

MEXICAN RURAL HISTORY  
SINCE CHEVALIER:  
The Historiography of the Colonial Hacienda\*

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Intellectual disciplines, very much like human beings, have life cycles. They are conceived and born, they progress through childhood, adolescence, and youth, they reach maturity, they enter old age, and some even die. Even if in the present case the simile is a grandiose one, and if the field of Mexican rural history can hardly lay claim to the status of being a distinct intellectual discipline, the main point nonetheless holds. After a long period of gestation and a halting but promising infancy, the field is standing firmly on two feet. It has a *problemática*—a set of questions, something resembling a research strategy, and a conceptual framework (much of it admittedly borrowed); it has an identifiable corpus of literature, and its practitioners recognize one another. Yet how mature is it, and where is it going? The purposes of this article are to review the development of the historiography on rural life in colonial and early national Mexico published during the last thirty years, focusing central attention on the study of the hacienda; to assess some of its findings, problems, and growing pains; and to make some suggestions as to where those working in the field might invest their energies in future. Within the overall topical organization of the essay, the literature on the classic Mexican hacienda is examined from thematic, theoretical, and methodological vantage points. These treatments are complementary rather than redundant because the questions historians ask, the explanatory schemes they use, and the sources and methods they rely upon are intimately interrelated, and such a prismatic analysis of a body of literature helps to point up its strengths as well as its weaknesses.<sup>1</sup>

DEFINITION AND PERIODIZATION

To begin at the beginning, what is rural history? The answer to this question is not so simple as it appears. In preindustrial societies, it can

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be argued that because most people reside in rural areas and make their living directly from the land, the rubric "rural history" properly embraces most social, economic, and even political relationships. If the term is to be useful, however, it must have a more restricted definition.<sup>2</sup> In this country, the term "agricultural history" is still much in vogue, although it carries with it the implicit meaning of a central concern with the technology and economics of production. In Europe the term "agrarian history" is generally used and implies a broader area including rural social structure. For the present, I will define rural history to mean the economic and social relationships of settled farming people living outside cities, specifically with regard to the production of goods from the land. In the Mexican case particularly, the category of "rural history," in either the colonial or national periods, has taken on a rather restricted meaning. It is no accident that this review deals so heavily with studies of the hacienda, and two of its purposes are to explore the reasons for this narrowness and to suggest that the boundaries of the term should be pushed outward in practice. Nonetheless, my central concern with the traditional Mexican hacienda seems justified because that is the subject to which most researchers in the field seem to have devoted their efforts. The same argument applies to this essay's obvious emphasis on the economic aspects of traditional rural Mexico and its landed estates.

Having opted for a fairly broad *a priori* definition of rural history, does it then make any sense to restrict the focus to the colonial period (1521–1821), or even to Mexico? To answer the latter part of the question first, there are two reasons to do so. First, a strong case can be made that colonial New Spain formed a coherent entity according to any number of criteria—ethnographic, geographic, administrative, economic, relationship to the metropolis, and so on. In fact, when one speaks of Mexico, one is usually talking about the core area of the viceroyalty because in the field of rural history, little has yet been produced on the northern extremes of the kingdom (Charles Harris 1964, 1975; Altman 1972, 1976; Myres 1969; Chevalier 1959, 1963; Cuello 1981), and even less on the southern margins (Hunt 1974, 1976; García Bernal 1972). Still, it is clear by now that Mexico's agrarian history differs from Peru's, for example, despite their similarities, and that the differences have been important. Second, I have felt it necessary to limit the field of inquiry in the present case so as to be able to manage a comparatively large literature in a reasonably critical way.<sup>3</sup>

The part of the question relating to the temporal aspect is somewhat thornier. Scholars have grown so accustomed to the traditional (and essentially political) division of Latin American history into colonial and national periods, divided neatly by the winning of independence, that it is something of a wrench, whether in writing or teaching, to think in terms of a periodization that may span political independence to

embrace a different view of long-term change.<sup>4</sup> For Mexico, it makes sense in some respects to divide the study of rural history into colonial and national periods. The evidence for such a demarcation is fairly persuasive and can be summarized briefly as: first, the birth of a national economic system, albeit weak and flawed, accompanied by a loss of external markets; second, a general economic contraction aggravated by a flight of capital out of the country; third, the ascendance of the creole landowning elite to national political power; fourth, the overnight destruction of the elaborate patriarchal structure of royal protectionist Indian policy.

Despite the persuasive evidence that points to the period 1810–1821 as some kind of watershed in the history of the Mexican countryside, the case for delineating such a periodization is at the least a questionable one for two reasons.<sup>5</sup> In the first place, the changes mentioned above may be seen either as insignificant for the structures of rural life and economy of Mexico or as already being under way before the consummation of political independence—that is, as not constituting changes at all. As to the loss of external markets, it is difficult to imagine that a temporary disruption and subsequent reorientation of export agriculture could have very much dislocated an agrarian economy whose resources were overwhelmingly devoted to internal markets. The post-colonial economic contraction may well have been underway before 1810, and even if it was not, it would surely have constituted the downturn or phase “B” of an “A-B” cycle (Coatsworth 1978). In either case, to disaggregate the trend is to lose the significance of the overall picture for the sake of a false clarity; what is important is not division but continuity. The creole ascension apparently did not alter basic productive or ownership arrangements in the countryside. Finally, the social position and living standard of the rural masses was slipping in some parts of New Spain by the end of the eighteenth century (Van Young 1981), so that the sweeping away of Indian protectionist legislation only aggravated a situation already in existence by the time of independence.

The second major and related reason for not accepting the wars of independence as a major watershed in the periodization of Mexican rural history is that the technology, productive arrangements, and social relations of production that were to continue to dominate much of the Mexican countryside up to the last quarter of the nineteenth century, or even up to the revolution of 1910, were already in place by 1780 or earlier. In analyzing change in a rural society, one typically analyzes structures that change, but at a glacial pace in comparison to events in the political sphere. The structures of rural life are a part of those “coral reefs of human relations,” to use the evocative phrase of Immanuel Wallerstein (1976, 3), that cannot be rushed along with essentially political periodizations or fortuitous events. Whether or not Manuel Godoy

was the queen's lover made some difference in the fate of Spain, but little, one suspects, in the life of a peasant or rural laborer in the Mexico of 1800. To divide the study of Mexican rural history at national independence is therefore to apply inconsistent and inappropriate criteria to the problem of historical periodization.

Much of the recent work on Mexican rural history, whether recognizing the limitations of the conventional wisdom on periodization or hewing to the more traditional line that breaks Mexican life in two at 1810, places a heavy emphasis on the eighteenth century, rather than on the seventeenth or the nineteenth. Why should this be so? One reason is certainly the impressive quantity and quality of the surviving documentation. A second possible explanation is a fascination with the apparent contradiction between the late imperial efflorescence in economic and cultural life and the dramatic impending doom of the Spanish imperial order. Reinforcing these factors are the Western predisposition to think of the eighteenth century as central to the modern experience and our tendency, under the impact of developmentalist thinking, to confuse dynamism, especially in economic life, with significance. To focus on the eighteenth century alone, without stating justifiable reasons for such a focus, is therefore to skew the results before undertaking the investigation and to make the answer implicit in the question. This nondialectic approach to the rural history of Mexico, based as it is on magical thinking about the importance of hundred-year spans of time, is particularly inappropriate when looking at the social and economic substrate of agrarian society. Indeed, one of the major findings of the last thirty years is that the eighteenth century has begun to lose its integrity.

Such criticisms about periodization notwithstanding, it nonetheless will become obvious to the reader that most of the works mentioned in this article cleave to the traditional temporal division of labor between colonial and national periods. For our purposes, this division seems justified because the present essay deals mainly with the historiography of Mexican rural life and not with its history. In several cases, however, recent monographs on rural history do overlap the national divide, some by considerable margins (Brading 1978; Couturier 1976; Semo 1977; Charles Harris 1975; Bazant 1975), and these will be considered along with the rest that fall within the traditional chronological boundaries of the colony.

#### DEVELOPMENT OF THE LITERATURE

Thirty years ago, François Chevalier told us everything we had always wanted to hear about "men rich and powerful" and the classical Mexican hacienda; he also concretized the image of the Mexican latifundio as it had emerged from the work of such scholars and polemicists as An-

drés Molina Enríquez (1909), Frank Tannenbaum (1930), Helen Phipps (1925), George McBride (1923), Nathan Whetten (1948), Eyer Simpson (1937), and Jesús Silva Herzog (1959), among others.<sup>6</sup> Chevalier, of course, was not working in a vacuum. Going beyond reformism, polemic, or stereotype, scholars such as Silvio Zavala (1948a, 1948b) had already begun to ask penetrating questions about the institutional structure of landholding and labor systems.<sup>7</sup> But it was Chevalier, with his painstaking approach to masses of previously unexploited documentation, who brought the great hacienda down from the level of abstraction to that of historical reality. Chevalier provided a *tableau vivant* of the feudal-looking north Mexican seigneur and the patriarchal, quasi-frontier society over which he ruled. Perhaps more important, Chevalier suggested an etiology for the seigneurial and patriarchal society that revolved around the great landowners in many parts of Mexico, but preeminently in the north: a seventeenth-century economic contraction in New Spain led by a decline in mining production. This economic contraction, reasoned Chevalier, forced agriculture, whose early prosperity had been linked to the prosperity of the mining centers, to turn in upon itself, thus reinforcing a tendency already present in Mexican agriculture toward extensive practices as opposed to intensive practices—that is, toward livestock-raising and away from farming. The shrinkage of markets, therefore, was seen to be at the root of an economic retrogression that in turn caused a feudalization process to set in, producing a system of great landed estates that differed from the embryonic capitalist agriculture characteristic of the boom times of the Mexican economy.<sup>8</sup>

The year before Chevalier's ground-breaking study appeared, Woodrow Borah published a provocative essay in which he extended the concept of a seventeenth-century depression to cover Mexico in general (1951). Borah's work, however, had a different origin from Chevalier's because it grew out of the early efforts of the Berkeley group of historical demographers to outline the course of Mexican population history during the century after the conquest.<sup>9</sup> In his scenario of a seventeenth-century depression, Borah tied the general Mexican economic contraction to the earlier Indian demographic collapse and a resulting labor shortage that adversely affected the pivotal mining economy of the colony. As for the agrarian economy of the late sixteenth and seventeenth centuries, Borah also tied the population decline and resulting labor shortage to the rise and spread of debt peonage as the characteristic labor institution of the Mexican countryside. He argued that the extension of credit by landowners plus the enforcement of the sanctity of debt by those same landowners and the Spanish state immobilized laborers and created a more reliable labor force in a labor-scarce situation.

Despite some differences of interpretation, the Chevalier and Borah hypotheses were not incompatible but complementary. They provided simultaneous cogent explanations for the most widely discussed characteristic of Mexican rural history since the Spanish Conquest: the dominance of the countryside by large, underproductive landed estates with patriarchal or feudal social structures and impoverished, servile labor forces. Furthermore, the hacienda and debt peonage were portrayed largely as the results of depression and economic retrogression, which seemed to transform apparent economic irrationality and "feudalization" into adaptive strategies, albeit not virtues. The two ideas dovetailed so well that they came to be known in the literature as the "Borah-Chevalier thesis" (Frank 1979; Mörner 1973). If modern writing on the rural history of Mexico has any identifiable starting point, it may well be the publication of the Chevalier and Borah studies. The two works together accomplished three important things. First, they provided convincing substantive explanations for the shape of colonial rural society in Mexico, and incidentally for many of its characteristics that would survive the colonial period. Second, they raised the level of the historiographical debate, sharpened it, and turned it away from the more immediate past (the Porfiriato and the era of the revolution of 1910) back to the concrete historical realities of earlier centuries. Finally, they provided a point of theoretical crystallization around which a lively discussion was later to develop. Thus Chevalier and Borah propounded a powerful model of a historical process that culminated in the great Mexican hacienda. In so doing, they oriented the ensuing debate toward revisionist criticism of their model and toward further investigation of the great rural estate. Thus the great landed estate offered the earliest, and in some ways the easiest, point of entry into the subject, and I would suggest that for this reason the continuing investigation of the theme largely retains that cast. During the next fifteen years or so, the picture that Chevalier in particular had painted of the colonial hacienda came to be accepted broadly as the conventional wisdom on the subject (Simpson 1967; Wolf 1959). It had the advantage, after all, of offering a not-too-radical departure from the traditional view of the Mexican hacienda because it had simply and elegantly told us what we had always thought was in large measure correct, providing us with an etiology for symptoms that had always been visible.

