The origins and mutations of the original contributions are thus embodied in the papers that follow. Emphasis on Peru remains, and no claim is made to list all relevant publications. The authors have evolved their own lines of treatment, with some minor overlaps and differences in focus. Indeed, while the contributors regard themselves as allies in the field, they do not wish readers to imagine that they have identical opinions on all subjects. Nevertheless, the papers are complementary, and elucidate many facets of a movement that is an important part of contemporary historical research and writing on Latin America.

T.F.M.

THE SOCIAL HISTORY OF COLONIAL SPANISH AMERICA: EVOLUTION AND POTENTIAL

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"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 to which social history is ordinarily contrasted. Since our main concern here is with practical historiography rather than with questions of genre, 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 the people who produce them.

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

The potential significance of colonial social history is easy to see. Formal institutions in colonial Iberoamerica were weak and spotty, lacking the manpower, mechanisms, and even generally the will to carry out the activist policies of their

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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 colonial history is or should be to a large extent social.

Though E. G. Bourne sensed all this over half a century 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 past five or ten years. Several major publications have recently appeared, and others are forthcoming. With related kinds of ethnohistory and detailed demography, social history can now be said to be a dominant strain among young and youngish colonial historians in the English-speaking world. There are as yet only isolated examples among Latin Americans, who still largely equate progressivism with the French school.

Why should such a movement emerge at just this time? One might imagine some connection with the persistent present-mindedness of the 1960s, apparently to continue in the seventies. 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 twentieth century.

During the past decade, scholars with an interest in the long haul of Latin American history were much more likely to get support from institutes and foundations if their projects could be shown to bear on the subject of 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 into being; the continuation of the process is what their further "development" must inevitably consist of. The study of colonial social history 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 colonial historians. To take the only example about which I have any 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 purely as a problem. On the conscious level, my main motivations around 1960 were: a deep sympathy for that combination of restraint and energy so characteristic of sixteenth-century Spaniards; and on the other hand a dissatisfaction with then current knowledge of Latin American history, which seemed

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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 writes me 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 than by the moral outrage of the developmentalists.

Probably neither outside pressures nor conscious inner motivations are crucial to the present movement. The time of social history has come largely because the field has worked its way through the sources down to those which have 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 Latin American colonial 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 certain kind 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 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 Latin American colonial field is now nearing the completion of 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 new, 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. Scholars at every step 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 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 men 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' troubles as a governor 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 represents one of the most real technical contributions we historians can make to our colleagues in anthropology, 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 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 certainly 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 colonial scholarship during the first half of the twentieth century. 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 first for their creative reconstruction and skillful presentation of important subject matter, 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 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; for the most part 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 stocktaking occurred regularly about a generation after Spanish impact in any given area, when the first great movement of creativity and destructiveness 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 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 town and province; 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 Negro slaves. Rural church activity in the more settled areas, indeed, was often largely a function of the encomienda and 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 very much open to question. It is my feeling that the great mechanism of Europeanization was not formal instruction but ordinary contact between Europeans and Indians, measured in man-hours, and that 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 local 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 1930s), 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, and 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. 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: 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.

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 true exhaustion of its characteristic sources. 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, if they are used in a different way. 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, concerning 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 built up by the institutionalists; as these phantoms then gradually dissolved under the light of approaching closer study of society, the "Tannenbaum thesis" of mild treatment of slaves in Latin America was left apparently 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 and North 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 superseded. Boxer's relative lack of preconceptions, his faithful reporting of detail, his attention to reports of men 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. Probably significant elements of the population remain untreated or unsuspected; the picture of others, such as the paulistas, is a typically one-dimensional "governor's portrait;" categories of analysis imposed from the outside will yield to ones growing out of the material on closer examination (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 Latin American colonial field. Some may think this 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 colonial 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 were myriad; they included everything from travelers' accounts to estate records, 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 memories were 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 time 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 several Brazilians, 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 layman the utter uselessness of broad, synthesizing generalizations on social matters presented by contemporaries. "The sons of the first 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 in the public utterances of a Southern politician, 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 Negro slaves escape from camp, and one becomes aware that there are Negro 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 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' 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 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 statistics and the use of computers, though there are some that do not, such as the minutes and other records of corporations—municipal councils, cathedral chapters, or charitable organizations.

