

TRENDS AND DIRECTIONS OF RESEARCH ON PHILIPPINE HISTORY: AN INFORMAL ESSAY*

NORMAN G. OWEN

AUTHOR'S NOTE

This paper does not in any way pretend to be a definitive account of the "state of the field." I have chosen to include the names of certain scholars in order to avoid boring and bloodless abstractions, but the names used were just those which occurred to me as I wrote, and the omission of others does not in any way imply a judgment as to their worth, or lack thereof. I do not regard this as a finished work; any suggestions or corrections sent to me (at the University of Michigan, Department of History, Ann Arbor, Mich. 48104) will be gratefully received.

The reader may wish to make allowance for my own perspectives on the field, which are shaped in particular by three factors:

(1) *I began studying Philippine history in the mid-1960's, and thus may tend to slight earlier achievements.*

(2) *My own research is on the last Spanish period, and I know that bibliography better than those of other periods.*

(3) *I am generally more interested in social economic change than in political, diplomatic, intellectual, religious, and cultural developments.*

In preparing to write this paper, I began by skimming through some of the bibliography of the field since World War II. While trying to sort out new "directions," some of which I will discuss later, I concluded that probably the single most important development has been the overall growth of the entire field, both in terms of the number of trained scholars working in it and in terms of the institutional and "infrastructural" support available to them. In the first decade after the war, there were very few historical studies produced, perhaps because in the excitement of independence, scholarly attention was focussed more on current events than on the distant past. In the second decade, 1956-65, a number of fine scholars emerged, such as Teodoro Agoncillo, John Phelan, Horacio de la Costa, Theodore Friend, Onofre Corpuz, Cesar Majul, Edgar Wickberg, Josefa Saniel,

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and Maria Lourdes Diaz-Trechuelo. Many of these pioneers had to make their own way in Philippine history; their formal graduate training was likely to have been under a committee of Spanish colonial, American diplomatic, and East Asian historians rather than Philippinists, or even Southeast Asian experts.

The third decade, now closing (1966-75) has brought to the front Grant Goodman, Oscar Alfonso, David Joël Steinberg, John Schumacher, Nicholas Cushner, John Larkin, Bonifacio Salamanca, Michael Onorato, William H. Scott, Serafin Quiason, Maria Rodriguez Baena, W.E. Cheong, and Leslie Bauzon, along with many other young scholars whose names are not yet well-known but whose accomplishments may be considerable. Without suggesting any comparison between the *talents* of these "generations," it seems clear that the younger scholars have had the advantage of better formal training in Southeast Asian history, enjoy vastly superior bibliographic aids and other supports to research, and are in a position to benefit from the pioneering works of their predecessors. And they are more numerous — besides those mentioned, there are others, not just Filipinos and Americans, but Australians, Englishmen, Spaniards, Mexicans, Russians, Japanese, and other Asians. Some of their research may be outstanding, some pedestrian, but the total accumulation of scholarly knowledge on Philippine history keeps growing at an ever-increasing pace; textbooks and syntheses are becoming out-of-date far more rapidly than ever before.

The intellectual and psychological reasons for this surge of interest in Philippine history are beyond my competence to judge. One can only speculate: Did the latest American involvement in Southeast Asia provoke an examination of this earlier venture? Was a new awareness of the Philippines created among Peace Corps Volunteers and their friends, or among servicemen, exchange students, former government officials? Were Filipinos driven to seek their national identity in their own past because "independence" by itself did not turn out to be the answer they sought? Has the whole "Third World" become so important in this age of global interdependence that the story of its development (and under-development) cries out to be discovered and told? Whatever the underlying cause, in practical terms the past twenty years have been a time of unprecedented individual and institutional support for the student of Philippine history. It is not cynical, I believe, to see some of the scholarly interest as a res-

ponse to this opportunity, to the hope of support in this fascinating field.