A serious research response to the Chevalier and Borah works took more than a decade to develop. This delay seems curious in view of the importance of their studies for the interpretation of colonial history. A number of partial explanations suggest themselves, but all in combination do not convincingly account for the gap. Among these are the diffusion time required for Chevalier's work to become generally known (it was not translated into Spanish until 1956, or into English until 1966);

the growing interest of scholars in the United States and elsewhere in other themes; the need for certain methodological advances to be made;<sup>10</sup> and the sheer lack of scholars in the field. Whatever the reasons, after the generation of some fruitful hypotheses in the early 1950s, the study of colonial rural history remained curiously stunted during the following decade or so.

Work certainly was being done, particularly on the economic and technical aspects of rural structures, some of it very good work, but a clear set of questions, a method, and a conceptual framework were slow to develop after the precipitous birth, or perhaps rebirth, of the field in the early fifties.<sup>11</sup> Nevertheless, during the fifties new theoretical perspectives were introduced or expounded that would become important later on, when the empirical base for the study of rural history had broadened somewhat. In 1950 a now little-cited article by Jan Bazant explicitly introduced the Marxist analytic of "feudal versus capitalist" into the debate on the nature of Mexican nineteenth-century economic development. Although Bazant addressed himself to the problem of the Porfirian era, the ground of his discussion was eventually extended to include colonial economic development, particularly the nature of the great landed estate. Chevalier, playing into this debate, built on his own previous work with a pair of essays (1959, 1963) in which he further sketched the quasi-feudal nature of the great north Mexican hacienda in the eighteenth and nineteenth centuries. Parallel to the introduction of Marxist historical categories, and touching them at some points, was an analysis offered by the anthropologists Eric Wolf and Sidney Mintz of the theoretical differences between plantations and haciendas (1957). Although based on contemporary materials rather than historical studies and including the Caribbean area as well as highland and lowland Mesoamerica, the Wolf-Mintz scheme for the first time disaggregated the social and economic characteristics of large landed estates. It posited a limited number of variables as determining whether a given region developed as an area of plantation agriculture (characterized by heavy capital investment, high levels of technology, an intensive labor regimen, and strong links to regional or international markets) or as one of hacienda agriculture (characterized by low levels of investment, technology, and labor utilization, as well as weak links to markets).

The first significant test of what has become known as the "Chevalier model" (or the "Borah-Chevalier thesis") came in 1964, with the publication of Charles Gibson's monumental work on the history of the Valley of Mexico, *The Aztecs under Spanish Rule*. Gibson was occupied with the acculturation process of the Indians of the valley during the colonial era, and consequently he approached the Chevalier model of the Mexican hacienda sideways and gave it a good kick from which it has never recovered. The hacienda was so important an institution in

the Valley of Mexico, Gibson implied, that it virtually obtruded itself upon his notice as he looked at the relations between Indians and whites. Eschewing Marxist or any other theoretical framework, Gibson took the road of eclectic, empirical historicism and devoted much of his long book to an examination of what agrarian structures were actually like in the Valley of Mexico. In retrospect, his findings were hardly surprising, but they diverged greatly from those of Chevalier for the north and near-north of Mexico. Gibson found not sprawling, underutilized estates, but a wide range in size variation among haciendas that were assessed more for their capital value than for their size. The Valley of Mexico had not an extensive livestock economy of low productivity, but a mixed livestock and farming regime that concentrated on cereal production and used irrigation widely. Gibson's findings pointed not to an autarchic agricultural economy that had closed in upon itself and had little connection with local or regional markets, but to a highly commercialized estate agriculture linked to the enormous market of Mexico City. Gibson found not the oppressive institution of debt peonage that had evolved in parts of the north to secure a scarce and mobile labor force, but rather that labor in the Valley of Mexico, at least by the eighteenth century, was relatively plentiful, that free-wage employment was more important than debt peonage, and that the hacienda represented shelter and security to the rural Indian laborer, not merely oppression and bondage.<sup>12</sup> In the conclusion of his book, Gibson pointed somewhat Delphically to the crucial importance of the hacienda in the history of colonial Mexico and to its historiographical neglect, exhorting us to make deeper and broader studies of rural history using the great estate as a central problem (1964, 406–7). Gibson was saying that his data on the Valley of Mexico might be unrepresentative (as in some sense anything related to Mexico City must be), but that we needed to know more about the country as a whole. Meanwhile, Lesley Simpson's introduction to the English edition of Chevalier's work at about the same time was saying that the French scholar had confirmed what had always been known anyway (1966, vi). Clearly there was a problem.

If Chevalier and Gibson could present substantially divergent images of the colonial hacienda, what did it mean for our understanding of Mexican history in general, and for the study of rural history in particular? What could account for the differences they saw in such a pivotal institution? The two scholars and others working in the field had not begun to make the pieces of the puzzle fall into place, but had disclosed a puzzle whose existence we had not noticed before. The studies of Chevalier and Gibson pushed the ensuing debate on the nature of Mexican agrarian development into channels where it has mainly run up to the present. Most important, they both focused on the development of the hacienda as the central thread of colonial agrarian



history, Chevalier explicitly and from the beginning, and Gibson by a process of elimination. Borah, Chevalier, and Gibson were not the only scholars to come to this conclusion or to lead in this direction. Methodological considerations and investigation on other themes in colonial social and economic history also seemed to point in the same direction. In the first case, the surviving documentation spotlighted the great estate as a historical entity, although there were other entities in the countryside that also left documentary residues (a good deal on Indian villages, less on small independent property-owners, market towns, and so on). In the second case, subsequent attempts to understand more about the social structure of colonial Mexico have also led in the direction of the great hacienda. In what may be called the socialization of elite studies, such investigators as Brading (1971) and Ladd (1976), who ostensibly started with different ends in view, were both drawn to the conclusion that the ownership of landed estates was inextricably intertwined with elite status.<sup>13</sup> These factors partially explain the degree to which the study of rural history has come to be identified with the study of the hacienda.

Given the emphasis on the hacienda within the field of rural history, which had emerged clearly both from an older body of literature as well as from post-1950 investigations, three themes or related clusters of problems had come to dominate the approach to the hacienda system by 1965. First, it was acknowledged that there was considerable temporal as well as geographical variation in the socioeconomic role of the landed estate in Mexico during the colonial period and the nineteenth century; a major problem was how to account for this variation. Second, the theoretical framework employed in attacking the problem of divergent development, which clearly resonated with the increasing amount of empirical data, concentrated on the importance of a limited number of variables and their interactions. These elements may roughly be categorized as production factors, market factors, and social-control factors. They are all at least implicit in the work of Chevalier, Gibson, and others, and were explicitly laid out by Wolf and Mintz (1957; see also Mörner 1973; and Van Young 1981). Finally, efforts were also being made implicitly to fit the development of colonial Mexican agrarian structure into some kind of broader analysis of historical change—to characterize Mexican colonial society as a whole by resorting to the character of its agrarian institutions. Chevalier (1952) and Bazant (1950) were not the first to characterize the colonial hacienda as “feudal”; the idea was clearly expressed at the beginning of the twentieth century by the Mexican sociologist Andrés Molina Enríquez (1909). But if the haciendas in central Mexico were different from the ones in the north and near-north, or if the haciendas of the eighteenth century were different from those of the seventeenth, what were they to be called? Embryonically capital-

ist? Transitional? Less feudal? Nonfeudal? Semi-feudal? Postfeudal but precapitalist?<sup>14</sup> It was at this point that Marxist analytical frameworks began to have some importance, becoming influential among even those who rejected Marxist categories. When Marxist scholars such as André Gunder Frank (1969) and Enrique Semo (1973) injected the entire debate over feudalism versus capitalism into the ongoing debate over dependency theory, the waters became at once more muddied and the discussion linked to larger theoretical issues about world economic history.<sup>15</sup>

Since the late 1960s, the study of colonial and early national agrarian history has flowered. Scarcely a year has gone by without the publication of at least one major book by a Mexican, an American, or a European, in addition to the scores of dissertations and journal articles that have appeared. A number of international conferences have been devoted to the agrarian history of Latin America as a whole or of Mexico in particular, and these meetings have published interesting proceedings.<sup>16</sup> What have we learned? I will answer this question by reviewing in broad strokes the major findings in the field. After that I shall look at the methods and sources that have been used up until now, and then briefly suggest some possible areas for future investigation.

#### SUMMARY OF RECENT RESEARCH

One would think that sharper definitions would be the first recognizable product of a rapidly growing field of research such as colonial Mexican rural history, yet sharp definitions have been the first victims of recent investigations. The most basic piece in the conceptual arsenal, the model of the hacienda, has been damaged almost irreparably. It is probably easier to say what a hacienda was not than to say what it was. It was not a capitalist family farm, like those found in many areas of the Western world today; nor was it a communal or cooperative enterprise like an intact Indian village; nor was it based upon peasant or subsistence agriculture, although in its most underutilized form it might appear to be similar to all of these. If one shifts from trying to define the hacienda in terms of a style of production to trying to define it in terms of a limited number of major variables presumably common to all instances, the problem becomes more difficult.

To anticipate my conclusions a bit, even if one takes the most catholic and nonrigorous view of the matter, the range of observed variation in the nature of agro-social units called haciendas was enormous during the colonial and early national periods. Furthermore, if in order to cast the theoretical net as widely as possible, one charts each of the major variables along a continuum ranging from "little" at one end to "much" at the other, instead of looking for their presence or absence, the category of "hacienda" is likely to become so broad as to be nearly

meaningless. What does this polymorphism suggest? For one thing, it suggests that the Mexican hacienda may have been an effect rather than a cause, its historical role that of a dependent rather than an independent variable, and that we have been studying rural history the wrong way around. Indeed, several authors (Chevalier 1966; Frank 1979) have asserted that the Spanish American hacienda was a child of the New World—a cultural and economic response to specific conditions encountered there by Europeans—rather than a form transferred intact from the Old World. If this assumption is correct, as the abundant research increasingly indicates, then much of the debate, revisionism, and *sturm und drang* over the nature of the hacienda has been misplaced. We will never fully understand Mexican agrarian history until we shift our focus outward from the hacienda to encompass other elements in the countryside and look carefully at rural economy and society as a system in which the rural estate was only one important part. Nonetheless, in reviewing broadly the recent findings in the field, I wish to address the known rather than the unknown, which involves looking at the hacienda.

Beginning with land, the most basic variable in the makeup of the traditional hacienda, recent research shows that rural estates differed tremendously in size and quality of landholdings. Certainly in the arid north, where the possibilities for irrigated agriculture were constrained by scarcity of water, weakness of markets, and shortage of labor, haciendas reached their greatest extensions, attaining the size and virtually autonomous status of principalities, with millions of acres under their control (Charles Harris 1975; Romero de Terreros 1956; Chevalier 1952; Altman 1976).<sup>17</sup> On the other hand, as Gibson early pointed out (1964, 289–90), size alone (or even in conjunction with other factors) was not enough to make a rural estate into an hacienda. Nearer the center of New Spain, the average size of estates seems to have decreased.<sup>18</sup> In a general way, hacienda size tended to vary inversely with the quality of land, even where location and quality were interrelated through the process of artificial improvement of fertility. In the Bajío and Guadalajara regions in the late colonial period, haciendas of two or three hundred thousand acres were considered enormous, and most were apparently much smaller, as they also were in the valleys of Mexico and Oaxaca (Brading 1978; Van Young 1981; Gibson 1964; Taylor 1972). As Isabel González Sánchez has shown, in the area of Tlaxcala at the beginning of the eighteenth century, haciendas of more than three or four thousand acres were considered inordinately large, while properties comprising as little as six or eight hundred acres were still considered to be haciendas (1969). To attempt to categorize the colonial hacienda strictly on the basis of size, therefore, as some earlier writers did (Phipps 1925) and as some modern authorities also suggest (Couturier 1976), seems risky. On

the other hand, haciendas tended to be larger than the other kinds of rural production units (communal holdings, peasant plots, ranchos, estancias, labores, and so on) that existed in the Mexican countryside. But how much does this tell us? It is rather like trying to explain the differences among human beings—between an adolescent and an adult, for example—on the basis of size alone.

What, then, of other production factors such as technology, capital, and labor? In the instance of technology, it is generally recognized that Mexican agriculture remained fairly backward during the colonial and early national periods. Yet just how backward was it, and was estate agriculture likely to have been any less backward than other types, or qualitatively different from them? It must be admitted from the outset that, with certain exceptions, we really do not know much about colonial agricultural technology. Exceptions to this statement are the practices of the Mexican Jesuits, from whom we have a manual of prescribed managerial and farming techniques (Chevalier 1950), the practices of Indian farmers, for whom we have scattered but intriguing data (Gibson 1964; Cook 1949); livestock raising (Charles Harris 1975; Serrera 1977); and hacienda sugar production, which because of its capital-intensive nature and the need for careful production accounts, has left behind a relatively detailed body of records (Barrett 1970, 1979a; Berthe 1966).