For about the last twenty-five years there has been a strong interest in documents like these, and the interest 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 the investigator into certain avenues of research regardless of his real main interest. One gets mainly 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; and one hears that Alvaro Jara will soon complete similar work on Potosí 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 will be 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 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 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 towards 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). Many 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 are generally held to be the field's most modern, advanced development. Certainly demography's potential is far from exhausted; 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 I believe no direct influence has been exercised in either direction. Franklin Mendels in a recent article (1970) tells how European historical demographers have been turning from the study of total population trends to more intensive investigations of smaller groups, going as far as the reconstitution of individual parishes and families. 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 as statistical demography and the close study of the lives of people. As to method, social history can perhaps make some contribution through helping to clarify and refine the categories. For the later period of census-taking, the latter eighteenth century, the place to start is no doubt an intense study of the use of increasingly elaborate terminology in the censuses themselves. As a sample from the work of Trent Brady illustrates, 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 woık 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 can do much to fill in what it meant to be "Spanish," "Indian," and "mestizo" at given times, so that we can better interpret the statistical results. Borah himself has given a good beginning to this enterprise in his masterly, almost unknown essay, "Race and Class in Mexico" (1954).

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 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, but 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.

Social history can probably help demography with the persistent problem of the conversion factor. Through such methods as dissection of individual parish records and the study of wills and trials, we may attain a much better idea than we now have of family size, and of the number of human beings corresponding at various times to such shifting units as *vecino, casa*, or *tributario*. A modest change in these units can mean a difference of millions in the statistical result. Rolando Mellafe (1970) is already well on the way to a better definition of such terms for sixteenth-century Peru, largely through detailed study of parish registers.

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. If a generation of undergraduates has been taught that Indian population decline was the reason for falling silver production and the rise of the hacienda, the fault is not 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 the other factors. As to mining, David Brading's new work on eighteenth-century Mexico (1971) and Peter Bakewell's thesis on Zacatecas (1968) prove very clearly, to my mind at least, that Indian population was a negligible factor. Silver deposits, mercury supplies, technology, finance, and organization seem to be the totality of relevant variables. Insofar as labor was important at all, 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 raw 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 only systematic work on the evolution of the hacienda in a single area, Mario Góngora's study of the history of landholding in Chile's Valle 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 simple connection between the granting of land and Indian population loss. François Chevalier, too, though he explained the hacienda largely as a response to depression and population loss, gave a 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 ideals, 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 arising 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 general 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.

At present it would appear that for the earlier period at least, smaller and more intense studies in the style of the new European work would be in order, to reconstitute more refined information from sources not organized to deliver it in any straightforward way. This effort would find a ready collaborator in social history proper, insofar as the two endeavors do not indeed simply converge into one.[3] In the era of late Bourbon census-taking, there are additional possibilities. The censuses themselves are so much more methodical and articulated that statistical-demographic work with them can doubtless 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. 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 compilation 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. Failure to recognize this caused social history pioneer Tomás Thayer Ojeda recklessly to identify the conquerors of Chile with Pasajero entries, 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 subsocieties. 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: 237–239), 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 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 recently by J. Ignacio Rubio Mañé (1966). The original document gives each individual's name, Spanish origin, and present occupation, and often his 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 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 are the famous sets of census surveys we call the *Relaciones geográficas.* However, they tend more to economic than to social detail, and a recent attempt by Alejandra Moreno Toscano (1968) to use the latest French quasistatistical methods on them has 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 L. 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 has done this, among other things, in his *Miners and Merchants in Bourbon Mexico* (1971), for the audiencia of New Spain, and Leon Campbell is publishing an article in the *Hispanic American Historical Review* (1972) with similar research on the audiencia of Lima. Both reach revolutionary conclusions on the question of whether creoles were deprived of high office in the Spanish Indies. Their sources are 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 members of the audiencias?

For a forthcoming work on the *Relação* or high court of colonial Brazil, Stuart Schwartz has 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.[4] These data, together with study of their activities, marriages, and investments in Brazil, make 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. A constant process of absorption of the judges into the upper levels of local Brazilian society emerges as one of the clearest trends. 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.

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. Actually important actions and trends are visible in council minutes, but without the social context necessary for understanding them. The debate between Boxer (1965) and Dauril Alden (1968) over whether or not Brazilian councils were aristocratic and self-perpetuating can never be settled out of those records alone. Though I would be amazed if Alden's position—that the councils were aristocratic—is not correct, the question must and can be settled by establishing family connections 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 dominated by 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 de 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 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 demonstrates 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 is to show the extent of urban-centeredness of the whole society, arousing the suspicion that Brazilian estate owners may have been as urban-oriented as their Spanish American counterparts, Russell-Wood retains the concept of a rural aristocracy as worked out in the last 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. We must study the mill owners in other sources until we know enough of their life style, residence pattern, and total activities to decide the question one way or the other.

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 the same characteristics as Misericórdia records. They can serve as an initial guide, and 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.

BEYOND INSTITUTIONAL SOURCES

The inadequacy of a single source for carrying out social-economic investigation has been apparent to many scholars for some time; therefore a search has gone on for another principle 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 are based mainly on a more or less coherent 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 practically eliminate selectivity. The great trouble is to find a group which includes humble people, yet is well enough documented. So far 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.[5] 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.