In the United States, this is associated chiefly with the rise of "area studies," at university "centers" funded by federal and foundation monies — very much a phenomenon of the 1960's, although some of them carry forward, with diminished thrust, into the 1970's. The Southeast Asia programs provided employment and a favorable academic environment for established scholars; they brought together bibliographic facilities, they published papers and journals, and they provided not only training, but funding, for many graduate students. In the mid-1960's when I entered the field, it seemed not only feasible but even farsighted to be embarking on the study of Philippine history. Now, in response to the general economic situation, as well as a kind of neo-isolationism, many of these programs are shrinking or collapsing for want of funds. Students are still accepted, but they find it much harder to get fellowships, and all but impossible to get jobs. It appears that this particular boom in the training of Philippine historians is almost at an end, although there are enough graduate students still in the "pipeline" to produce new dissertations for another five years or so.

Other developments in the United States may serve to cushion this trend. The proposed Philippine Studies Program at the University of Hawaii, if it is funded, may take up the slack left by the decline of other university centers. Money for fellowships and opportunities for publication still exist, and they have by no means fallen to pre-1960 levels yet. Furthermore, some of the infrastructural aspects of scholarship are stronger than ever. The formation of this Philippine Studies Committee of the AAS, and the publication under Mike Onorato of its *Newsletter*, have led to greater communications within the field than ever before, and this cannot help but stimulate the sharing of information and the cross-fertilization of ideas. In the bibliographic sphere, besides the *Bibliography of Asian Studies* and the mimeographed listings from the Cellar Book Shop (both of which seem timeless by now, although they are not that old), we have within the last few years also seen the Library of Congress catalogue of its Southeast Asian holdings, the guides to Philippine-American historical literature by Onorato and Stanley, and Shiro Saito's *Philippine Ethnography: a critically annotated and selected bibliography*. Other newsletters, working papers, and mimeographed lists tell us of the availability of Philippine newspapers, or the location of sources

on martial law, or books for sale in the Philippines, or research in progress, or microfilms available for sale or loan. In addition, American scholars who can get to the Philippines are able to take advantage of much of the infrastructural development there over the past decades.

My prognosis, then, for historical research on the Philippines within the United States is that the total number of trained historians will continue to grow for a few more years, but will then reach a plateau, and the total number of Ph.D.'s awarded to Americans in this field may actually be lower in the 1980's than in the 1970's. But because of their improved training in the field, because of the bibliographic guides and documentary holdings readily available to them, and because of the pioneering studies of the 1950's and 1960's, the scholars of the next decade or two may, in spite of any numerical decline, be able to do more research with less effort. I expect them to rewrite Philippine history almost completely from their improved vantage point, "standing on the shoulders of giants." Such a prognosis is speculative, not scientific; it is based on no inside information, and it assumes that there will be no radical change in the intelligence or character of scholars, nor in the mildly depressed state of the American economy. But if things go on the way they are going — which is, I suppose, what a "trend" means — this is what I foresee.

It would be, of course, quite parochial to view the rise and fall of American university programs as the sole or even the primary factor affecting the field of Philippine history. Interest in, and support for, the field still seems to be high in Australia, Japan, and several other countries not usually thought of in connection with Philippine studies, although none of these yet has a program or a "critical mass" of scholars comparable to those of the United States and the Philippines. More significantly, research opportunities and support for Filipino historians in the Philippines itself have been growing over the past twenty years, and are still high. We may be on the verge of seeing the center of gravity in Philippine historiography shift from the U.S.A. to the Philippines itself. Despite the numerous co-optations of trained Filipino scholars into the world of business and government, there are enough left in the academe to form a solid core of historical scholarship — Agoncillo, Alfonso, Foronda, Majul, Salamanca, Saniel, Tan, and others, plus such younger scholars as Luz Ausejo, Leslie Bauzon, Soledad Borromeo, Rosario Mendoza Cortes, Edilberto C. de Jesus, Jose Arcilla, and Maria Fe Romero, not counting

those still working in the United States (*e.g.*, Salvador Escoto, Milagros Guerrero), Australia (Reynaldo Ileto), and elsewhere.

