COMMUNICATIONS

To the Editor: November 7, 1988

Reviewing a book is not easy. On the one hand, the critic must tell the readers, as accurately as possible, what is in the book they have not yet read. And at the same time, the critic may provide a personal view on more fundamental matters. In Alan Knight's review of my book, *Le Mexique de l'Ancien Régime à la Révolution* (*HAHR*, 68:1, 139–143), fact and opinion are so inextricably confused that it seems necessary to try to distinguish one from the other. Obviously, Knight's view on fundamental issues is important, and he has every right to think the way he does. But I would also like to draw attention to the kind of argument he uses to support his view, so as not to lose sight of what is really at issue.

The heart of the matter is the meaning I give to the revolutionary events of the nineteenth and twentieth centuries and, more broadly, of "modern politics." The phrase "modern politics" should make it clear that I believe that in politics there has been a mutation similar to that which has taken place in other areas of human endeavor and which leads us to talk of "modern science" and "modern philosophy." In other words, as far as "Latin" countries are concerned (for I only want to talk here about these), the French Revolution ushered in a radically new way of thinking about society and politics.

It is not possible to dwell here on the nature of this novelty. What is clear is that all those who reflected on politics and society in the nineteenth century took it for granted as a result of the French Revolution and the advent in most European countries of the age of postrevolutionary society. From Benjamin Constant to Auguste Comte, via the French and Spanish "doctrinaires" Guizot, Balmes, Donoso Cortés et al., from Saint Simon, Karl Marx, and Alexis de Tocqueville to Edgar Quinet and the close-of-the-century positivists (to name but a few), all centered their reflection on an undeniable novelty. They offer different interpretations of the facts they examine, and sometimes their judgments are contradictory. Some want to halt the process, others aspire to take it further. They all agree that things are not the same as before.

One might concede that this is the case for Europe, but that Latin America belongs to another world; its problems are specific, Knight seems to think that one is guilty of "Eurocentrism" if one thinks of Latin America in terms of Europe's problems and draws comparisons. However, the reflections of Latin American thinkers in the nineteenth century revolve around the same topics as their European counterparts, and their interlocutors are those very European authors: Mora, Alamán, Zavala, and Otero in Mexico; Sarmiento, Alberdi, and Echeverría in Argentina; Lastarria, Bello, and Arcos in Chile, to begin with. Then came the great positivists—Sierra, Bulnes, and Rabasa in Mexico, the Venezuelans Vallenilla Lanz or Gil Fortoul, to quote only a few names from a host of authors—all obsessed with the same theme. Even if they are also concerned with the construction of the nation, we find again and again that the core of their thinking is the revolution, the construction of modern society, and the social resistances to this project. Like their European counterparts, some think that revolutionary ideals are utopian; others that they are desirable. For all, the uniqueness of the new age that followed the French Revolution is an established fact. For Latin America, as for Latin Europe, there was an "ancien régime," a revolution, and a revolutionary age. If we do not think twice about including Latin America in the economic framework of the Western world, why should we suddenly be inhibited when faced with cultural and political inclusion?

Of course, one may decide that revolutions should be considered mainly along socioeconomic lines. This vein has been abundantly worked over the last decades, but Europe has recently taken a social, cultural, and political approach to the revolution and the nineteenth century. Authors as varied as M. Agulhon, P. Nora, F. Furet, M. Ozouf, R. Halévy, and P. Rosanvallon have usefully overcome the socioeconomic standpoint and rediscovered the great questions (but not necessarily the answers) of nineteenth-century thinkers. What is a revolution? Wherein lies the radical novelty that all bear witness to? In a socioeconomic change? In the new social group in power? In the new way of thinking about society and politics? In a new political culture? In a new legitimacy?

Any serious observer of contemporary Latin American historiography can only be struck by the extraordinary lack of sociocultural and political variables. And yet politics, in the strongest sense of the word, is everywhere present, in current events and in all the sources. Is politics simply an illusion masking the real causes which are socioeconomic? Knight seems to think so, when he speaks of "hard socioeconomic logic" in connection with the revolt of the pueblos.

Before challenging this assumption, I should raise the question of the actors. Before knowing *why* a person acts, one might want to know *who* the person is. And even if someone does act according to "hard socioeconomic logic," it may be useful to know whether that person is alone or part of a group; whether the group is a random, momentary gathering pursuing a common interest or a structured and a permanent association. A Zapatist pueblo in Morelos is by no means the equivalent of a crowd in a city riot.

