Of Things of the Indies

Essays Old and New
in Early Latin American History

James Lockhart

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To Betsy and John

the extent that one could attempt something on the order of my "The Merchants of Early Spanish America."[37] Similar surveys could now probably be done with Audiencias and their judges, and possibly also with some portions of the ecclesiastical world, as well as in the very rapidly expanding subfield of the study of women and gender. Perhaps with Africans as well. With the crafts and maritime life, not yet.[38] I do hope, though, that as primary research on given topics across the centuries from first contact forward mounts (and it by no means needs to reach comprehensive coverage, but merely be enough to allow some triangulation), people in our field will take the opportunity to work out the longer trends it implies.

[37]Now in this volume as Chapter 6.

[38]Mining is unusual. The barrier in this case is not a lack of studies for the seventeenth and eighteenth centuries, but the paucity of knowledge about how the industry really functioned and was organized in the beginning.

2. The Social History of Early Latin America: Evolution and Potential

(1972, revised and expanded 1989)

"SOCIAL HISTORY" should be readily definable as the study of historical phenomena which transcend the individual and manifest themselves in human groups. But such a definition includes almost all meaningful history; it seems to fit precisely those political and institutional studies with which social history is ordinarily contrasted. Since our main concern here is with practical historiography rather than with precise definitions of broad genres, I will simply indicate through description and elimination the kind of history I mean.

Social history deals with the informal, the unarticulated, the daily and ordinary manifestations of human existence, as a vital plasma in which all more formal and visible expressions are generated. Political, institutional, and intellectual history, as usually practiced, concern themselves with the formal and the fully articulated. Social history bears the same relation to these branches as depth psychology does to standard biography. While it often discusses humble or obscure individuals, the correlation is not perfect, since study of the daily life and family connections of the famous is certainly social history, of a very valuable sort. Indeed, any branch of history can be converted into social investigation by turning attention from its usual main object of study, whether laws, ideas, or events, toward patterns in the lives and actions of the people who produce them.

Often it is impossible or undesirable to make a distinction between social and economic history. One can, however, discern a fairly distinct type of economically-oriented research which is concerned more with amounts and techniques of production than with the organization, conduct, and wider connections of the people involved. It tends strongly toward statistics and macrophenomena, and has a great deal in common with institutional history. In our field of early Latin America, it has often been practiced by French scholars and French-influenced Latin Americans. This useful branch of historical writing is also not our present concern.

The potential significance of social history for the so-called colonial period is easy to see. Formal institutions in early Iberoamerica, however important in adjudication and legitimation, were weak and spotty, lacking the manpower, the mechanisms, and even generally the will to carry out the activist policies of their counterparts in the twentieth century. Even when apparently locally influential, they were so only by virtue of grafting official attributes onto concentrations of social-economic resources, forming conglomerate structures of which the official aspect was more the symbol than the active principle. The main cohesive and dynamic forces of life were the needs, customs, techniques, and ideals of individuals acting in informal groupings; taken together these of course constitute society, so that early Latin American history is or should be to a large extent social.

Though E. G. Bourne sensed all this over eighty years ago, socially oriented studies have only recently come into their own. They were long nearly absent from the field (the national period lagging even farther behind), until finally the harbinger, demography, appeared after World War II, followed by full-fledged social history in the late 1960's. The years up to about 1975 saw the appearance of a series of major publications. With related kinds of ethnohistory and detailed demography, social history continued for some time to be the dominant strain among English-speaking historians of early Latin America and in some sense perhaps still is, though it has become broader and more eclectic. Latin Americans have largely continued to equate progressivism with the French school, though they read the English-language literature, and one or two major figures, above all Mario Góngora, have moved in that direction.

Why should such a movement emerge just when it did? One might imagine some connection with the persistent present-mindedness of the 1960's, strong echoes of which have stayed with us to this day. Social history touches a substream of continuity existing under more quickly alternating intellectual currents, governments, and even formal institutions. It therefore deals with matters that are of immediate relevance to the present. Scholars of our time in the social

sciences have maintained a preoccupation with twentieth-century "development." Social history certainly bears on development. It can show, for example, that the role of foreign entrepreneurs in Latin America was not only prefigured but fully anticipated by first-generation Spaniards and Portuguese in the colonial period. It can show that Spanish American cities in the sixteenth century had the same structure and function as today. A double process of migration to the cities and expansion of urban-European life outward, beginning in the conquest period, is what brought the Latin American countries as we know them into being; the continuation of the process is what their further "development" must inevitably consist of. The study of society in the early period quickly reveals the bankruptcy of the traditional-vs.-modern dichotomy in interpreting development.

Despite all this, it is not clear that development studies had much influence on early Latin American historians. To take the only example about which I have intimate knowledge, they surely had no influence on me. Rather, I always had a negative reaction to the condescension and restricted perspective that are involved in viewing another society and culture primarily as a problem. On the conscious level, my motivations in the early 1960's were: a deep sympathy for that combination of restraint and energy so characteristic of sixteenth-century Spaniards, not unrelated to my love of Renaissance music; and on the other hand a dissatisfaction with then current knowledge of Latin American history, which seemed to me to be miscellaneous, to lack a core. My intuition was that a close study of society would reveal that core and make sense of the whole. My colleague David Brading also wrote me once of his love for Guanajuato and everything concerned with it. Generally it appears to me that social historians are more likely to be motivated by a positive fascination with their subject and an intellectual curiosity about it than by the moral outrage of the developmentalists.

Probably neither outside pressures nor conscious inner motivations are crucial to the rise of the social history movement. The time of social history came largely because the field had worked its way through the sources down to those which have obvious social content. According to a principle which may be called the law of the preservation of energy of historians, scholars in a given field usually take the easiest (most synthetic) sources first. When the easiest source is exhausted, or at least when it ceases to produce striking new results, a new generation of historians takes the next easiest, and so on.

There is a cycle of sources, from more to less synthetic, with corresponding kinds of history. For early Latin American history, the main elements of the series are (1) contemporary books and other formal accounts, which we call "chronicles"; (2) official correspondence; (3) the internal records of institutions; (4) litigation; (5) notarial records. With the chronicles, a sort of narrative history is practically ready made; the scope of reference is then gradually reduced as one proceeds through the series until in the notarial records the historian is confronted with an individual item about one ordinary person on one day of his or her life. The sources also get less and less accessible as one proceeds down the list, both in the physical sense and in the sense of requiring more special skills for use. They become more primary, minute, local, fresh, and of more direct interest to social history.

The early Latin American field has now completed its first full cycle of surveying the (written) sources, and it is in this light that we can view the question of the relation of the newer, close social investigation to older types of work. Mainly we will find the succeeding stages complementary rather than directly contradictory; there is merely the difference between a less and a more complete view. One type of correction is usually necessary. At every step historians have assumed that the portion of reality they were working with was the whole reality, and have made generalizations accordingly. The non-existence of Spanish artisans, merchants, and women in the new areas was presumed from their near absence in the bare military narratives of the conquest. Scholars working in vast collections of metropolitan legal records came to imagine that the "state" was all-powerful, or at least all-important, while those working with ecclesiastical reports made the "church" the sole transmitter of Iberian civilization.

Epic and institutional history

The historians who used the chronicles to write the epic of the conquest and interpret the lives of the great conquerors did work of lasting validity, in the sense that their narrative facts are mainly correct, that they saw something of the sweep of the process and recognized many of the critical junctures. It goes without saying that commercial, technical, social, and ethnohistorical dimensions must be added to their picture. But aside from other dimensions, there is a strong element of social convention or tradition, unrecognized by the older writers, in the very ostensible acts of military conquest. The psychological portraits by Prescott or Ramón Iglesia, acute analysts of character though they were, often rested on a false supposition of the uniqueness of acts which were actually within a well defined tradition. Prescott thought only a spirit as daring as Cortés could conceive of Moctezuma's capture, not realizing that to seize the cacique was standard procedure. The social background juts into surface events in ways the older writers could not know. No one can fully understand Columbus's troubles as a governor of Spaniards without taking into account the abysmally low status and prestige of sailors in Spanish society, or the extreme Spanish contempt for foreigners.

The literary sensitivity of an Iglesia or a Marcel Bataillon is by no means outmoded; rather it requires extension. The art of subtle reading of documents represents one of the most real technical contributions we historians can make to colleagues in other disciplines, as the Swede Åke Wedin has shown, and it needs to be extended to all kinds of sources, not merely chronicles. But we have also seen by now that it is only too easy for textual criticism, in a vacuum, to outsmart itself. Sometimes the slightest glimmer of a contact with social reality, a triangulation through other kinds of documents, will answer a question more certainly than the most stunning textual pyrotechnics. For example, Bataillon used great virtuosity to impale the chronicler Pedro Gutiérrez de Santa Clara on his many plagiarisms, mistakes, and absurdities, and would surely convince a neutral, uninformed person that the writer had never left his native Mexico for Peru. Yet Juan

Pérez de Tudela (1963) points out many examples of the chronicler's sure touch on matters of social detail not to be found in his sources, proving his presence in Peru beyond reasonable doubt. From my own Peruvian research I could multiply the instances of Gutiérrez de Santa Clara's originality and authenticity.

The successors to the epic writers and commentators were the institutional historians who dominated Latin American historical scholarship during the time between world wars and somewhat beyond. With them too we find that the descriptive-analytical core of the work retains much validity, while the general perspective and conclusions need modification. The repeated occurrence of this phenomenon at every stage of the field's evolution is a negative commentary on the naive common belief that a work's ostensible "conclusions" are the most important part of it. Actually they are almost always the most ephemeral part. The classics of the field have had importance for their creative reconstruction and skillful presentation of important subject matter, including the identification of key concepts and patterns, and second for the method they used, as a model for other studies.

As one example, there is little wrong with Roberto Levillier's biography (1935–42) of late sixteenth-century Peruvian viceroy don Francisco de Toledo. The vicissitudes of the viceroy's official career and the extent of his legislation are there delineated in a way that for the most part we have no particular reason to change. But social history has made us aware that Viceroy Toledo did not single-handedly create the Spanish colony of Peru; in most cases he was merely codifying a state of affairs that had come about spontaneously in previous decades. The same holds true for such viceroys as Mendoza, Enríquez, and Velasco in Mexico. A process of rationalization and stock-taking occurred regularly about a generation after Spanish arrival in any given area, when the first great movement of creation and destruction had run its course. Robert Ricard's classic *Spiritual Conquest of Mexico* (1933) remains as a faithful reproduction of the mendicant orders' own view of themselves; it gives an admirably rationalized portrait of their internal history, aims, and methods—but not of their achievements. Basing himself only on friars' reports, Ricard thought the countryside was empty except for the friars and some very dimly seen Indians. We know now that the rural *doctrina* or parish was usually tied to an encomienda, and the encomienda in turn to an already existing Indian entity and jurisdiction; that the friars were outnumbered in the very areas where they were working by Spanish stewards and retainers of the encomenderos, by miners, small traders, and Hispanized black slaves. Rural church activity in the more settled areas, indeed, was often largely a function of the encomienda and of Spanish secular society. The widespread assumption, stemming from Ricard and other similar work, that the church was the primary conveyor of Spanish social-cultural influence is not tenable. In my opinion the great mechanism of Europeanization was not formal instruction but ordinary contact between Europeans and Indians, measured in manhours, and the primary Europeanizing agent was the local Iberian and already Iberianized population going about its daily business—not the church, except insofar as it was a part of that population.

Thus the institutionalists, until after World War II at least, mainly took their institutions at face value. They therefore produced an ideal picture, with emphasis on formal structure. The actual operation of the institutions at a local level, or indeed at any level, received little attention. It was inevitable that there would be a movement, dictated by both the sources and the logic of the subject, from this generalized and formalistic institutional history toward more individual or regional studies, and this movement, when the time came, had in it the germs of social history.