The growth of research infrastructure in the Philippines has been phenomenal over the same two decades, and continues to grow along with the production of scholars. The University of the Philippines and Ateneo de Manila University, in particular, have become major centers of scholarship on Philippine history and culture. They have assembled fine groups of historians and other social scientists engaged in teaching and research, they lead the way in the micro-filming (including microfiche) of rare books and documents, they are building up fine collections of Filipiniana, which are relatively well catalogued, they publish journals and monographs, they sponsor seminars and speakers. Neither school has yet the strength in the broader field of Southeast Asian studies that the best American universities do, but in the specific field of Philippine history, they provide excellent facilities and faculties. Other Manila schools, such as the University of Santo Tomas, University of Manila, and Far Eastern University, also sponsor and publish historical scholarship, though on a smaller scale. But the health of the field is suggested even more strongly by the growth of research at provincial universities such as Silliman (Dumaguete), Central Philippine (Iloilo), San Carlos (Cebu), Xavier (Cagayan de Oro), St. Louis (Baguio), Mindanao State and Dansalan College (Marawi City), and Notre Dame de Jolo (Sulu), among others.

Non-university support for historical research is also good. The National Library, the Lopez Memorial Museum, the American Historical Collections, and the Ayala Library have all published catalogues of their extensive Filipiniana holdings within the past few years; the National Archives and the Dominican archives are embarking on the far greater task of listing some of their thousands of manuscripts. Journals such as the *Philippine Historical Review* and the *Historical Bulletin* provide opportunities for scholars to publish the results of their research, as do commercial presses such as Solidaridad, Bookmark, and Alemar, which are in this respect well ahead of their American counterparts. A new popular serial, *Filipino Heritage*, has collected articles on various aspects of history and culture by some of the finest scholars in the field; its publication, anticipated this year, should help to compensate for some of the misinformation and outdated scholarship found in a few of the history texts still used in schools. The Filipiniana Book Guild and the Historical Conservation Society have made many of the more im-

portant historical sources available, in English translation, to a much wider constituency. Bibliographic aids are also plentiful: besides the catalogues of the major collections, there is one volume (A to O) of the *Union Catalog of Philippine Materials* in print, specialized bibliographies of imprints before 1699 (by Bernardo), Chinese in the Philippines (by Chinben See), and of Mindanao and Sulu (by Tiamson), and descriptive guides to archives abroad by Domingo Abella, Maria Lourdes Diaz-Trechuelo, and Helen Tubangui, among others. Re-publication of classic works such as Retana's *Aparato* and Blair and Robertson has made earlier bibliographic research more accessible to many scholars. Regional and national history conferences have been sponsored by the National Historical Commission and by local colleges and societies. The participation of "amateur" local historians has been encouraged, and their contributions, even when lacking in full scholarly rigor, have often helped the "professional" historians.

Perhaps the single most promising trend in historical research in the Philippines is the formation of the Philippine Social Science Council, which publishes the *PSSC Social Science Information* and administers the Modern Philippine History research grants. The council and newsletter provide a structure for communications among scholars, which is particularly important as the field becomes larger and more spread out, with researchers no longer concentrated in a couple of Manila universities. The grants are important because they provide (for the first time, so far as I know) some form of financial support for established scholars, so that they can take the time, even if only a term or two, to do further research. One of the great limitations of Filipino historiography so far has been that many scholars who have produced one good book (usually their thesis) are thereafter forced to bury themselves so deeply in teaching that they never get a chance to use their hard-won research skills on a second subject.

There are also some clouds in this generally sunny picture of historical research in the Philippines today. Martial law has brought about a certain amount of censorship, direct and indirect; it has led to the detention of some scholars and the self-imposed exile of others; it may have increased political polarization within the academic community, always an arena of personal and political friction. The National Archives, by far the most important source of primary materials on the nineteenth-century Philippines, has been virtually

closed to scholars for most of the past two years, with no end in sight. Meanwhile, the endemic problems of scholarship in a developing country continue, most of them having to do with the allocation of resources in a situation of scarcity — *i.e.*, money. Filipino historians are still shockingly underpaid and overworked in terms of their education and their potential earning power in other occupations. A job in business or in the bureaucracy may seem to offer, not only far more money, but the opportunity to contribute more directly to national development and even, in some cases, more time for research! The Modern Philippine History grants are only a partial palliative here; they are short-term, small in comparison with American grants (providing very little for travel and microfilming, for example), and few in number. They do not solve the profession-wide problems of low wages and long hours, and although there are other efforts to upgrade the pay and working conditions of academics, they have not achieved much thus far in the face of general economic instability.

