Introduction:
Background and Course of the New Philology

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PHILOLOGICAL ETHNOHISTORY dominates the present book. What is it? Where did it come from? Many people in Latin American studies who are aware of it and have been influenced by it would not be able to give a reasonable answer to those questions. Here I will try to provide some perspective on the movement.

The progress of the early Latin American historical field from 1800 on can be seen in terms of a movement through the sources, following the principle that I call the conservation of the energy of historians: that one first uses the easiest sources available and does the easiest things with them, then on exhausting the greatest potential of each source and method marches on to another one, only a bit more difficult if possible.[1] An easy source is not only one found in an accessible place, written in a legible script (maybe even printed), but synthetic, already containing generalizations, judgments, and connected narrative, as opposed to sources that are more fragmented, individual, narrow in scope. It would also be in a familiar language. On most of these counts indigenous-language sources come at the very end of the line, especially the mundane records that have tended to be central to philological work in ethnohistory. So it was natural that as across the nineteenth and most of the twentieth centuries the field worked its way first through chronicles, then in turn high-level official correspondence and reports, the internal records of institutions, litigation, and notarial records, all in Spanish, indigenous-language materials had to wait until last. During this long time the history of indigenous people primarily dealt with Spanish attitudes and action toward them, usually as expressed in high-level polemics, laws, and administrative measures.

A breakthrough came with the work of Charles Gibson, above all his *Aztecs Under Spanish Rule* (1964), bringing the history of Spanish administration down to such a local level that the outlines of indigenous corporate evolution and its impact on Spanish activity became clear on a grand scale. But no research could have been less philological.[2]

 The present essay is a further development of my article "La Historia en los estudios latinoamericanos: el camino a la Nueva Filología," in Salles-Reese 2005. I have reduced the first portion, which was meant to give literary scholars and anthropologists a notion of the evolution of the whole field of early Latin American history, and expanded the second part, devoted specifically to philological ethnohistory.

[1]See chapter 2 of Lockhart 1999, especially p. 30. For those not familiar with them, that whole chapter and also chapter 12 there can give further insight into how philological ethnohistory belongs to early Latin American historiography generally and is a natural if not inevitable part of its evolution.

[2]Or so it seemed. Gibson studied no original texts at length, as such, and was not expressly analytical about language or terminology. In the core of his research he rarely descended to the level of the individual, where such things become truly operative. The crucial Nahuatl sociopolitical term *altepetl* nearly escaped him. (*cont'd*)

Indeed, the very term philology has come to mean little to many historians working in the Latin American field. I have seen myself introduced on a program as the creator of the New Philosophy. I take it, for the record, that philology has to do with close, systematic work with written texts, leading in the first instance to editions, but going on to many kinds of research in which the texts are kept in the forefront of the mind even in the final product of the research and not ultimately submerged as they are in much demography and economic history, and even in some narrative, institutional, and social history.

Yet if what some of us have been doing is a New Philology, it was preceded at a distance by an old philology. Several scholarly phases and generations stand between us and it, but it is still a large corpus and even still consulted. It originated and reached its height early in the development of the field, for the easiest thing of all to do with sources is to publish them, that is, if one forgets about translation, commentary, etc. Document publication, epitomized by the Chilean José Toribio Medina, dominated the nineteenth century and continued into the early twentieth. The greatest monuments of this stage are the innumerable ironically named *Colecciones de documentos inéditos*, which of course by the very fact of appearing are *éditos*. The type of thing included was overwhelmingly correspondence to and from institutional agencies at a high level, or sometimes entire manuscript books. Close to Mexican matters, this stage was reenacted in a large way as late as the mid-twentieth century by Herbert Eugene Bolton and his mainly Jesuit successors writing on the north Mexican "frontier."

Some of the people who did so much document collection became fountains of knowledge and insight, prominent among them Joaquín García Icazbalceta, who a hundred years ahead of his time knew, among other things, all the essentials of the origin of the legend of the apparition of the Virgin of Guadalupe. But most worked mechanically and produced neither writings nor knowledge; they founded their reputations and self-esteem on how many documents they had found that others had not.

After the huge florescence of this rudimentary philology, there came a deep,

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But that was because it hardly appeared in his Spanish sources. He discarded the scholarly terminology of his day, such as "mission," and used the *doctrina* and *visita* that he found actually used in the Spanish records, with profound implications for history and ethnohistory, in a way very similar to the procedure of his more openly philological successors. To move from the clichés of the time, such as "village" or "calpulli," directly to the altepetl would have been a huge leap. But moving from the *cabecera-sujeto* unit to the altepetl was simply to view the same thing from two different points of view, as seen by the Spaniards and as seen by the indigenous people, and the methods by which the two were discovered were the same. Nor did Gibson use inappropriate terminology brought from European comparisons, such as "peasant," that was absent from his sources.

For further discussion of such matters see the chapter on Gibson and ethnohistory in Lockhart 1991. As important as *Aztecs* was and is, *Tlaxcala in the Sixteenth Century* (1952) was in some ways more advanced and more philological. It actually closely exploited a major Nahuatl document, the Tlaxcalan Actas, though for lists of names and not for the text itself, and it concentrated on a single situation, a major technique of later philologically oriented studies.

long, and justified reaction against it, at least in the world of international scholarship (as opposed to the histories of individual cities, regions, and countries as practiced by proud residents, which often continued in the old way). Publishing documents fell to the bottom of the scale in prestige; it tended to disappear from the higher levels of the historical profession, from the best historical journals and the history lists of good university presses. Against that background, today's far more analytical and sophisticated indigenous-language documentary editions, even after producing many results and having received a good deal of recognition, still face considerable resistance and miscomprehension.

Meanwhile as the field of Spanish American history over many decades gradually and somewhat unevenly progressed through narrative/epic, institutional/intellectual and demographic/statistical stages, it was far removed from either philology or ethnohistory. By the 1960s and 70s, however, things were changing. The slowly grinding machine of historical research was now beginning to process fragmentary and unsynthesized material at the level of ordinary individuals, including notarial records, quotidian litigation, internal estate records, and private correspondence. The practitioners were converging from various directions and in various countries; in Spain Enrique Otte with studies of individual merchants and merchant firms;[3] in Chile Mario Góngora studying conquerors and the details of estate organization; and above all in the English-speaking world Bakewell, Bowser, Brading, myself, and others, in various ways synthesizing individual lives and small organizations from scattered materials. Though it was hardly labeled at the time other than as social history, I call this sort of work career pattern history, which though moving in many directions toward a fresh analysis, tends to start with the shape of newly synthesized lives, assembling them into groups and types and discerning processes.