Many areas had a larger population than Chile, and it is 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 a book which will be published under the title *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 men 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, occupations and factions, 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 patriarchal existence. 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 bureaucratic and ecclesiastical assaults.

The group anatomy technique could have many fruitful applications, but in all probability it 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' 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. A natural subject for this technique is mining, which is a concentrated 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 a 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), 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.[6]

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 thesis "Silver Mining and Society in Zacatecas, 1550–1700" (1968), apparently soon to be published, worked through an impressive amount of local administrative, judicial, and even notarial records, and has written a complete set of chapters on varied aspects of Zacatecas industry, agriculture, society, and government. I have previously mentioned some important substantive conclusions of this study, and will have occasion to mention others when we come to speak of the hacienda.

As interesting as Bakewell's work is, David Brading's *Miners and Merchants in Bourbon Mexico* (1971), started slightly earlier, goes beyond it in two respects. Brading studied eighteenth-century Guanajuato on the basis of a survey of local documentation 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. Brading's interest in any event 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. Litigation containing inventories and testaments was one crucial source, but there were others.

Finding that his people's lives stretched far beyond Guanajuato, he followed them in the archives of Mexico City and Seville, and 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 national scale, without resorting to impressionism. One of the many social mechanisms which he demonstrates is that of the Spanish-born Mexican merchants who hand over their businesses to new immigrants (often relatives from the hometown), while the merchants' landed property and perhaps title go to their creole sons. Such insights, taken with Brading's previously-mentioned work on creole office-holding, at last begin to make structural sense of the famous creole-peninsular rivalry.

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. It is still not before the public, although I saw the substantially completed manuscript in Seville over five years ago. 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, uses the technique of synthesizing careers of many important individuals and firms from widely scattered data. He makes particularly enlightening use of private letters, infinitely more frank and revealing than official correspondence.[7] While Cubagua is somewhat peripheral and hermetical, 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, aside from exploiting pearls, was to organize a municipality, with the largest investors on the city council. These patriarchs started building large urban houses and acquiring 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, with many implications for the society as a whole. There is no 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 are the principal studies with social content.

Another sector standing out as a distinct unit of study are the Indians. Serious work on colonial-period 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 colonial period, as if they knew that Gibson would come. Their interest in late pre-Columbian society itself, as opposed to archaeology and the study of material culture, was slow to develop; they knew and know 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*, as in his broad-based *Aztecs under Spanish Rule* (1964), he more than anything else did institutional-jurisdictional history, often with economic overtones. 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 has little to do with social history, beyond setting a framework for it. In other ways there are 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, and rural in emphasis. Like the social historians, Gibson became an advanced skeptic about the active powers of the state, and produced an account of the evolution of the Indian communities in which broadly socio-cultural 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. (See also Karen Spalding's article in this issue of LARR.) And like the social historian again, Gibson aimed not so much at "conclusions" as at deep-going analysis that changes 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.

Studies analogous to Gibson's have begun in Peru. A strong impetus has come from the work of 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 colonial, and have great potential as a starting place for intensive local studies, in which the role of provincial Spaniards will hopefully be taken into consideration.

An important regional study on Peruvian Indians is Karen Spalding's thesis "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 establishes jurisdictional-administrative entities and socio-political categories and trends, all in all highly similar to 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 assert their weight. The types of documents sufficient for Gibson's purpose are not necessarily enough to penetrate into the ordinary lives of rural Indians. In the Haurochirí study, sophisticated use of "fragments of information included almost unconsciously by the author of a document" at least makes possible the nearest approach yet. Such a close view shows even greater change, internal variety, and mobility (especially geographical mobility) than Gibson's picture. It also reveals 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 appear to exist for the Huarochirí district that would yield a truly intimate portrait of Indian society below the level of the Indian lords.

At times Karen Spalding makes 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 to choose a locality for study on the basis of intrinsic interest. The most propitious situations often disappoint us. Recently (1971) I sampled 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. However, it soon became apparent 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. Such evidence is greatly instructive about the timing and nature of the Hispanization process, but once again the Indians escape us. I still believe that somewhere the main internal records of some truly Indian community will prove to have survived, permitting us to study the structure of their lives as we do that of other groups.

If mining and commerce are rather specialized, and Indians recalcitrant, the trouble with the great estate as a limiting principle for social history 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 city council member has some role in it. Thus study concentrating on the more purely agricultural aspects will fail 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 most of the elements of the broader social-economic pattern, particularly the multi-tiered social structure of the hacienda; 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 could he approach closely to the labor question, which turns on specific local detail. Despite his classic portrait of a social institution, and his sure grasp of essentials, there remained large lacunae, which Chevalier filled with current stereotypes (such as debt peonage), ideas familiar from European history (feudalism, capitalism, or Spanish national character), or shrewd guesses which 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, to discover that debt peonage did not live up to its name. Bakewell, merely by virtue of studying Zacatecas closely, has seen 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.