Any general prognosis for historical research in the Philippines must put it in the total context of Philippine society and economy. If the current situation remains stable, I would like to believe that the institutional support for history, the infrastructural aid of publication opportunities, bibliographies, research grants, conferences and the like, and the cadre of fine young scholars in the field would lead to the continuation of this exciting growth in Filipino historiography. But if the political situation deteriorates, it may lead to more censorship, to obstruction of research and destruction of resources, to limitations on travel and on interviewing, to more energy expended in polemics rather than scholarship, and perhaps to more scholars seeking permanent exile. And if the economic situation gets worse, with rising oil prices and falling export prices, the resources now allocated to “luxuries” such as historical research in all its aspects — salaries, university centers, libraries and archives, publications, etc. — may be diverted to other more “necessary” ends; we may see programs shut down, journals fold, and research projects abandoned. For this to happen just when the Philippines seems to be coming into its own in historical research would indeed be tragic.

So far this paper has dealt only with the “productivity” of Philippine history, not with the contents of what is produced. The major trend in this latter area is suggested by the major trend in the former, by the enormous increase in the number of scholars and the

volume of scholarly research in the field. With more scholars it is possible to explore more different kinds of history, a greater variety of regions, periods, sources, and approaches, to seek out the new without abandoning the old. When I began this paper, I thought I knew what the "mainstream" of Philippine history was, and how it had shifted over time; I came to realize this "mainstream" of mine was just one channel of a deltaic river spreading widely in all directions. "Classical" approaches continue even while other scholars are trying out new ways of looking at Philippine history.

I will hazard the generalization, nevertheless, that three broad tendencies in post-war scholarship can be distinguished, each more predominant at a certain point in time. The first of these, chronologically, is classical colonial history, focussing primarily on the motives and actions of the Western imperialists, both at home (Madrid, Washington), and in their colonial capital — Manila. This type of history is predominant before the war — in Spanish historiography, in many of the writings of American colonialists, and even in such classic institutional studies as Schurz on the Manila Galleon and Cunningham on the Audiencia of Manila. It was picked up again after the war, and carries through to the present; at worst, it degenerates into "What the Governor said to the Archbishop," but at its best it can illuminate the whole complex network of colonial motives, policies, and practices in the Philippines. Thus we have recent research on metropolitan decision-makers, such as Victor Balaguer and Theodore Roosevelt; on colonial governors-general, from Bustamente y Bustillo to Frank Murphy; on colonial institutions such as the Royal Philippine Company and the early Philippine Constabulary; on foreign residents in the country, from Spanish Jesuits to British merchants; on the whole range of development schemes, defense plans, and diplomacy as seen from the colonial perspective. Viewed simply as problems in colonial history, much remains to be written; there are, for example, growing numbers of monographs on the late 18th century (Bourbon reforms) and the early 20th century (Schurman through Leonard Wood), but no consensus on these eras has been achieved, and there is almost nothing in between. In absolute terms, the volume of scholarship on these classical colonial questions continues to grow. But I sense a shift in the field as a whole away from this approach. The best way to study Philippine history, many scholars believe, is not to spend more time on Basco y Vargas or William H. Taft, but to try to discover what the Filipinos were do-

ing, how their lives were changing and what they thought about it.

Traditionally, the study of the Filipinos in their own history — with a few honorable exceptions, such as Dapen Liang — tended toward gross generalizations combined with hagiographic works on Rizal. (One rather extensive catalogue of Filipiniana lists over 250 titles in English of Philippine “biography” to 1966. *One half* of these are on Rizal, and this does not include another 200-odd books, articles, and translations which treat him as a literary figure!) Modern “Filipino-centric” history, as I see it, really emerges in the late 1950’s and early 1960’s in such key works on the late 19th century and early 20th century as Agoncillo and Majul on the Katipunan, Malolos, and Mabini; Achutegui and Bernad on Aglipay; Schumacher on the Propaganda Movement; and Leon Ma. Guerrero’s unusually perceptive critical study of Rizal. For the American period, studies in the mid-1960’s by Friend, Steinberg, Goodman, Salamanca, and others played a similar role. These analyses of nationalism and political development were often made more sophisticated as historians came to employ the insights of social scientists such as Landé, Lynch, Guthrie, Jocano, and Hollnsteiner (most of whom were also working in the early and mid-1960’s.) The systematic analysis, rather than patriotic and filiopietistic chronicling, of the lives and thoughts of the Filipino national elite is still young; it is being ably carried out at present in such works as Vicente Pacis’ biography of Osmeña, Schumacher’s studies of Burgos, histories of Philippine nationalism by Agoncillo and Usha Mahajani, Renato Constantino’s intellectual biography of Recto, Ronald Edgerton’s study of post-war reconstruction, and the sections dealing with Federalistas, Quezon, and Osmeña in such colonial-oriented histories as those of Onorato and Peter Stanley. Another index of the importance of this “nationalist” approach is the re-publication of *La Solidaridad* and the works of Rizal, Burgos, Aguinaldo, Mabini, Ricarte, and Kalaw, as well as more modern memoirs by Quezon, Laurel, Vargas, and the like.

