themselves something of an innovation and provide a rare comparative glimpse of an important aspect of social life before and after the dismantling of the colonial regime.

An introductory chapter notes the tangible progress toward women’s equality made between 1790 and 1857 with respect to education, work, political mobilization, and government attitudes, and concludes that the notion of women’s social utility gradually supplanted the older ideal of female seclusion. In a thorough and lucid review of the changing legal status of women, Arrom notes that despite an implicit recognition of women’s competence, their inferior legal status was perpetuated. She finds evidence of improvement in the legal status of single and widowed women during the first half of the nineteenth century but not of married women. Arrom’s explanation is that the nuclear family was essential to the preservation of the hierarchical corporatist state and that effective control of the family in this setting required the inequality of husbands and wives. A man governed his wife and children just as he in turn was governed by the king. Yet Arrom notes that Hispanic society was not a “classic patriarchy” like Rome because not all women were directly controlled by men.

Single and widowed women enjoyed considerably more freedom, and while they occupied no well-defined place in the corporatist ideology, by 1811 they accounted for roughly a third of the city’s female population. A relatively late average age at marriage (22.7 years) and a low marriage rate, coupled with the fact that the typical woman’s husband died when she was forty all help explain the pattern. The incidence of marriage was lowest among the Spanish upper class, where there was a significant degree of female celibacy. Arrom speculates that upper-class women had more freedom from traditional roles than their lower-class counterparts because they were less likely to be wives and mothers and more likely to head their own households. Census data on employment show that more than a quarter of the urban women worked, mostly as domestics, cooks, laundresses, or workers in the retail food industry. Work was not highly valued by women, however, and was certainly not an avenue of upward mobility. Those who had moved into traditionally male occupations began to leave them after 1811 because of the economic recession and rising male unemployment.

The Women of Mexico City contains a good discussion of the divorce process. Arrom argues that a subtle change in attitude toward marriage occurred in the first half of the nineteenth century, as wives gained in status relative to their husbands. Yet at the same time, the expansion of liberal individualism also stimulated a more tolerant view of adultery by husbands (but not by wives) and a strengthening of the

double standard. Arrom concludes with a discussion of the *marianismo* complex, arguing that it was not introduced in Mexico until after the middle of the nineteenth century. Marianismo brought a belief in the spiritual superiority of women and a corresponding elevation of their status within the family, trends that reversed some of the gains they had made earlier in the labor force and in political mobilization. Thus in some ways, the late colonial period was more dynamic for women than the succeeding years.

More than just a case study, *The Women of Mexico City* achieves a breadth of synthesis that will appeal to a wide scholarly audience. Arrom skillfully knits together laws and legal commentaries, census material (working from a sample of the 1811 census), wills, and ecclesiastical divorce cases to support her arguments. Like any study, however, this one has its weak points. While Arrom attempts to deal with women of all class levels and ethnic groups, the book is slanted, perhaps inevitably due to the sources, toward the white, Spanish upper class. The author is able to show how female roles varied with economic status but fails to develop a convincing concept of class. Nor do readers learn much about the non-Spanish population; Indians and *castas* are mentioned, but they occupy a secondary place in the book. I also think it curious that there is no mention of possible differences in the statuses and roles of creole women versus peninsular women. On the whole, however, Arrom's study is a most impressive work that is bound to become a standard reference in the field for years to come.

The most quantitatively oriented of the dozen books under review is Claude Morin's *Michoacán en la Nueva España del siglo XVIII*. It is also the most explicitly economic in focus, employing parish registers, tax records, and tithe statistics to measure population growth, agricultural production, taxation, prices, the circulation of money, and the condition of haciendas. Morin takes an avowedly Marxist approach, arguing that many of the Bourbon reforms—particularly in taxation—benefited Europe but led to growing inequality in Michoacán. The cash economy was to a great extent socially circumscribed, and surprisingly little money was in circulation, with businesses operating primarily on credit. Morin even speaks of a process of "demonetization" closely related to the ascendancy of the export trade. On haciendas, too, he finds that salaries were rarely paid in cash and that debt peonage was common. The social relations of production are described as "almost feudal" (p. 268).

Morin concludes that in all fields of economic activity, new forms of organization introduced in the late eighteenth century led to an expansion of production that in turn fostered greater inequalities: the increasing impoverishment of many and the enrichment of a few. World-market integration had the effect of reinforcing "colonial feudalism,"

which persisted alongside petty commodity production and communal production in the Indian *pueblos*. *Michoacán en la Nueva España del siglo XVIII* has had a significant impact since its publication because it helps fill some important gaps in the historiography of Michoacán and the Bajío and because of its judicious use of quantitative sources. Morin is a useful counterweight to Leiby in assessing the impact of late-eighteenth-century economic reforms. Changes that looked quite positive from the perspective of the central bureaucracy often turned out to have negative consequences for economic development at the local level.

