CENTRAL ISSUES:
Social History and the Recent Study of Colonial Central America

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THE FRONTIER MISSION AND SOCIAL TRANSFORMATION IN WESTERN HONDURAS: THE ORDER OF OUR LADY OF MERCY, 1525–1773. By Nancy Johnson Black. (Leiden, the Netherlands.: E. J. Brill, 1995. Pp. 194.)

INDIAN PAYMENT IN KIND: THE SIXTEENTH-CENTURY ENCOMIENDAS OF GUATEMALA. By Lawrence H. Feldman. (Lancaster, Calif.: Labyrinthos, 1992. Pp. 92. $18.00 paper.)

GUATEMALA IN THE SPANISH COLONIAL PERIOD. By Oakah L. Jones, Jr. (Norman: University of Oklahoma Press, 1994. Pp. 344. $38.95 cloth.)

ENCOMIENDA POLITICS IN EARLY COLONIAL GUATEMALA, 1524–1544: DIVIDING THE SPOILS. By Wendy Kramer. (Boulder, Colo.: Westview, 1994. Pp. 293. $50.00 cloth, $19.95 paper.)

DEMOGRAPHY AND EMPIRE: A GUIDE TO THE POPULATION HISTORY OF SPANISH CENTRAL AMERICA, 1500–1821. By W. George Lovell and Christopher H. Lutz. (Boulder, Colo.: Westview, 1995. Pp. 190. $49.95 cloth.)

SANTIAGO DE GUATEMALA, 1541–1773: CITY, CASTE, AND THE COLONIAL EXPERIENCE. By Christopher H. Lutz. (Norman: University of Oklahoma Press, 1994. Pp. 346. $37.95 cloth, $16.95 paper.)

ARCHITECTURE AND URBANIZATION OF COLONIAL CENTRAL AMERICA: VOLUME I, PRIMARY DOCUMENTATION AND LITERARY SOURCES. By Sidney David Markman. (Tempe: Center for Latin American Studies, Arizona State University, 1995. Pp. 282. $35.00 paper.)

ARCHITECTURE AND URBANIZATION OF COLONIAL CENTRAL AMERICA: VOLUME II, A GEOGRAPHICAL GAZETEER OF PRIMARY DOCUMENTARY, LITERARY, AND VISUAL SOURCES. By Sidney David Markman. (Tempe: Center for Latin American Studies, Arizona State University, 1995. Pp. 342. $50.00 cloth.)

IMPERIAL PANAMA: COMMERCE AND CONFLICT IN ISTHMIAN AMERICA, 1550–1800. By Christopher Ward. (Albuquerque: University of New Mexico Press, 1993. Pp. 272. $45.00 cloth.)

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Unstructured comparison of books such as these—varied in nature and different in the types of research on which they are based—runs the risk of misleading and misguided conclusions. For this reason, the nine volumes under review here will be discussed in the context of the evolution and current state of social history as it relates to colonial Central America. I have chosen social history as a focus for three related reasons. First, formal institutions in colonial Latin America tended to be weak, semi-articulated, and lacking in dynamism relative to the informal social groupings around which colonial life revolved (as evident in the monographs by Wendy Kramer, Christopher Lutz, and Christopher Ward). Second, the irregularity of the primary sources (as reflected in the volume prepared by Lawrence Feldman and the two-volume work by Sidney David Markman), coupled with the richness of notarial documentation, favors an orientation toward social and cultural matters, toward the nature and product of human relations rather than an emphasis on quantified production, statistical abstraction, or institutional development (despite the statistical basis of Lutz's book). Finally, social history is the kind of history with which I am most familiar.

My concern with these five fairly brief monographs (fewer than two hundred pages of text on average) and four source guides is not to judge their contributions to social history but to discuss the degree to which they turn from studying institutions, laws, ideas, objects, and events to the individual protagonists and informal social groups behind such phenomena. I am particularly interested in the nature and treatment of sources, the approach taken (if any) toward indigenous societies, and the general state of the field.