But from the beginning, though originally of the whole group only Otte was doing anything like traditional document publication,[4] and it was rare to concentrate on entire documents or their genre traits, a certain kind of philology was an important part of the method. In tracing individuals one needs hallmarks: the titles, ethnic and gender characterizations, names of occupations, indications of citizen status that do or do not accompany the subjects' names. Discovering and studying the implications of these epithets was at once social history, intellectual/cultural history, and language research.

In this way it was discovered that the Spanish conquerors were not called soldiers during the conquest and really did not correspond to the general notion of soldier, and that the ecclesiastics were not called missionaries and were substantially different in their methods, aims, and relation to the rest of their society than the evangelists of a much later period who can legitimately be called

[3]Enrique and I had very different training and backgrounds and had never heard of each other when we met in the Archive of the Indies in 1965, but we immediately recognized each other, found that we shared a common method, and agreed on almost everything.

[4]I did have the honor of participating with Enrique in the publication of a collection of letters (Lockhart and Otte 1976), which we thought of more as reaching a wider audience than as primary scholarship, though it was partly that too.

missionaries. In this way the early presence of a substantial and varied corps of European artisans was detected and interpreted.

Consider what career-pattern research did with ethnic categories, recognizing that *zambo* was a rare and late term and that generally speaking mixtures of African and indigenous were called mulattos just like any other mixture of African, that the term *peninsular* was also very late and much more restricted in use than had been thought, and perhaps above all that the people who in the scholarship had long been called creoles were normally just Spaniards;[5] the word *criollo* was long primarily for people of African descent born outside Africa, and it continued to be applied in various ways to people of various ethnicities and even to things.[6] The rank terms *don* and *doña* and their absence were developed into a major tool of research, and their use proved not to be at all that attributed to them by older clichés. Professional and occupational titles were used in the same way, and also categories relating to gender and marriage status. Names were mined for their many, many implications of region and rank. General usage in all these matters changed in each generation, creating social trends that we can discover merely by tracking the epithets. They also varied with region, bringing to light important macroregional patterns. As a result it became standard practice among career pattern social historians to respect the exact original form of any name found in the sources, including its associated titles or lack thereof, to look at the ensemble for its significance, and above all to repeat it exactly when putting it in books or articles.

All of these are philological practices, and all carried over into work with indigenous sources, where the same kind of tracking became prevalent. To anticipate, don and doña turned out to be as laden with meaning in the indigenous world as in the Spanish, although they were not used identically in the two spheres. Among the Spaniards, one acquired the don and doña at birth; among the Nahuas they usually came only upon the attainment of prominence, above all high municipal office; thus though there were more doñas than dones among the Spaniards, the opposite was true with the Nahuas.

Tracing the exact form and use of terms started with epithets perhaps but went considerably beyond that. Any new or puzzling term was followed, such as in the conquest period *casa poblada*, which embodied the whole social ambition and large-scale household organization of the time. And one had to be just as

[5]A strong boost in this direction with the meaning of *español* was given by the work of Chance and Taylor, not following lives but observing in the same fashion the original categories used in censuses. See Chance 1978.

[6]Some scholars, especially outside history, still identify the crude social stereotypes of high-level institutional polemics with the categories of social reality; distinguishing the two can lead to more sophisticated and valid research, and the work of career pattern social history can help greatly in this respect. One might start with my *Spanish Peru* (Lockhart 1994) and the works of John Kicza (1983), Ida Altman (1989, 2000), Robinson Herrera (2003), and Catherine Komisaruk (forthcoming). It is gratifying that philological ethnohistory has attracted some attention in literary and anthropological circles, but social history has received far less if any, though it has great relevance as the background against which the literary, intellectual, and institutional commentary of the time was meant.

sensitive to the absence of expected words, such as the entire lack of the word *hacienda* meaning a large rural estate in the sixteenth century; the word was there, but it meant any valuable, often even movable property, especially merchandise. The near absence of the very term *encomienda* was full of significance.

Thus those of us who were doing career pattern history almost unconsciously became philologists. We even published some documents with comment when we found new types of sources such as the personal letters of sixteenth-century Spaniards; Otte published hundreds, usually with detailed analysis of the careers involved and the patterns shown. Except for not knowing indigenous languages, we were quite well prepared for indigenous-language philology.

At this time the field became acutely aware of the importance of the nature of different sources, that is, texts. Many social historians have been highly sophisticated and critical about texts since the 1960s, anticipating the somewhat parallel concerns of those sometimes called postmodernists in literature and anthropology, but in quite a different way. We know, have always known, that a certain text by a certain author has a purpose, an intention. When the Dominicans, late to the table and with few parishes, criticize the Franciscans, who arrived first and got more, we see what is up. But such matters far from exhaust the question of how to treat texts. Often the conscious or even subconscious intention of the originator of the text is not at the center of our interests.[7]

[7]This is as good a place as any to discuss some terminology originating in postmodernist quarters and now sometimes impinging on early Latin American history, which career pattern social historians and philological ethnohistorians have alike largely avoided, whether unconsciously from instinct or consciously on principle.

We do not speak of "privileging" certain texts. The expression implies an arbitrary choice for ulterior purposes. One member of this school asserted that we "privilege" Nahuatl texts, which is nonsense. We look in Nahuatl texts for things that are not in any others, of which the evolution of indigenous concepts is one important facet. The altepetl and the tlaxilacalli can be discovered and analyzed only in Nahuatl documents. The same is true of kinship terms and many other things, which in translations and other texts in Spanish are all spoken of using Spanish categories. When we study Spanish concepts, on the same principle we use Spanish sources, and as career pattern historians we have done so.

Postmodernists, new cultural historians, etc., often call non-Spanish people subalterns, a term derived essentially from British empire history, of India especially. Few situations have been less like the Latin American than the British presence in India. The word applies naturally only to auxiliaries and ignores a vital distinction between Africans, who were part of Spanish society, and the indigenous population. It also presents indigenous people purely from a European point of view and emphasizes only one aspect of their situation.