Another thoroughly researched regional study is *Rural Society in Colonial Morelos* by Cheryl English Martin. The author is mainly interested in finding out how sugar haciendas shaped the social history of the Morelos region, and to this end, she traces the interaction of haciendas, Indian villages, and small farms from the late sixteenth century to the end of the colonial period. Although the sugar haciendas were heavily indebted to creditors in Mexico City from the start, they grew steadily during the seventeenth century before encountering a financial crisis in the 1690s. Instability of ownership and financial troubles followed, as in other regions of New Spain, although a dramatic recovery occurred after 1760. Sugar prices rose, the market situation improved, and some sugar was exported for the first time since the sixteenth century.

Society receives as much emphasis as the economy in *Rural Society in Colonial Morelos*, and unlike most of the other books reviewed here, Martin's study devotes ample coverage to Indians and *castas*. Parish registers are used to good effect to study settlement patterns, migration, *congregaciones*, and population trends. An intriguing chapter on hacienda labor shows that slaves and Indians were brought closer together than in other New World sugar colonies. Slaves formed ties of marriage, concubinage, and *compadrazgo* with Indians and free mulattoes, and by the eighteenth century, slaves enjoyed a relatively privileged position, despite the generally harsh tenor of labor relations. Especially welcome is a chapter on late colonial Indian villages. Haciendas were able to grow by exploiting the land and water of the lowland communities, but they drew most of their migrant labor from villages of the Morelos highlands. Major lowland Indian towns received migrants of all racial categories and managed to survive as "Indian" communities, despite the fact that Indians were in a minority in all of them by the end of the colonial period. Indian caciques and other village leaders often cooperated with local Spaniards and hacendados, finding it more profitable to rent land and house lots to them than to other Indians. Some of this income became personal gain, but some was used for public purposes.

While Morin finds heightened economic inequality and exploitation in late colonial Michoacán, Martin points to a marked increase in social conflict in Morelos. In a concluding chapter focused on Yau-tepec, she shows how economic reforms led to a free-for-all over land and water among small farmers, Indians, clerics, civil officials, and hacendados. The absence of a Spanish *cabildo* in the region was likely a contributing factor to the conflict. In all, *Rural Society in Colonial Morelos* is a solid and illuminating work on agrarian social history. While it lacks the breadth of synthesis found in Arrom, Morin, and Hamnett (discussed below), the longer time span covered provides a compelling picture of social and economic relations in this important sugar-producing region.

In *Roots of Insurgency: Mexican Regions, 1750–1824*, Brian Hamnett takes on the challenging job of addressing the causes of the War of Insurgency of 1810–1821 from the perspective of regional social tensions. He concentrates on the provinces of Puebla, Guadalajara, Michoacán, and Guanajuato, each of which became a major theater of action. His concern throughout is primarily with regional tensions and conflicts surrounding the “resident elite” of merchant-investors, mine operators, municipal councilors, and landowners, as well as the “provincial bourgeoisie” of lawyers, clerics, intellectuals, writers, and doctors. An important thread running through the book is the expansion of mercantile finance into the hinterlands and the central role of the merchant-investors in the genesis of much social unrest. This observation does not imply that identical processes appeared everywhere, for Hamnett is clear about the uneven development between northern and southern parts of New Spain, as well as factors internal to specific provinces. He examines many kinds of conflict, including those resulting from government abuses, land conditions, mining, deterioration of living standards, agricultural crises, and *caciquismo*. Many districts were trouble spots long before the insurgency and continued to be for many years afterward. Local incidents were unconnected except when briefly transformed by the leadership of Hidalgo and Morelos into the semblance of a national movement.

While Hamnett presents no new “theory” of the insurgency, he succeeds in bringing readers closer to the grass-roots level and in showing how scattered and diverse local grievances were. He argues that several short-term and long-term factors came together in 1810: dearth and dislocation, military unpreparedness, and especially a political loss of control by the central government. With this crisis at the center and uncertainty in provincial capitals, Hidalgo and Morelos provided the necessary leadership that made political insurrection possible. Yet when they were gone, the movement fragmented and the original ideological and nationalist superstructure of revolt rapidly disintegrated. Hamnett is not sanguine about the ultimate result: “It seems that in Mexico in

the 1810s we are witnessing a classic case of failed revolution" (p. 201). *Roots of Insurgency* is a welcome alternative to the usual histories of independence movements, which tend to be nationally focused and preoccupied with alienation and ideology. The study furthers understanding of not only the insurgency but the social and political conflicts in the provinces in the late colonial years.

It is difficult to sum up in a few words a collection of works as diverse in content and methodology as those considered here. Overall, I would have to say that the substantive local and regional studies stand out as superior to the synthetic works, and that if any theme predominates, it would be the increasing inequalities, tensions, and economic exploitation of the last sixty years or so of colonial rule. An exception to these trends was the increasing equality of women, at least in Mexico City, which occupied a privileged position as the dominant capital. In general, however, it is becoming increasingly clear that provincial economic development in New Spain during these years brought with it many social tensions and inequities often associated with dependent development in more recent periods. Now that a good number of regional studies are available for diverse parts of colonial Mexico, Brian Hamnett's research strategy of systematically examining a limited number of key questions in the context of several different regions provides a promising model for future work.