It is worthy of note that that massive monument of institutionalism, Clarence Haring's *Spanish Empire in America* (1947, actually conceived in the 1930's), though it has done perhaps more than any other work to reinforce the notion of a rigid and powerful Spanish state, was written with some realization of limitations and exceptions, as well as a strong sense for such tidbits of social history as appeared in the administrative sources Haring used.

The intellectual history written from closely related sources—official correspondence and pamphleteering—stands in much the same relation to social history as does formal institutionalism. Mainly concerning the controversies over Indians and the encomienda, it is unassailable as the history of a polemic, but tells little about the social reality beneath it (even though that dimension was often crucial in determining the positions of the participants in the debates). A certain sophistication is required to keep the two aspects apart; many readers of the intellectual historians have failed to maintain the distinction. In the case of the most famous exponent of this branch, Lewis Hanke, there was little enough excuse for readers' confusion. Hanke said repeatedly that he was studying attitudes and ideas.[1] In one memorable passage of *The Spanish Struggle for Justice* (1949, pp. 84–85), which struck me forcibly when I first read it as a graduate student, he not only disavowed any firm conclusions on the encomienda as a functioning organ, but put his finger on the lack of sources which would elucidate its ordinary operation and the necessity of locating such sources, if any should exist.

[1]This I always took to be Hanke's main emphasis, but at times (1971) he has implied belief in a stronger impact of laws on behavior than he had expressed earlier.

The social historians are not absolute skeptics about the power of law and formal ideas, nor are they in the main guilty of a simplistic distinction between law and reality. If there is such a thing as reality, then law is clearly a part of it. The frequently used term "social reality," as contrasted with law, is merely a convenient expression meaning the potentially observable, verifiable behavior and behavior patterns of an existing population. No one imagines that law is without relation even to this more narrowly defined social reality. But social history teaches us three basic lessons about the relationship: (1) Most Iberian legislation was reactive. If there was an ongoing dialectic of law and social reality, law's role was usually that of the antithesis. (2) Iberian law tended to be descriptive rather than actively formative. Charles Gibson has called it "an approximation of historical happening, or a commentary upon it" (1964, p. 235). (3) Legislation was written in a highly formulized style that almost amounted to a code, and even when directly influential could not and cannot be taken literally. For these reasons as well as for more obvious ones, it is generally hard to deduce much about social reality from a given law or debate prior to knowledge of the whole context. Most of the general principles of interpretation one can draw up are negative, such as the principle that legislative reiteration indicates noncompliance.

Social history in search of sources

Institutionalism held the stage for so long that an intellectual dissatisfaction with it became manifest rather far in advance of that true exhaustion of its characteristic sources that had helped bring the epic stage to a conclusion. Though there were still viceroys' biographies unwritten and Audiencia correspondence unread, important scholars in the field began to strain in the direction of social or economic history, usually staying as close to traditional sources as possible.

The simplest way was to work with legislation on social matters. This was the method of Richard Konetzke, who compiled relevant royal cedulas (1953–63), and also wrote some articles about social trends, based on these and similar materials. While valuable as one kind of formal institutional history, such work is no closer to social reality than any other legislative study. In fact it is more distant, for in the whole panoply of idealistic and quixotic royal ordinances, social legislation has a peculiar unreality. Not only were Spanish social concepts more archaic and artificial than concepts of administration and commerce, but the crown knew and cared far less about the amorphous society of the Indies than about the administrators it appointed or the export economy that produced its revenues.[2]

Actually, royal ordinances can be a very valuable source for social history. The cedularies of the Spanish crown contained decrees of two different kinds, *de oficio* and *de parte*. The former, ordinances of a general nature addressed to governors, were the main concern of investigators like Konetzke. But the more voluminous de parte decrees, concern-

[2]Guillermo Céspedes del Castillo (1957) used mainly metropolitan administrative sources to produce an exceptional synthesis of Spanish American social history for the sixteenth and seventeenth centuries. It is interesting to observe on the one hand how much Céspedes was able to achieve through intuition and sophistication, and on the other hand how the nature of the sources often forced even an acute, socially oriented scholar into a legalistic and metropolitan stance, with consequent retention of several received ideas that would be hard to defend today.

ing individuals, do not suffer from the typical shortcomings of the general legislation. Usually they are in response to petitions, and whether they concern the recovery of property, the grant of a coat of arms, or a recommendation, they give authentic information about a dimension of the individual in the Indies that is often not otherwise documented.

Frank Tannenbaum's *Slave and Citizen* (1946) was also an attempt to do social history with legal-institutional materials. It took for granted the strong, active state and church of the institutionalists; as these phantoms then dissolved under the light of closer study of society, the "Tannenbaum thesis" of mild treatment of slaves in Latin America was left with little to stand on. Yet Tannenbaum did have enough knowledge of Iberian society to see and emphasize that the Iberians possessed a living tradition or social convention of slave holding, and that having such a tradition was very different from having none. While research was pulling one leg from under Tannenbaum's thesis, it was adding support to the other. Though "mild" and "harsh" are probably the worst imaginable categories to measure it, there was a difference between Latin American slave-holding practice even in structurally similar situations, at least as to rate and manner of the absorption of the slave group into European society. Herbert Klein in *Slavery in the Americas* (1967) started with much the same sources and perspective as Tannenbaum, but, by virtue of some quite solid statistical information about employment patterns and relative positions of blacks and mulattoes in the later period, plus a more specific geographical focus and the sheer force of intuition, he put much more emphasis on social-economic practice than had Tannenbaum.

One of the most outstanding feats of legerdemain performed in the field at this stage was that of C. R. Boxer, who without going in any way beyond the traditional sources of "empire history" as practiced by the British—travelers' accounts and correspondence from both local and metropolitan officials—produced preliminary social-economic surveys (1952–65) of much of colonial Brazil that have long retained their usefulness and still cannot be considered entirely superseded. Boxer's relative lack of preconceptions, his faithful reporting of detail, his attention to reports of people on the spot, and his English traveler's eye for social and commercial significance, whatever the main thrust of the documentation, were all great strengths. Nevertheless, it is likely and indeed in some cases apparent that Boxer could not utterly transcend the limitations of the sources and the genre. Significant elements of the population remained untreated or unsuspected; the picture of others, such as the paulistas, is a typically one-dimensional "governor's portrait"; categories of analysis imposed by Boxer from the outside are now yielding to ones growing out of closer examination of the material (thus "plantation," or the anachronistic, Anglo-Saxon notion of "prejudice").

Another striking phenomenon of the period when the field was groping for social history sources was the work of Gilberto Freyre. With Freyre it becomes obvious, as was already implied to a certain extent with Tannenbaum and Boxer, that the sources are not necessarily and absolutely the only factor dictating what kinds of history are done. Some fields develop more clearly in this fashion than others; when there is a high degree of political or ideological interest in a subject, its study may veer far indeed from that steady march through the sources which, though perhaps blind, is natural, organic, and in a sense logical. Such deflection has been minimal in the early Latin American field. Some may think this is for the better, some for the worse. I do not deplore it. National image building is doubtless a valid and creative human endeavor; at the same time, it is often antithetical to the search for truth and the investigation of historical reality. Freyre's work is a major example of the intrusion of twentieth-century political-intellectual movements on pre-independence historiography. For historians, his writing was valuable above all as a stimulus toward social investigation, directing attention as it did away from formal structures while emphasizing the centrality of informal institutions, social conventions, and the ethnic-cultural make-up of the population.

Freyre's sources appear quite varied (with travelers' accounts after all at the core), but they do not merit prolonged discussion because he used them with impressionistic levity, as a guide to a private vision, in the manner of a novelist. One feels that in a sense his childhood was his most basic source, and that the rest was used to illustrate and bring alive a world already imagined. That world is childlike in its lack of any time dimension. The task of deciding what the primary chronological reference is in *The Masters and the Slaves* (1933) is exceedingly difficult; apparently the time is more than anything else the early nineteenth century, whether Freyre realized it or not. At any rate, one can deduce from subsequent writings by Boxer, Stuart Schwartz, and others, that Freyre projected a late and idealized version of the "plantation" back onto the whole colonial period, totally ignoring and implicitly denying the long and dynamic evolution of the sugar-producing complex and accompanying population.

It is unlikely that any basic advance could have been made toward an understanding of Iberoamerican society as long as the main sources used were legislation, chronicles, and official correspondence. Not only was little social information to be found there; until other sources gave a context, what social data did exist were unintelligible. It is difficult to convey to a layperson the utter uselessness of broad, synthesizing generalizations on social matters presented by contemporaries. "The sons of the conquerors are impoverished." "All Spaniards in the Indies are considered hidalgos." "Creoles are deprived of high office." Not one of these statements, found repeatedly in correspondence, is anywhere near the truth. Critical reflection might have convinced scholars that such dicta were not literally true. But even after we have concluded that a general statement is affected by formula or bias, what meaning can we attach to it? None, until we have either direct and reliable information on the careers of large numbers of individuals of the type referred to, or a relatively exhaustive statistical survey (the latter being in most cases impossible). Once a notion of the real state of things has been in some manner obtained, it is instructive to return to the original statement, which one now for the first time fully comprehends. Comparing the reality with the statement reveals the political position or other interest of the person who made it. In this way one can also acquire a

sense of the vocabulary being used; contemporary recipients of governors' reports understood them far better than we usually do today.

One redeeming feature of these sources was their inconsistency. While in general they built up a version of society approximately as distorted as the picture of the twentieth-century United States purveyed in political campaigns for the presidency, at times there was occasion for concrete references greatly at variance with their general tenor. Particularly in the chronicles one is forever meeting with suggestive examples: the black slaves escape from camp, and one becomes aware that there are black slaves; some Spanish women get killed, and their existence too is authenticated; guns are manufactured for approaching hostilities, and one becomes aware of the extent of local Spanish technical know-how and self-sufficiency. The most striking such anomaly in the chronicles, the building of brigantines for the siege of Tenochtitlan by Cortés's forces, led C. Harvey Gardiner to write a forward-looking biography (1950) of the man most responsible, ship's carpenter Martín López.

On the edges of institutionalism

At length, the field moved on to sources containing fuller, more direct, and more systematic information about various aspects of life—not necessarily leading to "social history" in our more specific sense; more often indeed, to economic or macrodemographic research. Such sources were of many kinds. Most typically they were the internal records of a well defined organization or institution, consisting of day-to-day entries which might have become the basis of reports, but themselves did not have that character. These sources include such documents as registers of emigrants or departing ships, parish records, tribute and tax rolls, and the like; census records, where they exist in any detail, are a somewhat similar source. Most materials of this nature lend themselves readily to a statistical approach, though there are some that do not, such as the minutes and other records of corporations—municipal councils, cathedral chapters, or charitable organizations.

Ever since the end of World War II there has been a strong interest in documents like these, and it will indubitably continue. The quest for organized and centralized sources is legitimate. Concentrated materials are not merely more convenient for the scholar; they make it possible to attain a higher degree of completeness, or to deal with longer time periods and broader areas, without exceeding human limitations. But inevitably the more centralized source is more manufactured, tampered with; it tends to force one into certain avenues of research regardless of one's real main interest. We get primarily what the persons writing the records were interested in communicating, rather than the facts and patterns they took for granted, which are usually precisely the matters of greatest interest to social history. In a statistically-oriented source, what is left out is often forever beyond reach. The more unorganized, haphazard, and miscellaneous the source, the more likely that one can discover new basic patterns, suspected or unsuspected—not as the principal subject matter of the document, but as a byproduct, something to be picked out by a perceptive reader. Of course the limitations of any one source need not be a serious matter if historians will stop the practice, too prevalent in our field, of basing a whole approach on one kind of source alone.