This approach to Philippine history, though still important and even growing, is, I believe, about to be overtaken by a third wave, one which has only begun to gain strength within the past decade. This is an attempt to go beyond the definition of the Philippines as the story of the Manila-based elites, to a history of all the Filipinos, in the provincial towns, in the barrios, even up in the hills. The

roots of this new movement are many and tangled. They would include the influence of social sciences, particularly of anthropology; the realization that many of the Filipino national leaders did not seem to represent the ideas and interests of their constituents in the countryside; the Marxist perspective on society, which looks for differences between classes as a prime factor in historic change; the nationalistic ideal of finding out what "the people" were thinking and doing; the frustrations of trying to write "Filipino-centric" history before the nationalist movement emerged in the late 19th century; and the example of social historians of the West and of other Asian countries, who were opening up exciting new vistas of peasant movements, finding the voice of the voiceless masses.

In a broad sense, Philippine historians had tried to deal with the questions of the masses and their welfare for a long time; but without specific research on the subject, the answers were likely to be superficial. Many historians had to deal in generalities, based on their own perceptions or those of the authorities they quoted, whether colonial officials or Filipino politicians: the Filipinos did/did not resist the Spanish conquest and conversion, they were/were not better off in the 19th century, did/did not generally support the Revolution, liked/disliked the Americans, benefited/suffered from American rule, etc. The economic historians, although often over-concerned with government policy and policy-makers (particularly with the fascinating debates on tariff bills in the U.S. Congress), did at least try to substantiate these generalizations with some quantifiable evidence on the terms of trade, population growth, rice consumption, agricultural productivity, and other indices of per capita welfare. But further refinement did not come until a number of scholars, beginning in the late 1950's and early 1960's, began to go back to the primary sources and to re-examine them closely and rigorously for specific evidence on specific questions about a certain group of people living at a certain point in time. Pioneers of this approach included John Phelan, in his study of Hispanization; Felix Keesing, in his *Ethnohistory of Northern Luzon*; David Sturtevant, in his work on the "Guardia de Honor" and other peasant movements; and Canute Vandermeer, in his work on the historical geography of Cebu. But the full potentials of the approach were not realized until Edgar Wickberg and John Larkin began to explore the extraordinary resources of the Philippine National Archives, turning up hundreds, even thousands of old manuscripts that could be

brought to bear on precisely such questions as they were asking. The 19th century in Philippine history had been seen as a blur of misgovernment out of which a righteous rebellion of nationalism suddenly exploded; Wickberg and Larkin were able to expose at least some of the whole complex process of economic development and social change which occurred in this transitional period.

The new focus which this approach to Philippine history took seems obvious in retrospect; it requires some effort to realize that it was largely ignored or thought impossible only about fifteen years ago. So long as the subject was in Manila, whether colonial officials or Filipino elite, it was possible to treat the "Philippines" as more or less a unit, and the "Filipinos" as a generally undifferentiated mass, save for the token recognition that the non-Christians were different (perhaps not properly "Filipino" at all). But once the focus was shifted away from Manila, it was immediately apparent that, even within lowland Christian areas, there were strong and significant differentiations. One immediate aspect was geographic — provinces and regions differed in ecology, in language and culture, in religious and political administration (some had friar estates or military *comandancias*, others did not), in security, in proximity to Manila, in ease of transportation, in the whole course of economic and political change. Local history had traditionally been left to amateurs, to antiquarians who delighted in the dates of foundings of towns and appointments of bishops, lists of the names of town priests and gobernadorcillos, local folklore and unauthenticated tales of religious miracles and revolutionary heroes — or so it had seemed to the professional historical community. But Keesing, Vandermeer, and Larkin began to show that it was possible to write a real history of these different areas and to explore significant changes in local society, each developing according to its own pattern of political and economic interaction. Larkin's study of the Pampangans, far more ambitious historically and based on a much wider range of documentary sources than the others' works, was influential well before its publication in 1972, and he became the chief prophet and godfather of the genre. It was an idea whose time had come.