The notion of the actor would therefore seem to take precedence over other causal elements. But how can we understand the actors? By examining their actions, by studying who the individuals are who act together, and by determining how they are linked together. Apart from a few random groupings, what is invariably noted is the existence of structured groups acting in a coherent and permanent way in the social and political field. These group structures, with their specific ties, working rules, values, and "imaginaires," now make up a vast field of study that one may call "forms of sociability" and which has acquired more and more importance in recent European historiography.

When studying these forms of sociability, I made a distinction between traditional actors and modern actors, a distinction I propose in chapter 3 of my book. The contrast traditional/modern is not an a priori grid of my invention; it emerged from the heterogeneity in the human groups I studied. The difference between a family clan and a revolutionary club is obvious to all. It is not the result of "dichotomous mentalities" either, as Knight would have it, but emerges from the group and organizational structures in question and from the patterns of their imagination. Tradition and modernity are not vague intuitions. They refer to clearly definable codes of social organization. They point back to those very same realities that nineteenth-century thinkers, such as Tocqueville for example, called "Ancien Régime et Révolution."

Does this mean that the group typology one can establish refers to chemically pure agents? Obviously not, and a large part of my study consisted in underlining the paradoxical relationships that existed between the "traditional" and the "modern" actors and what I called the "hybrids." All of these actors belong to the same society, they are all the society, but it is important in carrying out an analysis not to mistake the actors and the logic that impels them according to their group. A well-known hacendado for example, at the head of a vast clan of relatives, friends, and clients, may belong to a revolutionary clan; but that same man will act according to two different logics, as the case may be. He will act as a "citizen" in his relationships with other members of his club, but as a traditional head or leader in relationships with the members of his clan. I could give many more examples of such interplay between the modern world and the traditional world; I study them at some length to shed light on phenomena such as caciquismo, but Knight makes no mention of this, when as a reviewer he should be telling the reader what the bc ` has to say. His omission is designed all the more effectively to reduce my interpretation to a mere antinomic opposition of "mentalities."

What is at issue, of course, is not a matter of words; it is a matter of the conceptual tools one uses to interpret facts that may indeed be well known, but which have not yet been accounted for. It is not enough to say that things such as clientship or family favoritism are well-known phenomena, in Mexico or elsewhere, or that "the presidential archives are full of such stuff." These data are indeed omnipresent in the sources, but they have to be accounted for and not swept out of our field of vision, or from our global accounts, simply because we do not know how to deal with them. Are these phenomena specific to Mexico? Obviously not, and this is precisely why it is possible to draw analogies with what is to be found in other places, Europe for example, or in other times such as the Middle Ages and the ancient world. Why should it be forbidden to draw the parallel between contemporary forms of clientship or "recommendation" and that described by Cicero in Rome? Or to compare the military ties under a nineteenth-century caudillo with those of medieval vassalage? Why is this dubbed mere "rhetoric"? Analogical does not mean identical. An analogy is a proportionality or a basis for comparing relationships. It enables one to see precisely what things have in common and what they do not. The study of history can only be comparative. This does not mean that contemporary patronage is the direct heir to Roman clientship or that the military under the caudillo are the descendants of our medieval "vassals." The conclusion surely is that the Latin world is one in which personal ties have an extraordinary intensity and permanence; ties which impinge forcefully when one travels back upstream through the centuries. That is why these relationships can be called "ties of a traditional sort." On the other hand, we would be wasting our time if we sought, in classical antiquity or in the Middle Ages, forms of sociability analogous to a freemasons' lodge or a revolutionary club. That is why these forms of sociability may be called "ties of a modern sort."

There is no need to dwell further on this subject, or to provide more definition of terms. More than a chapter of my book is devoted precisely to defining the concepts that Knight dismisses as empty and vague. I shall leave it to other enquiring minds to judge whether the typology I have elaborated is useful in explaining the political phenomena of the nineteenth and twentieth centuries. For what counts in a model of interpretation is that it be relevant and enable one to understand a little better what the sources are showing us. With regard to relevance, Knight's criticism that I did not study the campaign to moralize Mexicans during the Porfiriato, in the domains of "drink, gambling, bloodsports, prostitution," would be well founded if my aim had been to describe social life in Mexico at that time. It may even be granted that the moralizing drive had an influence on the revolution, for society functions as a whole. But are these facts more relevant in accounting for the Mexican Revolution than understanding who the social and political actors were in nineteenth-century Mexico, or showing how a "fictive democracy" could exist in the nineteenth century as a result of the coexistence of actors belonging to two different systems, each with a different logic when it comes to action?