A more intensive type of investigation is obviously called for. One way is exhaustive study of the great estate in one locality or subregion at a time, but such research is often difficult to differentiate from a total study of the region. 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. Most of the properly social investigation, even of so basic a matter as the city-estate link, remains to be done. However, the complete maps of holdings at various dates, and the lists of owners and dates of sale, constitute an unparalleled individual opportunity for social history. With this much of a start, one might trace readily the social profile and interrelationships of the owning families, and relate, with great completeness and subtlety, their holdings to their familial fate.[8]

Another tack is 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 obvious. 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, the records are usually only land-titles and litigation papers about land. They represent a beginning, but nothing more. Practically 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).[9] Such records can be informative in the extreme, as may be seen by the use made of them by Stuart Schwartz in working out the number and productivity of the tenants of Sergipe do Conde in the Bahia area over a long time span.[10] Even the tenants' names are given, and Schwartz is able to deduce with reasonable certainty the numbers of slaves they owned. A resource with this much potential cannot be ignored; we must 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. Still, as a control, the internal records of an estate never owned by the Jesuits would be worth their weight in gold, and such may yet prove to exist. Even when estate records seem relatively complete, it is important to seek out the personnel in other documents to do justice to the estate's multiple dimensions and avoid a new formal institutionalism.[11]

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 cities, but also includes the Hispanic elements of the estates, plus the rural ecclesiastics and some others. Though unwieldly, 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 blacks, who everywhere accompanied the Iberians, so that the study of them by themselves is very arduous and sometimes artificial. 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 dynamic principles of change are contained within it. It includes the cutting edge of acculturation: 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.

One interesting study of a segment of the Spanish world is Peter Boyd-Bowman's ongoing Puebla project. After summarizing and indexing the notarial records of Puebla from 1540 far into the sixteenth century, Boyd-Bowman is preparing a book to be entitled "Profile of an Early Colonial City: Puebla in 1554." It will focus its material in as complete as possible a physical, social, and economic anatomy of Puebla in the single sample year of 1554, when most of the town's long-term characteristics had already taken shape. The work's most prominent feature will be a series of life sketches of 302 men known to have been vecinos or citizens of Puebla in that year; information on them will also be subjected to statistical compilation.

Research of this intensity approaches the group anatomy technique discussed above. Among the citizens are many relatively humble people, so that the sample is wide and the results revealing. Yet the narrowness of the focus on one locality alone, and a rather exceptional one at that, will leave us wondering about the nature of development in New Spain more generally, until further centers can be at least tested, particularly Mexico City, which is not only crucial in itself, but will throw much light back on Puebla.

I have attempted to study the Hispanic society of one major region for about a generation, the conquest period, in *Spanish Peru, 1532–1560* (1968). The method was essentially to read widely in many types of sources that reflect the ordinary activities of people at all visible levels, assembling numerous examples of careers and contracts, then sorting out the main social types and processes. 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 the *primary* record of any local Hispanic society is to be found in its public or notarial documents, but for discovering the true contours of lives many other sources must often be added. The biographies I put into *Spanish Peru* as sample 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 multi-dimensional, reliable picture that still adds up to one unified pattern. Particularly important among other types of documents are local trial records.[12] 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 the researcher can locate the same individual in both notarial records and litigation, he has the main elements needed to discover that individual's life pattern and function. If he can find fragments of a dozen such lives with similar morphology, he has discovered and documented a social type, usually nearly identical 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 of administration to that development, and the important role of humble Europeans and others whose existence had hardly been recognized.

As to Indians, 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 Indian 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. At present I am eyeing the project of a social history of Mexico around the beginning of the seventeenth century, and after a preliminary survey of the sources estimate it will take me most of a decade, if I ever finish at all. In the seventeenth century a provincial center is usually still easy enough to study; one gets the feel in a week or two of work. But Mexico City is hard, and the whole is somewhat staggering.

On the periphery of Iberian occupation, countries or large subregions are probably still quite practical arenas for this approach in the middle colonial period. After some months of studying Yucatán in the latter seventeenth century (when the notarial records begin), my student Marta Hunt has in-depth information on the estate structure from top to bottom, knows who the dominant urban families were, and has good career samples at all levels.