At present, besides the works already mentioned, there are histories of Pangasinan, Negros, and Panay in print; articles on Ilocos, Batangas, and Misamis; journals, such as *Leyte-Samar Studies* and the *Ilocos Review*, primarily devoted to local/regional history and culture; Ph.D. dissertations on Nueva Ecija and Cavite; numerous

smaller studies on other provinces; and major research underway on Cebu and Negros (two historians on each), Cagayan, Ilocos, Kabikolan, Samar, and Iloilo, that I know of. Two distinguished historians whose previous works had been on national history have just published major works on the non-Christian areas — William H. Scott on the uplands of Northern Luzon, and Cesar Majul on the Muslim South. Other ethnohistoric studies are in print and in progress on Cotabato, Sulu, and other southern provinces, and although recent fighting may have disrupted research in some areas, it has also produced a new wave of official interest and funding for research on the Islamic areas. In both this region and the Mountain Province(s), there is a small but growing awareness of the importance of preserving local legends and the memories of old men before they disappear.

Even among studies which are not strictly “regional” or local, there is a stronger sense of the complexity of Philippine society, an attempt to differentiate the previously undifferentiated “Philippine” nation. Research which in an earlier period might have been purely institutional or national now seems to try to put events in the regional context — the tobacco monopoly, the friar estates, the Hukbalahap movement are studied by scholars who are quite aware that these existed in the Cagayan Valley, or Cavite and Laguna, or Pampanga, not just “somewhere in the Philippine countryside.” Other lines of approach may concentrate on the differences between rural and urban development, with its implication that the cities may have had a very different historical experience from the rural barrios; or on ethnic groups, Chinese, mestizos, creoles, Filipino in-migrants from other provinces; or on classes, distinguishing *illustrados* from ordinary *principales*, and both of these from ordinary *taos* or *timawas*, to say nothing of the *vagamundos* who existed on the margins of official society. Once any sub-group is studied, some new differentiations may appear; thus Wickberg points out the gap between the handful of wealthy Chinese merchants and the hundreds of petty clerks and agents they employed, while other scholars are studying specific cities now within the general framework of “urbanism.” I genuinely believe that, within five or ten years, there will be scholarly histories of almost every region in the Philippines as well as a number of other studies focussing on settlement size, ethnicity, and class, so that a new synthesis may be possible. Already informal comparisons, based on the sharing of unpublished research, have proved interesting; the “com-

mercialization" of agriculture, which seems to begin in central Luzon in the late 18th century does not really affect Kabikolan until the 1820's or 1830's, and does not reach Samar in force until the 1870's, so the pattern of nationalism and revolution which occurs is quite different. I believe that when we look back then on our current generalizations about the "Filipinos" and how they thought and behaved at a given point in time, we will find them crude at best, and downright foolish at worst. Of course there will be similarities as well as differences, and it is not unreasonable to hope that out of these various specific studies will come a clearer picture of those elements which are more nearly "universal" in Philippine history, not merely local or transitory.

This new approach to history calls for the utilization of new sources, and for the re-examination of old sources in new ways. The standard published works in Spanish and English (including the ever-valuable Blair and Robertson) and the great repositories such as the Archivo General de Indias, in Seville, and the U.S. National Archives, in Washington, are still very much part of the repertory of historians. Within the past decade or two, there has also been increasing use of a number of other archives and libraries containing the same general types of evidence — the Philippine National Archives, above all, but also the Lopez Museum, Ayala Library, National Library, and Dominican archives in the Philippines; the Newberry Library in Chicago and the Lilly Library in Indiana, as well as special collections at many other American universities and libraries; in Spain, the Archivo Histórico Nacional and the Museo Naval in Madrid, the Franciscan archives in Pastrana, the Augustinian in Valladolid, and the Jesuit archives in San Cugat del Valles. In all of these archives, as well as others such as the Archivo General de la Nación, in Mexico, there is still much to be done in the way of preliminary surveying; in some of them, bundles of manuscripts exist that have not been opened since they were first tied up over a century ago. But the major trend that I see is not the sheer number of documents available, though that is impressive. It is the way these documents are used. For those who considered Philippine history to be essentially the story of Legazpi, Rizal, Taft, and Quezon, there was a certain tendency to skim through documents rapidly in search of the Big Names of history, and to ignore everything else as not worth the time required to read it. Others approached research willing to accept the emphases found in the documents themselves, to deal with con-