To the postmodernists every statement is narrative, with the implication that it is a story composed for the purpose of gaining some advantage. To us that is only a small aspect of human discourse, which is channeled even more meaningfully by genre, convention, evolving language, and many other forces. This way of looking at things also and above all destroys the hard-gained and indispensable distinctions between narrative, analysis, skeletal or statistical presentation of facts, and exhortation or appeal, and also between narrative, institutional, and social history, to mention only three of the well defined types. These distinctions organize our understanding both of the original texts and of works of historical scholarship. (*cont'd*)

One of the primary things we learned about sources at this juncture is that legislation, regulations, and policies are declared intentions but cannot be presumed to have been carried out; in the Spanish American context in fact the presumption is very much in the other direction. One of our surest rules is that legislative iteration means noncompliance. The postmodernists who do a kind of cultural history around and sometimes in our domain have forgotten this lesson or never learned it; they often return to the mistakes of the most naive of the institutionalists, taking it for example that because some Jesuit confessional manuals show an ambition to penetrate deeply into indigenous people's intimate secrets, influence their thoughts and actions, even change their consciousness, the mere declaration of the policy and intention means that it happened.[8] And this despite the fact that most indigenous people got one confession a year, or not that, and that the priests often hurried them through, devoting more time to Spanish customers. Actually, declared policies and instructions not only were mainly not carried out, they often weren't even meant as they read, or were copied mechanically from elsewhere, or were written as evidence that one was doing one's job and deserved promotion, or as part of institutional rivalries, etc. This skeptical attitude toward high-level official statements, this tendency to start with the subjects of the study at the most basic and direct level and get to what is said about them by distant others late or never, has also been characteristic of philological ethnohistory.

By having a field of interest far broader than the conscious or subconscious

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Those of this persuasion often speak of intertextuality, referring to the undeniable and important fact that language structures of all orders pass from one language to another, one register and genre to another. But we view this as etymology; in the new context such features are quickly naturalized and are as authentic an expression of those making the utterance as other parts of it that may have been present for a much longer time. In Nahuatl, at the level of words, abundant evidence both phonological and semantic tells us of the full integration of new terms.

Some of the apparatus of postmodernism has spread so far that nearly everyone uses it, even if unconscious of its origins and not always, thank goodness, as it was originally intended. A linguistic expression is a construction and has been called that since long before postmodernism; but it was not thought of as being constructed *by* someone, unless, as Heidegger would say, by the language itself. Each person, group, and language expresses things in a way not to be confused with the phenomena referred to, but rarely as consciously and with the intention to manipulate as in, say, "chauvinist pig."

The language cannot do without the word "power," but one will find it much less frequently in our writings than in some others. For some postmodernists all communication is political, a power play, and so they talk a great deal of power, often without any proof that the intended manipulation is at all successful. For us intentionality and influence-grabbing are, to be sure, a quite universal part of utterance, but only its surface. The things we tend to be interested in lie deeper, beyond the normal domain of "power" (such as the ability of indigenous structures to survive and shape Spanish structures), and postmodernist talk strikes us as over-conspiratorial.

[8]It is the same mistake that Robert Ricard once made with the promotional literature of the mendicants and later François Chevalier with Jesuit instructions about haciendas, and to which the French would seem to be prone.

intentions of the writer, or the so-called "voice," both career pattern work and philological ethnohistory enter an area where the criteria are different, where some questions at the surface level may remain unsolved or unaddressed, but unsuspected and important things are discovered in a virtually unchallengeable way; we look in documents for the equivalent of fingerprints and DNA. The encomienda was well on its way to becoming an artificial though unchallenged scholarly concept when it was seen that people in Peru of all shades of opinion called "encomiendas" *repartimientos*, which helped lead to an entirely different view of the phenomenon and its affinities. The Franciscans and Dominicans, Motolonia and Las Casas, fought bitterly, but in Mexico both orders called an indigenous parish a doctrina and not a mission and never called anyone a missionary, and it was facts such as these that put mendicant activity in the framework of indigenous organization and the general Spanish civil presence. In the constant struggles between Nahua factions inside given communities, each side would present diametrically opposing views as to their merits and as to certain facts, but both equally used the vocabulary of *altepetl* as the overarching entity and *tlaxilacalli* for its parts that enabled us first to recognize and then to understand the operation of the Nahua world's master entity. All of these recognitions are at the root the same thing.

Thus the New Philology happens on the way from career pattern history to ethnohistory. From career pattern work it was clear that the structures, patterns, and trends of a society could be investigated most meaningfully only in sources in the language of the people concerned, at the time of interest, mentioning and telling many unplanned details about a large variety of actual people, exemplifying in actually used words hundreds of the organizing concepts of the society and culture. At a less technical level, the practitioners of social history from the late 1960s forward believed strongly that it was necessary and desirable to include in research and analysis all elements of the population of areas studied, giving an emotional urgency to something that was already happening in the natural movement of the field through its sources.

Indigenous people, especially those away from cities, tend to escape the Spanish records that had been used until then. Hence the search for indigenous-language records, which were found in large numbers in Mesoamerica and in greatest bulk among the Nahuas. These sources were in a way much like those already used, with notarial records basic, but they were also different enough that a specific philology was almost forced into existence. Nahuatl documents only rarely mention the same individual frequently enough to allow us to trace whole careers through an abundance of that person's appearances, and where everyone is named Ana Juana or Pedro de la Cruz, it becomes even more difficult. Using our method of examining the details of a text minutely and deducing a great deal through the context, conventions of the time, logic, and intuition, we often manage to reconstruct a great deal about an individual's life, family, and general situation from a single testament, the most typical document. And testament clusters and other crossreferences in some thickly documented situations are making more things possible than we once imagined. But in general the more narrowly career pattern aspect of the method had to take second place while the

philological aspect expanded to compensate, and also, because the documents were so hard and varied so from one time and place to another, a great deal of effort had to go into simply deciphering the words.[9]

We came, then, to what I one day called a new philology, and a certain number of people have picked up the term. There have been many "new" movements of course, and there is even right now another whole New Philology in editing Renaissance and medieval manuscripts, more sophisticated than its predecessors but still restricted largely to editing texts. Let us spend a while considering what is new or special about this New Philology concerned with indigenous-language texts.[10]

Let me particularly emphasize here that the fact that some others had begun to work with indigenous-language documents does not mean that they had anything to do with the New Philology, were predecessors of it, or were converging on it. Their interest was mainly preconquest, the disciplinary affiliation primarily with literature and anthropology, the method, when one can speak of method, mainly a pure traditional philology. It is not a major interest of mine to delineate schools; I favor anything that will produce knowledge and insight. But the New Philology was and remains highly distinct in its origins in career pattern social history and in several hallmarks: mundane indigenous-language documentation, which it first discovered and exploited, as the primary context;[11] concentration on individuals and concrete situations; the whole set of methods coming out of career pattern research; the stages of postcontact cultural evolution and the language analysis that discovered them; an awareness of the pervasiveness of cellular organization; a tendency to converge again with early Latin American social history more generally. Anderson and Dibble, Garibay, León-Portilla, and Horcasitas are revered names, but they lacked all the above characteristics.