Region and Center

The historical definition of colonial Central America as a region is problematic for a number of reasons, a difficulty reflected in the books under review. George Lovell and Christopher Lutz's annotated bibliography of demographic studies, *Demography and Empire: A Guide to the Population History of Spanish Central America, 1500–1821*, follows the borders of the colonial Audiencia and Reino de Guatemala. This area included what are today Chiapas and Belize to the north and stopped approximately at Costa Rica's border with Panama to the south. The authors also cover in a couple of their entries Yucatán, which was briefly subject to Guatemala in the sixteenth century. Sydney Markman's focus is the same, with Panama and Yucatán included only in entries that relate also to the provinces of the Audiencia.

But both the Lovell and Lutz compilation of secondary sources and Markman's two volumes listing primary sources are heavily weighted in favor of the provinces comprising modern Guatemala. This bias reflects

the fact that the sedentary indigenous population was concentrated in this area. Thus Spanish settlement was most dense here and the production of written records and concomitant historical literature most intense. For similar reasons, within Guatemala, the south and the Highlands are the regions represented best. This is also the location of almost all the *encomiendas* granted during the period discussed by Kramer. Feldman's focus is narrower still, with most of his encomienda data coming from a few provinces running across the center and east of the Highlands. Likewise Oakah Jones, while making occasional references to other corners of the Audiencia and claiming "to concentrate on what is today's republic of Guatemala during the Spanish colonial period" (p. xii), follows his Spanish sources in focusing overwhelmingly on the colonial heartland of the southern Highlands.

This tendency of studies of the wider Guatemalan or Central American area to give most attention to a small region is paralleled by a high proportion of regional and subregional studies in the literature on colonial Central America. Examples here are Lutz's history of the sometime provincial capital and seat of the Audiencia and Nancy Johnson Black's study of the small Tencoa region in western Honduras. One notable recent illustration of this pattern is the publication of what is arguably the magnum opus of one of Guatemala's most distinguished scholars. Robert Carmack's *Rebels of Highland Guatemala* (1995) crosses temporal barriers but focuses on the single community of Momostenango. By concentrating on a narrower time period within the colonial era, other recent outstanding works have been able to widen their geographical scope. Yet their focus remains within the confines of a Highland subgroup or subregion.[1]

Such a regionalist focus may indeed be historically rooted and justified. Besides, one does not expect a single work to treat every topic and every province. Nevertheless, what is lacking in the books under review here and in the field in general is new scholarship that addresses questions of center and region, tackles a significant time period and area, and uses the glue of a particular thematic focus while illuminating multiple aspects of colonial society (Kramer, Lutz, and Jones could be taken as partial exceptions). Exemplary steps have been taken in this direction by a few Central Americanists, most notably by Murdo MacLeod (1973), William Sherman (1979), and Adriaan van Oss (1986).[2] But the field has yet to produce a quantity of scholarship having this ambition that compares with the out-

1. Examples are Orellana (1984), Lovell (1985), Hill and Monaghan (1987), Hill (1992), and Few (1997). Fowler (1989) is a distinguished example from a neighboring area. Also see Lovell and Lutz's *Demography and Empire*, which is, as far as I can tell, faultlessly comprehensive.

2. McCreery (1994) is a contribution of this nature but deals only partially with the colonial period. Martha Few's recent dissertation (1997), while confined to the Santiago area over one century, belongs methodologically in this category, in many ways representing a brand new chapter in colonial Guatemala's historiography (also see Herrera 1997).

put on other Spanish American regions—central and southern Mexico, for example.[3] A significant weakness of Jones's *Guatemala in the Spanish Colonial Period* is his inadequate incorporation of the methodologies, source genres, and discoveries by scholars of the neighboring provinces of New Spain and colonial Mesoamerica.

Christopher Ward's *Imperial Panama: Commerce and Conflict in Isthmian America, 1550–1800* illustrates the issue of region and center in a slightly different manner. Ward laments the fact that Panama has been left in a historiographical void (p. xi), seen by Central Americanists as part of Gran Colombia or "perhaps an economic and political appendage of the viceregal court at Lima, while Colombianists and Peruvianists view Panama as part of a distant periphery" (p. 199). Ward's stated ambition of filling the void is partly realized through his detailed economic study of the Portobelo trade fair, but he leaves Panama's regional affiliation as ambiguous as ever. Ward seems concerned to understand and present colonial Panama as a separate and fairly self-integrated social, political, and economic entity, yet his study shows how Panama's particular economic and geographical circumstances frustrated its local development along expected lines. Much wealth passed through Panama (as Ward details well), but little remained to become the basis for the full-fledged urban-rooted Spanish American society that quickly evolved in other regions. By consolidating colonial centers, these regions resisted long-term marginalization (see, for example, Altman and Lockhart 1976; Lockhart 1991). Ward argues persuasively that because "Panama's period of commercial activity was limited to three or four weeks every two years" (p. 191), colonial Panamanians failed to become wealthy, especially during the Portobelo boom period of the late sixteenth and early seventeenth centuries. The resulting inadequacy of the trade-related infrastructure (roads, warehouses, fortifications, and troops and supplies for defense) in turn hindered the consolidation of a prosperous local elite.