troversies between regulars and seculars, or complaints of *caciquismo*, or debates on tariffs or on education as representing the important issues of the day. This at least has the advantage of getting into the thought patterns of those who kept the records, whether colonial officials or nationalist politicians. But to get at the history of a whole society, we must look for evidence on people who left very little in the way of personal documentation, and who were generally considered unimportant by those who kept the records, who misunderstood them at best and hated them at worst.

Thus the new historians are looking more for reports of conditions in the countryside, on the friar estates, for price fluctuations, for patterns of behavior — migration, rebellion, revitalization — which will suggest something of the thoughts of those whose words are lost, for petitions and court cases and voting patterns of individuals whose names mean nothing at first, are not found in any textbook and probably never will be. There is plenty such material in the old sources, and it is by his use of these, rather than his opening up of new manuscripts, that John Phelan won acclaim for his study of the *Hispanization of the Philippines*. Some archives are better than others for this approach; it is precisely the greatness of the Philippine National Archives that it consists of materials apparently considered too unimportant to be sent to Madrid for decision! Thus it is rich in routine reports, petitions, disputes, and local elections which tell us about ordinary Filipinos, while the corresponding archives in Spain are top-heavy with such “important” matters as debates in the cabinet over colonial policy (which was often a dead letter in practice) and the appointment, promotion, and salary disputes of every Spanish official.

A number of other sources, often thought unimportant or unusable by traditional historians, have been utilized by some of the recent regional and social historians. Local politics can be followed through vernacular newspapers and pamphlets. Detailed reports on the Huk-balahap and other peasant movements were found in the military archives at Camps Aguinaldo and Crame, before these sites were put to other purposes. Researchers are interested in interviews and family papers not just of Famous Men and a few others who knew them well, but of a much broader range of Filipinos — participants in rebellions, in local politics, pioneers on a new frontier, witnesses of unspectacular economic change. The “Historical Data Papers,” in the Philippine National Library, are compilations of fact, folk-

lore, and fantasy compiled in the early 1950's by schoolteachers; in terms of "national" history they are both irrelevant and unreliable. But a researcher immersed in local history can test some of the more questionable assertions against evidence from other sources, and can use these papers, with some hesitation and prudence, both for facts not available elsewhere and for at least a suggestion of the local attitude toward happenings; they have become a valuable source.

Potentially the most extreme case of new history using sources differently is the parish records; baptisms, marriages, and deaths. If these have been used at all in the past, it has been only to verify the date of birth or parentage of a national hero — Aglipay, Rizal, or Burgos. But might not they also be used to estimate birth and death rates, age at marriage, life expectancy, in- and out- migrations, frequency of epidemics, and other indices of welfare of an entire community? In Europe and the United States, such studies, particularly where they can be carried to the point of "family reconstitution," have revolutionized our knowledge of pre-industrial society. With a little additional data, which the archives may contain, these records can also tell us about occupations, about social mobility, about the harvests from year to year. Can this be done in the Philippines? The answer is not yet clear. Preliminary and rather rudimentary studies so far have yielded varying results; some hypotheses seem reasonable and worthy of further explorations, while others have backfired to any conclusion suspect. Within the next few years, Peter Smith, and perhaps others, will attempt an analysis of a few parishes in much greater depth; meanwhile, the Mormons are going ahead with their 10-year project to microfilm all the church records (before 1925) in the Philippines, a potential boon to further research in the field. Whether or not this line of inquiry proves fruitful, it is at least an imaginative *attempt* to see beyond the confines of Washington, Manila, and even the provincial elite, and understand the lives of all the Filipinos as they lived in towns and barrios, were born and married and died.