Luis Reyes indeed came onto the same ground, almost simultaneously, and he contributed mightily with his translations and sometimes also with his analysis, but he too had an entirely separate profile, showing none of the diagnostic traits of the New Philology. He was, however, uncannily close in some ways. I first met him in Puebla in 1969. He had been given the assignment of cataloging and collecting early notarial documents in Spanish, the same kind of source I had used in my *Spanish Peru*. In fact, I gave him a copy of the book, which he had already heard of, and he showed a quick understanding. At that time I had not begun to

[9]See the discussion of these matters in Lockhart 1991, chapter 10. At that time it was not yet clear that a kind of situational analysis closely akin to career pattern work, concentrating even more intensely on an even smaller documentary relic of a life, would continue to be a central element in much philological research. See especially Pizzigoni 2007 for an example.

[10]See the article of my beloved friend and colleague Matthew Restall (2003) on the New Philology, which contains much of interest but little about the methods and characteristics of this movement, a fact that helped move me to write much of what is said here.

[11]Having worked with a broad range of mundane documents previously accounts for much of the success that some of us have had in translating texts in more elevated registers more realistically.

study Nahuatl, nor was he himself working on it at the moment, and it did not come up.[12]

The New Philology's interest from the beginning was in the time of the documents, not just in precontact times as had been the case with the anthropologists, or with the Mexican nationalists/literati/historians Garibay and León-Portilla and their school. The domain of investigation was also different from that of the linguists, who have tended to concentrate on living informants. The crucial factor wasn't any preconceived interest in any particular time period but a recognition that a corpus of documents tells things primarily about its own time and is much less informative about any other.

In its whole first phase or scholarly generation the work concentrated mainly on mundane documents,[13] the equivalent of the kinds of records used for career pattern social history of the Spanish world, very different from the codices and ecclesiastically sponsored texts used until then.

Linguistic analysis, even quasi-quantitative, was used to establish the chronological pattern in the adaptation of Nahuatl to Spanish as seen in Nahuatl texts of all kinds. This happened very early in the game, in *Nahuatl in the Middle Years* by Frances Karttunen and myself (1976), the same year as the first large publication of mundane Nahuatl documents, *Beyond the Codices*. Also nearly at the same time began the close analysis of the use and meaning of key indigenous terms, starting with the *altepetl*, the local indigenous state, and the terminology associated with the household, both sets very different from the approximately equivalent concepts in Europe.

The movement involved new translation techniques and results, partly arising from experience with a new broader corpus, partly from the revival and extension of Nahuatl grammar. Much of the grammatical work was separate, done by Nahuatl grammarians coming out of linguistics, language study, anthropology,[14] but much has occurred among us as well (Carochi 2001, Lockhart 2001). Our translations have been less literal, with a fuller recognition of idioms. Translations in the style of Garibay or Anderson and Dibble might say, for example, "You are exhausted, fatigued," which corresponds to the structure and literal sense of the

[12]Some German scholars, most of them with precontact interests, recognized Luis and helped train him. Thus a good deal of his work had the expected precontact orientation. Later, often again because of the interests of sponsors, he was drawn into unearthing and translating postconquest mundane Nahuatl texts, and he achieved a great deal in that line, sometimes doing his good by checking over the work of whole teams of less talented translators, or being ghost translator for scholars innocent of Nahuatl. He has to his credit having first recognized in the sources what an artificial concept *calpolli* is for altepetl subdivisions generally, especially postconquest, and having advocated the use of the term mainly seen in the sources, *tlaxilacalli*.

[13]Nevertheless, Susan Schroeder's work on the annalist Chimalpahin was part of the first main wave both as a dissertation and as a book (see below).

[14]Above all Andrews 1975 and Launey 1978. Scholars with an interest primarily in grammar and language have not often gravitated toward the New Philology, but consider John Sullivan, originally in Spanish, who came into Nahuatl through Andrews and my own efforts in Nahuatl grammar. See especially Sullivan 2003, an important edition bearing many of the hallmarks of our movement.

original phrases; we say "Welcome," because these words are always used when someone arrives and a welcome is called for, and also we finally found in a remote corner of the great *Vocabulario* of fray Alonso de Molina an entry that gives precisely that definition. And since we have learned more about Nahuatl syntax, above all about the meaning and use of particles, our translations are not chopped up into small unconnected phrases; instead we produce the at times very long sentences that capture the sustained nature of much Nahuatl rhetoric and add much to the sense. It may seem paradoxical that at the same time that we understand better and with more subtlety what is peculiar to Nahuatl, it turns out more similar to Spanish, English, Latin, or Greek than it seemed in the age of the old so literal and non-syntactic translations.[15]

As we have seen, the New Philology is full of carryovers from career pattern history, above all the combination of the investigation of conceptual terminology, names, and titles with the reconstruction of careers and situations, even though the proportions are changed. Related is the technique of going through a whole, often scattered, documentation, not just a selected bit of it, bringing many different things into unexpected conjunction and providing a context for new insights and recognition of larger patterns.

This research at once macro and micro has led to some results which are very broad indeed and yet specific, and such results are used in further investigation; two of the most prominent are the notion of cellular organization and the stages of postcontact linguistic and general development. Many readers of this book will know all about these concepts already, but some I imagine won't, so I will characterize them briefly. Cellular organization refers to the fact that in the Nahua world almost anything, whether concrete, social, intellectual, or artistic, tends to be divided into separate equal independent parts often held together by an order of rotation, a numerical scheme, or both, and thus quite distinct from corresponding things in the Spanish or indeed European tradition. Cellular organization applies to the altepetl, to the household, to the land regime, to songs, to annals. In some realms it faded with time after contact, in others it remained strong, and the difference is in itself highly informative for cultural history.

The stages refer to three phases of adaptation to Spanish things first discovered in the Nahuatl language and then seen to exist in all the branches of life in the Nahua sphere to a greater or lesser extent. Stage 1 is the first generation, 1519 to about 1540 or 45, characterized in language—to simplify—by no change at all, and though there were vast upheavals in this time, the Nahua conceptual world in general did not yet make adjustments. Stage 2 went a hundred years to 1640 or 50, in language involving a great many Spanish loanwords but no other marked changes, while more generally the changes and adaptations took place at a corporate level. Stage 3 was from then on, involving the borrowing of Spanish verbs, prepositions, conjunctions, whole idioms, new sounds, and much more,

[15]Major examples of the newer style of translation can be seen in Karttunen and Lockhart 1987, Lockhart 1993, Sousa, Poole, and Lockhart 1998, and Chimalpahin 2006. See also, to a large extent, the translations of Nahuatl theater by Sell, Burkhart, and Poole.

corresponding to a widespread bilingualism. More generally, changes and adaptations were now primarily at the level of the individual. All of this was discovered through investigating the Nahuas; since then it has been seen that changes of the same type and tendency took place also in parts of Mesoamerica where other languages were spoken, though they were less systematic, with less distinct stages. I refer of course to the work of Matthew Restall with the Yucatecan Maya and of Kevin Terraciano with the Mixtecs (see below).