The historiographical view of Panama as neither a significant center in its own right nor meaningfully attached to its regional neighbors would thus appear to be justified. This is not to say that colonial Panama should be ignored. On the contrary, it should be studied not despite its marginal status but because of it. In the process, Panama should be compared with other regions of Spanish America where differing economic circumstances determined the processes of social development. Two examples that come to mind are Yucatán, with a pattern of development similar to central Mexico's only slower and less complete (Hunt 1974; Patch 1993), and the boom-and-bust example of Cubagua (Otte 1977). The most pertinent example for potential comparison, however, might be Highland

3. Examples are such works as Chance (1978), Bricker (1981), Farriss (1984), Martin (1985), Clendinnen (1987), Lockhart (1992), Patch (1993), Cope (1994), Boyer (1995), Kellogg (1995), Stern (1995), and Taylor (1996). Another effort along such lines is Restall (1997).

Guatemala, or even just the Audiencia capital itself, now that Lutz's study of it is readily available.

Source and Method

In commenting on the irregular nature of the primary sources for colonial Latin America, I meant the frequent occurrence in archival material of gaps in chronological sequences; the variations in genre formation, usage, and survival; and the unreliability of record-taking methods from the viewpoint of modern social science. As a result, two methodological considerations come to mind that should be central to colonial Latin American scholarship, both illustrated in the books under review here.

First, the reading of the sources and (where applicable) their transcription, translation, presentation, and analysis must be carried out with comprehension and care. This point is particularly important in the publication of source guides, whose sole raison d'être is to provide other scholars with bases for original study. This is true despite Feldman's disclaimer that *Indian Payment in Kind: The Sixteenth-Century Encomiendas of Guatemala* is "neither a study of Indian societies nor hispanic rule . . . [but] a geographical, economic, and demographic framework to study the facts . . . , a finder's aid for the colonial investigator" (p. ix). Feldman must be applauded for tackling a challenging document, the tribute assessment on Guatemala's encomiendas prepared under President Alonso López de Cerrato (1548–1551). Yet Feldman's rudimentary presentation of the information it contains makes *Indian Payment in Kind* more useful as a starting point for research, a collection of citational clues for archival investigation, than as a substitute for such work.

Given Feldman's stated intentions, one cannot reasonably lament the lack either of articulate discussion of entries (such as that offered on secondary sources in Lovell and Lutz's guide) or of extensive and well-contextualized analysis (such as that distinguishing Kramer's monograph). Still, a source such as the Cerrato report would be better placed in an analytical work such as Kramer's (which draws on various encomienda records, primarily *cédulas de encomienda* and the *probanza* and *residencia* files of individual encomenderos). Or it might fit well in a source guide such as Markman's *Architecture and Urbanization of Colonial Central America*, which features hundreds of entries representing dozens of genres (from chronicles as well as archives), organized thematically (Volume I) and geographically (Volume II), with entry summaries that are clear and often analytical (rather than in note format).[4] Markman's entries have been se-

4. In fact, Kramer discusses the Cerrato report (pp. 17–18), while several entries in Markman's guide cite edicts of the Cerrato presidency. Lovell, Lutz, and Swezey (1984) contains much of the information presented in Feldman's *Indian Payment* while also featuring extensive analysis.

lected according to a particular focus, that of architecture and urbanization, just as Lovell and Lutz are primarily concerned with population history. But both works adopt a laudably broad definition of their foci, permitting the inclusion of entries relating to almost every topic imaginable and making both projects immensely useful to students of colonial Central America.