For economic history, the documents in organized series represented an obvious and important opportunity to determine amounts and trends for shipping, production, and prices. Such work as that of Pierre Chaunu on Atlantic shipping (1955–56) represents a very adequate response to this challenge (see also the work of Alvaro Jara on silver mining production). The pitfalls—lacunae and unofficial activity—are clear, and therefore relatively easy to compensate for. Both ships and silver mines were finite in number, highly concentrated geographically, and under quite close surveillance. We have every reason to accept the results of such research, particularly as to trends. It is always of interest to show their correlation, or lack of correlation, with trends in other aspects of society. For the rest, one can only say that work of this kind is not the answer to a social historian's prayer. By its nature it turns toward the most centralized, formally structured aspects of the imperial establishments; its sources are characterized by a paucity or total

lack of information about people and informal practices. Chaunu's work was actually in a way a step backwards for social history, since it redirected attention to metropolitan agencies and the sealanes, subjects which the field was tending to abandon as relatively peripheral to the story of the formation of Latin American society. As for silver mining, one can easily maintain that it was more than peripheral. Yet though it provided the economic base of European-style society in large parts of Spanish America, it also stood somewhat apart from the mainstream of social evolution in city and countryside. We know even in the twentieth century how easy it is for the mining sector to become an enclave. There is much greater direct significance for social history in such work as Enrique Florescano's study (1969) of the price of maize in eighteenth-century New Spain. Even here, because of the depersonalized nature of the source, almost no advance is made toward directly understanding the functioning of the haciendas that produced the maize.

Drawing social significance from serialized documents proved to be the special domain of the demographers, above all of Woodrow Borah and associates at Berkeley (1948 to present). Some may find it surprising that this branch of investigation appears here, only part way through our survey of sources and methods, when demography and the quantitative approaches connected with it have been held by many to be the field's most modern, advanced development. Certainly demography's potential is far from exhausted even today; but there is something beyond it, and no one type of endeavor can or should hold our exclusive attention forever. Macrodemography is very closely related, in both source material and method, to the kinds of economic research mentioned just above. Borah indeed has a distinguished record as an economic historian; he has worked, among other things, on price trends in colonial Mexico (1958).

It will emerge in the remainder of this paper that large-scale demography has been succeeded in part by an attempt to look more closely at various sides of social reality, as reflected in more local or less organized sources. This movement is parallel to and contemporary with a similar trend among European historians, though there the subsequent work has often itself remained primarily quantitative (see Franklin Mendels 1970). European historical demographers turned from the study of total population trends to more intensive investigations of smaller groups, going as far as the (rather skeletal) reconstitution of individual parishes and families, and in recent years demographers of Latin America have increasingly done the same sort of thing. The motivation is partly lack of confidence in the results obtained by macroinvestigation, partly the fact that knowledge about gross trends often reveals little about the structures and patterns of most interest to social historians.

Given Borah's own writings on method, this is not the place to treat demographic sources and methods per se. But surely there should be a dialogue between two approaches as closely related in subject matter and underlying theme as statistical demography and the close study of the lives of people. As to method, social history can make a contribution through helping to clarify and refine the categories. For the later period of census-taking, the place to start is no doubt an intense study of the use of increasingly elaborate terminology in the censuses themselves. Changes and ambiguities in the census-takers' use of categories can tell us as much, about both social concepts and social realities, as the raw figures. In work on the earlier period, the categories remain broad and relatively undefined, in striking contrast to precisely articulated procedures of source analysis and extrapolation. To get at the content of these categories, one must go beyond the "demographic" sources proper. Advances in broad-based, close study of people's lives have gradually done much to fill in what it meant to be "Spanish," "Indian," and "mestizo" at given times and places, so that we can better interpret the statistical results, but much still remains to be done in this respect.[3]

Nor are the actual census categories the only ones important for interpretation. Other tools of analysis, such as the category "labor," need refining. For example, demography

[3]Borah himself gave a good beginning to the enterprise of category analysis in his penetrating, almost unknown essay, "Race and Class in Mexico" (1954).

tells us that the Indian population of Mexico declined while the black population rose. Nothing is more natural than to suppose (with David Davidson, 1966) that the blacks were brought in to replace Indians, labor to replace labor. In lowland areas where the Indian population almost disappeared, this interpretation seems to hold true. But for an area such as central Mexico, investigation of what blacks were doing shows them mainly in various intensive, skilled, or responsible activities, and certainly not replacing Indians in the maize fields. Once the role of the blacks is understood, we can interpret the increase in their numbers as related to (1) the growth of the Spanish world with its infinite need for auxiliaries; (2) the continuing lack of enough fully acculturated Indians for such posts; (3) the growth of wealth and capacity to buy slaves. There seems hardly any relation to raw Indian numbers.

The demographers have not made their own special sources and subject matter into a total explanation of Latin American history to quite the same extent as did many of their predecessors, although the tendency can be detected. If a generation of undergraduates was taught that Indian population decline was the reason for falling silver production and the rise of the hacienda, the fault was only partly that of Woodrow Borah, who in *New Spain's Century of Depression* (1951) was careful to weigh other factors as well. But until various close studies of a more local nature could be made, it was really impossible to weigh other factors. As to mining, Peter Bakewell's work on Zacatecas in the sixteenth and seventeenth centuries (1971) and David Brading's on Guanajuato in the eighteenth (also 1971) prove definitively that the size of the overall Indian population of the country was a negligible factor. Silver deposits, mercury supplies, technology, finance, and organization seem to be the totality of relevant variables. Insofar as labor availability was important at all as a determining factor, it can only have been skilled, experienced labor; even at the bottom of the curve, there were enough Indians, many times over, to work the mines, if simple numbers would have availed. The case of the great center of Zacatecas is striking. The town grew, mines extended, and production went up, while the Mexican Indian population went down. Zacatecas hit a peak in the first three decades of the seventeenth century when Indian population was reaching a low point, then declined through the later part of the century as Indian population began to recover.

For the hacienda, such clarity cannot be attained. Indian population decline was certainly important in affecting the chronology of the hacienda's development in some areas; that it hastened its rise was all Borah ever said. But the first systematic work on the evolution of the hacienda in a single area, Mario Góngora's study of the history of landholding in Chile's Valley del Puangue (1956), not only presents resources, markets, and the growth of the Spanish world as the essential motor of development, but specifically denies any straightforward connection between the granting of land and Indian population loss. Later William Taylor (1972) and Robert Keith (1976) were to publish regional studies from which one can deduce, in the first case, that the lack of a strong local Hispanic market severely retarded the development of the mature hacienda system, and in the second case, that the presence of such a market soon brought it into existence. François Chevalier, too, though he like his contemporary Borah tended to explain the hacienda largely as a response to depression and population loss, gave a relatively full-bodied portrait of the institution, which, by showing its social coherence and interconnection with every stratum and aspect of Spanish American society, implied that it might in some form be almost coterminous with that society. My own work on Peru in the conquest period (1968) showed that social and economic goals, patterns of behavior, and types of organization usually associated with the hacienda were then already dominant, in loose association with the encomienda, so that with the predictable growth of Spanish society the predominance of something like the hacienda seems inevitable.

The general perspective rising from closer studies is considerably different from that which arises from macrodemography. It would appear from the former that trends in pure numbers of people rarely if ever actually originate social patterns; such trends work mainly to accelerate or decelerate developments rooted in society, culture, and technology. Even in the matter of the timing of important trends, close substantive studies usually give at least as much importance to the growth of the Spanish world (only partly a demographic phenomenon) as to decline in Indian population, despite the great disparity in sheer numbers in the early and middle colonial periods. We become aware that with the broad, unarticulated demographic categories necessary for the earlier period, a correlation cannot be presumed to be an explanation. It does, however, represent a hypothesis and direction for research.

Smaller-scale, more intense statistical studies can reconstitute more refined information from sources not organized to deliver it in any straightforward way, and by the 1970's such studies were ceasing to become a rarity. Edgar F. Love (1971), for example, studied exhaustively the ethnic status of the marriage partners of blacks and mulattoes in a Mexico City parish for a hundred-year period in the seventeenth and eighteenth centuries. While the results were not unexpected, Love established the nature and extent of mixture among lower urban groups beyond all reasonable dispute. For eighteenth-century Chile, Jacques Barbier (1970) used the extensive genealogical literature on the Chilean upper class to carry out a study which measures statistically the degree of endogamy of holders of titles, offices, and property entails, making quite fine distinctions of category and chronology. Love's study sticks close to the parish register, while Barbier ranges more widely and makes illuminating use of more substantive accounts of the careers of certain individuals, which is of course easier to do with a group much written about.[4] Barbier's article was one of several indications around this time that by the mid-eighteenth century an upper group of creoles was in a position of considerable wealth and great official influence almost everywhere in the Indies. Research of these types could readily and usefully be combined with the career pattern approach of social history proper, but such has rarely if ever happened. In the era of late Bourbon census-taking, there are many possibilities. The censuses themselves are so much more methodical and articulated that statistical-

[4]This work and more has since been incorporated into Barbier's book *Reform and Politics* (1980).

demographic work with them could serve as a time-saver and a sensitive guide for more direct studies of social configurations.

Some of the sources of the serial type seem to lend themselves not only to statistical compilations, but to a more immediate study of social patterns. Outstanding among these are the registers of permits issued to New World emigrants, such as the Archive of the Indies began to publish in its *Pasajeros a Indias* (Bermúdez Plata, 1940–46). The entries give not only the emigrant's name but usually the names of his parents, his birthplace, and his destination. Sometimes occupation and other particulars are added. Seeing the apparent potential of such a source, Peter Boyd-Bowman (1964–68) made an ambitious attempt to go over from the purely statistical approach of compiling discrete bits of data which lack intelligibility except in the aggregate, to the large-scale accumulation of actual case histories, each of which would have its own directly perceptible pattern.[5] It would thus be possible to manipulate large amounts of data while retaining direct comprehensibility at every step, with a consequent gain in reliability of conclusions and interpretations; above all, patterns would emerge that could never come out of more atomistic data. But the documentary base proved unable to sustain this pretty dream. The entries of the Seville registers do not tell enough about the emigrants; so few occupations are listed as to give a false impression. Above all, the broad Atlantic stretches as an unbridgeable gap. Though Boyd-Bowman has utilized various supplementary sources, there is no equally methodical and centralized listing in the Indies; as a result of the limited repertory of names and deliberate repetition of the same name in families, it is usually impossible to identify a given person in the New World with his homonym in the Pasajeros, even if you can find him, without other kinds of evidence. Failure to recognize this caused social history pioneer Tomás Thayer Ojeda recklessly to identify the conquerors of Chile with Pasajero entries,

[5] A brief statistical summary of this and later compilation subsequently appeared in Boyd-Bowman's "Patterns of Spanish Emigration to the Indies until 1600" (1976).

bringing about an extraordinary longevity among that group.

For macroresearch, then, the Pasajeros have turned out to be useful mainly in the same way as other sources of this type: as the basis of statistics on gross trends for the kinds of data they provide systematically, above all information about regional origin. In many other respects, Boyd-Bowman's *Indice* is an excellent research tool. The absolute volume of migration will probably long remain uncertain, but the work of Boyd-Bowman and others has established that the general order of magnitude was greater, at an earlier time, than once thought. From the beginnings, there were enough Iberians in the New World to form complex, largely self-contained societies. Also the quick transition from an initial, geographically determined, overwhelming Andalusian predominance to a broad cross-section of Castilians is a very clear trend. The significance of it is not so clear; we are as far as ever from knowing whether Spanish American civilization can be said to have Andalusian origins.