As I say, though cellular organization and the stages represent very important substantive results in both substance and theory, they are also research tools, situating things we find in their own most basic context and putting them in a perspective unattainable coming from Spanish sources or other approaches to the indigenous world. For example, in the sixteenth century and the first half of the seventeenth the traditional kinship terms were still in use; one said *teiccauh*, younger brother, sister, or male or female cousin of a male, making no gender distinction among younger relatives but specifying the gender of the person from whose point of view the relationship is calculated. By the mid-seventeenth century the traditional terms were disappearing, and instead we see *hermano*, *hermana*, *primo hermano*, and *prima hermana*. When in a group of testaments from a settlement in the region of Xochimilco in the eighteenth century I see numerous examples of *teiccauh*, I can say that this particular settlement kept kinship terms of Stage 2 into full Stage 3, even though in other respects the texts are typical of their own stage. In work on Nahuatl testaments of the Toluca Valley from the seventeenth and eighteenth centuries, a whole series of changes between about 1650 and 1710 can be summarized by saying that although phenomena of Stage 3 appeared at the earlier date, the process was not complete until the first decades of the eighteenth century (Pizzigoni 2007).

I mustn't entirely neglect the idea of Double Mistaken Identity.[16] It is not as precisely applicable a notion as cellular organization or the three stages. Rather it is a catchy way of saying that instead of the all too common notion of syncretism, the crux of the matter is the degree of convergence, and that where there is a great deal of convergence as between Nahuas and Spaniards, though it is inevitably always partial, much will happen at the points of convergence, each side tending to treat matters as familiar, leading on to developments which are partly new, partly old, and can't be attributed firmly to one side or the other. An important implication, as with most of our work, is that such things cannot possibly be planned, any more than the three stages of linguistic and cultural development, which no one even recognized as such at the time, much less tried to direct or legislate; that these phenomena are rather parts of a broad autonomous movement involving two (sometimes more) populations and cultures. Planners such as the half-imagined Vasco de Quiroga had nothing to do with it. The church itself was only one part of a much larger Spanish presence gradually making itself felt in every aspect of indigenous life, and the indigenous presence also colored Spanish life. Indigenous culture is an equal determinant of things despite the apparent and

[16]Touched upon in several of my publications, but see above all chapter 4 in Lockhart 1999.

in some aspects very real Spanish dominance, as we see the moment we look at indigenous areas with different cultures and consequently very different results after contact, such as the striking difference between central and northern Mexico. These are crucial principles, of which philological ethnohistorians and indeed many others are aware. Their operation varies almost infinitely with the different degree of convergence not only in whole societies, but in the multifarious components of a single one, so that one must be utterly flexible, prepared to recognize Double Mistaken Identity in textbook perfection in one case and find it entirely absent in another. If one wants to dispense with it altogether, I have no objection. It has perhaps received more than its fair share of attention.

Cellular organization and the stages could be discovered and studied only by means of a universal approach including all the documented times and regions, which was made necessary by the apparent lack of a massive and cohesive documentation illuminating the lives of individuals and the trajectory of small-scale organizations. (This is the short explanation for my largest and most basic book on the Nahuas, Lockhart 1992.) Nevertheless, bit by bit we came to realize that after all there were relatively large and concentrated documentary nuclei for certain particular subregions or altepetl, giving rise to some important studies of individual communities. They do not turn out the same way as they would if done by an anthropologist. As far as possible they continue the method of tracing multiple individual lives to see regularities. They have a philological basis which often leads to important publications in addition to the "main" or monographic study.

Thus it was with the first of them, by Sarah Cline on the altepetl of Culhuacan in the late sixteenth century, focusing for the first time through truly primary sources on home life and the land regime (mainly chinampas in that case), based on a collection of Nahuatl testaments which was also published (Cline 1986, Cline and León-Portilla 1984). A few years later there appeared the study by Robert Haskett (1991) on the region now called Morelos, including a number of altepetl but above all Cuernavaca and Tepoztlan. It focuses on indigenous municipal government and its personnel, but it is nearly equivalent to a study of local society. It was based on the overall local Nahuatl documentation, which Haskett collected and studied from the beginning as far as independence.

Another work in this series is Rebecca Horn's book (1997) on Coyoacan, a large complex altepetl with a Nahuatl documentary heritage exceptional for its size and variety. The texts included the largest known collections of bills of sale for land. With these resources, and also using the Spanish documentation on Coyoacan—an important innovation I will mention again—she constructed a portrait of local indigenous society and its relations with the equally local Hispanic sector until mid-seventeenth century, including, in other words, all of Stage 2.

Now Caterina Pizzigoni has undertaken the study of indigenous society in the Valley of Toluca in a time centering on the first half of the eighteenth century, using various sources but above all a large collection of Nahuatl testaments brought together from various repositories (with much help from Stephanie Wood). As in the case of Cline, she focuses on the household in all its dimen-

sions, with a certain emphasis on the roles of the two genders, but corresponding to her sources she covers a larger region across a longer and later time. She produced an interesting doctoral dissertation (Pizzigoni 2002) and now has completed a philological publication (Pizzigoni 2007) to which I will return. It is a natural thing that at first these local studies should have been done each one isolated from the others, but now the process is beginning in which each new author seeks comparative perspectives, consulting the earlier studies.