Some have questioned how efficient the Jesuits' vaunted management techniques actually were (Blood 1972; James Riley 1973, 1976; Konrad 1980).<sup>19</sup> In any event, one needs to distinguish between management practices and farming technology; and under the latter head, it now seems unlikely that the Jesuits enjoyed any particular edge over their lay competitors. In the case of Indian farming, we know from comparisons of contemporary descriptions of peasant farming technology with modern anthropological accounts that techniques were fairly intensive, highly productive, adaptive, and simple (Cook and Borah 1974–80; Palerm 1971; Oscar Lewis 1963). If traditional Indian maize agriculture (or small-scale agriculture in general) differed significantly from hacienda agriculture, it was probably not so much in terms of basic techniques as economic organization. We know, for example, that Indian farmers and non-Indian rancheros employed irrigation in producing wheat (Van Young 1981). What ultimately differentiated Indian and some ranchero farming from estate agriculture was their organization on the basis of household economy, a typically peasant mode with the general absence of paid wage labor and the presence of wholly different categories regarding use of labor power and calculations of profit and loss.<sup>20</sup> On the other hand, the generally low productivity of estate agriculture is most often ascribed to the economic environment, especially to the weakness of market demand in a country with an overwhelmingly rural population (Florescano 1971a), rather than to a lack of

technological knowledge. It is thus a case of another continuum rather than a rigid dichotomy or a dual economy, with a roughly similar technology available across a wide spectrum, but with access to the means of production distributed very unevenly.<sup>21</sup> As for livestock-raising, we know quite a bit about normal technology—that is, about the breeding and handling of horses, cattle, and sheep (Charles Harris 1975; Serrera 1977), and even something of the institutional context of herding and transhumance (Dusenberry 1948a, 1963; Serrera 1974a, 1977; Matesanz 1965; Miranda 1958). What we know little about (with certain exceptions such as Simpson 1952 and Crosby 1972) is the interaction between technology and the environment—for example, the ecological effects of large-scale stock-raising. Sugar production is perhaps the one area of agricultural technology in which the techniques and efficiency of small and large producers are likely to have differed markedly. Yet even in this activity, technological improvement did not take place in the form of a revolution, but in small increments, similar to the revival of colonial silver mining (Sandoval 1951; Barrett 1970; Berthe 1965b, 1966; Brading 1971; Chávez Orozco 1950).

If the profile of the traditional great estate is blurred on the technological account, when one looks at patterns of ownership and investment—at capital in relation to the agrarian economy—the image sharpens a bit. Modern researchers have revealed a good deal about the sources and functions of capital investment in colonial and early-nineteenth-century agriculture, as well as about the social meanings of large-scale landownership. Moreover, much of what we have learned undermines the older stereotypes and conventional wisdom about landed wealth and its place in the social order. For example, it is clear by now that during most of the colonial period and on into the nineteenth century, landed wealth alone did not guarantee elite status. Chevalier (1952) discussed the relationship of estate ownership to mining and officeholding and suggested that when the mining sector contracted during the seventeenth century, landownership offered something of a haven, *faute de mieux*, for capital. Since Chevalier's work, numerous studies have pointed out that capital usually flowed from the official bureaucracy, the professions, and above all from commerce and mining into agriculture, not the other way around.<sup>22</sup> These studies have also proved that large-scale agriculture required periodic injections of capital and the capacity to wait out unfavorable market situations, so that self-sustaining, purely agricultural fortunes tended to be rare (Brading 1978; Florescano 1971a; Van Young 1981). A recent work of synthesis on the history of the colony suggests that a secondary elite may have existed whose fortunes did depend solely upon landholdings and who therefore could not claim status amongst the wealthiest of families (McLachlan and Rodriguez 1980).

If ownership of the land could not underwrite elite status, what was the social and economic significance of the great estate in Mexico? Modern research is bringing us closer to a multicausal view of this complex question. Certainly it would be a mistake to discount the prestige aspects of large-scale landownership simply because such ownership alone could not insure elite status in colonial society.<sup>23</sup> The studies cited above and numerous others all suggest that social and political power and ideas about status and hierarchy were intimately linked to control of the land in traditional Mexico at least up until the revolution of 1910. It may even be that in the provinces outside Mexico City, where access to certain forms of great wealth and to the levers of political power were limited, the association between elite status and landownership was even closer.<sup>24</sup>

In any case, the notions about prestige and the social values associated with the objective bits of behaviors that the historian can see are ultimately very difficult to ascertain. In the present context, the fact that colonial and nineteenth-century landowners did not often prate about the prestigiousness of landownership should not be surprising. Such prestige would have been one of the unarticulated assumptions of a society in which, if the *droit de seigneur* no longer existed, a seigneurial mentality certainly did; the parvenue might have talked of it, but would have been seen as vulgar in doing so. Furthermore, we know enough about human societies in general to understand that it is often very difficult to separate the social and economic aspects of a given institution or occurrence. We know, for example, that in colonial Mexico the various ecclesiastical corporations (convents, monasteries, sodalities, episcopal bureaucracies, and so on) recruited their personnel from among elite families and accumulated enormous wealth in the process of providing an outlet for conspicuous social consumption. But the Church also acted as a banker in a prebanking age, financing large-scale agriculture at the same time that it drew off surpluses from that very sector of the economy.<sup>25</sup> In this case, then, the social and economic functions of the same colonial institution were indissolubly linked. In still other instances, behaviors that appear to be aimed at noneconomic goals or that appear to be economically irrational may in fact turn out to have economic ends or to be adaptive and rational in the context of the behavior.<sup>26</sup> This situation would certainly be true in the cases of the apparently irrational behavior of peasant households and the monopolizing character of land acquisitions by large-scale landowners. The point is that considerations of prestige in prerevolutionary Mexico were not necessarily incompatible with considerations of profit or with maximizing economic behavior.<sup>27</sup>

Regarding capital in relation to the traditional hacienda, if gaining prestige and maintaining elite status were not incompatible with rational

economic behavior, what was the economic *raison d'être* of the great estate, and how did it function? It has become something of a truism since Gibson's 1964 study that landed estates were intended to be profit-making enterprises. The older view of the matter—that landowners up through the revolution of 1910 lacked the capitalist mentality, that landed estates were intended only to underwrite elite-status aspirations, and that market-production and profits were exiguous—was put most forcefully by Molina Enríquez in his celebrated statement, "La hacienda no es negocio" (1909). The revisionist view that the Mexican hacienda was from the beginning a profit-oriented business enterprise and that it was moreover integrated into the growth of the western capitalist economy has been put most boldly by André Gunder Frank (1979).<sup>28</sup> This latter view has become so widely accepted that most researchers seem to be turning their energies away from the feudal-capitalist dichotomy, in which the debate over the great landed estate has been couched, toward a study of the factors that limited the profitability of a particular enterprise or group of enterprises at a given time. Notwithstanding, it seems premature to write an obituary for the feudal concept of the traditional Mexican hacienda. Is there any sense in which this venerable label may still be valid?

The answer all depends on what one means by the term "feudal." In relation to the rural history of Latin America and of colonial and nineteenth-century Mexico in particular, the term feudal has been taken to encompass a cluster of interrelated characteristics that are supposed to describe, in Marxist parlance, both the mode of production and the social relations of production of the traditional great estate.<sup>29</sup> The model itself is the medieval European manor, the object of enormous amounts of scholarly energy.<sup>30</sup> The characteristics most often cited to support the resemblance of the traditional Mexican hacienda to the European feudal manor are the political and juridical powers of its owner, the low degree of orientation to markets (sometimes associated with nonmonetary or natural economies), the fixed and servile nature of its labor force, and the patriarchal flavor of its social relations. Going by these criteria, the models of the European feudal manor and the Mexican hacienda show a rather low degree of congruence, except in their social structures. The political and juridical power of the estate's master never was formally a part of the institutional structure of rural society, but seems to have grown as an accretion of customary usage. This situation stands in marked contrast to the explicitly legal framework of the classical European feudal system, with its complex claims of sovereignty and reciprocal obligation. The closest New World analogue to the European feudal system was the *encomienda*, a fief *manqué* that played a tremendously important role in the early history of Mexico, but one that never developed the autonomous legal status of the feudal fief, and which we now

know was not a land grant (Zavala [1935] 1973, 1940; Simpson 1966).<sup>31</sup> This statement is not meant to minimize the actual political and juridical dominance that landowners often exercised locally, which was considerable despite its informal bases. Again, the most appropriate framework for this situation is a gradient or continuum, with the variable to be charted being the relative degree of institutional attenuation in the countryside. That is to say, the greater the population density of a given region and the more numerous the competing foci of political and juridical power (Indian villages or other settlements, royal jurisdictions, ecclesiastical establishments), the lower the degree of landlord hegemony. This gradient would extend from the Indian south (Taylor 1972, 1976), through the Valley of Mexico (Gibson 1964; Tutino 1975, 1976a, 1976b), to the north central and western central regions (Brading 1978; Bazant 1975; Couturier 1976; Van Young 1981), and to the northern part of Mexico (Chevalier 1952; Charles Harris 1975).

The putative nonmarket orientation of Mexican great estates in the colonial period and the early nineteenth century has been mentioned above. Much of the research since Gibson's work on the Valley of Mexico has stressed the market orientation of great estates, and some important recent findings do deal with the response of the traditional great estates to market forces, whether contraction or expansion (Florescano 1969b; Brading 1978; Serrera 1977; Charles Harris 1975; Van Young 1981). One reason for the newer emphasis on market orientation as opposed to the older image of autarchy (Chevalier 1952) is the use of actual hacienda account books rather than descriptive materials as primary evidence. Market orientation and profitability are not necessarily synonymous, of course, but calculations of profit and loss based on production decisions in a market economy are certainly related to our understanding of the manner in which the traditional hacienda functioned within a given context. The fact that the Mexican hacienda did not always produce a profit does not necessarily mean that considerations of profitability did not enter into its management. To equate low profitability with feudalism, as we have done for so long, would be to follow the reasoning that because Chrysler Corporation has stopped generating profits, it is necessarily a capitalist enterprise no longer. To analyze accurately the functioning of the colonial and nineteenth-century rural economy, we have of necessity begun to look at production units within an economic context. To characterize the Mexican hacienda as feudal on the basis of its low market orientation at certain places and times is to confuse cause with effect.

As to the servility and permanence of estate labor forces, it is perhaps on this point that revisionist views have gone farthest in attempting to debunk the conventional wisdom about life in the countryside before 1910 (Cross 1979; Bauer 1979). To anticipate a bit the upcom-



ing discussion of rural labor, it is true that these rosier visions of work in the countryside sometimes strain our credulity. The black image of the *tienda de raya* as an exploitative institution, for example, refuses to be lightened in our minds by attempts to rehabilitate it into a kind of relatively benign country store.<sup>32</sup> Nonetheless, it seems fairly clear from recent work on this theme that rural laborers had a more complex relationship to estates and less resemblance to European serfs than had once been thought. The latter point means that the dissimilarity cut both ways; if formal enserfment was missing, so was hereditary tenure. Altogether, the complexity and mobility of the rural labor force in the traditional Mexican agrarian economy matched that of land-tenure arrangements.

We are left, then, with the fourth major characteristic that has been presumed to draw the traditional hacienda and the feudal manor together onto common ground: the patriarchal flavor of their social relations. There does appear to be a substantial similarity here, despite the enormous range of variation observed. The hierarchical and paternalistic social organization of the traditional hacienda, its function as a surrogate community, the mediating role of the *patrón vis à vis* the outside world, and the affective bonds and loyalty that often bound estate populations together all strongly summon up the image of European preindustrial communities on the land. This view is not meant to idealize the traditional hacienda as a social organization. We know from recent work that social conflict was endemic in the Mexican countryside, ranging from the ordinary pushing and pulling characteristic of any society through criminality, brigandage, and vagabondage, to small and large uprisings.<sup>33</sup> Social relations on haciendas could certainly be expected to exhibit that ambivalence and those contradictory centrifugal and centripetal tendencies that structure any society. But the patriarchal flavor is still there, leaving the conceptual problem of how to characterize a social order where many of the economic criteria for a feudal definition do not apply, but where social relations on the land partake of a feudal form. The answer to this puzzle is that what is patriarchal is not necessarily feudal. Or, if we wish to retain some vestige of the feudal model for Mexico at the risk of producing a theoretical monstrosity, we may say that the traditional hacienda was economically capitalist (or precapitalist) but socially feudal.<sup>34</sup> In any case, a number of reasons can be adduced for the patriarchal nature of hacienda social relations. First, some element of paternalism or patriarchalism would seem characteristic of large agro-social units in most rural societies, feudal or not, where a number of dependent individuals live under the dominion of a landlord. Second, the relative attenuation of institutional bonds in the traditional Mexican countryside must have encouraged the pluri-functional authoritarianism of landlords and their surrogates as points of social crystallization in

the rural environment.<sup>35</sup> Third, the patriarchal flavor of the traditional hacienda probably also grew out of the dual need to acculturate the Indian population and to exert enough social control over it to insure regular access to its labor.