At one time I had a rather strong distrust of the Pasajeros as a source for investigating trends of immigration, particularly for working out short-term shifts and trends for individual areas. The reasons were several. Those who have watched the process closely know that many people who received permits to go to the Indies actually never left Spain; others, a very large proportion, went to areas different from their declared destination. On the other hand, many or most emigrants for one reason or another went unregistered; at least this is a natural conclusion to draw from the fact that only a small fraction of the names of Spaniards in Peru in the conquest period can be located in the Pasajeros. All this induced me to carry out a separate investigation of the origins of Peruvian Spaniards (1968, pp. 237–39), counting all I could find in all types of sources, with the single criterion that they must have been physically in Peru. The resulting list turned out to have a regional distribution very close indeed to that seen in the entries in the Pasajeros giving declared emigrants to Peru for the same years. Apparently the biases balance out or are irrelevant, and the emigrant registers are a trustworthy and sensitive indicator of trends in overall volume and regional composition.

Whenever and wherever similar registers can be found on this side of the Atlantic, they can easily become the basis for both statistical surveys and systematic observation of careers, informal structures, and behavior patterns. An outstanding example of such a document is the complete or nearly complete listing of the European-born Spaniards in Mexico City in 1689, published by J. Ignacio Rubio Mañé (1966). The original document gives each individual's name, Spanish origin, and present occupation, and often the approximate wealth and time of arrival as well; Rubio Mañé also located over half of the Spaniards in local parish records of marriages and burials. Even a quick statistical overview of the data shows much of interest: for example, how very few European Spaniards were in government, and how many were in humble positions. A year of research, tracing these same individuals in all kinds of other Mexican documentation, would enable one to write an illuminating book. On the basis of multiple career patterns, it would show the nature of the first-generation Spaniards' ties to and role in the local society and economy, and it would help in the process of refining what have been two of the field's most corrupt categories, "creole" and "peninsular."

Another source which might have the potential for a macrostatistical approach on the basis of perceived patterns rather than discrete phenomena is the famous set of regional surveys we call the *Relaciones geográficas*. However, they tend more to economic than to social detail, and an attempt by Alejandra Moreno Toscano (1968) to use French quasi-statistical methods on them led neither to new conclusions nor to appreciable refinement of old ones. Like the emigrant registers, the Relaciones geográficas seem primarily helpful as research tools, as one resource we can use in reconstructing individual careers and local communities, but insufficient in themselves.

Institutional sources of various kinds also offer opportunities for close studies of administrative entities and personnel, with inevitable social implications. The social aspect first became an important and integral part of institutional studies with John Phelan's *Kingdom of Quito in the Seventeenth Century* (1967). At root a study of Quito's Audiencia, and by no means mainly intended as social history, the work nevertheless gives a full-length portrait of the president, Antonio de Morga, with attention to the shape of his career as typical of advancement patterns, and with much evidence of the intertwining of his official and social life. Aside from the usual materials, Phelan made an intensive study of *visita* proceedings, which with the related *residencias* loom ever larger as an important source for the crucial informal activities of officials.

The next step was to deal systematically with the careers of larger numbers of officials. David Brading did this, among other things, in his *Miners and Merchants in Bourbon Mexico* (1971), for the Audiencia of New Spain, and Leon Campbell shortly thereafter published an article (1972) with similar research on the Audiencia of Lima. Both reached revolutionary conclusions on the old question of whether creoles were deprived of high office in the Spanish Indies. Their sources were standard, except for close attention to visitas and other litigation; anyone who had had the perspicacity to distrust creole correspondence could have discovered these facts long ago, by merely asking the question: *who* were the members of the Audiencias?

For his *Sovereignty and Society* (1973), dealing with the *Relação* or high court of colonial Brazil, Stuart Schwartz discovered and exploited an even more centralized and informative source, the metropolitan files of appointment dossiers, which give the judges' social and regional origins, education, and previous careers. These data, together with study of their activities, marriages, and investments in Brazil, made it possible to assemble capsule biographies of large numbers of officials over a long period of time. As a result, one can establish the important interrelation between social status or regional origin on the one hand and official activity on the other. An ongoing process of absorption of some of the judges into the upper levels of local Brazilian society emerges as a clear trend. Others, however, eventually went on to different assignments in the transatlantic Portuguese world. This is social history pure and simple; at the same time, it gives the main elements for explaining the nature of the court and its official activity. A mere study of the court's formal organization and of the legal philosophy and intellectual caliber of its members could never give as good a basis for understanding the general role and profile of the tribunal.[6]

On the face of it, the minutes of local corporations appear promising for social history, since they deal with relatively large numbers of people in a rather normal daily situation, nearly outside the great hierarchies. But municipal council records, the most common such source, have led to little beyond conventional formalistic treatment. Social historians who have wandered among these materials have usually become disillusioned with them, largely because of their aridity in personal detail; even so, establishing council membership is always a good tool in identifying and tracing the trajectory of important local families and interests, when done in conjunction with other types of research on the region (as seen for example in Bakewell's 1971 study). The debate between Boxer (1965) and Dauril Alden (1968) over whether or not Brazilian councils were aristocratic and self-perpetuating could never be resolved out of those records alone. The question must be settled by establishing family connections and status through study of other local documents. Once a deep study of a local society has been made, the dry council records come to life. Where before one saw only the admission of so-and-so to the council, one may now recognize that a body of estate owners is admitting its first merchant to membership, with all that action implies.

A source of this type that is much more amenable to social history are the records of lay brotherhoods, at least those of the Portuguese *Casa da Misericórdia*, as proved by A. J. R. Russell-Wood in his *Fidalgos and Philanthropists* (1968). These documents include a much larger slice of life than those of the municipal councils; the Misericórdia was a prestigious organization which was led by the rich, but also admitted

[6]Carrying the evolution a step further, Mark Burkholder and D. S. Chandler later published a book (1977) based on a survey of dossiers for all the Spanish American Audiencias from the late seventeenth century to the end of the colonial period; in a study of this scope, of course, they were not able to look closely into local ties and activities of the judges in the many different areas.

plebeian members and was in contact with the poor. Because of legacies left by members, the Misericórdia transcribed many testaments and property inventories, and admission procedures often necessitated a rather realistic and explicit evaluation of social status. Thus one gets at least a glimpse of family fortunes over some generations, of changing investment patterns, of the degree of social mobility, and of some important social types or categories.

On the basis of the brotherhood records, Russell-Wood clearly demonstrated the existence of a complex urban society in Brazil from an early time, a society amazingly like its Spanish American equivalent in organization, function, and tendency. Whatever its economic base, colonial Brazil was no mere rural plantation society; the master-slave dichotomy is not an adequate analytical tool to comprehend it. What we see is a complete European-type urban-oriented society in operation, with a strong tendency to grow because of entry of European immigrants into its middle levels and Africans into its lower levels.

The Misericórdia records have the great advantage of containing social data in a highly concentrated, centralized, and usable form, so that one can quickly get a long time perspective; Russell-Wood ranges over some two hundred years. On the other hand, Misericórdia documentation is not sufficient in itself; no type of document really is, but there *are* collections which concern the main activities of the people who figure in them and thus catch them head-on—administrative sources for administrators, estate records for estate employees. The Misericórdia was not anyone's main activity, and it is not possible with its records alone to construct good career samples, so that very basic aspects remain untouched. For example, though one thrust of the new material was to show the extent of urban-centeredness of the whole society, arousing the suspicion that Brazilian estate owners might have been as urban-oriented as their Spanish American counterparts, Russell-Wood retained the concept of a rural aristocracy as worked out in the previous generation by Boxer and Freyre. The Misericórdia records contribute literally nothing to this subject, except that the putative rural aristocrats belonged to at least one urban organization, the

Misericórdia. It was left to later scholars using wider sources to approach closer to the matter.[7]

Apparently the records of other brotherhoods will be even less able than those of the Misericórdia to serve as the sole basis of social investigations. The Misericórdia had a dominant, nearly monopolistic position among the Portuguese organizations; no single Spanish brotherhood could compare with it, and some of its functions were carried on in Spanish America by various branches of the church proper. There the brotherhood records are often consolidated today in archives of the *Beneficencia*. Taken together, these materials seem to have much the same characteristics as Misericórdia records. They can serve as an initial guide, and they permit surveying rather long periods of time; but though a fresh and intimate source, they are too peripheral to large areas of social life to suffice alone.

A special phenomenon in the field during the early 1970's was the appearance of Murdo MacLeod's *Spanish Central America: A Socioeconomic History* (1973), which put the historiography of that region on a new and higher level. Clearly affected by Borah and the French school, interested in demography and economics, as well as in society, as the title proclaims, MacLeod nevertheless produced a book which is essentially based on central governmental reports in the institutionalist manner. The focus was changed, but not the method, though much greater sophistication was exercised in interpreting officials' statements than in earlier institutional studies. MacLeod's procedure was fully justified by the need to start from scratch and provide an initial framework in a significant area that had until then been virtually a historiographical vacuum. What he was able to extract from

[7] Rae Flory's extensive doctoral dissertation (1978) surveys northeastern Brazilian social types and structures in the late seventeenth and early eighteenth centuries, placing the sugar mill owners in a much wider and more realistic context, on the basis of notarial and other local records. It is lamentable that this fine and innovative study has never been published. An article by Flory and David Grant Smith (1978), using copious individual examples in addition to a statistical overview of the groups' characteristics, shows extensive merchant-planter interpenetration.

the materials he used was primarily a macroeconomic survey of the region over two centuries, no mean achievement, but the internal organization of the main estate types and commercial endeavors remained to be studied, not to speak of society itself in either Spanish or indigenous spheres.

Beyond institutional sources

The inadequacy of a single source for carrying out social-economic investigation gradually became apparent to many scholars; partly for that reason, a search has gone on for other principles of limitation, to restrict the field of vision enough to allow deep exploration in all kinds of sources, yet include a coherent universe. Actually, the documentary base has continued to assert its strength, and most such studies in the end rely mainly on a more or less compact and homogeneous body of documents.

One rather straightforward way to attack the problem is to do a complete anatomy of a small, well defined group of people, both as a random sample of society and as a primary observatory for certain patterns operating at small-group level. By choosing strategically located samples one can achieve results with a broad significance and yet virtually eliminate selectivity. The great trouble is to find a group which includes humble people, yet is well enough documented. So far almost the only such studies are of contingents of conquerors, who started from obscurity but later became associated with wealth and notable deeds, and thus appear with regularity in all kinds of documents.

The first study of this type was the pioneering book of Tomás Thayer Ojeda, *Valdivia y sus compañeros* (1950), which examines the group characteristics of the conquerors of Chile, as to both backgrounds and later careers. Thayer Ojeda had emerged from a quasi-genealogical tradition to produce a monumental collection of biographies of a very large portion of the Spanish Chilean population in the conquest period (1939-41), but lacking statistical compilation or systematic discussion of trends. While he sought his individuals in all known sources, he wrote as a Chilean-Hispanic patriot, and thus de-emphasized artisanry and mercantile activity while rather easily accepting claims of nobility. Out of a lifetime's research and publication he accumulated enough data to put together a list of Chile's first conquerors that seems literally complete, with compilations of vital statistics and career patterns.[8]

Many areas had a larger population than Chile, and it is

[8]Eulalia Lobo (1967) carried out a useful social statistical analysis of Thayer's work, but any such attempt must naturally remain the captive of Thayer's outmoded categories and his other distorting tendencies.

An important documentary resource for biographical studies is the large corpus of memorials of services performed (*probanzas de méritos, de servicios*) presented by candidates for honors, titles, grants, or offices. Since they constitute statements by interested parties and are in the form of questionnaires accompanied by duly sworn testimony, they have, as a source, much in common with litigation. Probanzas are extremely easy to misuse; Gardiner, for example, was taken in by his subject Martín López's self-serving probanza campaign. An inflated, unbalanced biographical sketch can be readily concocted merely by rephrasing the questionnaire. Reading a dozen memorials will convince the scholar that all honors seekers claimed to be brave if not heroic, fanatically loyal, of noble birth, and impoverished after having spent their last penny in the service of the crown.