From the beginning both genders appeared in abundance in the documentation, as they do in the so basic and numerous testaments. The resources produced studies which without always having a gender emphasis showed both genders operating together in the same framework (this could be said of all the local studies I have just mentioned). They contribute greatly to the trend, also outside ethnohistory, in which even those who have a very strong specific interest in women's history are giving equal attention to both genders and to their commonalities and common framework as well as to distinctions and different functions. This was very much true of Cline (1986); in Sousa (1998), a broad work of primary research is directly focused on indigenous women in large parts of Mexico, yet men and the general framework receive virtually as much attention, for good reason.[17] Pizzigoni put the women of the Toluca Valley in the title of her dissertation (2002) even though large parts are more on society and gender, but her testament publication (2007) and her ongoing monographic work are openly in a broader context, without at all giving up a strong interest in women. Indeed, an important school of broad-based gender history is growing up in works like these.[18]

After enough work with mundane documents, it became apparent that other types of indigenous texts were not as different as they seemed, and also, though some of them had already been translated, that there was almost as much left to do with them to attain a good understanding as with mundane texts. Many of the same techniques can be applied to annals and ecclesiastically sponsored texts as to the rest, leading to intellectual history of a recognizable sort, with much interest in categories expressed in indigenous words. Susan Schroeder's book (1991), based on the work of the annalist Chimalpahin, studying concepts of sociopolitical organization in Chalco and the Nahua world generally, is a major example. Let me say in passing that this book shows that even inside the New Philology the phases do not always obey the theoretical chronological sequence. It can be said that the majority of those working within the group at first dedicated themselves above all to mundane documents and only later began to study Nahuatl texts of other types, but this innovative book by Schroeder was part of the first cohort of

[17]The 1998 dissertation has been expanded and is well on its way to publication.

[18]The movement is not confined to indigenous-language research. Gauderman (2003) has all the same characteristics, and so does the work of Komisaruk on Guatemala (2000, forthcoming). All of these books and dissertations have in common that they have a much wider archival base and criteria of selection—no different from other works of philological ethnohistory and career pattern social history—than previous works on women, which were virtually always restricted to one particular institutional source, unless they resorted to mere official reports and belles lettres.

historical monographs of the movement beginning with Cline's 1986 book on Culhuacan. Now another work (still not published) has been written complementing that of Schroeder, focusing on the contemporary annals of Chimalpahin and its use of ethnic and gender categories (Namala 2002; see also Chimalpahin 2006).

Another genre of Nahuatl documents other than the notarial are the so-called *títulos primordiales*, which ironically were put together or at least set down (above all, apparently, to confirm altepetl land rights) in a late period, my Stage 3, though presenting themselves as done in the sixteenth century. They have an almost folkloric flavor and are among the most difficult texts that have been seen. Stephanie Wood, after having written many articles about these documents, recently published an intellectual history of another type, almost of popular culture, focusing on the indigenous view of the conquest and of the Spaniards that is expressed in the títulos and other texts (2003). Robert Haskett (2005) has added a book on the primordial titles of Cuernavaca, the first example of a multidimensional study of a specific local corpus of this type.

Work with all these various kinds of texts, little known and sometimes in unknown genres, from the beginning led to philology in the traditional sense, the publication of the documents. At first, as in the anthology *Beyond the Codices* (1976), it could seem very similar to the old philology with different materials, with relatively little commentary, but soon we grasped that the public did not see in the documents the things that to us appeared so clear. Thus the analytical and linguistic component grew ever larger, to the point that the distinction between monograph and edition in many cases became obscured or irrelevant.

The first example, already well developed, was an edition (already mentioned) of a rare collection of some sixty Nahuatl testaments issued in Culhuacan, an altepetl of the Valley of Mexico, around 1580. The documents included aside from routine matters many bits of rich human expression, a whole world of until then unknown terms describing lands and household, and some clusters of testaments of relatives scattered through the collection. In the edition (Cline and León-Portilla 1984) each testament has its own introduction that highlights and briefly explains some of its salient features, which otherwise even experts would be likely to miss. This method turned out to be of great advantage not only for Cline's monograph on Culhuacan in these years (Cline 1986) but also for students and scholars who continue to return to the book as a resource for many purposes.[19]

Another publication (also already mentioned) has now followed and extended the tradition of *The Testaments of Culhuacan*. Caterina Pizzigoni (2007) has completed an edition of a collection of Nahuatl testaments of the Valley of Toluca, most of them from the eighteenth century, with the title *Testaments of Toluca* (it includes almost a hundred documents from two major parts of the valley in

[19]See the comparable edition of Maya texts by Matthew Restall referred to in n. 23 below. See also the edition of some very early Nahuatl census documents by Cline (1993), with a large preliminary study of a mainly demographic nature that could very well be a monograph.

various years, collected by Pizzigoni and Stephanie Wood). Here too each testament has its own introduction, but on a larger scale, speaking not only of salient points but reconstructing the overall situation of the testator (rank, family, property) far beyond what would ever occur to a reader not so conversant with the material. The introductions also speak of linguistic phenomena and of the varied styles of the notaries from different districts in different decades. In this way a whole local indigenous world emerges and speaks for itself, and the corpus can also be used as a rich source for research of many kinds. It also makes possible comparative studies on a larger scale, which Pizzigoni has begun, making comparisons with the testaments of Culhuacan. As more materials receive this kind of edition, exhaustive comparison can be carried further.

Editions in our style dealing with Nahuatl sources of a more traditional type also at times approach the nature of monographs in the matter of extensive analysis and important conclusions. An example is *The Story of Guadalupe* (Sousa, Poole, Lockhart 1998), an edition of bachiller Luis Laso de la Vega's Nahuatl book of 1649, reaching the conclusion (contrary to Mexican national legend but confirming the opinion of Joaquín García Icazbalceta in the nineteenth century) that it was written by Laso de la Vega based on and often following literally the Spanish version of the story published a year before by his friend Miguel Sánchez. Language analysis was a big part of the method, along with philology's traditional detailed comparison between the two texts.

In my *We People Here* (1993), an edition of several Nahuatl texts on the conquest of Mexico, I had some traditional philological concerns, such as reproducing for the first time the Spanish as well as the Nahuatl of Book 12 of Sahagún's Florentine Codex, with English translations of both, in four columns. But basing myself at first on surprisingly distinct styles and vocabularies, I also became aware that the famous Book 12 divides into two very different parts, with the first and much better known part, more used for centuries by historians, being the later in origin and more legendary.[20] The first part, taken quite literally, has been the basis of views of the initial indigenous reaction to the Spaniards' arrival from Prescott virtually until today.