We now arrive back at the question of labor as a variable in identifying the traditional hacienda as an economic type. The case can be made that labor use and relationships are the key to understanding the evolution of the classical Spanish American hacienda (Bauer 1979; Van Young 1981; Mörner 1973; Womack 1979; Villamarín 1975). In attempting to analyze the distinctions between haciendas and other types of agricultural production units in Mexico, the use of wage labor on a fairly lavish scale by haciendas and its minor importance in family farming seems crucial, although the evidence on this point is still not plentiful (Brading 1978, 150; Van Young 1981). But the origins, sequence, and nature of rural labor systems have received considerable attention in the research of the last twenty years or so. In general, that research has tended to confirm Gibson's 1964 formulation of the problem of labor sequences, that the progression of *encomienda* to *repartimiento* to wage labor was a response to the shrinkage of the labor pool that is ultimately assignable to Indian demographic collapse (Van Young 1981; Mörner 1973; Florescano 1971a; Frank 1979).

The Mexican *encomienda* assuredly had come under considerable scrutiny long before Chevalier's work initiated the modern study of agrarian history as such. The works of Zavala (1940, 1973), Miranda (1941–46), and Simpson ([1950] 1966) not only delved into the institutional structure of the sixteenth-century *encomienda*, but provided some hints as to its function within the burgeoning colonial economy. The most compelling attempts to fit the *encomienda* into some sort of coherent theoretical scheme have come from Marxist historians, who sometimes have characterized it as the linchpin of a "dual economy imposed by the conquerors" (Frank 1979) and as "tributary despotism" (Semo 1973).<sup>36</sup> The *repartimiento* system of forced wage labor, short-lived as it was, is depicted by most writers as having linked the age of the *encomienda* to that of debt peonage. Although less well studied than the *encomienda* system that overlapped it, the *repartimiento* usually receives some attention in studies of rural history as a transitional form between tributary and wage-labor systems (Simpson 1938; González Navarro 1953; Gibson 1964; González Sánchez 1966; Taylor 1972; Van Young 1981). Overlapping, in its turn, with the labor *repartimiento* was the free wage-labor system, which has been traditionally viewed as dominated by the debt-peonage arrangement of the classic Mexican hacienda. But some questions about this institution are implicit in its description. Was it free? Was it for wages? Was it a system?

The questions about debt peonage in the colonial and early na-

tional periods turn not so much upon its origins or purposes, as upon its subsequent development and the degree of regional variation in labor practices. On the whole, the early findings of Zavala (1948b), Borah (1951), and Chevalier (1952) on the beginnings of the debt-peonage system have been little disputed. According to them, the rural wage-labor system was already in place by the end of the sixteenth or the beginning of the seventeenth century (see also Verlinden 1970) and would be increasingly associated with debt servitude thereafter. In this interpretation of the origins of estate labor practices, the evolving labor system was a response both to the increasing demands for commodities and labor by the Spanish population and to the declining capacity of the Indian population to meet those demands adequately. The labor-scarcity situation that prevailed during the first half of the seventeenth century not only gave rise to the necessity of fixing the laboring population on the land, but is also said to have pushed wages up (Gibson 1964). There is something of a contradiction here, however; one that is so far unresolved and that tends to undermine the labor-scarcity theory about the origins of debt peonage. The rising trend in wages for most types of labor implies a competitive market situation where some degree of physical mobility of the sellers of labor (rural workers) must have occurred.<sup>37</sup> If this situation was indeed the case, then wages, whether money or perquisites, would have served the same function as the institutional constraints on mobility associated with debt peonage—the attraction and holding of a labor force. If debt peonage was increasing in importance at this time, then wages should not have risen. Thus, the two trends were not redundant, but contradictory. The possibility does exist that debt itself was an index of the success of rural laborers in driving up their wages—that increasing debt was a sign of bargaining power, a scenario that has been suggested by several researchers (Gibson 1964; Brading 1978). The converse would be that when the bargaining power of the laborer declined, either due to an oversupply of labor or a weakening in the market position of large estates, the level of debt would tend to drop. This result seems to be what did occur in some areas during the eighteenth century, as the push of demographic pressure, especially in Indian landholding villages, replaced the pull of easy credit in recruiting estate laborers (Van Young 1981).

The supposed harshness and pervasiveness of debt peonage has come under a great deal of scrutiny in recent research. From this heightened interest and from the use of new sources a number of qualifications about “classical” peonage have developed. First, as I have suggested above, the status of resident laborer on an hacienda is now seen as having offered the Indian or mestizo peasant a measure of material and social security at certain times and places (Gibson 1964; Bauer 1979; Brading 1978), although the conditions of material insecurity may have

been partially created by the drive of large landowners to undercut the subsistence base of an independent peasantry by acquiring ever greater amounts of land (Wolf 1959; Lockhart 1969; Van Young 1981). Second, it has been suggested that elsewhere in modern Latin America, the peasant village could exist in a symbiotic relationship with landed estates by using money earned in the rural wage-labor economy to underwrite its traditional communitarian mode of existence (Favre 1977). Whether such a situation ever obtained in Mexico has never really been addressed in the recent investigations, but the possibility should be considered. Third, it has been established that debt peonage was not the only, or necessarily the most important, labor arrangement prevailing in the countryside, and that it existed in a dynamic relationship with other forms, including temporary wage labor, service tenantry, renting, and sharecropping (Katz 1980; Brading 1978; Bazant 1973, 1975; Cross 1979; Konrad 1980). During the late colonial period, temporary or seasonal wage labor seems to have been increasing in importance in many areas under the pressure of demographic growth in the countryside. Furthermore, the same studies that have illuminated the complexity of labor and tenure arrangements have indicated that being a debt peon was not necessarily less desirable than being a precarious renter or sharecropper. Finally, a good deal of doubt has arisen about the entire *raison d'être* of the system—the putative equation between debt and immobility. Some researchers have found exceedingly harsh conditions in certain areas of the country, primarily the north (Charles Harris 1975), where physical coercion of laborers, high debt levels, the enforcement of the sanctity of debt, and limitation of physical mobility of laborers all went together. In late colonial Oaxaca, debt levels were high and physical coercion on haciendas frequent (Taylor 1972), but the evidence as to limitation of mobility is equivocal. In areas of central Mexico during the late colonial and early national periods, much evidence indicates that although laborers may have been physically abused with some frequency, per capita debt levels were not particularly high, that laborers regularly absconded without liquidating their debts, and that mobility was not limited to any great degree (Brading 1978; Gibson 1964; Van Young 1981). Taken as a whole, the evidence on debt peonage suggests that where labor was in short supply, either because of a manpower scarcity or a strong peasant subsistence sector, peonage could be relatively harsh (e.g., Bauer 1979). Where manpower was plentiful, the logistics of maintaining a permanent estate labor force might conduce to some degree of debt peonage, but on the whole, the institution was likely to be less pervasive and less harsh.

Having examined very broadly the research on a number of different aspects of rural social and economic structure in Mexico and having pointed out the polymorphism of the Mexican hacienda, I return to

the question of definitions. It turns out that the hacienda is hard to describe, but you know it when you see it. The emphasis in the newer work on rural history, one very much present in the work of Chevalier, is on the dynamic aspects of rural society and economy, rather than on static typologies. Furthermore, as suggested above, the hacienda begins to look less and less like a cause and more and more like an effect. Metaphorically speaking, the Mexican great estate may be seen as a kind of pivot upon which larger historical processes and relations between groups turned. Depending upon how one is inclined to conceptualize large historical trends (and disclaiming for the moment as fortuitous any resemblance to Straussian structuralism), several pairs of oppositions may be identified: those between man and the environment, between cities and countrysides, between elites and masses, and between Indians and whites. How much do we know about these dichotomies in relation to the rural history of Mexico in general, and the evolution of great landed estates in particular?

Insofar as man in opposition to the natural environment is concerned, we do not seem to know much at this point and what we do know has not been well integrated theoretically. The natural environment in work on rural history is generally taken as a given, passive but largely intractable. Works that are keenly analytical of other aspects of the rural environment virtually ignore the role of natural conditions in forming social and economic institutions, at best nodding in their direction with an introductory chapter on "Man and Land," "The Natural Setting," or something similar (Taylor 1972; Van Young 1981). Some exceptions are found in colonial and nineteenth-century agrarian history, but such efforts have grown not out of the work of rural historians per se, but from that of historical demographers and bio-historians. The work of the "Berkeley school" particularly stands out for hinting at a broader vision of the ebb and flow of man-environment relations over long periods of time and emphasizing Malthusian crises and resource depletion (Simpson 1952; Cook 1949; Cook and Borah 1974–80; Crosby 1972). The peculiar constraining conditions of the natural environment in Mexico and their influence on the development of "hydraulic" society there have been examined by anthropologists and archaeologists (Denevan 1976; Palerm and Wolf 1972; Sanders and Price 1968; Wittfogel 1972, 1981). The historical geographers, who might have explored this issue, have mostly been interested in the spatial organization of cities and regions or with the workings of agriculture itself (David Robinson 1979; Bataillon 1971; Barrett 1970). To date, no real attempt has been made to apply any theoretical framework to agrarian structure in the colonial or early national periods.<sup>38</sup> In our studies of the traditional hacienda, we have been so preoccupied with patterns of land tenure, labor, and exchange that we have largely ignored technological matters.

We have also failed to explore the idea that the hacienda system itself was a technique that may have reflected a reduced capability on man's part to exploit environmental resources and certainly represented a different vision of man's place in nature than did pre-Spanish agrarian arrangements.

The study of regions in Mexico—specifically of cities in relation to their hinterlands—as an aspect of rural history has advanced a good deal further. Setting aside the fact that Mexico's complex topography occasioned in every village, hamlet, and hacienda (at least in preindustrial times) a tendency toward a *sui generis* character, the studies of regional and agrarian historians have delimited a number of identifiable regions in the country that experienced divergent paths of economic and social development. In the simplest version, this regional scheme would include a prenorthern fringe (it is no longer fashionable to call this area the Spanish Borderlands), a far north, a near north, a core area (including the Valley of Mexico), a dry west, a humid west, a near Indian south, and a far Indian south (Altman and Lockhart 1976). Many of these regions and their subregions centered on cities, or river valley systems, or mining areas (Taylor 1972, 1976; Bakewell 1971; Super 1973; Tutino 1976b; Gibson 1964; Brading 1978; Van Young 1981). Despite the fact that a number of regions within Mexico have been identified on a descriptive or phenomenological basis (as showing a more or less identifiable character within themselves), we still have not gone far in analyzing the internal structure of regions, with some exceptions (Florescano 1969a; Van Young 1979b, 1981; Brading 1978). Some work has been done on defining the relationship between Mexico City and the provinces (Kicza 1983; Brading 1971), and on the economic geography of colonial Mexico (Moreno Toscano 1965, 1968; Moreno Toscano and Florescano 1974). On the whole, however, we still know little about urban hierarchies within regions or about internal linkages, market networks, or characteristic regional agrarian structures. What we do know tends to undermine the view of dependency theorists that Mexico's only motor of development was its silver production, which has been characterized as an enormous funnel through which wealth was siphoned off to the Old World. The study of regions and world economies suggests, if not complete autarchy, at least a more complicated system of linkages among the regions of Mexico and a heretofore unsuspected economic vitality in which the flexibility of local agrarian structures played a substantial role.

The social dimensions of landownership—the significance of control over land for the distribution of prestige, wealth, and power in Mexican society—have been much discussed, both in case studies and in the literature dealing with elite composition in the colony and young nation (Brading 1971; Ladd 1976; Chevalier 1952; Serrera 1977; Lindley 1982). Most research suggests that relative deprivation and slippage

accounted for economic change and social cleavage in pre-Porfirian Mexico, not the effects of absolute deprivation. As the notion of the seigneurial mentality as the major motive for large-scale landownership has given way to a more realistic, nuanced view of dynastic considerations tempered by economic concerns, residual questions have been left behind about the social implications of concentrated land-tenure arrangements. What was the nature of patriarchal landlord rule in the Mexican countryside, and how did it compare with that in other parts of Latin America?<sup>39</sup> It is often assumed that power over land normally transmuted itself into political power at the local and even the national level, yet the concrete evidence for this kind of reflexive landlord political dominance is remarkably thin. Recent research does seem to indicate that, at least at the local level, landlords exercised a great deal of power, even in pre-Porfirian, pre-cacique Mexico (Charles Harris 1975; Tutino 1975, 1976a; Van Young 1981).<sup>40</sup> But to say that landlords and their kin sat on local *cabildos*, served as royal magistrates, or sent their children into the Church with some regularity is to fail to examine sufficiently the intricacies of landlord influence in the countryside. Much evidence indicates that the landowning group, if it can be called a group at all, was certainly not homogeneous socially or politically, and that at times it was riven with internal rivalries and schisms (Van Young 1981). Some of the blank spots in our picture of the social meaning of landownership may never be filled in because the behaviors that might illuminate these fascinating dark corners elude the historian's grasp.