But there is no need to despair; much is still to be learned from probanzas. As incredibly distorted as they often are, they hardly ever contain outright falsehoods. If a person claims presence at a battle, we may assume he was there, though we should not forget that he may have been on the wrong side. A probanza usually gives a usable chronological-geographical framework for a life. Although the language of the genre is inflated, it has its own conventions; the experienced investigator can do quite well at estimating social rank from the convoluted statements. Thus a man described as a "persona honrada y principal, cristiano viejo, temoroso de Dios, hijodalgo," will infallibly prove to have been a rather humble fellow, because if he had been sure of his hidalgo status he would have put it first. Each claimant picked as witnesses his relatives, friends, compatriots, and political allies; witnesses were usually required to give their birthplace. If the researcher has gained any familiarity with the society he is studying, the witness list is pure gold, since it can reveal the claimant's regional, social, and political affiliations with great precision. A careful reading and the comparison of the witnesses' statements will not only tell far more about the claimant than is in the mere questionnaire, but will often unearth as much or more social-biographical information about the witnesses and the whole circle as about the principal figure.

not likely that any large number of scholars will have as many years to devote to preparatory work as Thayer did. Usually there must be a unitary base document which at least establishes the identity of the group. In his *Grupos de conquistadores en Tierra Firme* (1962), Mario Góngora utilized a document which did that and much more. A contemporary survey of the first encomenderos of Panama (found in a residencia, by the way) included not only their names, regional origins, and time of arrival in the Indies, but frank declarations of occupation or of the father's social rank. On the basis of this material, Góngora was able to carry out a sophisticated analysis of the Spanish conqueror as a social type or types. But the Panamanian documentation did not allow construction of further career patterns.

I, too, have attempted this genre in *The Men of Cajamarca*, a study of the 168 Spaniards who seized the Inca emperor at Cajamarca in 1532. Their names all appear on a treasure distribution list preserved because of the enormous amounts involved. Their subsequent eminence makes it possible to trace the lives of most of them, from the Pizarros to the expedition's black crier and piper, and to show the patterns common to them all.

The people taking part up and down this path of conquest from Panama through Peru to Chile were much the same: groups with great internal diversity of social and regional origin, occupation and faction, each expedition an operating microcosm of Spanish society. Their behavior was highly stereotyped. Motives of adventurousness have to be practically eliminated from the reasons underlying their conduct. They acted on a rational view of their own self-interest, all aiming at the same kind of permanent and seigneurial eminence. According to their wealth and social degree, they chose between a governorship, a splendid life in their Spanish birthplace, or an encomienda in the Indies. In any situation that had lasting attraction, they were not easily swept aside, but played an important role in founding a Spanish society and setting social patterns which survived all subsequent governmental and ecclesiastical assaults.

The group anatomy technique could have many fruitful applications, but it has remained and in all probability will remain restricted principally to groups at the higher levels of society, simply because of lack of exhaustive information at the lower levels. Indeed, Schwartz's study of the Relação and Brading's of miners and merchants of Guanajuato involve virtually complete group anatomies. At any rate, any document which gives a complete listing of a group of human beings, whether governmental, residential, or commercial, has immense potential and deserves the close attention of the historian who happens to stumble upon it.

Another approach has been to concentrate on the study of one broad topic, a sector of economic activity or of the population, with well defined limits of time and region in view of the depth and breadth of the reconstruction and the records used. A natural subject for this technique is mining, which is a highly concentrated and profitable activity generating a disproportionate amount of records. Our first close-up portrait of the operation of any branch of Spanish American production or commerce was Robert C. West's study of the Parral mining district of northern Mexico (1949), followed by his treatment of placer mining in New Granada (1952). Because West was a geographer, his work was anything but social history, yet its emphasis was on describing and understanding basic operations rather than on statistics. West's work began a tradition of mining history which has veered more and more toward the social aspect without, as is natural, ever abandoning concern with prices, wages, and production. One such work is Alfredo Castillero Calvo's *Estructuras sociales y económicas de Veragua* (1967),[9] in which the author not only writes the internal history of the Panamanian gold mines, but uses comparisons with West to trace the main lines of a common Spanish American gold mining society, and investigates the lasting consequences of the mines for the towns and farms of western Panama.

[9] A somewhat related development was the preliminary work of Lewis Hanke, often in collaboration with Gunnar Mendoza, on Potosí, with an emphasis more social than technical. Hanke edited chronicles, described and gave samples of archival materials including notarial records, and provided provisional syntheses. See Hanke ;, 1965, and Capoche 1959. In later years Peter Bakewell took up hallenge and provided us with two full-scale books (1984, 1988) mary, socially oriented research on the Potosí mines.

A further development came when two Englishmen set about the study of the great Mexican silver mining centers, Zacatecas and Guanajuato. Here the available documentation is far more detailed, varied, and voluminous than for a site like Parral. P. J. Bakewell for his 1971 book on Zacatecas to 1700 worked through an impressive amount of local administrative, judicial, and even notarial records, and wrote a complete set of chapters on varied aspects of Zacatecas industry, agriculture, society, and government. The work was the result of the most complete survey of the local records of a given district, its people, and its economic activity that had been carried out to that time, and it correspondingly gave the fullest and most integrated portrait of a local society and economy, laying bare its basic interlocking processes and their trends over a period of some generations. On the other hand, with such a strong and valuable regional concentration, the interregional connections of the district, especially commercial links with firms in the Mexican capital, went uninvestigated. I have previously mentioned some important substantive conclusions of this study, and I will have occasion to mention others when we come to speak of the hacienda.

David Brading's *Miners and Merchants in Bourbon Mexico* (1971) deals with eighteenth-century Guanajuato on the basis of a survey of local documentation almost as thorough as that of Bakewell, except that he did not make as much use of notarial records. These were substituted in part by a detailed and refined local census. But Brading's is a very different, broader book, starting with government at the national level, proceeding to a thorough investigation of the organization of import-export commerce based in Mexico City, and extending to the silver mines only as a necessary last step. Brading's interest was especially in immigration and the upper levels of mining and commerce. He began systematically to follow careers, families, and important firms, integrating information from wherever he could find it until he had multidimensional portraits of large numbers of important figures. Merchant guild litigation containing inventories and testaments was one crucial source, but there were others. Finding that his people's lives stretched widely over the Mexico of the time, he followed them equally in the archives of Seville, Mexico City, and Guanajuato, and he investigated their connections with the Consulado, the administration, and the nobility. In the end, on the basis of patterns visible in multiple, exhaustively studied examples, Brading succeeded in describing recruitment patterns, social trends, and structure of both mining and international commerce on a countrywide scale, without resorting to impressionism. One of the many social mechanisms he demonstrated is that of the Spanish-born Mexican merchants who hand over their businesses to new immigrants (typically relatives from the hometown, specifically sent for and trained up), while the merchants' landed property and perhaps title go to their creole offspring. Such insights, taken with Brading's previously-mentioned work on creole officeholding, brought a new understanding to replace the old creole-peninsular stereotypes.

Another in this series of works is Enrique Otte's study of Cubagua, the island base of the Caribbean pearl fishery in the first half of the sixteenth century.[10] Otte demonstrates here, as in his many articles, how much coherent social-economic detail exists in the Archive of the Indies, if one is willing to explore the sections Justicia, Contaduría, and Contratación. Otte, like Brading, used the technique of synthesizing careers of many important individuals and firms from widely scattered data. He made particularly enlightening use of private letters, infinitely more frank and revealing than official correspondence.[11] While Cubagua is somewhat peripheral and hermetic, Otte does full justice to social aspects. The book will stand as a demonstration of what Spaniards would do if put on a desert island. What they did on this one was to concentrate on the primary (and only) export industry and organize a municipality, with the largest investors in the pearl industry on the city council. These quasi-patriarchs started building large urban houses and forming estates, until the pearls gave out and the whole venture was abandoned.

Extractive industries and related commercial activity thus make a very practicable framework for social investigation,

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[10]Otte's *Perlas del Caribe* was in many ways complete, and I became acquainted with the work, more than a decade before its appearance in 1977.

[11]In the classification of sources above (p. 30) there might logically have been a category "private correspondence," falling just after official correspondence. But the Iberians have never been noted for letter-writing, memoirs, and the like, and what they did write in that style was far more exposed to loss than was more public documentation. The most diligent searches have not to this date unearthed enough coherent collections of private correspondence to serve as the basis for an approach; thus the field was spared an otherwise probably inevitable period of domination by shallow biographies of luminaries.

Published collections of letters, even the best, such as Porras Barrenechea's *Cartas del Perú*, or Pérez de Tudela's edition of the Gasca-Pizarro papers, have been mainly official in nature. Any private letters they contain usually proceed from trial records into which transcriptions were introduced as evidence. The archives contain many more such letters, but although they are enormously suggestive, they are so scattered that it is not likely that they can ever be other than welcome windfalls for more broadly-based research projects.

The most consistent correspondents were perhaps the merchants, who were also forever involved in litigation. Some of their letters are almost book-size, and at times they can be found close enough together to be studied systematically. Here Otte has led the way in creating the genre of the article which is part synthesis and comment, part publication of letters (1966b, 1968).

Otte has also discovered and exploited (1969) the letters which New World settlers sent back to relatives in Spain; if the recipients later decided to emigrate, they would present such letters to the officials, causing them to be preserved. Some lots are sufficiently concentrated to serve as the beginning of an extraordinarily intimate, if somewhat impressionistic and one-sided portrait of settler society in a given locality (Otte 1966). In any case, the letters possess remarkable human interest, and the settlers appear in a fresh perspective as immigrants building up their own version of the myth of a new land of opportunity. We also see an aspect of the settlers' vocabulary and conceptual equipment that is otherwise hidden from view.

Feeling the desirability of making a wider public acquainted with these appealing materials and some of their significance, Otte and I published a collection in translation, with extensive comment, in 1976 (*Letters and People of the Spanish Indies*). Much later Otte published a monumental collection of sixteenth-century private letters (1988).

with many implications for the society as a whole. There is no inherent reason why the tracing of lives cannot be extended down at least as far as the ordinary mine workers; even without doing this, Brading and Bakewell have given us a reasonably articulated and realistic picture of that stratum. But for all the importance of mining, it is somewhat detached from the general evolution of society. The same kinds of considerations hold true for import-export commerce, of which these very works of Brading's and Otte's were until recently the principal studies with social content.

Another sector standing out as a distinct unit of study are the Indians. Serious work on postconquest Indians began at almost the same time as mining studies, with Charles Gibson's *Tlaxcala in the Sixteenth Century* (1952). One might expect that there would have been a corpus of anthropological writing on which to build, but such was not the case. Anthropologists had studiously avoided the postconquest period, as if they knew that Gibson would come. Their interest in late pre-Columbian society itself, as opposed to archaeology and artifacts, was slow to develop; they knew approximately as much about this subject as we would know about sixteenth-century colonial society if we had only the *Recopilación de Leyes* and some late chronicles. Therefore Gibson essentially had to start from scratch. His methods and sources (though he was anthropologically well informed) were not those of anthropology, but of Latin American history as it had been developing over the years. His field of interest was wide; nevertheless, in *Tlaxcala*, and even more in his broad-based *Aztecs Under Spanish Rule* (1964), he more than anything did institutional, corporate, jurisdictional history (although at a new level). Such an approach almost imposes itself when a new area of the discipline is being opened up, not to speak of the extraordinary difficulty of getting at the actual lives of Indians.

Thus in one sense Gibson's work had little to do with social history, beyond setting a framework for it. In other ways there were close affinities. If the bulk of Gibson's documentation was in one way or another administrative, it was also very much at a local level, rural in emphasis, and relating to people who if not exactly uniformly humble had at

least, up to that time, been largely left out of historical consideration. Like the social historians, Gibson became an advanced skeptic about the active powers of the state, and he produced an account of the evolution of the Indian communities in which broadly sociocultural forces are the active principle, with formal law only the legitimizing stamp added after the fact. Above all, Gibson worked at the same level of profundity as the social historian. Accepting few ready-made categories, he resynthesized his categories out of the actual usage of the time. And like the social historian again, Gibson aimed not so much at "conclusions" as at deep-going analysis that changed our very way of viewing things. This is the rationale of Gibson's passion for detail; some of those who do not understand it would do well to imitate it.