The multiplicity of sources listed in Markman's two volumes suggests the second methodological consideration germane to my discussion, namely the efficacy of using more than one type or genre of primary source in order to check, balance, enrich, and enliven a given piece of analysis. Every study's sources must be limited, and the resulting work is thus limited by them.[5] Wendy Kramer's treatment of the official records of Guatemalan encomenderos in *Encomienda Politics in Early Colonial Guatemala, 1524–1544: Dividing the Spoils* represents a skillful manipulation of unstudied sources. In the first major study of the early decades of encomendero rule in Guatemala, Kramer compares each successive encomienda distribution, using Huehuetenango as a case study. The conventional view of Pedro de Alvarado as unique in his extensive domination over early conquistador society in Guatemala[6] is significantly revised by Kramer's exposure of the roles played by Alvarado's kin and prominent compatriots. She thus enlarges scholarly understanding of the foundational years of colonial Guatemala[7] and further illuminates the unexhausted topic of the political culture of conquistador and encomendero society in Spanish America.[8] Yet the paucity of source variants means that Kramer can write only a limited kind of political narrative, rather than "a complete social history of the first group of Spaniards" or an examination of "the economics of the encomienda" (p. 23). As a result, her interpretation of the conquest process in terms of definition and pace is possibly overly determined by the encomendero perspective (a question to which I shall return).

The limitations of a narrow source base can often be effectively offset through judicious use of another type of primary source of indirect or partial relevance or through adroit use of secondary material. Thus methodology becomes as important as source multiplicity. Kramer, for example, makes up for the limitations of her sources by mining them with ex-

5. My own book *The Maya World* (1997) was based almost entirely on primary sources written in Yucatec Maya. These sources were of many different genres and offered a unique and original perspective on the social history of colonial Yucatán. They determined the study's contribution as a complement to rather than a substitute for Nancy Farriss's *Maya Society under Colonial Rule* (1984), which was primarily based on (and limited by) a different set and type of sources.

6. For an example, see Lutz, who quotes William Sherman (pp. 13–14).

7. Kramer here goes beyond MacLeod (1973) and Sherman (1979), complementing Carmack (1973), Orellana (1984), and Lovell (1985).

8. The study thereby joins such landmark studies as Thayer Ojeda (1950), Góngora (1962), Lockhart (1972a), Davies (1984), and Himmerich y Valencia (1991).

action and intelligence. Conversely, Jones compensates somewhat for his relatively unsophisticated and old-fashioned treatment of sources by consulting a fairly wide range of archival and secondary materials. Nancy Johnson Black's study of a colonial Mercedarian mission in western Honduras combines archaeological evidence with an analysis of the institutional archival sources of the Order of Our Lady of Mercy.

But the prospect of Black's multidisciplinary mix, coupled with the opening chapters' subtle anthropology-oriented review of theoretical and background issues, raises expectations that are not fully met in *The Frontier Mission and Social Transformation in Western Honduras: The Order of Our Lady of Mercy*. The archaeological evidence turns out to be minimal, leaving Black with the slim base of the seemingly dry Mercedarian records on which to construct her ambitious account of the cultural impact of missionizing on the missionaries themselves. How this reader wished for another source that might have personalized the analysis. One example must suffice: a paragraph entitled "Chastity" reveals only a series of regulations regarding Mercedarian behavior, with nothing on patterns of violation or enforcement of the rules, no sign of the attitudes or varying practices of individual friars, no cases or names at all (p. 102). To be fair, Black is not alone. One finds more named individuals in Ward's tables than in his text. Jones features Spanish individuals only. And Lutz reduces Santiago's inhabitants to numbers in tables and patterned categories in his text.[9]

Despite the empirical slimness of her study, Black's integration of primary sources on her specific topic with secondary material on a wide range of related topics amounts to a significant contribution to the subfields of mission and frontier history. Similarly, Ward endeavors to make extensive use of historical literature so as to make the most of his archival materials. *Imperial Panama: Commerce and Conflict in Isthmian America, 1550–1800* is in effect an attempt to integrate four different studies: a synthesis of secondary works on colonial Panama, mostly on its economy, aimed at undergraduates; a historiography of colonial Panama for specialists; an appraisal of the Spanish imperial economy as it related to Panama and particularly the annual Portobelo trade fair (using official records from the *Contaduría* and *Contratación* sections of the Archivo General de Indias); and a study of Panamanian fortifications (adapted from the author's dis-