By the eighteenth century, the difficulties have greatly magnified. Another student, Paul B. Ganster, has undertaken a social investigation of Lima in the mid-eighteenth century, realizing from the beginning that a broader topic was doubtless out of the question. He encountered such a mass of notarial records that until they are thoroughly indexed, it will probably be impossible to use them as a starting place. While some individuals recur in the records, they do so in too small a context. Ganster has adopted the very reasonable research method of identifying individuals first in more organized sources such as Consulado records and the Beneficiencia, after which he may seek them out in the notarial archives and trial records. Very probably for the eighteenth century it will prove necessary to study one stratum or function of Hispanic society at a time, despite the dangers and limitations involved.

ASPECTS OF BIOGRAPHICAL TECHNIQUE

The work of Otte, Brading, Schwartz, Boyd-Bowman, and this author 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 and social institutions. But collective biography is beginning to occupy a place 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 atomistic. Nor is it static. Discovery that training black and Indian helpers is a standard part of the successful 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 re-

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search, 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 often 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 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 written down.)

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 patriarchal nature of society aids research, for inventories and wills of the patriarchs 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 is in close contact with the Iberoamerican cities and towns, or with any other place that has preserved records of a trial court and a notary. The difficulty is not to be encountered with lower groups as opposed to upper, so much as with rural 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, 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 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.[13]

Multiple biography does not involve, as some seem to think, any attempt to prove that property-owning or kinship are the final determinants in men's affairs, any more than studying a chess game implies a belief that the pieces make the moves.[14] 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, to deduce what they thought. Such a survey most often points neither to economic determinism nor to blind behaviorism, but reveals instead the force of widely-held social conventions, as discussed below.

SOME GENERAL AND THEORETICAL CONSIDERATIONS

Having now at least sampled most of its known documentary resources, the field will be freer than before from documentary determinism. It may follow its whim, or general intellectual currents, or better yet, the inner logic of the subject.[15] It is thus perhaps fruitless either to recommend or to predict directions for further investigation. Still, a large proportion of recent social 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. This tradition has gained enough momentum and has enough advantages that I think it likely to continue for some time. On the supposition that it will, 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; the reciprocal correction and complementing of the various types bearing in on a single subject matter results in balance and validity 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 cover the whole Latin American colonial period; nor is the whole movement likely to do so, even if it goes on for some time. It becomes necessary to give some attention to general strategy in the choice of topics, using a process of triangulation to plot long-term and multiregional developments.

Such work must be read in the same spirit. Latin American historians have fallen into the habit of calling any book which is not a text by the slightly pejorative term "monograph;" and for any study on a subject ostensibly less inclusive than a national entity through all eternity, they have begun to use the even more damning word "microhistory." "Microhistories" are deemed to be nothing but "case studies" which can eventually add up 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 observable *only* at the local level.

The social historians from Gibson on often proceed directly from observation of individual examples to the formation of concepts and categories, whether social types, ideals, 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 is altering them. It has by now become clear that most of the basic categories first used to understand the colonial period 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 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 variations 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 find myself increasingly antagonistic to the imposition of concepts from the outside, particularly from the social sciences. At a time when there are strong challenges to research in general, even to the very idea of an independent, fair-minded investigation of the truth, it is certainly not very iconoclastic to say that social science worship may no longer be the last word. In other branches of our discipline this has already been said, though perhaps for somewhat different reasons. My objections are not so much humanistic, for I, like the social scientists, want to study broad and general patterns, and believe in a kind of modified social-cultural determinism. But I think that social science can bring in rigid concepts from a different context, which correspond to nothing, and impede 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 Latin American colonial situation. Social science ideas 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 will only repeat the mistake made with the anachronistic concepts the field is just overcoming. 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.

It is hard to see how colonial social history has any real alternative to proceeding inductively. We do need a body of theory or general statements on the nature of each of the constituent elements of the society that concerns us: the urban-Iberian world; the Indians; the great estate; the export economy. Also there is a thorough dichotomy of central and peripheral areas that is susceptible of systematic expression, and much to be said about that series of processes we have been content to call "acculturation." In each case it seems to me that the best results can be obtained by sifting the common elements and secular trends out of the Latin American materials before us, rather than hastening to identify the Latin American phenomena with something outside.

An able first attempt in this direction was made by Lyle N. McAlister some time ago (1963); his essay may be viewed as essentially a rationalization of social thought articulated by Spaniards of the colonial 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 has understandably been somewhat overtaken by the decade of scholarly production that it helped to stimulate. More recently, I have sketched out a provisional interpretation of the Spanish American great estate (1969) as a unitary, coherent, organizational type characterized by a relatively unchanging social core and overall function, with simple principles of evolution and variation. Robert Keith (1971) has contributed to the same subject with a discussion of tensions inherent in the structure of the encomienda and hacienda, because of their deviation from stable ideal types. If scholars carrying on primary research will continue to draw out the wider implications of their work, they can gradually create a corpus of theoretical writing that is truly relevant to the field. There is no better vehicle for this kind of matter than the too easily scorned genre of the interpretive article.[16]

Generalizing about generalization quickly becomes a tenuous enterprise, but some observations may be made by way of warning or preliminary guide. Recent work in many different areas and time periods points to the rich diversity and complexity of Iberoamerican society. Perhaps this trait was merely the result of geographical expanse and manifold ethnic origins. But perhaps one can go so far as to say that colonial Latin American society was characterized by numerous fine distinctions and gradations more than by sharply differentiated groups of people, whether European-style economic classes or North American-style impenetrable racial groups. Status groupings, ethnic groupings and patriarchal groupings as often undercut as reinforced each other.