In due course, studies analogous to Gibson's began in Peru. A strong impetus came from the work of the anthropologist John Murra. His publication and analysis of amazingly detailed visitas or inspections of Indian provinces stand within anthropology's tradition of primary interest in pre-Columbian times, but the visitas themselves are early post-conquest, and they have great potential as a starting place for intensive local studies, although such have not to date materialized in the form one might have envisioned.

An important regional study on Peruvian Indians is Karen Spalding's dissertation "Indian Rural Society in Colonial Peru: The Example of Huarochirí" (1967). Its scope might be described as halfway between *Tlaxcala* and *The Aztecs*. With sources much like Gibson's, it established jurisdictional-administrative entities and sociopolitical categories and trends, all in all quite reminiscent of central Mexico. As the title indicates, one object of the study was to go beyond the Gibsonian emphasis to concentration on society proper. At this point sources once again asserted their weight. The types of documents sufficient for Gibson's purpose were not necessarily enough to penetrate into the ordinary lives of rural Indians. In the Huarochirí study, sophisticated use of "fragments of information included almost unconsciously by the author of a document" at least made possible the nearest approach yet. Such a close view showed even greater change, internal variety, and mobility (especially spatial mobility) than Gibson's picture, some of which must be attributed to different approaches, some to true differences between the regions. Spalding's approach also revealed somewhat more about both indigenous social organization and the Spanish social-cultural impact on the Indians, whether through migration to cities and mines, or through the presence of a growing Spanish element in the country. However, no records appeared to exist for the Huarochirí district that would yield a truly intimate portrait of indigenous society below the level of the Indian lords.[12]

At times Spalding made skillful, somewhat wistful use of notarial records from the provincial town of Huánuco, which happens not to fall within the Huarochirí district. Apparently we will have to seek meaningful detail on rural Indians wherever we can find it, rather than choose a specific restricted locality for study purely on the basis of intrinsic interest. The most propitious situations often disappoint us. My own first venture in the direction of ethnohistory came in 1969 when I sampled some very complete notarial and judicial documents from the latter sixteenth century in the Mexican provincial center of Toluca, which was then still nominally an Indian town, with an Indian cabildo and a *corregidor de indios*. It soon became apparent, however, that the core of Toluca was occupied by a thriving, dominant Spanish community; both the notarial records and the corregidor's court proceedings mainly concerned that group, and so did the related piece I published subsequently (1975). Such evidence is greatly instructive about the timing and nature of the Hispanization process, but once again the Indians escaped us. It was only later yet that Indian-language records were to open up the central Mexican indigenous world

[12]The work I am speaking of here is distinct from Spalding's *Huarochirí: An Andean Society Under Inca and Spanish Rule* (1984), although the latter does originate in the dissertation. The published book manages to approach yet closer to the lives of individual Indians in some respects and shows several kinds of increased sophistication and breadth, yet it also manifests certain problematics that did not attach to the dissertation, and several valuable parts of the earlier work did not find a place in the book, so that one still needs to consult the dissertation for some purposes.

in a quite different way, permitting us to study the structure of these people's lives somewhat as we do with other groups.

In the years when Spalding was working on Indians in Peru, Frederick Bowser was doing the same with blacks in that country in the time from the conquest to 1650, resulting in *The African Slave in Colonial Peru* (1974). Bowser gave us the most complete, reliable, informative book yet to appear on Africans in any Latin American country during the early period. After this work, there could be no doubting the primary attributes of the black population in Spanish American central areas—intensive or skilled work, proximity to Europeans, an intermediate or auxiliary function in the Spanish-Indian context, a slow absorption into the lower levels of Spanish society. Deriving patterns from an overflowing wealth of individual lives and cases, Bowser demonstrated that it is possible, for early Latin America, to do the social history of a tightly defined and lower-ranking group over a long time span without sacrificing the depth and subtlety of insight that come from large-scale use of freshly synthesized careers. The book almost appears to have been done in two stages; there are large institutional-statistical sections that have relatively little to do with the social core and would seem to have been written earlier, in a different spirit, although ultimately Bowser reinterpreted or discounted most of the utterances of officials and interest groups in the light of the social reality he so deeply studied. After all, many of the monuments of this age of miracles in early Latin American social history showed strong signs of the still incomplete transition from institutionalism.

If mining, commerce, and Africans are rather specialized topics, and Indians recalcitrant, the trouble with the great estate as a limiting principle is that it does not limit. It stretches in every direction; its primary function is the connection of city and country, into both of which it looms importantly. Every social type from community Indian to Spanish city council member has some role in it. Thus study concentrating on the more purely agricultural aspects fails to include large and vital segments of the functioning entity.

Perhaps it was then both inevitable and appropriate that the first major studies of the subject should be wide ranging.

Freyre we have already mentioned. A great advance came with François Chevalier's study of the Mexican hacienda in the middle colonial period (1952). In the Marc Bloch tradition, Chevalier brought a far sharper temporal and geographical focus to his work than Freyre; he surveyed a vast amount of relatively direct documentation in the Mexican archives, including provincial ones. Such a wide net caught many of the elements of the broader social-economic pattern, particularly the multi-tiered social structure of the hacienda (although the important temporary laborers eluded his vision). But since he proceeded on such a broad front, Chevalier was not systematic at the local level. No one area was surveyed exhaustively; rarely if ever did Chevalier follow a single family or hacienda through various kinds of sources, nor did he use internal hacienda records, preventing him from approaching closely to the labor question. Thus despite the real gains there remained large lacunae, which Chevalier filled with current stereotypes (such as "debt peonage"), ideas familiar from European history (feudalism, the Spanish national character), or shrewd guesses that could be off the mark when they were made without a sufficient context. Thus it was left for Gibson, not studying the hacienda per se but working far more intensively on a smaller area and using hacienda payroll records, to discover that debt peonage did not live up to its name. Bakewell, merely by virtue of studying Zacatecas closely, saw that Chevalier's idea of ruined miners retiring to their self-sufficient haciendas is highly unlikely. What happened first to the ruined miner was the confiscation and forced sale of his hacienda.

It was obvious that a more intensive type of investigation was called for, concentrating on one locality or subregion at a time. Even projects of this type would be tantamount to the study of the whole region, so that further limitation was necessary. The first important post-Chevalier hacienda research tended to stick close to land tenure. After a large research effort on local land titles, Mario Góngora in his study of the evolution of the hacienda in a Chilean valley (1956) did not proceed far beyond landowning, land use, and markets. The latter aspect, however, showed Spanish estates reacting to markets much more rationally than in Chevalier's picture. Then in his *Encomenderos y estancieros* (1971), following the careers and connections of estate holders through the entire local documentation, Góngora got inside estates, showing their rationale, inner structure including labor, and evolution in form over time responding to changing conditions. In this study, Góngora tied later estates to the encomienda of the conquest period much more meaningfully than Chevalier had managed to do.

William Taylor's *Landlord and Peasant in Colonial Oaxaca* (1972) was also at root a thorough land tenure study, although it involved serious attention to the general history of the region and especially of its Indian population at the corporate level (Taylor was a student of Gibson). Taylor provided an exhaustive series of individual reconstructions of all kinds of estates (including those held by Indians and church entities) over a period of generations up to the end of the colonial period, at least in landholding aspects. He found estate development retarded in comparison with central Mexico. Indian communities and their traditional rulers retained large amounts of land to the end of the colonial period, while Spanish properties were mainly not consolidated or stable. At this point it was not yet clear how much of the deviance of Taylor's findings from those of Chevalier was regional variation, how much actual correction, but Taylor set the field on the road to investigation of both questions, leading eventually to the realization that estates everywhere were more dynamic, volatile, and frequently traded than Chevalier had imagined, yet there was in fact important regional variation as well, estates regularly becoming consolidated earlier where there were more Spaniards and better local markets. Robert Keith's *Conquest and Agrarian Change* (1976), concerning the development of the hacienda system on the Peruvian coast, could be described as mid-way between the methods and outlook of Chevalier and the later more rigorous regional studies. At any rate, it gave a most useful example of the evolution of estate forms from a time of small or medium holdings to larger estates in response to the growing market of Lima.

Another tack was to attempt as complete as possible an investigation of one estate or enterprise. The advantages of this procedure, starting with depth and ending with an easy extension through a long time period, are apparent. Unfortunately, most estates failed to preserve enough records to support such an approach. Hundreds if not thousands of wealthy Latin American families today have what they imagine are colonial hacienda records, but actually, when these are earlier than the nineteenth century, they usually consist only of land titles and litigation papers about land. Virtually all the coherent internal estate records preserved in both Spanish America and Brazil seem to be of Jesuit properties or such oddities as the Cortés estate.

Ward Barrett's study of the Cortés estate's sugar hacienda (1970) covers the whole colonial period, is breathtakingly thorough on many aspects of production and general operation, and is far less purely "geographical" than one might expect from its author's disciplinary affiliation. Even career patterns are not ignored completely, and we learn much about the labor force at all levels. The general impression is of a rational, sophisticated, market-oriented business, very different from Chevalier's view. Barrett believed that the Cortés hacienda was less anomalous from the point of view of technology and management than in many other respects. This strikes me as true, though I would imagine the technology to be yet more standard than the management.

How informative Jesuit estate records can be is seen in the use made of them by Stuart Schwartz (1973) in working out the number and productivity of the tenants of the sugar *fazenda* Sergipe do Conde in the Bahia area over a long time span.[13] Even the tenants' names are given, and Schwartz was able to deduce with reasonable certainty the numbers of slaves they owned. The result was a revolutionarily new view of what a "plantation" really was and what kind of an arena it represented for Portuguese-African interaction. A resource

[13]Schwartz's article on *lavradores de cana* was part of a large research project on the Bahia sugar complex, centered on Sergipe do Conde, which led in due course to his *Sugar Plantations in the Formation of Brazilian Society* (1985). Schwartz did so much additional research in so many dimensions that he was largely able to transcend the question of Jesuit peculiarity.

with this much potential could not be ignored. It was clear that we should exploit what the Jesuits have given us, always with an eye to distinguishing what is Jesuit peculiarity and what is more general. Many basic processes must have been the same in a Jesuit enterprise as in any other. Valuable studies of Jesuit estates have continued to appear, and doubtless more will be seen in the future. Still, as a control, detailed internal records of any estate never owned by the Jesuits are worth their weight in gold, whenever and wherever they appear. Even when estate records seem relatively complete, it is important to seek out the personnel in other kinds of documents to do justice to the estate's multiple dimensions and avoid a new formal institutionalism.

The remaining large constituent element of Iberoamerican society that can be studied as a separate unit is the whole broad portion of it that was European or Europeanized at any given time—what the Spaniards called the *república de los españoles*. This entity tends to be roughly identical with the network of Spanish cities and towns, but also includes the Hispanic elements of the estates, plus the rural ecclesiastics and some others. Though unwieldy, it has great internal coherence; the rural members look to the towns, and the towns to the capital. All types and functional groups stand in close relation to each other, so much so that almost all of them need to be seen to make any one of them truly intelligible, while the whole makes a rounded unit.

On the practical side, to take the Hispanic world as one unit recommends itself because the subgroupings within it all appear together in the same collections of records; to investigate any one, the researcher must read the same mountain of papers that is the source for all. This tends to be the case even for a group as distinct as the Africans, who everywhere accompanied the Iberians, so that the study of them by themselves is very arduous and sometimes artificial. The same applies very much to the study of women. As a subject, the Hispanic-urban world—though no more the totality than the other elements—recommends itself for its centrality to the process of social evolution. It is at the center not only physically and demographically but also in the sense that many of the dynamics of change (marginalization, market formation, etc.) are contained within it. It includes the cutting edge of cultural interaction: those indigenous people who are in daily contact with Iberians.