9. Although anecdotal case examples may not replace well-articulated patterns drawn from multiple cases, neither does selective use of such samples compromise a work of social science in which parish records have been converted into marriage indices (to take an example central to Lutz's book). In fact, a balance between these two source-types and methods is often the most effective. For example, Cope (1994) might be viewed as centered between Boyer (1995) at one extreme and Lutz's *Santiago* on the other (comparing three recent studies that focus mostly on urban non-elite colonial society). By the same token, Robinson Herrera's recent dissertation on Santiago (1997), in relying primarily on case examples, complements Lutz's study.

sertation and based on sources from the AGI, especially from *Mapas y Planos*). The resulting volume, although somewhat disjointed, tends to be readable, informative, and persuasive.

"Indians" and Spaniards

While none of these books are primarily works of ethnohistory,[10] all except Ward's *Imperial Panama* deal to some extent with the indigenous population. And well they should: the majority of the Central American population was indigenous, even at the end of the colonial period (see Lovell and Lutz's introduction). As throughout colonial Mesoamerica and the Andes, native individuals and communities in Central America participated heavily in forming colonial culture and society. The encomiendas Feldman and Kramer focus on were grants of Mayas. Black's Mercedarian mission existed to proselytize the Lencas. Jones's colonial Guatemala and Lutz's Santiago were built, sustained, and populated largely by Mayas. Furthermore, scholars can no longer claim to lack what once retarded the profound investigation of native societies: an inadequate historiographical context, an insufficient variety of theoretical frameworks to guide interpretation, and a poor knowledge and understanding of source materials. Yet the degree to which the books under review reflect the new historiographical potential is varied.

Black cannot reasonably be criticized for the paucity of attention given to Lenca society and culture in her study. She understands that the Mercedarian sources are appropriate for reconstructing the response of the order to the mission experience, rather than the response of the Lenca. Black does not neglect using secondary sources to provide some indigenous context for the mission story. Still, some readers of *The Frontier Mission and Social Transformation in Western Honduras* may be disappointed by its failure to contribute much to the ethnohistorical trend in what has been recently termed "the New Latin American Mission History," whereby greater emphasis is placed on native perspectives (Langer and Jackson 1995; also see Jackson and Castillo 1995).

Likewise, Kramer should not be faulted for privileging Spaniards over Mayas in discussing the conquest period because her book is intended as little more than an account of encomienda distribution using encomendero sources. Nevertheless, one glimpses some encomendero influence on Kramer's perception of the Spanish Conquest. Like her source subjects, she tends to view it as a singular initial event whose consolidation (through such policies as encomienda distribution) provoked "native uprisings" and "outbreaks of rebellion amongst overworked encomienda Indians" resulting in the "slaughter of Guatemalan encomenderos." These

10. They are not *ethnohistory* in the sense that the term is used by colonial Latin Americanists, to refer to the study of the continent's indigenous peoples.

events, however, "never reached the scale of the massacres" in Yucatán's "Maya revolt of 1546–47" (p. 227).[11] Her characterization of these stages of the conquest as revolts (in both Guatemala and Yucatán) reflects the Spanish sense of just entitlement through claim and conquest as the assertion of that claim, with indigenous resistance not a rejection of Spanish claims but a revolt following their alleged acceptance. Indeed, the Spanish use of terms such as *sublevación* and *pacificación* served to bolster their spin on what was from the indigenous perspective an open-ended and protracted negotiation involving various strategies of contestation and adaptation.[12] Indigenous accounts of the conquest contribute to a more balanced view of the period, but Kramer dismisses them because they were "often written down years after the actual events . . . , focus on the histories of their own people and mix folklore with facts" (p. 27). She speaks as though Spanish accounts were none of these things.[13]

Kramer's *Encomienda Politics in Early Colonial Guatemala* complements Lutz's *Santiago de Guatemala, 1541–1773: City, Caste, and the Colonial Experience* in a number of ways. First, Kramer concentrates on the decades before the founding of Lutz's Santiago (on the Panchoy site that is now Antigua). Second, Lutz directly influenced Kramer (one of many students and scholars who have benefited from his role as a central patron and mentor in the field). Third, both books represent kinds of social history that have played important parts in the evolution of the discipline but seem mildly dated today.[14] Finally, neither book chooses to give serious attention to the impact of colonial processes (whether encomienda imposition or urban development) on Maya society and culture.