In view of all this, it seems a little ironic that so much effort in past social theorizing in Latin American history has gone into the search for, designation, and ranking of fixed groups or strata. I think it would be well to put less emphasis on strata or blocks, and more on principles of organization and long-term tendencies. (Let it be understood that a social type is more a principle than a group.) Neither society nor conscious thinking about its principles is of one piece; there are divergent forces in society, and contradictions in its principles of organization. There is no need to postulate equilibrium in any given social system, much less one as new and plural as Iberoamerica's in the colonial period. The main antecedents, early modern Iberia and the Indian societies, were themselves each relatively heterogeneous. It is therefore appropriate to separate out individual trends and forces, then use them in a flexible way to explain varying situations.

Such an approach makes for a very healthy awareness that social principles are not the social reality itself. It may be that some such principles are simply observed regularities, but most are best considered as ideas or habits in the minds of people of the time, some of which may have served as tools to shape reality, while others may have been used mainly to try to explain it. I have the impression, for example, that whereas patriarchalism was a genuine, pervasive force in colonial Iberoamerica, notions of estates and corporatism have more the flavor of archaic explanatory theory.

Whatever the role of explicit social theories, there were other kinds of ideas, held broadly and perhaps unconsciously, far beneath the level of theory, that did clearly and visibly affect actual standard behavior. On every hand the workings of received tradition are evident; this is more than a pure matter of ideas, but can be viewed through that lens. One important tradition-maintaining mechanism is the pattern of ambition that each individual sets up for himself. Though these ambitions or social ideals are only rarely articulated, by following people's lives we can find out what they are; patterns of failure are particularly instructive. In other words, the technique of collecting career samples leads not only to the definition of social functions and types, but to the discovery of uniform social-psychological molds.[17] Such configurations are not vague and "idealized," but mercilessly specific and detailed, often varying little over long periods of time. In the Spanish American colonial world the most outstanding, widely-held social ideal was patriarchal-seigneurial, involving urban residence, large rural properties, livestock, servants, slaves, honorific office, and several other requisites. This ideal was imitated, with necessary modifications, by the lower levels of society. Associated with other social-economic functions we find goals just as specific and revealing, if less far-reaching. Separating the goals from the individuals who hold them helps make us aware that a very rigid and tenacious role definition is compatible with much mobility and growth in society.

It is becoming apparent that conscious innovation in colonial Latin America—perhaps in any new society—was at a minimum in the strictest sense. Whenever possible, existence was kept literally the same; beyond that, whatever was susceptible of treatment on analogy was so treated. The observation holds equally for all the main ethnic groups involved. It follows that antecedents are of the greatest importance, whether they be Iberian, Indian, African, patterns coming out of the general movement of European expansion, or continuities extending from early colonies to ones established later.

The state of scholarship, however, often prevents our drawing the longer lines. For the moment, the social history of Iberoamerica is more advanced than that of Iberia itself, in both the Spanish and the Portuguese spheres. While many persons engaged in research on this side of the Atlantic are struck by the settlers' conservatism, and intuitively presume wholesale carryovers, they can establish little connection between their personae and that odd assortment of poor nobles, priests, administrators, beggars, and Jews who still largely people the Spanish histories. Whenever Iberian research of adequate intensity appears, comparability emerges instantly. So far such work mainly concerns Seville. Enrique Otte has shown (1965,

1967) that the forms of commerce in the Spanish Indies were those of Seville. Ruth Pike's work on the Sevillan Genoese (1966) reinforces the point; the field eagerly awaits publication of her general social history of Seville in the sixteenth century. Her article on black slaves in Seville (1967), in conjunction with similar surveys of the subject for Puebla by Boyd-Bowman (1969) and for conquest Peru by this author (1968: 171–198), yields the extremely important conclusion that the whole complex of Spanish treatment of Africans reached maturity in Iberia, and was transferred to America intact, without discernible variation.