At what order of magnitude should this unit be studied? The multiplicity of sources tends to put a sharp limit on extension through time, though the smaller the region one takes, the longer the time one can cover. In *Spanish Peru* (1968) I attempted to study the Hispanic society of one major region for about a generation, the broader conquest period up to about 1560. The method was essentially to read widely in many types of sources that reflect the ordinary activity of people at all visible levels, assembling numerous examples of careers and contracts, then sorting out the main social types, processes, and popular concepts. Following individual lives through all sorts of records was a crucial technique, as with Brading. Aside from study of governmental records kept on a countrywide basis, the local documentation of three important centers was surveyed, and much advantage was taken of the fact that people from all regions were constantly in the capital.

The totality of the records proved to have coherent detail on the careers not only of the famous and the wealthy, but of the obscure and humble, down to artisans, mariners, free blacks, and urban Indians. Information on the lower groups tended to come predominantly from notarial records. Indeed, the social-commercial-economic aspect of private life leaped out of these documents so strongly as to give me the impression during research that my study rested almost wholly on the notaries, while the rest was only elaboration and duty. I would still assert that its public or notarial documents represent an indispensable, basic, comprehensive tool in writing the history of any local Hispanic society, but for discovering the true contours of lives many other sources must often be added. Any document that places a specific individual at a specific place at a specific time is grist for the mill. The biographies I put into *Spanish Peru* as samples of career patterns contain references to all the various kinds of sources we have referred to above, as do the files on other individuals that did not actually enter into the book. Each source gives a new aspect of the person's life, making possible

a multidimensional, imaginatively reconstructed picture that is still reliable and adds up to one unified pattern.

Particularly important among other types of documents are local trial records.[14] One studies them not to find out who was guilty, but to explore a social milieu. Almost any local trial reveals the internal structure of a whole social circle, often including even servants and children. If you are fortunate enough to locate the same individual in both notarial records and litigation, you have the main elements needed to discover that individual's life pattern and function. If you can find fragments of a dozen other such lives with similar morphology, you have discovered and documented a social type, nearly coterminous with a social subgrouping and a social-economic function.

To indicate in more substantive terms the kind of results one can achieve, I believe that *Spanish Peru* made sense of the Hispanic element of sixteenth-century Spanish American society in the Peruvian (= central or classic) variant by revealing its main constituent types, their functions, and their relation to each other, as well as pointing to the extreme earliness of protonational development, the near irrelevance

[14]Visitas and residencias have most of the same characteristics. So do Inquisition trials, with the added advantage of their close attention to the whole life history and even genealogy of the principal figure, a feature not present in much ordinary litigation. Nevertheless, traditional writers on the Inquisition usually read only the trial sentences, or if they went so far as to read the testimony, only employed it to redirect focus on the institution, or at most on certain groups as the objects of persecution. Despite their advantages of richness, intimacy, and frequent concern with humble people, much sophistication is required to put Inquisition records to work for social history. To a very large extent the situations and social types appearing in these records are marginal or abnormal. Often the witnesses are of more interest than the accused. Some authors who have made progress toward social utilization of Inquisition trials are Wiznitzer (1960), Liebman (1970), and most especially Greenleaf (1969). The latter, by not taking the proceedings at face value, using trials to put together meaningful life histories of the principals, and connecting the Inquisition data with data from outside sources, was able to show the political and economic motivation of much Inquisition litigation and to throw light on the overall functioning of early Mexican society.

of administration to that development, and the important role of humble Europeans and others whose existence had hardly been recognized.

As to indigenous people, Spanish local documentation of the conquest period is highly revealing on the subject of urban Indians, and there is enough to show that the main mechanisms of both Hispanization and economic exploitation were established very early indeed. There is, however, distressingly little about rural Indians per se in any of the kinds of documentation used for *Spanish Peru*. But as one comes forward in time, the characteristics of the records change to the degree that Spanish society grows and expands. By the seventeenth century, the Spanish world has extended into distant regions to the extent that a study of it will also include fresh insight into local indigenous society.

With the gains come new difficulties. The sheer volume of records and their geographical dispersion can make the *Spanish Peru* approach impracticable. One must wait longer for the same individuals or firms to reappear. In the seventeenth century a provincial center is usually still easy enough to study; one gets the feel and recognizes some people in a week or two of work. But Mexico City is hard, and the whole is somewhat staggering, as I found out in some preliminary archival surveys in 1969. Away from the Spanish American viceregal capitals, large subregions are still quite practical arenas for this approach in the middle colonial period, as seen in studies by John Super (1973) of the Querétaro region, by Marta Espejo-Ponce Hunt (1974) of Yucatan, and by Rae Flory (1978) of Bahia.[15]

By the eighteenth century, the difficulties have greatly magnified. Paul B. Ganster undertook a social investigation of Lima in the mid-eighteenth century. He encountered a forbidding mass of notarial records and finally decided to limit himself to the secular clergy, producing a study (1974) which combined an analysis of the institutional side of the hierarchy with career pattern information on the clergy at all

[15]Summaries of the studies by Super and Hunt later appeared in Altman and Lockhart, eds., *Provinces of Early Mexico* (1976), and an expanded version of Super's work was published in Spanish in 1983.

levels, greatly enriched by his earlier explorations in the notarial archives and other non-ecclesiastical records, so that his subjects could be placed in a well understood context of family and broader society. Eighteenth-century studies have continued to be thematically restricted in the main, so that whatever the difficulties, the need for wide-ranging research on the society of that time is as great as ever.[16]

Aspects of biographical technique

Much of the work of Otte, Brading, Schwartz, Bowser, Bakewell, and myself can hardly be imagined without the use of a method on the order of multiple or collective biography. In no case is such the sole technique, and the same procedures of typification can be applied to objects other than people, particularly to contracts, popular concepts, and social institutions. But collective biography has been sufficiently central and distinct to warrant discussion of its special nature. The effect of following the careers of several apparently similar individuals (usually "ordinary" rather than famous ones; but it makes no difference) is to reveal and make intelligible a repeating pattern, one that is usually in the first instance what was called above a social type, or type of life history with characteristic contours. The approach aims directly at understanding a general principle of the operation and articulation of society, and is thus the opposite of

[16]Some years after the time frame covered here, John Kicza through massive research in notarial and other records, and also through the courage to be selective once trends were clear, bolstered further by the denser social statistics that the late period can generate, produced a general career-pattern survey of Mexico City society in the last several decades before independence. Most, though not all, of his work is represented in his 1979 dissertation, and one large chunk of it was published in 1983 as *Colonial Entrepreneurs*. The Kicza corpus is a uniquely valuable monument in eighteenth-century studies and cries out for imitation by those with large capacities and ambitions. It provides a general framework for a myriad of more specialized works, often showing that patterns imagined by their authors to be peculiar to a particular sector in fact pervaded the whole. It also looks at the country from the center, a refreshing and necessary perspective after what has now been a wave of rural and provincial research.

atomistic. Nor is it static. Discovery that training black and Indian helpers is a standard part of the successful Spanish artisan's life is tantamount to putting one's finger on an important element of social dynamics.

Since multiple biography deals with more than one individual, it appears to have a quantitative aspect, and in a sense it does, but not necessarily in the same way as social statistics or demography. The object of attaining redundancy in the biographies is not to survey a certain percentage of all exemplars of the type, but to get a sense of the trend of repetition. The principle is not unlike that of a wind sock, which is reliable although it tests only a millionth part of the air. It is important to understand that collective biography requires neither a complete knowledge of all actual careers, nor completeness within the sample career, to yield significant results. The role of career samples within social history can be compared to that of skeletons in physical anthropology. A dozen career outlines do as much to delineate a social type as two or three skeletons a physical type. What percent of a total prehistoric population a small number of sample skeletons represents is a question we do not ask, nor do we quibble over some missing bones. For many kinds of basic, general, even subtle analysis, a few skeletons are as good as several hundred. For other research, such as range of age and size, a larger sample is needed. In the same way, career samples can give an exact idea of a certain social stratum's general life habits, relation to other groups, and overall function, without always telling us much about the group's absolute numbers or whether its living standard is rising or falling at the moment. The method does give us a good understanding of *why* the group might be of a certain size or have a certain living standard. There is more supplementation than duplication between statistical work on straightforwardly demographic data and a biographical approach, even if the two methods employ many of the same sources. One approach seeks trends by aggregating atomistic data; the other cuts across the data to resynthesize basic units. It is the difference between counting dinosaur finds and reconstructing dinosaurs.

A career outline need not contain every detail of a person's life to be useful. In just such a manner a skeleton reveals little

about external characteristics, but has its own individuality and subtlety, and sets certain limits for the other traits. If we have a birthplace and date, a marriage, a property inventory or two, some hint of length of residence, organizational affiliation, and friends, we can carry out many kinds of analysis and correlation, and see how the life hangs together, unhindered by the fact that we do not really know the individual intimately and that much of his or her activity was never reflected in any written document. (A deep study of local litigation will often give the researcher a good sense even of thoughts and events that were not ordinarily recorded.)

While it might appear that career sampling would be difficult or impossible at the lower levels of society, such is generally speaking not the case. Doubtless upper groups are better represented, but much litigation survives in which people as lowly as blacks, Indians, and mariners figure as principals, not to speak of their frequent appearance as witnesses. Many of their apprenticeships, loans, wills, and work contracts are in the notarial records. Their names appear on the lists of sodalities, and on payrolls. The patron-client nature of many types of organization aids research, for inventories and wills of the heads of families and enterprises mention people far down the scale, when it comes to debts and legacies. In short, there is no real difficulty in acquiring a sufficient redundancy of multidimensional career samples for any social type that was in close contact with the Ibero-american cities and towns, or with any other place that has preserved records of a trial court and a notary. The difficulty is to be encountered not with lower groups as opposed to upper so much as with rural groups as opposed to urban. Nothing but hard work separates us from an excellent structural understanding of the urban and semi-urban poor. It is in the countryside that the records tend to forsake us, often leaving institutionalism, impressionism, analogy, and quantitative extrapolation as the only possible approaches.

The difference in applying biographical technique to upper as contrasted to lower urban-oriented groups is that with the upper groups one can approach near enough to completeness that even ephemeral and external characteristics can be integrated into the picture, and standard quantitative techniques can be used. Even with the lower groups, one can often do much toward measuring relative frequencies and thus change or variation, but far less than for a restricted group like high court judges, holders of entails, or encomenderos. As to intimacy and subtlety, as to that combination of suggestive detail and the eternally human which calls up an almost physical presence, the records of the humble yield little or nothing to those of the prominent. No situation has ever come home to me more than that of eleven-year-old Ana mestiza, who lived in semi-adoption in the house of a poor Spanish woman of Lima in 1560, along with the latter's black slave woman and female Indian servant. All the women were more or less abandoned, and they lived from boarding transients. The grown-ups were often gone in the evening, shopping, gossiping, or prowling, and Ana whiled away the time dancing alone in the empty house until the others returned.[17]

Multiple biography does not involve, as some seem to think, any attempt to prove that property-owning or kinship are the final determinants in human affairs, any more than studying a chess game implies a belief that the pieces make the moves.[18] But people of all types express only a small part of what they think; much of what they do express is lost; and even what remains is affected by self-interest and camouflage, so that we must resort to the record of their behavior, however incomplete it is, to deduce what they thought and what their language meant. Such a survey points neither to simple economic determinism nor to blind behaviorism. Instead, it often reveals the force of widely held social conventions and other unexpressed, often subconscious mental constructs.