There has been an important anthropological contribution at this level, outside the New Philology proper, with monographic research as well as editions, and some rapprochement has taken place between the two circles. Louise Burkhart opened up for serious research the field of Nahuatl texts done under ecclesiastical auspices, such as sermon collections and confessional manuals. She began (Burkhart 1989) to analyze the Nahuatl terms and phrases employed to translate Christian concepts, recognizing in them a continuity from precontact times as well as a role for the Nahua aides of the priest-philologists. The work in some respects resembles that of Schroeder. The anthropologist Susan Kellogg (who has a position in a department of history) published a book on law and cultural transformation centered on the study of a corpus of Nahuatl testaments from Mex-

[20]One could also mention here the edition of the Bancroft Dialogues by Karttunen and Lockhart (1987) with a very substantial preliminary study on the rhetoric and linguistics of Nahuatl elevated speech.

ico City (Kellogg 1995). Later Kellogg collaborated with Matthew Restall, a prominent practitioner of the New Philology in the Mayan culture area, to publish an anthology of indigenous testaments from various regions, with transcriptions and translations of some. On a larger scale and again in the area of ecclesiastically inspired texts, Louise Burkhart has entered into association with Barry Sell, who comes out of the New Philology, and with the historian Stafford Poole, also close to the movement, to publish the most sophisticated edition yet seen of Nahuatl theater (Sell and Burkhart 2004, Sell, Burkhart, and Poole 2006, with more volumes to come).

Perhaps I should say here a word or two about the general situation with history and anthropology. For a long time anthropology concentrated on archaeology, i.e., precontact times, and on the other hand on ethnography, the study of living peoples, leaving the intervening centuries to history, which showed no interest in indigenous peoples and languages. In fact, anthropology in due course moved in a tentative way into the realm of postconquest, written sources in indigenous languages in advance of history, as with the Anderson and Dibble translation of the Nahuatl of the Florentine Codex (Sahagún 1950–1982) or Fernando Horcasitas' collection of Nahuatl theater (1974). But these scholars tended to be marginal within anthropology itself, almost refugees from it. Since that time there have arisen figures like John Chance working on demography and Spanish-language ethnohistory of Mexico of whom it is hard to say whether they are anthropologists or historians. Burkhart (and in Andean studies Frank Salomon) function equally within the history field, and even in indigenous languages, though with a sharper anthropological profile. The mutual comprehension between the two groups, historians and anthropologists, working on postconquest matters is very considerable, one could almost say satisfactory.

Meshing with the larger group of anthropologists studying the precontact situation is mainly yet to be achieved. Though the work of philological ethnohistory is inevitably about the postconquest period in the first instance, it contains much patently preconquest material and in many ways is the best potential source for sociocultural matters in the late precontact period. In Mesoamerica anthropologists studying the precontact situation have been much concerned with what holds groups together, because from pure archaeology that is often not at all obvious, and they have treated so-called elites as a separate and often nearly omnipotent element, again because they are so prominent in the archaeological record. Postconquest ethnohistory, with evidence showing a broader range of activity more closely observed, though at a later time, would suggest that elements left out of consideration by the anthropologists—language, microethnicity, cellular organization—account for both fusing and fissure of indigenous groups far back into precontact times. It would also suggest that "elites," as important as they were, were often ultimately an expression of ethnic units and necessary to the functioning of entities based on rotation and nesting, so that groups would reconstitute them if something brought about their demise. And though through philology we know that preconquest Nahua central Mexico was organized into the inherently independent, quite idiosyncratic entities called altepetl, that word and concept hardly circulate among anthropologists studying that time; instead they

tend to use the loaded, in many respects inappropriate concept "urban." Likewise our studies tend to show that the distinction between states and chiefdoms so pervasive among anthropologists is an illusion.

Art history, which for Mesoamerica has much in common with anthropology, also has a relation, potential and actual, with philological ethnohistory. At one time scholars in that field, like anthropologists, devoted themselves almost exclusively to precontact phenomena, but in the last generation they have begun to explore the postcontact era as well, often retaining an interest primarily in the indigenous side of things. A powerful force in this respect has been Cecelia Klein of UCLA, for all of the works of particular interest here were first done under her direction.[21] The major publication to date is by Jeanette Peterson (1993), on frescoes, patently executed by indigenous artists, in mendicant churches. Indigenous-language research does not enter in importantly, but the images themselves are treated in many of the same ways that philologists treat alphabetic texts; also with the same meticulousness and the same attention to broader implications. The work, though not so intended, is a perfect illustration of Stage 2 in the arts, and perhaps even more so of the workings of Double Mistaken Identity.[22] In the wake of Peterson have come some works (Leibsohn 1993, Kranz 2001, Cosentino 2002) which while still centered on images involve full-scale work with Nahuatl texts in various ways, bringing the two aspects into close conjunction within the same interpretive framework, which is often to an extent precisely the framework of the New Philology.

At the same time, scholars from within the movement have been giving more attention to including the pictorial dimension as an integral element within the overall analysis. This began in my *The Nahuas* and has become more general, especially well developed in the work of Terraciano and Wood.

Returning to my characterization of the New Philology, it tends to a sort of broad interconnected cultural history operating very close to the people and the documents, hence at the same time tending to be social history. It shares with anthropology an interest in just about everything and the belief that everything is connected with and affects everything else, but operates on written records and analysis of written language within a framework quite innocent of anthropological frameworks, or as we would say preconceptions.

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[21]Of course we must not let ourselves forget the great works of Kubler (1948) and McAndrew (1964) on mendicant churches, which represent a formidable precedent and more than that, for although both scholars were hopelessly Ricardian and institutionalist, through their meticulous primary investigations and insight they contributed aspects to analysis decades later in terms of the stages and cellular organization. See Lockhart 1999, pp. 361–63.

[22]I hope I will be pardoned an anecdote illustrating some interaction between an art historian and a philological ethnohistorian. When I once attended a slide talk given by Jeanette, she pointed out that when the usual Mesoamerican speech scroll represented song in the pictorials she was discussing, it was covered by a series of cross-marks. Having just been studying Nahuatl song and having discovered that the canonical form had eight verses, one of the manifestations of cellular organization, I hazarded a prediction that the lines would form eight compartments. On examination they actually did, with some one less or one more, just like the songs.

All of these traits and tendencies I have been talking about are in reference to the Nahua world, but of course the same methods and perspectives can and should be applied to any indigenous group for which contemporary sources written in its own language exist, and that was envisioned from the first, by way of finding out to what extent the Nahua example might be unique or paralleled elsewhere. Yucatecan Maya was studied (albeit less successfully) in the UCLA history department almost as early as Nahuatl with precisely that in mind. The desirable expansion has already taken place for two additional major areas of indigenous language and culture in two key works by Matthew Restall (1997)[23] and Kevin Terraciano (2001) on the Maya of Yucatan and the Mixtecs, which make a trilogy together with my *The Nahuas*. Nor should we forget the book of Peter Sigal (2000), who on the basis of very difficult Yucatecan Maya texts carried out a complex analysis of postcontact sexual concepts which at the same time represents something new in the general intellectual or cultural history of the indigenous people of Mesoamerica. The indigenous world of the Andes would be a marvelous arena for research in the style of the New Philology if a large enough supply of similar sources could be located. Some are in fact already known, and I myself have made a provisional attempt to interpret Quechua linguistic evolution in the context of the three stages of the Nahuas (Lockhart 1999, chapter 8).