The inherent worth of a technique of comparison or, more prosaically, spot checking, is not in doubt; it was implied indeed in my previous concern for strategy in the choice of research topics. However, the comparative approach has frequently proved sterile or misleading. Too often it has meant that the comparer abdicates all responsibility for the validity, or even range of applicability, of the works he is comparing. We cannot let ourselves forget that historical comparisons must be made in the first instance not between directly observed phenomena, but between scholarly writings. Naive comparison of different types of research leads to wildly erroneous conclusions.

It may be objected that no one can muster sufficient expertise to test the validity of research in several different areas at once. This is true in part, but the comparer can still bring a critical attitude to his endeavor, and should at least acquaint himself with the *types* of sources and methods used, so that he will know what to expect. While sophistication can do much, the outlook for fruitful comparison is usually dim where one cannot find similar work on similar sources in the areas of interest. The comparer may perhaps justifiably grope in the unknown as long as his investigation is intercontinental in scope. But for work confined to Latin America the standards should be higher, even if the basic principles are the same. Indeed, generalizing or thematic work within Iberoamerica is probably not best considered "comparative." The very term implies going outside one's ordinary purview. Our normal field of vision should be no less than all of Spanish and Portuguese America. Social-economic investigation is making clear that Iberoamerica is one unit in which regions ignore political borders, while uniform rules of regional variation and chronological evolution apply to the whole, Spanish America and Brazil alike.[18]

Our field seems to have arrived at a stage where the most important tasks—choice and execution of research projects, establishment of basic concepts, and substantive generalization or "comparison"—all demand neither detail-shy theoreticians nor purely document-oriented investigators, but flexible minds who can see the general within the particular.

NOTES

1. This I have always taken to be Hanke's main emphasis. However, in a recent statement (1971) he implies 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 for the most part guilty of a simplistic distinction between law and reality. If

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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: 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 its whole context. Most of the general principles of interpretation one can draw up are negative, such as the principle that legislative reiteration indicates noncompliance.

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 he probably would not defend today.

3. Some articles along these lines are already appearing. Edgar F. Love (1971) has 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 are not unexpected, Love establishes the nature and extent of mixture among lower urban groups beyond all reasonable dispute. For eighteenth-century Chile, Jacques Barbier (1970) has 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. In general, Barbier's article reinforces the growing belief 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.

4. The general outlines of this work may be seen in Schwartz' recent article (1970).

5. Eulalia Lobo (1967) has carried out a useful social statistical analysis of Thayer's work, but any such attempt must naturally remain the captive of Thayer's now 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 de méritos are extremely easy to misuse; an inflated, unbalanced biographical sketch can be readily concocted merely by rephrasing the questionnaire. Reading a dozen probanzas 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 a probanza. As incredibly distorted as they often are, probanzas hardly ever contain outright falsehoods. If a person

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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 probanzas 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 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 as about the principal figure.

6. A somewhat related development is the preliminary work of Lewis Hanke, often in collaboration with Gunnar Mendoza, on Potosí, with an emphasis more social than technical. Hanke has edited chronicles, described and given samples of archival materials including notarial records, and provided provisional syntheses. See Hanke, 1956, 1965, 1970, and Capoche, 1959.

7. In the classification of sources above (p. 4), 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 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, are 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 fragmented 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 (see Lockhart, 1968: 89), 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, and part publication of letters (1966b, 1968).

Otte has also discovered and begun to exploit (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 (1966a). 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. Whatever else their value, a volume of these letters would make a far more useful and appealing teaching aid than many of the problems anthologies with which we are deluged.

8. Several American doctoral students seem to be studying haciendas in some specific locality. A forthcoming book by William B. Taylor, *Landlord and Peasant in Colonial Oaxaca,* of which I have seen only the conclusion and bibliography, is based on a thorough survey of all known local documentation, plus much in Mexico City and Seville. It covers the whole colonial period, with emphasis on land tenure. Taylor rivals Góngora in thoroughness in

this respect, while going further than his predecessor toward a general regional study. He finds estate development retarded in comparison to central Mexico. Indian communities in the area retained most of the land and even the livestock, to the end of the colonial period, while Spanish properties were mainly not consolidated or stable before the nineteenth century. The main characteristic of the situation seems to be the interrelated operation of three factors: a non-intensive export product, cochineal, with little else in the way of marketable resources; few Spaniards; and a relatively dense, well-organized Indian population.

Robert G. Keith recently completed a dissertation on the origins of social and land tenure aspects of the hacienda on the Peruvian coast, on the basis of varied documentation including notarial records, law suits, and *composiciones*. Keith traces the evolution from reliance on the encomienda through a time of small or medium holdings to the rise of plantation-like haciendas by the seventeenth century, and shows how the development relates to a broad spectrum of factors including demography, geography, markets, crown policy, and local politics. Keith A. Davies, a University of Connecticut doctoral student, is investigating the relation between Indian agriculture, small Spanish farming, and haciendas in selected valleys of the Arequipa region. The Editor has also brought to my attention a University of Texas thesis on the holdings of the Sánchez Navarro family in northern Mexico in the late colonial and early national periods, by Charles H. Harris, III.