The relations between Indians and whites, one of the key issues in understanding the historical evolution of Mexican society, has surfaced repeatedly in the recent work on colonial and early national history, which is hardly surprising because the newer work inherited the preoccupation of the older. Nor is the consideration of this dichotomy a repetition of the one just discussed, that of elites and masses. Indeed, the degree of congruence between race and class in colonial Mexico has been the subject of a good deal of controversy (Borah 1954; Chance 1978; Chance and Taylor 1979; McCaa, Schwartz, and Grubessich 1979). Whatever view one takes of the matter, however, it is clear that changes in access to land and its use impinged in major ways upon the acculturation process that has been going on in Mexico since 1518. As indicated by recent research, the major proposition here seems to be the continued seizure of Indian resources by non-Indians, and two corollary propositions: the increase of demographic pressure in the countryside until at least the close of the colonial period, and the increasing internal differentiation within Indian society. Regarding the seizure of Indian land resources, the main questions concern the rhythm of this process up to the Liberal era of the mid-nineteenth century and its effects in undermining the integrity of the traditional landholding Indian community.

Some authors have suggested that the pace of legal and illegal acquisition of Indian farming lands continued unabated during the late colonial period (Florescano 1971a, 1971b), while others have pointed to the period of rapid Indian depopulation (up to about 1650) as the time when haciendas achieved their major expansion (Gibson 1964). Not all regions experienced the same degree of land concentration, of course, as Taylor's 1972 work on Oaxaca has notably shown.

On the other hand, the general agricultural and economic expansion of the eighteenth century, which saw the recapitalization of haciendas in many areas and the development of previously unused lands, applied new pressures to the structure of village and peasant landholding, even where comparatively little land actually changed hands (Brading 1978; Van Young 1981). On the whole, what the *encomienda* and tribute systems effected in the sixteenth century, and the massive occupation of Indian lands added in the seventeenth, the economic and demographic expansion of the colony achieved in the eighteenth—the cumulative expropriation of Indian peasant resources and the creation of a rural proletariat (Van Young 1981; Konrad 1980). The hacienda, with its complicated structure of resident and temporary labor and its service tenantry, sharecroppers, renters, and slaves, therefore became the major locus of both class and ethnic conflict during the colonial period because making a laborer out of a peasant constituted an attack on his cultural identity as well as economic exploitation. Indian society became increasingly differentiated during the colonial period (Gibson 1964; Taylor 1970, 1972, 1979; Carrasco 1961, 1972; Wolf 1959), so that members of village elites were often co-opted into Spanish society, and landholding within the traditional system tended to undergo concentration as well. The resilience of Indian communities and their doggedness in maintaining their identities in the face of such pressures are truly impressive and merit further study (Miranda 1966; Taylor 1979; Van Young 1981, forthcoming).

#### METHODS AND SOURCES

Historians of rural Mexico have mostly been well served by their eclecticism—their willingness to borrow approaches, methods, and concepts from other historical and social science fields. To the degree that history as a whole is an invertebrate discipline, with a relatively limited range of theoretical concepts where the analysis of social change is concerned, such borrowings are necessary. Researchers investigating the rural history of Mexico have emulated models drawn from other historiographical traditions and have embraced anthropological theory for its explanatory power and quantification for its technical power. The most influential models in rural history are apparently European ones that are



predominantly French. The French tradition of rural history, starting at least with Marc Bloch's work (English edition 1966), has lent a number of characteristic concerns to the study of Mexican agrarian history. Among these are the influence of demographic trends (a concern that also emerged from recent Mexican historiography itself), the geo-history of regions, the concrete relations between haves and have-nots in rural society, and the relationship of social and economic change in the countryside to larger changes going on within colonial society. It must be admitted that U.S. historians of rural Mexico for the most part have kept away from doing work on the grand French scale (Gibson's *Aztecs* still comes the closest here), perhaps partly because of the structure of French academic careers and partly because of divergent methodological orientations.

The attempt in the last fifteen years to put rural history on a quantitative basis has been very important. The term "quantitative" is used advisedly because the manipulation of numbers generally appearing in works on agrarian history is mostly prestatistical and technically very simple. In practice, the most complex operations likely to appear in such investigations are graphs, the use of log scales, moving averages, and age pyramids (Florescano 1969b; Barrett 1970; Gibson 1964; Konrad 1980). Also, enormous numbers of tables have become de rigueur in almost any self-respecting work on rural history. In statistical terms, these techniques are the equivalent of counting on one's fingers and toes. Still, the use of quantitative data (notably absent in the 1952 work of Chevalier) has lent a degree of credibility to the historians' use of descriptive terms such as *little*, *much*, *large*, *rapid*, *frequent*, and the like. Of course, a number of widely acknowledged problems are involved with the counting of historical data, including the reliability of the data, the reliability of our understanding of the data, the homogeneity of the data, the adequacy of time series, and so on.<sup>41</sup> All of these problems are exacerbated by the nature of the data available to the rural historian, data that because of the decentralized character of rural society and agricultural production units, are likely to be fragmentary, heterogeneous, and of questionable reliability.<sup>42</sup> Up to this point, computers have not been widely used in manipulating such data, mostly because of the relative thinness of the information available, and econometric techniques have not been employed for the same reason.<sup>43</sup> Ultimately, of course, any kind of quantification, whether primitive or highly technical, will depend for its significance on the judgment of the researcher (the field of quantitative history is littered with the bodies of too many self-victimized practitioners to deny this simple truth). Despite these problems and caveats, the overall effect of quantification in the study of Mexican rural history has been to raise the level of investigation and to reinforce enormously the validity of generalizations about agrarian soci-

ety. This trend is particularly true because questions about the Mexican hacienda have involved primarily its economic nature, and quantitative techniques have been indispensable in answering these questions. On the whole, I hope that there will be more counting in future research, not less.

A second important methodological development has been the introduction into studies on rural history of theoretical applications from anthropology. American anthropologists, who are somewhat less cautious about delving into historical questions than historians have been in adopting anthropological frames of analysis, have produced some of the most interesting and provocative studies of Mexican local history and syntheses of general Mexican culture history (Oscar Lewis 1963; Marvin Harris 1964; Wolf 1959). Still, the enormous theoretical and ethnographic literature on peasants has had a considerable impact on the study of historical agrarian structures. In some cases, this influence has been implicit and relatively diffuse (Gibson 1964; Lockhart 1969, 1975), and in some cases explicit (Brading 1978; Taylor 1972, 1979; Tutino 1976b; Van Young 1981, forthcoming). Particularly important have been ideas about the corporate nature of Indian village society (Wolf 1957, 1959, 1971), the forces of conflict and cohesion within it, and its relationship to the outside world of state, Church, and non-Indian landowners. But anthropological concepts have also pointed up the importance of kinship and the pervasiveness of familism as major social characteristics at the top of colonial and early national society in Mexico (Brading 1971; Lindley 1982), and such concepts have been applied to the study of haciendas as social communities. On the latter theme, the recent prize-winning work of anthropologist Herman Konrad on the famous Jesuit hacienda of Santa Lucía has given us a virtual ethnography of a colonial estate, one admittedly based on the unusually rich documentation left by the Jesuit order (1980). Konrad's anatomy of hacienda society reveals a complex system of function, status, and ethnicity heretofore largely unexamined by researchers, and one that was probably characteristic of larger rural estates in Mexico and elsewhere.

The limited application of anthropological concepts in the study of Mexican rural history so far probably has as much to do with the training and conservatism of historians as with any inappropriateness of theory for data. This caution is not inapposite because historians are understandably reluctant to use theories generated from the study of contemporary (in this case peasant) societies in studying historical ones for fear of introducing anachronistic assumptions into their analysis. But some theoretical guides to the perplexed are essential in studying rural societies such as Mexico, which appear on the surface to be made up of an enormous number of discrete units, yet also seem to change under the impact of larger and sometimes unseen forces.

As the study of Mexican rural history has advanced in the last fifteen years or so, three basic approaches to research have evolved, all of which employ virtually any technical apparatus or analytical framework. The three approaches may be called entrepreneurial, sectoral, and regional.<sup>44</sup> Although these three different types of rural history are methodologically distinct in theory because they ask different kinds of questions, they are often not so distinct in practice. Entrepreneurial history in the agrarian context follows closely in the style of business history, in which the central focus is often a single enterprise or corporation. In the case of entrepreneurial history, the unit studied is usually a single rural estate (Badura 1970; Barrett 1970; Couturier 1976; Semo 1977; Konrad 1980), or a group of estates administered mainly as a unit (Blood 1972; Charles Harris 1975), or the agricultural enterprises of one individual or an entire family (Altman 1972; García Martínez 1969; Romero de Terreros 1943). More important than the flexible distinction between these subtypes is the common feature upon which they rely for the cogency of their approach: continuity in estate management or in the identity of the production unit, or both in combination.

In order to do this kind of study, the investigator must have at hand a relatively substantial and continuous documentation, although one suspects that in practice the approach is more often dictated by the availability of the data than vice versa. The likelihood of such bodies of documentation having survived depends heavily on factors that may make the entity being studied unrepresentative of rural production units as a whole. In principle, this probability means that when looking at traditional rural estates, the property must either have been so large and wealthy or owned by a family of such resources that it was not divided or sold for long periods; that it was entailed or otherwise kept off the market; or that it belonged to a privileged and continuing corporation (usually an ecclesiastical corporation). As recently as Chevalier's work (1952), these ownership characteristics of the Mexican hacienda were not considered exceptional, but more recent research (Taylor 1972; Brading 1978; Van Young 1981) has shown that a high degree of ownership instability was more the rule than the exception. It usually follows that estates that continued in the same hands for long periods of time were anomalous, not typical of the countryside as a whole, and that the research sample therefore will have been preselected so as to skew the results of the investigation. Because of this built-in bias factor, one may be permitted to doubt the typicality of the Cortés sugar estate in Morelos (Barrett 1970), the Jesuit hacienda of Santa Lucía (Konrad 1980), the Sánchez Navarro latifundium in northern Mexico (Charles Harris 1975), or even the five *potosino* estates studied by Bazant (1975).<sup>45</sup>

Notwithstanding these problems, entrepreneurial studies have provided important insights into the economic and social life of the

Mexican countryside and will undoubtedly continue to do so as new caches of documentation are found. First, even though survival may have indicated an atypical success, agricultural production units in similar environments faced similar technical problems, and the responses to those problems are likely to have been of the same nature. Thus, for example, we learned a good deal about northern ranching techniques from Charles Harris's 1975 study and about colonial and nineteenth-century sugar-making technology from Barrett's 1970 work on the Cortés sugar estate at Atlacomulco. Second, particularly where economic or business data are concerned (wages, prices, production levels, profits, and the like), entrepreneurial studies offer at least some corrective to the fragmentation of data available in most cases. The magisterial study by Barrett is an example of what can be accomplished if the surviving documentation is good and the researcher imaginative. Such completeness is not always possible, however, because massiveness of documentation cannot always be equated with quantitative value, especially where the surviving material is long on administrative correspondence and short on accounts (Charles Harris 1975). Third, entrepreneurial studies can show us the detailed inner workings, both economic and social, of the agro-social units known as haciendas: how production decisions were made, how the haciendas were managed, how laborers were recruited and paid, what social relationships were like in the countryside, how estates fitted into their owners' other activities, and what was the social dimension of landownership.