Some general and theoretical considerations

Having by about 1975 at least sampled most of its known documentary resources, the field was henceforth somewhat freer from documentary determinism; the time since has seen an increasing eclecticism. Still, a very large proportion of recent history in our field, including much research without biographical orientation, can be described as the close study of a certain segment of social reality, with broad documentation, concentrating on categories and types as embodied in specific examples, each of which is seen as an organic entity rather than as an ingredient for aggregate statistics. I have some reflections to offer to both authors and readers of such studies.

For writers, nothing could be more important than to widen the documentary base, not so much in sheer amount as in variety. Each kind of source imposes its own distinct perspective and contains its own distinct subvariety of the

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[17]Archivo Nacional del Perú (Lima), Real Audiencia, Procedimientos Penales, legajo 1, trial of Isabel Gómez.

Perhaps this is the point at which to insist that women are not only in themselves an essential part of any balanced treatment of social history, but through their presence or absence, their marriages, dowries, activities, and property-owning, are essential to measuring the quality and velocity of general social development, as well as any individual male's rank, prosperity, and affiliation (the converse is also true). Women appear regularly in all the Iberian sources from cedularies to litigation; though their exclusion from most active professions is a handicap, career skeletons and even full, intimate portraits are not much harder to produce for women than for men of the corresponding social categories. Some examples may be seen in Lockhart 1968, pp. 150–70, 197–98, 210–11, 216.

[18]Such suspicions run through Lawrence Stone's 1971 article on prosopography. The differences between the biographical tradition that has grown up in early Latin American historiography and the more established one that Stone describes, mainly for English and classical historiography, are most suggestive. The Latin American work is far more primary, in part no doubt because of the lack of preceding preliminary writings, but with profound effect on the quality of the result. The nature of the Latin American (Iberian) documentation is also vastly different, particularly when it comes to lower groups. The Europeanists tend to use "prosopography" for upper groups, statistics for lower groups, whereas several Latin Americanists have managed to synthesize careers from primary data across the board. Many of the weaknesses Stone found in the collective biographical approach come from an attempt to tie surface politics too closely to the social-economic self-interest of the actors, something most of its Latin Americanist practitioners have never tried to do. The two biographical traditions grew up quite separately. I had not heard of Stone until after I had written *Spanish Peru* and *The Men of Cajamarca*, and though I have since become cognizant of his work I have not followed it. The early Latin American biographical tradition since the 1960's is a distinct school with much to contribute to general historiographical method.

general language and conceptual equipment. The reciprocal correction and complementing of the various types bearing in on a single subject matter results in balance, validity, and insight unattainable with a one-source approach. It is obvious that such investigation must be restricted in scope if it is to operate at the necessary depth. Therefore it cannot normally cover whole consecutive centuries or vast regions. Even the totality of such studies, though by now very substantial and ever growing, offers nothing approaching complete "coverage," and perhaps never will. It becomes necessary to give some attention to general strategy in the choice of topics, using a process of triangulation to plot long-term, multiregional developments or patterns of uniformity and variation.

Such work must be read in the same spirit. In the usage of some scholars, any book which is not a general text is called by the slightly pejorative term "monograph"; and any study on a subject ostensibly less inclusive than a national entity through all eternity may be considered a "microhistory." "Microhistories" are deemed to be mere "case studies" that can add up eventually in an almost mechanical way to "macrohistory." Actually this is a thoroughly false perspective. There are general patterns at a local level, and many important general patterns are observable *only* at the local level.

The social historians from Gibson on have often proceeded directly from observation of individual examples to the formation of concepts and categories, whether social types, ideals, concepts embodied in special language, conventions, patterns of behavior, or principles of organization. Such a procedure immediately attains a deeper stratum and a greater universality than either the substantive "idea" or the substantive "generalization." Rather than manipulating concepts, social history has altered them. It gradually became clear that most of the basic categories first used to understand the postconquest centuries were anachronistic and naive projections of a later onto an earlier time: Indian village, plantation, soldier, missionary, debt peonage, and many more. Backward projection of a recent past had resulted in a false impression of extreme rigidity and lack of movement.

Social history together with demography has largely destroyed this picture and has begun to replace it with a reconstruction of secular trends and regional variation in terms of more adequate categories, hoping to do justice to unity without denying complexity.

Such an endeavor implies an accompanying elaboration of theory. In the great scholarly emporia of Europe and North America one can acquire ready-made theories at bargain prices. But personally I have from the beginning been antagonistic to the imposition of concepts from the outside. In the 1960's and 70's such concepts and procedures came primarily from certain of the social sciences—sociology, economics, political science. My objections were not so much humanistic, for I, like the social scientists, want to study broad and general patterns (though seen originally in fleshed-out, living examples), and I believe in the ultimate autonomy of social-cultural evolution as something comparable to and related to the evolution of human language, beyond the conscious planning or even awareness of any individual or set of individuals. But I think that social science brought in rigid concepts from a different context which often corresponded to nothing and impeded a fresh view of historical reality.

The social sciences as we know them arose in industrialized countries in the late nineteenth and twentieth centuries; they often presuppose an easily available, trustworthy informational base, and strong, uniform institutions. Nothing could be further from the early Latin American situation. Ideas from the social sciences can serve to inspire interpreters as well as any others—but no better. Each new external element must be tested critically for applicability; otherwise we are in danger of repeating the mistake made with the anachronistic concepts the field has now overcome. Unsophisticated work on insufficient sources with the newest European methods and concepts is, as Rolando Mellafe (1970) has said, nothing but an amusing intellectual game. In the event, the historians of early Latin America never did become excessively influenced by the social sciences, developing instead their own procedures and ideas, their own tradition, adapted to their topic. A new generation of notions from the outside has arisen, however—from European-based postmodernism, from studies of other areas of European expansion—and the struggle goes on in a different form.

Theoretical and general statements on early Latin American social history, based on the materials and writings of the field itself, have not been entirely lacking. An able first attempt in this direction was made by Lyle McAlister in 1963, before the real onset of the social history movement. His essay may be viewed as essentially a rationalization of social thought articulated by Spaniards of the early period, in legislation, official reports, and travelers' accounts. Interpreted in this fashion, the article is as valuable as ever. As an analysis of actual social organization and dynamics it was understandably overtaken by the subsequent scholarly production that it helped to stimulate. In 1969 I published an article sketching out an interpretation of the Spanish American great estate as a unitary, coherent organizational type characterized by a relatively unchanging social core and overall function, with simple principles of evolution and variation.[19] In the introduction to Altman and Lockhart, *Provinces of Early Mexico* (1976), I expanded on this analysis, introduced the concept of the trunk line, and carried out other types of general analysis of social organization and evolution. Since that time I have gone considerably further in that direction.

Epilogue
(1989)

The present article originally appeared in 1972. It has since become clear that the time around 1965 to 1975 was a golden age of social historiography in the early Latin American field, with the appearance on the scene of Bakewell, Bowser, Brading, Russell-Wood, Schwartz, Spalding, Taylor, and myself, and the publication of major works in new genres with new methods by almost all of this group. Through reading dissertations and still unpublished manuscripts, I was able to catch most of these developments in the original article; now I have made a few additions, bringing the time

[19]Now Chapter 1 in this volume.

covered up to about 1975 or 1976. I have also done such rewording as was appropriate to make 1988–89 the temporal perspective. Portions of the final section were omitted, partly because they contained recommendations for research tied to the period of original composition, and partly because I have since further developed some of the theoretical analysis therein contained (Lockhart 1984, applied extensively in Lockhart and Schwartz 1983).

In the time since 1975, mainline career-pattern, small-entity social history has continued to appear and make its contribution. A great deal of it has taken the form of studies of rural estates. A perhaps even larger stream has been diverted into ethnohistory, done as social history of Indians, but with new methods and perspectives required by the circumstances. A rapprochement with anthropology has taken place in this respect, up to a point, and though there are problems with the relationship it is a much more comfortable and natural one than the earlier interaction with disciplines oriented essentially to Europe and the United States. The time after 1975 saw a rash of eighteenth-century studies, often social-institutional in nature, but this wave seems to be subsiding. An important new development is a greater emphasis on cultural studies, some of it coming from a new and more analytical school of literary history, but much of it growing directly out of the "social" history movement, which was perhaps somewhat misnamed from the beginning. Of the work on Mexican Indians I am presently completing, four of the core chapters are "social," and four appear more "cultural" (though at a deeper level I make little distinction).[20] In a note to the original article, I said the following:

> Just as social conventions can be distilled from examples of behavior, popular concepts can be studied in examples of speech. A surprising amount of ordinary usage is contained even in the highly formulized notarial records; but the greatest repository is litigation. Spanish legal clerks came at times amazingly close to capturing the actual word-flow of witnesses deposing before them. The possibilities of such

[20]I was speaking of the work later published as *The Nahuas After the Conquest* (1992).

a source are practically unlimited. It seems to give us the means of finding out what key words such as "creole" meant in the ordinary usage of various people at various times. We could then grasp the rhythm and context of the evolution of such terms, which are not at all as they appear in the limited, overdrawn, sometimes uncomprehending statements found in travelers' accounts, chronicles, and legislation, the main source for this kind of intellectual history to date.

Thus if social history is converging with demography and anthropology on the one hand, it is approaching cultural-intellectual history on the other. In both subject matter and source material, socially oriented research is close to giving early Latin American intellectual history a depth and generality that have so far escaped it. There are still major problems of technique. Despite its overall richness, court testimony is a very diluted source; unlike a behavior pattern or a social convention, a concept embodied in a word cannot be seen fully exemplified in a single carrier.

To conclude I will quote a passage from a review I recently wrote:[21]

> The category "social history" is no longer generally valid. In certain subfields, the term still has much meaning on the level of conversational usage. Thus in early Latin American history it means specifically work which features fleshed out portraits of actual individuals and organizations and stands in contradistinction to demography; but the sense of the expression varies from field to field. The designation was originally justified in that widespread dissatisfaction with political/administrative/elitist/military/diplomatic history first took the form of an urgent feeling that more people, indeed the entire population, should be included. Far more important, however, was the need to go beyond celebrating events as such and accepting the actors' conscious rationale for their actions. The pursuit of a deeper truth has by now led scholars in a vast number of directions, investigating all the concerns of the humanities and the social sciences in time depth.

[21]Lockhart 1987.

3. Letters and People to Spain

(1976)

ONE NOTES gratefully the growing success scholars are having in estimating the numbers, regional origins, and some other characteristics of the emigrants from Spain to America in the sixteenth century. Later emigration stands open to study by the same means—primarily the compilation and analysis of data gathered by governmental agencies of the time. Already we see a movement of truly mass proportions, far more broadly based as to region and social recruitment, far more sustained than we once had reason to think. The implications are both deep and broad, bearing in the most direct way possible on the manner of creation of Spanish America, the main lines of the evolution of early modern Spain, and the general or comparative history of emigration.[1]

Interpreting the movement is no easy matter. One cannot use gross data of the type available for direct measurement of the impact of emigration on either place of origin or point of destination, much less for meaningful discussion of the question of whether emigration was "good" or "bad" for Spain. Aside from some work of an economic and statistical nature, the social history of early modern Spain is virgin territory, far less explored than Spanish America itself; in measuring the effects of emigration the scholar faces the logically contradictory problem of measuring the impact of a loss on a vacuum.

In any case, there is a whole area of interest left untouched by the emigration statistics. What of the emigrants' expectations and motivations?[2] What of contacts preserved with

[1]A whole vein of older work drawing conclusions from supposed differences in English and Spanish emigration patterns is now thoroughly outdated. It emerges increasingly that early modern English and Spanish emigration movements were quite similar in kind, and further that the European emigration wave of the nineteenth and twentieth centuries contains practically *nothing* new, i.e., that the entire set of movements represents a single phenomenon.

[2]J. H. Elliott asks such questions in *The Old World and the New, 1492–1650* (1970).