9. 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. Barrett believes that the Cortés hacienda is 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. See also the forthcoming work on the Cortés estate in the conquest period by G. M. Riley (1972).

10. Schwartz' substantial study of the lavradores is part of his ongoing major research project on the Bahia sugar complex, centering on Sergipe do Conde. James D. Riley has completed a thesis (1971), not seen by me, on the management of Jesuit estates in the Valley of Mexico in the eighteenth century.

11. For further critical-bibliographical discussion of the history of the great estate, see Lockhart, 1969.

12. 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, most writers on the Inquisition have read only the trial sentences, or if they go so far as to use the testimony, only employ it to redirect the focus on the institution, or at most on Jews as persecuted people. 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 Greenleaf (1969), Wiznitzer (1960) and Liebman (1970).

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

Given the preoccupations of the time in which I write, perhaps it is necessary at this point to insist that women are not only an essential part of any balanced treatment of social history, but through their presence or absence, their marriages, dowries, activities, and property-owning, are an absolutely indispensable measure of the quality and velocity of general social development, as well as of any individual male's rank, prosperity, and affiliation. Women appear regularly in all the Iberian sources from cedularies to litigation; career skeletons and even full, intimate portraits are not much harder to produce for women than for men of the corresponding categories. Some examples may be seen in Lockhart, 1968: 150–170, 197–198, 210–211, 216.

14. Such suspicions run through Lawrence Stone's recent article on prosopography. The differences between the biographical tradition now growing up in colonial 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, 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. Many of the weaknesses Stone finds 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 are not likely to try to do in any case.

15. A possibility, and one that I personally find attractive, is to do a second generation of studies of types already done—viceroys' biographies, and the like—with more intensive use of wider documentation, and more awareness of the broader context.

    Another avenue is the study of social explosions. The sad historiographical mishandling of the Túpac Amaru revolt does not condemn such research in principle. It is true that a sustained interest in revolts as revolts will tend to turn any area into an apparent hotbed of rebellion. On the other hand, revolts generate an incredible amount of testimony and litgation, often reaching into all aspects of society, and perhaps revealing distinctions not ordinarily visible. The object of interest would be not only behavior manifested in the upheaval, but the back reflection on normal processes. John Phelan's ongoing work on the *comuneros* of New Granada should be an illuminating pilot study.

16. Despite the appearances, the social theory of the Latin American colonial city must be newly erected from its foundations. The considerable theoretical discussion of the issue, by figures as distinguished as George Kubler (see Morse, 1971, for bibliography), has been principally within a legal, formal, esthetic framework. Most general treatments derive their information from the chronicles, with resulting emphasis on the institutional and touristic sides of the city. Richard Morse brings breadth and sophistication to urban analysis, but even in his latest survey of the subject, his interpretations of the colonial city are applied to essentially institutional-intellectual data. He shows no inclination to take cognizance of the social content of the city, as can be seen by the absence from his newest bibliography (1971) not only of the recent urban-social surveys (Russell-Wood, 1968; Lockhart, 1968), but even of the somewhat socially-oriented recent municipal council studies (Boxer, 1965; Moore, 1966). A discussion of the city which takes urban social reality as a starting point, then integrates the legal aspects, will arrive at a drastically different vision of the city's nature and function than one which starts with imperial-legal sources and considerations, while ignoring society.

17. 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 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 and chronicles, 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 colonial 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.

18. A work which moves in the direction of treating Latin America as a social-economic and ecological unit rather than "comparing" is Magnus Mörner's book on race mixture (1967). The study is illuminating, but Mörner himself is fully aware of the extent to which any thematic work must depend on uneven previous scholarship and already existing categories of analysis.

Two books have come into my hands at the last moment, too late for me to assess them fully or even digest them. Brian R. Hamnett's study of administration and the cochineal industry in the Oaxaca area at the end of the colonial period (1971) could have been discussed above in the section "On the Edges of Institutionalism." In many ways a conventional British administrative study, it achieves novelty and significance through its concentration on a single remote subregion, and its exploitation of the correspondence not only of major officials, but of the local *alcaldes mayores* and *subdelegados*.

Mario Góngora (1970) has now carried out much substantive social investigation related to estates, the lack of which I above pointed to in his earlier study of the Puangue valley. Liberated from the constriction of land tenure, Góngora uses mainly litigation to reconstruct individual estates in all their economic-social flesh and blood at various times in the epoch 1580–1660, delineating the types of people and estate forms, and masterfully bringing out the larger patterns or continuities in the gradual, unplanned transformation of the Chilean countryside.

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