Sectoral history, although heterogeneous because of the nature of its subject matter, is not quite the catchall category it sounds. Its common thread is that it isolates one aspect of relationships in rural society, typically an economic one, and examines it closely in terms of its history and connections with other aspects. Sectoral history is preeminently analytical in approach, but generally does not attempt to provide an integrated whole system when the analysis is completed, as do the other two approaches to rural history. Rural labor in the countryside of traditional Mexico had been much studied in this fashion, beginning with the institutional and later economic studies of the *encomienda* (Zavala 1973; Simpson 1966; Miranda 1941–46), working up through the *repartimiento* labor system (Simpson 1938; González Navarro 1953), and culminating with a widespread interest in debt peonage (Zavala 1948b; Verlinden 1970; González Sánchez 1966; Katz 1980; Bazant 1975; Cross 1979). Land tenure has also been studied in this way (González Sánchez 1969; Osborn 1973; Coatsworth 1974). In much the same way, investigations of one type of agricultural or livestock-production activity may be included within this category (Serrera 1977; Sandoval 1951; Barrett 1979a), as well as price history, or the structure of rural credit (Linda Robinson 1979, 1980). Marketing activities of rural estates and other production units

also fall under this rubric, and recent research in this area has brought to light much concerning the importance, mechanisms, and structure of local markets, in particular (Barrett 1974; Florescano 1969b; Van Young 1979b; Kicza 1982). Institutional studies, although often limited to formal structures, legal regulations, and policy formulation, can nonetheless illuminate aspects of rural economic life (Serrera 1974b; Hernández Palomo 1974). This type of endeavor has the vices of its virtues. It often provides a detailed look at an aspect of rural life that might be given short shrift in a study of broader scope, and it may allow a less cluttered and more direct comparison with the same single variable in other contexts. On the other hand, if one disaggregates the production process, for example, and looks at only one variable, analytical problems are likely to develop. The mutual influence among production factors will be lost to the observer, and a certain reductionist bias may creep into the argument, producing an analytical situation where the tail wags the dog.<sup>46</sup>

Regional history is at once difficult and rewarding to produce. Perhaps the difficulties inherent in this approach, such as complicated problems of definition and conceptualization, the manipulation of large bodies of heterogeneous data, and the typically long timespans involved, have discouraged those interested in agrarian history from often attempting it. In any event, works falling under the other two rubrics have demonstrably dominated the field. Chevalier's study, although it dealt with New Spain as a whole, was preeminently regional in emphasis, with its heavy concentration on the northern areas. Gibson's 1964 study of the Valley of Mexico, although not explicitly agrarian in focus, nonetheless relates the history of a region. Several recent works on Mexican agrarian history also make the region the theater of their analysis (Taylor 1972; Tutino 1975; Brading 1978; Van Young 1981; Hunt 1974; Altman and Lockhart 1976; Morin 1974; Serrera 1977; Prem 1978; Medina Rubio 1974; Leslie Lewis 1977; Hurtado López 1974; Hamnett 1971b; Florescano 1965b).

The advantages to this approach to agrarian history are several. First, it allows one to look at fairly large segments of rural society as systems, which means that the *sui generis* quality of individual production units or rural settlements may be controlled to some degree when viewed as interacting with other entities of a similar order in the regional context. In this manner, for example, the relative advantages and disadvantages of small- versus large-scale producers in relation to a regional market or markets may become clearer (Brading 1978; Van Young 1979, 1981). Similarly, the flow of manpower or land resources between the peasant subsistence sector and commercial agriculture is best observed from the perspective of a relatively large area, as opposed to a single employer. Second, regional agrarian history allows one to examine the

complex reciprocal relationships between cities and their hinterlands. This advantage is particularly important in that it illuminates two key issues in the history of Mexico: the growth of cities and the nature of great landed estates. In the latter case, regional agrarian studies are increasingly demonstrating that haciendas as production units had another alternative to supplying mining areas in boom times or sinking into a torpor during slow times: they could send their products to local urban markets. The study of the structure of these regional markets and their influence in mediating the transfer of resources from the countryside to the city is one of the most interesting, and so far least researched, aspects of Mexican rural history. Third, and related to the second point, is the fact that regional history in Mexico is inherently important. The pervasiveness of regionalism in Mexico—the stubbornness and distinctiveness of regional identities and loyalties and their frequent projection onto the national scene in the form of political conflict—is one of the most important themes in Mexican history. The *patria chica* has often received the attention of Mexican scholars writing about their own country. This concern has produced a rich, if uneven, body of literature on local history, some of it inevitably touching upon agrarian themes (Amaya 1951; Lancaster-Jones 1974).<sup>47</sup>

An important conceptual problem has emerged along with the growth of interest in regional agrarian history—the very basic one of defining what a region is. In practice, those writing regional history rarely attempt such definitions, but historians do occasionally grapple with this issue and produce some interesting results. In the volume edited in 1976 by Altman and Lockhart entitled *Provinces of Early Mexico*, the reader was asked not to conceive of regions as abstract entities, but to operationalize the concept by envisioning Mexican regions as highly individualized mixes of a limited number of variables. Geographers, more used to dealing with the concept of the region in a technical sense and more accustomed than historians to admitting that history occurs in space as well as in time, are also more likely to treat the concept in some kind of coherent a priori fashion (Linda Robinson 1979). Yet how elaborate or rigid a definition do we need to sharpen our ability to frame questions about geo-historical regions? Something as simple as defining a region as a geographical space with a discernible internal structure would probably do very nicely. If no a priori definitions of region are used, regional rural history tends to become the story of what goes on in a given area, rather than the story of a region as an identifiable entity.

Along with innovative methods and an increasingly clear delineation among the major approaches to Mexican rural history has come the use of an ever-larger variety of primary historical sources. The general trend here has been away from documents of a formal institutional nature, which were generated at a high level and offer concentrated kinds

of information, in favor of the informal, the mundane, and the dispersed. Government and other official reports, laws and edicts, travelers' accounts, and the published works of older historians and chroniclers have given way in more recent research to account books, notary records, land suits, and the like. Much of this innovation can be traced to the work of Chevalier and Gibson, and recent investigators have taken the eclecticism even further. The obvious advantage of using documents generated on the local level in the course of everyday life, and one much prized by social historians, is that the observer of traditional rural Mexico can begin to observe the actual working of social and economic life in the countryside as opposed to dealing with what contemporary observers or government officials thought that life was like. The disadvantage of utilizing such sources lies precisely in their diffuseness and in the problems of gathering, manipulating, and making sense of large masses of possibly heterogeneous data.

Estate account books and all kinds of administrative and business correspondence have proved to be of enormous value in beginning to solve the puzzle of the traditional hacienda in Mexico. This kind of documentation has been used to advantage by a number of researchers (Semo and his students, 1977; Bazant 1975; Cross 1979; Brading 1978; Barrett 1970; Konrad 1980; Tutino 1976a; Charles Harris 1975). Of this type of material, account books are certainly the most valuable because they allow at least partial reconstruction of the internal workings of rural estates. Where other, less structured documents are also used, accounts can serve as a sort of central armature around which to build a picture of the hacienda as a rural production unit. Accounts are likely to provide data on production decisions and amounts, levels of profit and loss, costs, wages, and market prices. The longer the run of the accounts in terms of years, of course, and the greater the degree of their homogeneity, the more useful they are likely to be, although even fragments can prove valuable (Charles Harris 1975; Van Young 1981). As important as they are, hacienda accounts may nevertheless have some tendency to introduce biases into the historical reconstruction of rural economy.

Since the late 1960s, notary records have come to the fore as one of the most valuable sources of information for the social and economic history of rural Mexico.<sup>48</sup> Such documentation is particularly valuable for writing the history of Mexico's rural estates that, whatever else they may have been, were preeminently a form of property. Wills, inventories, and instruments of rental and sale can provide a synchronic view complementary to the diachronic one given by accounts. Records of dowries, mortgages, ecclesiastical encumbrances, and other forms of liens may enable us to follow the history of a family's fortunes, the fate of a single estate, or even the evolution of an entire region over fairly

long periods of time. Notary records have been used with great elegance by Brading (1978) in his study of the Bajío region, and in studies of elite family enterprises by Lindley (1982) and Kicza (1983). Although notary records have proven to be an enormously fruitful source of information, they pose two major problems in their use. First, if either of the two approaches other than entrepreneurial history is being used, one must find and manipulate a very large number of such records to achieve any significant results because notary documents are likely to convey a fairly low density of information. Second, the social holes in the notarial net are large, which is to say that the majority of the rural population will not show up in these records because the scale of their economic transactions were too small to warrant the costs and trouble of going to an official notary.

A third category of documentation that recently has been much used in researching rural history is judicial records of all kinds. Among these would be civil litigation over business arrangements and debts (Van Young 1981), as well as criminal records dealing with patterns of rural crime, violence, and protest. The latter have been used recently with great effect by Taylor (1979) in his study of rural drinking, homicide, and rebellion in Oaxaca and central Mexico during the colonial period. This documentation can give us a picture of social (as opposed to strictly economic) behaviors, of the elastic web of everyday life in the countryside, and of the strains engendered by class relationships and economic change. As for agrarian structure, litigation over land has proved to be an immensely valuable source of information on a whole range of topics including the physical shape and extent of landholdings, the use of land, and the competition among various rural production sectors for economic resources (Gibson 1964; Taylor 1972; Brading 1978; Van Young 1981).<sup>49</sup>

Finally, statistical materials produced by governmental and ecclesiastical agencies have helped provide a quantitative base for the discussion of Mexican rural society and economy. Fiscal records of various kinds, which were kept by municipal and other levels of government to tax and regulate economic life, can provide not only an idea of what men thought the sphere of government action should be but also a picture of the ebb and flow of production, consumption, and prices in the rural sector. For example, records kept by city governments relating to the grain and meat supply have been used by several researchers (Florescano 1969b; Van Young 1981; Barrett 1974).<sup>50</sup> Serrera has used fiscal records relating to the long-distance livestock trade to reconstruct the livestock industry of a large part of western central Mexico (1974a, 1977). Ecclesiastical tithe records, where they can be found in unbroken series and sufficient quantities, have provided vital information on the



movement of production and prices in the countryside over long periods (Borah 1941; Hamnett 1971a; Pastor et al. 1979; Brading 1978; Brading and Wu 1973). Population records generated both by secular and ecclesiastical agencies have revealed much about demographic movement in the Mexican countryside and its relation to economic change, not only during normal times but during periods of disruption and crisis as well (Cook and Borah 1974–80; Brading and Wu 1973; Hamnett 1971a; Florescano 1969b). Valuable as they are, these statistical sources have not yet been widely used, and much remains to be done with them. Other kinds of useful records also remain unused by searchers on Mexican rural history. For example, *alcabala* (royal sales tax) records might allow one to reconstruct the volume and direction of commercial traffic in rural districts, to say something about the propensity of rural people to consume manufactured goods, and to form a clearer picture of rural living standards as reflected in consumption patterns.

But what of people living outside haciendas in the traditional Mexican countryside? The types of documentation mentioned have been most effectively used so far in reconstructing rural economic life, particularly the internal organization and function of haciendas within it. Documentary biases and theoretical concerns have reinforced each other in this respect. But the same kinds of sources can be utilized to look at the lives of small property-holders (*rancheros*) and rural middlemen (muleteers, hacienda administrators, merchants, minor officials, and so on), as well as the internal structure of Indian and non-Indian villages (Van Young 1978, forthcoming; Taylor 1979). To cite but one example, litigation over land can reveal much about not only titles to property but also about the way in which land was used, ideas about its legitimate usage, the social distribution of wealth in the countryside, and even the social relations among individuals. To take another example, testaments of small farmers and ranchers, rural middlemen, or individual Indian peasants, although likely to occur less frequently than those of urban-dwellers or wealthy *hacendados*, can reveal the same kinds of detailed information about wealth, social standing, and family. The problem in reconstructing the lives of ordinary rural people is not so much in the nature of the information as in its relative scarcity when compared to that available on wealthy landowning families, individual haciendas, or corporate entities such as peasant villages. The metaphor used above of the holes in the notarial net would apply equally well to most kinds of documentation that might be used. Such a bias in the data must inevitably lend, as I have noted elsewhere (Van Young 1981), a certain static, snapshot quality to descriptions and analyses of rural socioeconomic groups below the level of the hacienda or pueblo. This problem of historical evanescence is certainly not unique to colonial Mexico, but I

would suggest that it places definite limits for even the most imaginative researcher on what we can hope to know about the lives of rural people.<sup>51</sup>

#### SOME SUGGESTIONS FOR FUTURE RESEARCH

As previously noted, the literature on Mexican rural history has developed very unevenly in its temporal coverage, with an increasingly large bulge appearing on the eighteenth century. This situation should be remedied if we want to achieve a truly balanced picture of Mexican rural development. The seventeenth century is still for the most part terra incognita, despite the stimulating pioneering efforts of François Chevalier and Woodrow Borah (see also Israel 1979). If, as we are beginning to suspect, the "century of depression" was not so depressed after all, despite the overwhelming evidence of continued native population decline, what was happening in the Mexican countryside and between city and country during this period? Was it an era of strong autochthonous regional growth, or was New Spain suspended in a century-long torpor until the dynamic eighteenth century came along? In terms of agrarian structure, with partial exceptions, the seventeenth century has not yet found its historian. At the other end of the period, the growing literature on the late colonial era seems to point strongly to the role of agrarian structure in establishing the preconditions for the outbreak of the independence movement in 1810. In researching this question, David Brading (1973b, 1978) has done much to illuminate the period, but his work has tended to jump rather than bridge the gap. A gap there was, even if one regards the Hidalgo revolt and subsequent wars of independence as epiphenomena floating above larger underlying processes of change. Still, the fact that the basic agrarian structure of Mexico may have emerged after the wars of independence largely the same as before does not deny that agrarian conditions were of great potential importance in producing a period of political, economic, and social disruption. The linkages here are not at all clear, and they are to be uncovered by looking at completely different sources than rural historians have been wont to consult. As far as the national period is concerned, we have yet to take a close look at the putative conditions of chaos and retrogression that are said to have prevailed in large parts of the Mexican countryside during the formative decades of the young republic (Rodríguez 1980), although a number of excellent studies of rural structures have overlapped this period (Brading 1978; Semo 1977; Bazant 1975; Tutino 1975).