The last remark requires some qualification. Kramer focuses on the encomenderos, not on their encomienda subjects, while Lutz devotes considerable space to indigenous society and culture where they dovetail with demographic issues. For example, Lutz conveys much that is new and important about settlement and marriage patterns in Santiago's seg-

11. Jones adopts this perspective with even less ambiguity, dating the conquest from 1524 to 1530 and dubbing the years 1530–1541 as "aftermath" (pp. 18–29). A more complex analysis of the conquest is merely outlined in a single paragraph and the topic quickly resolved with the assertion that "the Indians had been defeated by 1541" (p. 30). Lutz avoids the problem in his opening chapters by refraining from giving an end date to the conquest and keeping focused on his analytical target, the city of Santiago (pp. 3–44).

12. Thus the conquest of Yucatán could be dated variously, with a good possibility being 1517–1570 (per Clendinnen 1987; see also Restall n.d). A similar reevaluation needs to be carried out for Guatemala, although an important contribution in this direction is Bricker (1981), which is not cited by Kramer.

13. While accounts of the conquest in Mayan languages tend to date from later in the colonial period, that tendency does not invalidate their various perspectives. Kramer's use of terms such as *folklore* and *tribal* in reference to the Mayas is regrettable.

14. Its choice of topic, sources, and method makes Kramer's work reminiscent of the new institutional history of the 1960s and 1970s, which featured a social-biographical element. Lutz's book is updated and revised from his 1976 dissertation and his *Historia sociodemográ-*

regated and later multiracial communities. For this reason, all colonial Latin Americanists interested in questions of race, ethnicity, and urban classes will wish to look at *Santiago de Guatemala*. Colonial Guatemalanists have probably already read it, many in the 1982 Spanish-language edition. Yet one wonders whether reducing inter-ethnic relations to quantified patterns and paradigm testing—as painstakingly-researched, well-presented, and revealing as they are—may miss much of the complexity and humanity of miscegenation and other intercultural processes. Of potential relevance to the Santiago case are the paradoxes of solidarity and internal conflict within social groups (as explored by Stern 1995), as well as the seemingly inconsistent perception of racial categories by *casta* individuals (as discussed by Cope 1994). Many readers will wish to compare Cope's study of Mexico City with Lutz's *Santiago* for their contrasting findings on the pace and extent of racial homogenization[15] and their differing methodologies. Some readers, however, will wish that Lutz had tackled some of the questions asked by Cope regarding the perception and meaning of racial designations and the development of class identities. Additional pertinent questions about the forms and functions of family and gender are addressed in 1995 publications wholly or partially on Mexico City by Richard Boyer, Susan Kellogg, and Steve Stern. Non-Spanish individuals and voices—central to such concerns—are seldom seen or heard in *Santiago de Guatemala*.

These criticisms of the treatment by Black, Kramer, and Lutz of "Indians" (as they tend to call them) are nonetheless carping comments relative to the significant contributions of these books on their primary topics and the egregious handling of ethnohistorical issues by Jones. His *Guatemala* is, to put it bluntly, strikingly Hispanocentric. This slant is illustrated by the overtly colonialist perspective of the chapters entitled "Spanish-Indian Relations" and "The Spanish Legacy," also by the marginalization of the Mayas throughout the book. The topic of "Spanish-Indian Relations" is so entirely characterized in terms of labor that a quarter of it actually discusses "Black Slave Labor." Even if this chapter were to be judged as a survey of economic relations only, it still completely ignores the role of il-

fica de Santiago de Guatemala, 1541–1773 (South Woodstock, Vt.: CIRMA, 1982). It nonetheless remains a good example of the quantitative approach to social history that originated in the 1950s and climaxed in the 1970s, an approach related to the new institutionalism. Lockhart once characterized this type of history as being "on the edges of institutionalism" (1972b, 13).