Philological ethnohistory came out of the social history of the Spanish world; it led by its own momentum into a very separate realm. Yet the original purpose was to achieve a more complete understanding of a whole complex multiethnic society. So in due course attempts were made to include the Spanish side, both Spanish sources and the Hispanic people who were in such close contact with indigenous people. We have now seen work simultaneously on Nahuatl and Spanish sources as with Rebecca Horn (1997) and Caterina Pizzigoni (2002), or on Spanish sources alone where there are no or virtually no relevant indigenous-language texts, as with Robinson Herrera on sixteenth-century Guatemala (2004), Catherine Komisaruk on Guatemala just before independence (2000, forthcoming), Kimberly Gauderman on women in Quito in the seventeenth century (2003), Edward Osowski on Nahua corporate religion in the last decades of the eighteenth century and first decades of the nineteenth (a book is in preparation), all beginning to develop new techniques according to the nature of their sources.[24] Among the Nahuas indigenous-language documents tend to become scarcer after

[23]See also the edition by Restall (1995) of a collection of testaments in Yucatecan Maya, with an apparatus very similar to and already thicker than that of the Testaments of Culhuacan. The phenomena and the new insights are very similar in principle to what is seen in the collections of Cline and Pizzigoni, but significant differences emerge in almost every department, calling for comparative treatments at some point in the future.

See further Karttunen 1985, which does for the postcontact evolution of Yucatecan Maya much of what is done for Nahuatl in Karttunen and Lockhart 1976.

[24]Leslie Offutt did a dissertation and book on the overall society of Saltillo, with a natural emphasis on the Spanish sector, in the career pattern tradition from Spanish sources, then later collected and has begun to work on a rich cache of Nahuatl testaments from the associated San Esteban de Nueva Tlaxcala, a far northern Tlaxcalan colony.

around 1770 (though new discoveries are still being made), whereas other sources multiply, and research must be adapted to the situation.[25] Many of the techniques of philological ethnohistory still apply, and after all, many came from Spanish-language social history in the first place.

The two types of work are complementary, often presenting mirror images. In indigenous-language work one sees the indigenous patterns and concepts on the inside and can deduce many things about the nature and effect of the Spanish presence; in Spanish-language work one sees indigenous people only in the Spanish context but can deduce much about trends in their indigenous context, especially now that it is better known than before. One of the main questions we ask of indigenous phenomena is whether they are Spanish-influenced, and only through comparisons with a well-studied provincial Spanish sector can that question be answered.

Because of the double nature of the complex, in which the new and the old can be the same thing and the adoption of something Spanish can be a means of retaining received culture, the question of continuity and change is inevitably subtle. Some observers took it at first that because we philological ethnohistorians found much, much more continuity than had been seen without indigenous sources, we were giving overwhelming emphasis to continuity, almost denying change. That was never so. The three linguistic stages, with which so much of it began, are a description of ever growing and deepening Spanish influence on Nahuatl, though always conditioned by the nature of the Nahuatl language relative to Spanish, leaving a readily identifiable and distinct Nahuatl to this day. The changes often, indeed usually, involved active participation and initiative by the Nahuas themselves. And the same is true of all the other departments of life affected by the process. The philological approach finds much change, and for the most part in the direction always imagined; it finds however that for generations many basic indigenous patterns remained virtually unaltered, sometimes with transparently superficial renaming, sometimes not even with that; that when deeper Hispanic influences came, their success usually depended on commonalities with the indigenous tradition; and that even the mature variants surviving on into our days have elements in common with the indigenous tradition even though we cannot always assign them that origin with security.[26]

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[25]Miriam Melton-Villanueva, presently a doctoral student at UCLA, has discovered a substantial body of Nahuatl testaments from the Toluca Valley in the first three decades of the nineteenth century. The size of the corpus and the fact that two distinct settlements are involved may lead to a thorough reassessment of the question of how long the tradition of using Nahuatl survived in a major way.

[26]I will append here a few words apropos of some remarks by Eric Van Young in a review article (2004). Direct rejoinder is not a favorite mode of mine; let it be remembered that in the early Latin American historical field, which boasts an amazing line of unusually distinguished works, nevertheless critical and historiographical acumen has been and is so nearly absent that the rare practitioner who has it in abundance, Eric being such a person, commands attention and perhaps deserves a response.

Eric recognizes the place and nature of our movement in several ways which I appreciate and to which I have no objection. I wish to elaborate on just three points:

First, he provides a sort of bottom-line assessment of the performance of the group by the number of "books" it has produced. The number of significant works that a person or school has written, if not made into the sole criterion of assessment, is well worth our attention. But, aside from the fact that other things have appeared since that writing, Eric means by a book exclusively a primary research monograph. I maintain that the group has produced twice as many significant works as Eric concedes, the remainder being editions of various kinds of texts with as much analysis as many monographs and new conclusions that have reoriented whole sectors of the field, not even taking into consideration the vast contribution inherent in a new kind of translation, which is not to be compared with translation from a modern European or even a classical Mediterranean language. The field as a whole must learn to read and assess philology differently.

Second, Eric says that members of the group tend to think the task is done when a translation is achieved. Where do the many, many pages of new and deep analysis accompanying the texts fit into such a view? Many of the editions were done precisely on the way to a monograph consisting of analysis, and when completed the two make an inseparable set each contributing something unique. Nor is translation the only way we treat texts. The three stages were deduced from texts most of which had not been translated at all. The only meaning I can attribute to Eric's statement is that we do not always or mainly go into the "political" and "power" aspect of texts we have translated, seeing that as superficial and not always corresponding to our interests. It is as though one would complain of a scientist studying the genome of lettuce that he had done nothing, meaning that he had not gone into whether or not the lettuce was safe for supermarkets.

Indeed, and third, Eric says that the group seems to be more modern than postmodern. This is correct insofar as one accepts the terms. But the category "modern" as used by the postmodernists is their own invention, and I do not accept it. It attributes imaginary negative qualities to a rational/intuitive manner of proceeding that is universal in great science and scholarship and is not peculiar to any one period, nor will it ever be antiquated, as the term implies. Aristotle and half the greats of classical times were "modern"; so was Horacio Carochi in the seventeenth century; so are many if not most good minds at present, and will be in the future.

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