A number of other important topics remain to be researched, but they can only be mentioned here in a cursory way. On the economic side, we still generally lack any kind of systematic price history for the

colonial period and pre-Porfirian Mexico. A notable exception is the pioneering work of Enrique Florescano (1969b) on agricultural cycles and maize prices during the eighteenth century, which relies upon Mexico City records and may therefore be unrepresentative of New Spain as a whole. It is also true that data on prices can be gleaned from a number of specialized works (Barrett 1970; Borah and Cook 1958; Brading and Wu 1973), but so far we have nothing resembling the careful, massive history of prices compiled by European economic historians for the early modern period. Until we do, we will be unable to construct an overall secular trend in Mexican economic history in general, or in rural economy in particular. Well-developed price history will also be necessary for any systematic attempt to assess the changes, if any, in rural living standards before the middle of the nineteenth century, although some notable efforts have been made in this direction on the basis of partial data (Gibson 1964; Cross 1979; Cook and Borah 1974–80). The question of price history and rural living standards also plays into the theme of what may be called the “peonage puzzle,” which has been addressed by a large number of historians from Borah (1951) to the more recent work of Katz (1980), Bauer (1979), Florescano (1980b), and González Sánchez (1980), among others. How did the institution of debt peonage change over the course of the eighteenth and nineteenth centuries, evolving from an apparently symbiotic, if not entirely benign, labor arrangement during the colonial period to the *bête noire* of the Porfiriato?

On the noneconomic side, we still lack studies that emphasize the social history of agrarian structures. What were the social and political props of landowner hegemony in the countryside, for example, and how did the traditional patriarchal structure work itself out on a day-to-day basis?<sup>52</sup> Exactly what were the much-mentioned mechanisms of social control in the countryside? In the absence of effectively organized police power, how were the gulfs between white and Indian, rich and poor bridged? The opposite side of this coin would be the social history of deviance, crime, and rebellion—the chronic and critical signs of strain and protest—about which William Taylor (1979) and a few others have begun to do such interesting work. We also know relatively little of other production units or agro-social communities in rural Mexico aside from large estates, but we do know that the colonial countryside was a great deal more complex socially than we had previously thought. It should be possible, for example, to do studies of individual Indian villages and towns on the basis of eclectic sources—studies that would parallel in depth of detail those that have been done by modern ethnographers (e.g., Osborn 1970). At the same time, our historical picture of rural districts will never be complete until we know more of the rural middlemen—the priests, provincial merchants, lower royal officials,

muleteers, hacienda functionaries, and rancheros—who mediated many of the links between landowners and landless laborers, whites and Indians, producers and consumers.

Much has been accomplished on the theme of Mexican rural history, and particularly on the traditional hacienda, during the last thirty years, but much remains to be done. The major issue is how those doing work in the field will invest their energies. We must ask ourselves how much value can be found in further studies of single rural estates, or of the structure of elite family enterprise, or even of major geo-historical regions? In a sense, this problem is one of epistemology and the value of generalization in the social sciences. But even putting such abstruse matters to one side, we do seem to be at the point where we may be learning more and more about less and less. There has been some tendency for us to fragment what we know, partly because much work in the field has adopted an empiricist, antitheoretical bias. The lesson we have learned from this practice is an important one—that traditional Mexico was enormously diverse and that its evolution was a complicated process, full of reversals and side eddies. But the time has come for us to begin to reintegrate the agrarian history of Mexico on the basis of what we know at this point and to start asking questions about what we do not know, rather than repeating ad infinitum the exercises at which we have already become skillful.

## NOTES

1. Areas of study and major works that bear in an important way on rural economic and social structure (such as urban history, demographic history, and ethnohistory) are touched upon in passing, but do not enter into the central focus of this article. Ten years ago, Magnus Mörner's 1973 major article in the *Hispanic American Historical Review* discussed the literature on the Spanish American hacienda produced until then, with emphasis on the colonial period. Much of what Mörner had to say then about work on Mexican rural history in particular and Spanish American agrarian structure in general remains true today, and many of the themes he highlighted continue to claim the interest of workers in this field. At risk of duplicating some of what he wrote a decade ago, I have chosen a start-from-scratch approach in the present essay, both because the interdisciplinary readership of the *LARR* seemed to demand it, and because my own views differ at some points from Mörner's and my arguments need to build their own momentum.
2. This observation is not meant to minimize the importance of a "holistic," "systemic," or "total" approach to the study of rural societies in the past. The French social historians, with their concept of "histoire totale," have sought to integrate social, economic, and political factors, and to show how these factors interact one upon the other or covary, but within a specifically agrarian context. See, for example, Le Roy Ladurie 1966 and Goubert 1960.
3. A strong case can be made that colonial Spanish America should be treated as a whole because the concepts, methods, and sources employed by rural historians of the area, and the models they apply, refute, or modify (e.g., Chevalier 1966) appear to share much in common. In fact, the reader of the present article will find allusive comparisons between work on Mexico and other areas of Spanish America. But the

in-depth analysis of a single large literature such as that on Mexico seemed to preclude work on such a scale, at least for the present.

4. On the general question of periodization in Mexican history, see the articles of Enrique Semo 1978 and Borah 1979; see also Brading 1978.
5. The arbitrariness of this temporal division has been noted by Mörner (1973, 215).
6. The priority of the work of other scholars, going back to Humboldt and Lucas Alamán, was recognized by Lesley Simpson in his foreword to the English translation of Chevalier's book (1966, vi) when he observed, "There is little that is startling or unexpected in the book, and, indeed, there could hardly be."
7. By institutional history, I refer to the explicit frameworks within which social and economic interactions occur, represented by written laws or regulations that may or may not be congruent with accepted usages. For an eloquent discussion of the difference between institutional history and social history, see Lockhart 1972.
8. We were later told that the economic contraction in New Spain was an echo of the seventeenth-century depression that gripped Europe (Chaunu and Chaunu 1955–59; Stein and Stein 1970). Lately, some doubt has been cast on this theory of linkage between economic depression in the Old World and the New (Bakewell 1971), but the fact is that we still do not know enough about the economic history of seventeenth-century Mexico to tell one way or the other. See more on this below, especially as it relates to rural history. On Mexico and the "general crisis," see Israel 1979.
9. The investigation of this phenomenon was subsequently deepened and expanded by Cook and Borah 1974–80. Furthermore, the general scenario that the Berkeley historical demographers developed for central Mexico has been applied elsewhere in Latin America, most notably to the Andean area (Dobyns 1963; Wachtel 1977; N. D. Cook 1981). A lively debate has developed over this issue; for some interesting criticism, see Rosenblat 1945, 1967; Henige 1978; Zambardino 1980. Despite criticism, the general scenario sketched for Mexico by the Berkeley school, and extended to other areas of Latin America by other scholars, is still widely accepted in both specialized and synthetic literature (McNeill 1976; Sánchez-Albornoz 1974). One reason for this continuing acceptance is that the scenario of high numbers and rapid decline explains convincingly much about the shape of colonial society.
10. I am thinking here specifically of notarial records, which have proved exceedingly valuable in the reconstruction of rural economic life, despite the fact that they are time-consuming for the historian to use. Notarial records began to be used systematically only in the late 1960s; see Lockhart's *Spanish Peru* (1968).
11. U.S. historians and geographers invested much effort in the history of ranching and its technology (Denhart 1951; Morrisey 1949, 1951; Bishko 1952; Brand 1961); price history received some attention (Borah and Cook 1958); the older type of fairly traditional local history continued to be written (Amaya 1951); and Lesley Simpson 1952 and Eric Wolf 1959 produced two broad-ranging essays on central Mexican economic history.
12. In fairness to Chevalier, it should be remembered that he had paid some attention to the problem of regional diversity in estate structure and had very briefly suggested the symbiotic nature of debt peonage (1952). But the emphasis of Chevalier's work was so clearly on the northern hacienda and its social matrix that in the process of distilling a model or ideal type of Mexican hacienda out of his work, other investigators have tended to simplify his argument on the theme perhaps more than it deserves.
13. The study of elite groups and their roles in colonial society was certainly not new, as is demonstrated by the large amount of scholarly energy devoted to the lives of viceroys, churchmen, prominent nobles, great family clans, and so on. What Brading and others have done in "socializing" elite studies (apparently under the influence of European social historians like Lawrence Stone [1965]), however, is to investigate the internal structure and origins of such groups in terms of a class or estate model of society.
14. For an excellent, succinct discussion of this debate, see Mörner 1973, 208–12.
15. See also Bartra 1974; Carmagnani 1976; and for a provocative short discussion of the trends in the historiography of the Mexican hacienda, see Brading 1978, 1–13; see also Mörner 1973 and Bazant 1972.

16. Where these conferences have not been completely devoted to either Mexico or the theme of agrarian history, these topics have figured prominently: Rome, 1972 (Florescano 1975); Cambridge, 1972 (Duncan and Rutledge 1977); Pátzcuaro, 1978 (Frost, Meyer, and Vázquez 1979); El Colegio de Michoacán, 1982 (see Zamora in the forthcoming publication from this conference).
17. In an age when the scholars studying these estates count themselves fortunate to be able to afford double lots in crowded university towns, the sheer size of the northern haciendas is truly impressive. They are sometimes described in comparative terms such as "twice as large as Belgium" or "three times as big as the entire state of New Jersey."
18. In a general way, one might apply the model of von Thünen's rings on an enormous scale to the whole of New Spain, replacing a single urban center with the variable of pre-Conquest Indian population density; one would presumably see an inverse relationship between the size of holdings and increasing density. For applications of this model at the regional level, see Brading 1978, 20, and Van Young 1981, *passim*; see also Ewald 1977.
19. For some comparative perspectives on Jesuit management and farming techniques, see Cushner 1980 on Peru; see also Berthe 1966 on Mexico.
20. The idea of the household economy was developed by the Russian economist A. V. Chayanov in *The Theory of Peasant Economy* (1966). For a concise summary of Chayanov's work, see Kerblay 1971, and for an interesting application to Mexico, see Brading 1978.
21. I do not mean to say that access to the means of production and technology are unrelated in the case of colonial Mexico, or that technology constituted an exogenous variable in the rural economic system. Economies of scale in agricultural production, for example, relate the distributional and technological variables because access to capital and markets as well as considerations of productivity would dictate the appropriateness of any given technology.
22. Even in the north, where one might expect the pattern of self-sustaining landed wealth to prevail, most evidence supports the view that landed and other kinds of wealth were complementary rather than mutually exclusive; see, for example, Charles Harris 1975, and Altman 1972, 1976. For other areas in Mexico, see Brading 1971, 1978; Ladd 1976; Van Young 1978, 1981; Lindley 1982; Kicza 1982, 1983; and the older, but valuable, work of Romero de Terreros 1943.
23. On this issue, see Mörner 1973, 192–94.
24. There has been a tendency to assume that although the colonial (and later national) capital occupied a clear position of primacy within the urban network of Mexico, social life in provincial cities emulated life in Mexico City, albeit on a reduced scale. This assumption seems to me a questionable one, given the fact that the institutional life of the provinces was somewhat attenuated in comparison to the capital city, and that the very primacy of the capital would have a tendency to deform provincial society so that its resemblance to that of the primate city would have been imperfect at best. For some consideration of this anomaly in a slightly different context, see Van Young's forthcoming article in the *Memoria de III Coloquio*.
25. On Church lending in agriculture, see Van Young 1981; Linda Robinson 1979, 1980; Costeloe 1967; and Florescano 1971a (which tends to skirt the issue), among others. The question as to whether the enormous amount in liens held by ecclesiastical corporations on rural properties in Mexico actually represented loans of liquid capital—that is, money—secured by real property, or mostly donations of income to the Church, or a combination of both, is complicated and so far unresolved. Arnold Bauer (1971) has pointed to the implications of this largely technical difficulty, and his forthcoming volume will address itself to this point, among others, regarding the economic history of the Church in Latin America. On the general issue of agricultural credit in Mexico, see also the documentary collection edited by Chávez Orozco 1953–58.
26. In analyzing patterns of agrarian property and production, particularly in terms of the feudal-capitalist dichotomy, researchers sometimes fallaciously try to have it both ways at the same time. For example, Robert Keith implies that in sixteenth-century

coastal Peru, where large-scale agriculture was profitable, capitalist considerations of economic maximization were at work, but where agriculture was not profitable, feudal principles governed the economic decision-making process (1976). Such a dichotomy, which is not ascribed to any type of temporal or developmental gradient, but simply to the absence or presence of a market, seems unlikely. Furthermore, in the case of the north Peruvian coast, which is seen by Keith as feudal and nonmarket oriented, as well as in other cases of backward areas sharing the same characteristics, our lack of knowledge about the technology in use (particularly extensive livestock-raising practices) possibly makes characterizing these areas as feudal somewhat risky. That is to say, acquiring land for the apparent social aggrandizement of the possessor may in fact be indicated as a rational technological strategy in an environment that dictates extensive land-use practices.