My prognosis for future directions in Philippine history is tentative and twofold. First, I believe that the current trend toward local history, toward the detailed study of specialized segments of Philippine society in specific periods of time, will continue strong for another decade or so. The major thrust of research so far has been toward the Spanish period, particularly the 19th century; an equivalent attempt to write good 20th century local history is only

just under way. The study of the 20th century demands more work in oral history and vernacular sources, comparatively less in archives and Spanish documents. It will both benefit and suffer from the fact that the issues and personalities it deals with are still alive — people will be able to talk about it, but they may not be willing to. It is a field in which Filipinos will have some obvious advantages, in command of local languages, potential contacts with informants, and simply in proximity to the areas being studied; although the US National Archives will remain an important source, no trips to Spain are necessary for this! But beyond all these details, there is the hope that the study of 20th century local history in the Philippines will come closer to almost any other kind of history to understanding how Filipinos *perceived* historical change. Other approaches at best give us certain behaviors of the masses and certain articulations of national leaders, but this has the potential to go one step further, to tell us how Filipinos saw their own history as it happened.

The second aspect of my prognosis is the continuation of the current trend toward the proliferation of approaches. No kind of history will die out, although new ones will be added. Colonial history is still an active endeavor, and for every “Philippinist” who lays it aside to get closer to the Filipino there will be some scholars in Seville or in some American History program who will pick it up again and carry it further. Similarly, our analysis of the national elite has barely begun, is still limited to a dozen or so key figures, many of them seen from only one viewpoint. How much do we really understand about Aguinaldo, the Federalistas, Sumulong, Roxas, even about Quezon and Osmeña before 1907 and how they climbed so far so fast? What about the Filipino secular priests, who date back to the Spanish period, or the Filipino military, or the bureaucracy as it developed — not just policies, but personnel, who they were, how they got there, what they tried to do? The biggest story yet to be written in Philippine history is a scholarly analysis of the national oligarchy in the 20th century and its political, economic and familial linkages as they developed over time. Almost every Filipino and Philippinist knows (or believes he or she knows) something about this, but who will put all the pieces together? Local history, meanwhile, will continue to thrive, and even when it is no longer the “vogue,” it will be an active field of research for those who don’t mind not being in the vanguard.

The new approaches yet to come are a matter for speculation, not any kind of "trend," and readers of the *Philippine Studies Newsletter* know as well as I what a wealth of proposals, over a broad range, there are. We may see more in the way of military history, demographic history, cultural history (connecting with recent folklore studies), pre-Hispanic history (rewritten in the light of current archeology), perhaps even cliometrics, which seems quite promising to me, and psychobiography, which does not. If I knew what the bandwagon of the future was, I would jump on it now, or at least recommend it to my students. There is one general tendency, however, which I feel quite sure will continue to increase — Philippine history will come to be more fully integrated with the world of Southeast Asia, seen neither as an adjunct to Spain or the United States, nor yet as an isolated and unique phenomenon. Parallels, as well as differences, will be seen among the different groups of lowland Christians; then between them and the other Filipinos ("Moros" and "Igorots"); then with the rest of the Malay world; then with Thais, Burmese, Vietnamese, and other Asians who share so many of the same elements of village society and culture, of ecology and economy, of colonial experience and the frustrations of nation-building and neo-colonialism. We may find, for example, that the abaca-growing smallholders of 19th century Kabikolan have much in common with the pioneers on the Burma rice frontier at the same time, while the Pampangans are closer to the Javanese, with whom they share conditions of population pressure and a rice-and-sugar ecological symbiosis. Peasant movements, urbanization, problems of tenantry, formulations of nationalism will be viewed in this wider context; if we break down Philippine history into regional or sub-regional parts, it is not with the purpose of isolating and fragmenting it, but of analyzing it more clearly, examining case studies for potential wider comparison. This effort has begun in the United States; it is less developed so far in the Philippines, where some texts still imply that Filipinos were Southeast Asians only before 1565 and 1946. But it will continue, and comparisons will be drawn. Some, no doubt, will be shallow and insignificant, but others, I believe, will strike a spark, open new insights, and point the way to yet newer directions for research on Philippine history.