15. A comparison is especially desirable regarding the Oaxaca-Santiago differences suggested by Lutz, based on his reading of Chance and Taylor's study (see Chance 1978), in which the breakdown of racial segregation among the urban underclasses was much slower in Santiago. Another interesting point of comparison is that of urban riots. Cope (1994) discusses the 1692 Mexico City riot at length. Lutz suggests that Santiago never experienced such unrest because it effectively controlled the food sources of the surrounding valleys and because the casta underclasses enjoyed limited opportunities for social mobility. Both factors coincide in an interesting discussion of the black market, for example (see Chapter 6).

legal forced-purchases, the *repartimientos* (shown by Patch 1993 to be so crucial in neighboring Yucatán).

Through Jones's lens, Mayas and Africans existed in colonial Guatemala only in the context of a submissive and impersonal utility to the Spaniards' grand colonial project, whose achievement was "the establishment of [Guatemala's] territory, language, religion, society, and customs" (p. 267). The Maya contribution amounts to "Indians' arts and crafts" and the fact that "their descendents are everywhere apparent" (p. 266). This simplification of cultural and demographic developments is as crude as it is divorced from historical and historiographical realities. Studies published in the last two decades whose comprehension would have greatly informed Jones's view of the Maya population are too numerous to list here but would include the work of Louise Burkhart, Robert Carmack, Inga Clendinnen, Nancy Farriss, Kevin Gosner, Serge Gruzinski, Robert Haskett, Robert Hill, Susan Kellogg, Jorge Klor de Alva, James Lockhart, George Lovell, Sandra Orellana (given cursory use), Susan Schroeder, and William Taylor.

Of this sample of colonial Mesoamericanists (if I may call them such), half are in some sense Mayanists. Of those, only a few have used Maya-language sources to inform their analysis, the most extensive such endeavor being Robert Hill's use of Cakchiquel materials. Yet even Hill's pioneering work has barely touched the surface of a profound philological and historiographical potential. Significant quantities of notarial records exist in Cakchiquel, Quiché, and no doubt other Guatemalan Mayan languages,[16] sources that might be used to enhance scholarly understanding of indigenous culture and society just as the study of Nahuatl and other native-language sources is now revolutionizing colonial Mexican studies.

It should be noted that Jones's book has much to recommend it if approached as though it were an older work emphasizing the primacy of institutions and events, rather than a work whose interpretation and emphases should be considered a contribution to the field. For example, it serves as an informative and clearly written introduction to such topics as the main structure, events, and protagonists of Spanish colonial government, Hispanic intellectual culture, and colonial architecture (it is nicely illustrated with maps and plans from the AGI). Coverage of earthquakes

16. I have seen only a small number of notarial records in Cakchiquel and Quiché (taken from the Archivo General de Centroamérica in Guatemala City). But I have gathered from conversations with Martha Few, Robinson Herrera, and Robert Hill that many more are to be found in the AGCA and that some exist in U.S. collections. Because the AGI contains a significant quantity of notarial material in Nahuatl, Yucatec Maya, and other Mesoamerican languages, it is quite possible that this archive also contains records in Guatemalan Mayan languages.

and other natural disasters of the period will interest devotees, who will also wish to consult Feldman (1993).

In conjunction with the Spanish-language sources used by Jones, Kramer, Lutz and other scholars, the further study of Maya-language sources will surely produce a more complete and textured picture of Guatemala's multicultural colonial society. Black's discussion of the frontier in western Honduras could be viewed as analogous to the historiographical frontier between philology-based ethnohistory and other types of social history based primarily on sources in Spanish or written by Spaniards (see especially pp. 158–62). The dismantling of that frontier by colonial Mexicanists is now underway, but in Central America, contact itself is only now being initiated. This analogy breaks down with respect to the asymmetry of Spanish-indigenous contact. As Black reveals, the experience of contact and cultural interaction was not always positive for the Spaniards, let alone for the indigenous population. The slant of Jones's book shows how one-sided the colonial experience could and can be seen. Yet as Lutz reveals for Santiago, interaction resulted in a plural and segmented society that was nevertheless dynamic and in many ways cohesive. Similarly, the ongoing production of—and miscegenation between—different types of social history using varied sources will no doubt result in something pluralist yet dynamic, segmented yet cohesive, which will contribute to colonial Central America's evolution as one of the most exciting fields in Latin American studies.

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