27. Far from being incompatible, prestige and elite status can be argued to have insured access to credit and capital, and even to the labor force and markets. For example, it would be interesting to know if those great landowners who most often made pious donations to the Church, and sent their sons and daughters into its ranks for purposes of social prestige, had preferred access to ecclesiastical loans; unfortunately, we do not yet know enough about patterns of ecclesiastical lending to answer this question. On ecclesiastical lending in the region of Guadalajara, see especially Linda Robinson 1979, 1980; on personal credit and family linkages in the same region, see Lindley 1982 and Van Young 1981; for Mexico City, see the 1982 and 1983 works by Kicza and his 1978 doctoral dissertation.
28. Frank takes the view originally espoused by Silvio Zavala, and by Jan Bazant even earlier (ca. 1950), that the Mexican hacienda was always capitalist rather than feudal in nature (Molina Enriquez and others via McBride, Simpson, and the rest). The book's major theoretical argument that the growth of the hacienda system in Mexico was a response to the exigencies of Mexico's integration into the "world capitalist system" is the weakest part of his treatment because Frank's own evidence tends to indicate the opposite—that Mexican agriculture in general, especially during the seventeenth century, was developing based on a kind of autochthonous growth that was not strongly linked to the outside world, even through the mining sector.
29. For an interesting recent discussion of the theoretical issues involved from a Marxist perspective, see Cardoso and Pérez Brignoli 1979 (especially vol. 1, chap. 1), various essays in Florescano's 1979 compilation, and Semo 1973.
30. See Bloch 1966; Duby 1962; Kula 1970; North and Thomas 1971; and for a comparative treatment of the European manorial system and Latin American landed estates, see Kay 1980. On northern New Spain, see Algier 1966; see also Möner 1973.
31. For a discussion of similarities and differences between the mature *encomienda* and the hacienda, see Lockhart 1969 and Keith 1971.
32. It must be admitted that the revisionist tendency insofar as rural labor institutions are concerned seems strongest among non-Marxists, particularly North Americans, possibly because among the latter, Marxist ideas have been somewhat less influential than social science concepts of a functionalist stamp. For the Mexicans, this kind of "pure" social science may seem a luxury that accords ill with the historical legacy that they live with on a day-to-day basis.
33. The literature on deviancy and social protest in prerevolutionary Mexico is not yet well developed, although efforts are being made in that direction by a number of historians. William Taylor, in particular, has broken much new ground with his comparative studies of regional patterns of deviancy, crime, and rebellion (1979, 1981a, 1981b; see also Klein 1966); the 1957 study by Martin, some interesting passing remarks by Florescano (1969b), and the 1965a article by Berthe all provide useful information on vagabondage and banditry in the countryside. My brief 1980 study gives a detailed descriptive account of a murder on a mid-eighteenth-century hacienda, while Bazant 1975 provides an interesting account of an uprising on a mid-nineteenth-century estate in the San Luis Potosí region. An enormous gap in the literature on late colonial and early national Mexican history exists because of the lack of studies relating late-colonial agrarian conditions to the uprising of 1810 and the

general social composition of the insurgent movement. As recently as fifteen years ago, Hamill's otherwise excellent study of the Hidalgo revolt virtually ignored the role of social and economic conditions in the countryside in fomenting the movement for independence from Spain, choosing to concentrate instead on elite-group grievances and mobilization (he later acknowledged the emergence of new research themes in the 1981 reprint edition of the book). The once-over-lightly treatment accorded social and economic conditions in the countryside by Domínguez in his 1980 comparative study of the Spanish American independence movements is less understandable, especially in view of the rich body of work that was produced in the interim. Since 1966 some notable, but tentative, steps have been taken in attempting a social analysis of the insurgent movement and in relating it to change and strains in the countryside (see the earlier article by Wolf 1957; Di Tella 1978; Taylor 1981a; Hamnett 1970, 1980; Tutino 1980; Brading 1973b, 1978; Florescano 1978). For two countrywide treatments of agrarian unrest during the nineteenth century, see Meyer 1973 and Reina 1980.

34. On this theoretical point, see Mörner 1973, 210–12.
35. By "attenuation of institutional bonds" I mean simply that population, and therefore the web of overlapping social relationships, were likely to be somewhat thin in rural districts, which left a vacuum most easily filled by the rural estate. A rough measure of this thinness would be figures on population density. For comparative purposes, let us take Mexico and France around 1800. France, with a population much larger than New Spain's (about 27 million) but a territory much smaller (about 213,000 square miles), had a population density of 127 people per square mile. The core area of Mexico, on the other hand (including the intendancies of Mexico, Puebla, Oaxaca, Guanajuato, San Luis Potosí, Zacatecas, Durango, Sonora, Yucatán, Guadalajara, Veracruz, Valladolid, and the *gobierno* of Tlaxcala, but excluding the northern zones of New Mexico, Texas, California, etc.), had a total area of about 800,000 square miles and a population of approximately 5,760,400, giving it a density of 7 people per square mile (Florescano and Gil 1974, 97–98). In its most crowded provinces, like the Intendancy of Guanajuato, Mexico's population density never rose above 50 people per square mile. Even allowing for the effects of differential patterns of urbanization and the controversiality of the overall population figures for Mexico, the difference in magnitude between the two countries is enormous. This contrast with European population patterns is not often mentioned, but it must have meant that the degree of isolation and the intimacy of social relations in the country districts were considerably greater than in contemporary Old World regions.
36. It is interesting to note that the *encomienda* as an antecedent of the great estate figures much less prominently in the recent writings on the agrarian history of colonial Mexico than in those on Peru (Burga 1976; Keith 1976; Ramírez-Horton 1977; Davies forthcoming; Lockhart 1968), or even on Chile (Góngora and Borde 1956; Góngora 1970; Bauer 1975). Some exceptions to this generalization can be found (Lockhart 1975; and some of the essays in the 1976 Altman and Lockhart collection).
37. We still know relatively little about wage and price movements in Mexico before 1700 (Borah and Cook 1958; Gibson 1964; Barrett 1970); what we do know is considerably complicated by questions about nominal levels, silver production and outflows, and the general monetary situation within the colony. As with so many other questions, the data become much more plentiful, and the research along with them, as we move into the eighteenth century (Florescano 1969b; Pastor et al. 1979; Hamnett 1971a; Brading and Wu 1973; Galicia 1975). The early decades of the nineteenth century are relatively well covered regarding rural wages (Charles Harris 1975; Bazant 1975; Tutino 1979; Cross 1979), but our knowledge of price levels is weak up to the era of the Porfiriato. Agricultural cycles and crises, with their typically sharp price variations, have received a good deal of attention, particularly in the work of Florescano 1968a, 1968b, 1969b and Brading and Wu 1973.
38. Would Wittfogel's 1981 analysis of "hydraulic" societies as applied to colonial and postrevolutionary Mexico, for example, produce a scenario of agro-social involution during the colonial period and most of the nineteenth century, followed by a re-



- surge of hydraulic organization and its accompanying state and social structures? No one has yet attempted such an analysis.
39. To date most Latin American historians have shied away from attempting comparative agrarian history or comparative studies of rural social structure. Sociologists and anthropologists have been characteristically bolder in generalizing and using the comparative method (Bartra 1974; Stavenhagen 1970). The comparative treatment of political upheaval and revolution in terms of underlying historical agrarian structure, done so eloquently by Moore 1966 for Europe, the United States, and Asia, has not advanced very far for Latin America or Mexico in particular (Wolf 1969; Landsberger 1969). See also note 33 above.
  40. The explicit study of such power relationships for the colonial era has not really been tackled for Mexico, but for other areas efforts are continuing to be made (Ramírez-Horton 1977, work in progress; Klarén 1973; Stein 1957).
  41. For a good discussion of these and other problems, see the introductory chapters on method in Florescano 1969b.
  42. Somewhat better results may be obtained with data generated by centralized recordkeeping agencies, or by corporate entities with an unbroken existence over a long period of time. In the first case, effective use has been made of price, tax, and other records maintained by the Church (tithes) and by the agencies of municipal government (*albóndiga*, *pósito*, and *abasto* records, for instance). For some examples, see Florescano 1969b; Brading 1978; Barrett 1974; Van Young 1979, 1981; Hamnett 1971a; Pastor et al. 1979. In the second case, the records of Jesuit-owned estates are particularly useful; see, for example, Konrad 1980; James Riley 1976; Blood 1972. On the Marquesado del Valle, see Barrett 1970; García Martínez 1969; G. Michael Riley 1973.
  43. One of the problems faced by the investigator of rural history is to construct some kind of series or otherwise to structure large bodies of data that appear to be non-quantifiable because each bit of data is discrete. This difficulty is most notably the case with notarial records, which have proved of great value in such studies; however, computer manipulation of such data is possible (see, for example, Hyland 1979).
  44. These three basic approaches were briefly outlined by Lockhart, albeit in a less refined form and not applied exclusively to the history of landed estates (1972, 23, 27).
  45. On the Jesuits, see also Tovar Pinzón 1971; Maya 1976; and Benedict 1970; on other ecclesiastical corporations, see Lavrín 1966, 1973, 1975; and Ewald 1976.
  46. Such is the case, for example, with the otherwise interesting 1977 work of Serrera Contreras on the livestock industry of the Guadalajara region during the late colonial period. Although his data are extensive and his treatment of them informed, Serrera's basic analytical error has been to focus almost entirely on the development of the livestock industry and export trade, excluding other equally, if not more, important factors such as the growth of local markets, land-use patterns, agricultural activities, and labor practices. The result is that his work ultimately fails to explain convincingly even the changes within the sector he chose to study. See Van Young 1979, 1981.
  47. It should be said that within the tradition of intensive subregional studies are found at least two identifiable variants. The first, usually referred to as local history, bears a distinctively antiquarian stamp and often has the style and flavor of diminutive *historia patria*—passionate in tone, narrative rather than analytical in approach, and appreciative of local eccentricity and character. Any good-sized town or city in Mexico is likely to have its chroniclers and apologists. The second variant, of which Luis González has been recently the most articulate exponent, is microhistory; two excellent recent examples are González's *Pueblo en vilo* (1968) and Heriberto Moreno García's *Guaracha* (1980), both of which deal with very circumscribed areas in Michoacán, mostly during the nineteenth century. The difference between the two variants is that local history of the traditional, antiquarian stamp usually ignores broader regional or national themes as reflected in local realities, while microhistory attempts to see the general in the particular, although in practice its exponents' interest in the grain and

- color of local life often goes well beyond what would be required by a straightforward case-study approach.
48. One of the first to use this kind of documentation in a systematic way was James Lockhart in his studies of early post-conquest Peru (1968 and 1972).
  49. For a discussion of the usefulness of land suits as a source for rural economic history, and of their limits, see Van Young 1981, 316–18.
  50. For other work on the meat and grain supply of colonial cities that is also based mostly on institutional sources, see Dusenberry 1948b; Chávez Orozco 1954–59; Florescano 1965a; Calvento Martínez 1966; and Vázquez de Warman 1968.
  51. For a somewhat more sanguine view of this question, see Lockhart 1972, 31. The 1981 edited collection of the life histories of ordinary people from various areas of colonial Latin and Anglo America by David Sweet and Gary Nash shows what can be done along this line, but I would maintain that the limits of this kind of social history are still fairly narrow.
  52. Mörner asked this same question ten years ago (1973, 193), and it still appears to be unanswered.

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