This leads me to a few remarks on the expedients used by Knight in a debate which ought perhaps to have remained within the limits of a difference of opinion, but which did not. In Knight's review, every attempt was made to discredit a work whose central thesis he does not share. Of course, Knight has every right not to share my conclusions. What he has no right to do is to deform the contents of the book through innuendos and omissions. Let us quickly see how he goes about this:

1. My study does not deal with "the period stretching from the Bourbon era to the revolution of 1910–1911," but more modestly with the nature and the origins of the Mexican Revolution. This led me to study the Porfiriato in greater depth than expected in order to

see whether the revolution represented a real break with the past. This meant examining the bases on which it was built, and the reasons why the Díaz regime held good until 1910. In order to get a deeper grasp of the Porfiriato and of the way the revolution began, I proposed to identify the social and political actors of that time. I tried to take a look at them in the long time-cycle, as in an *overview*, so as to understand why they acted the way they did. This led me to build an interpretation model for a certain number of political phenomena in the nineteenth century, but not to write the history of that century and even less of the eighteenth century. By misrepresenting my aim, the reviewer can accuse me, of course, of not making use of the fundamental work by . . . on the colonial era. Knight might just as well have reproached me with not supplying a complete bibliography on ancient history, because I quote Cicero and Augustus!

2. Further bibliographical criticism. I make no mention in my book of basic studies in the English language. My study, as is clearly stated on the opening page, is the unmodified publication of a state doctoral thesis, written in 1982 and submitted and defended in early 1983. The English studies Knight would have me cite were being written at the same time. Perhaps he was simply being careless when he criticized me for not mentioning a truly fundamental work by Nancy Farriss, which dates from 1984, or when he omits to say on what essential points these studies invalidate my conclusions or hypotheses.

3. Concerning the sources, there should be no ambiguity; they are clearly laid out in the introduction and in Annex VIII. The basis of my study being a general prosopography of the political actors (7,838 to be exact) during the Porfiriato and the Mexican Revolution, I have privileged general sources which alone lend themselves to this type of corpus. One cannot carry out a general prosopography of the whole political class of a country (how many have been done so far?) from merely local archives. Knight's archives are clearly not the same as mine, otherwise he would have done the work I have done. My main sources were official printed documents, the *Diarios de los Debates* of the two chambers, which does not mean that they were easy to find or quick to collate. Ideally, for so many people studied, once the corpus of political positions had been composed, it would have been useful to be able to get hold of personal files like the *hojas de servicio* found in archives. It was possible to find a certain number, but the majority either do not exist or are for the moment impossible to get hold of. My only regret was not being allowed at that time to make use of defense archives, but many other scholars met with the same refusals. I made use, accordingly, of memoirs, biographies, and local histories, which offered openings beyond merely dry biographical data, to scarcely known or ignored social and political phenomena which emerge from the accumulation of local and individual examples. This is the case with that typically modern form of sociability constituted of freemasons' lodges. They emerge as the backdrop to quite a few social and political phenomena in Mexico City, Puebla, Oaxaca, Nuevo León, Yucatán, and Guadalajara. I tried to show their texture of implantation, and it is little short of lying to assert, as Knight does, that the only proof I provide of their importance is an analogy with the Spanish Cortes in 1931! As I specified in my work, our knowledge in this area will remain indirect and partial, as long as freemasonry sources are not studied. These sources exist; I am working on them, and I shall soon be able to show that my hypotheses were justified.

5. A last remark about sources and one which shows up Knight's tendentiousness. Reading his review, one is led to think that my book is nothing more than a political analysis based on the prosopography, and an analysis of discourse and ideology, but not calling on social and economic data. Knight neglects to mention that I made use of a large number of quantitative sources—demographic, social, and economic—and legislative sources of the Porfiriato. These sources generated chapters 5, 6, and 7, and Annex V, as well as several developments elsewhere—in all 200 pages. There are also 69 pages of statistics, graphs,

and maps on migration and population patterns; the relative importance of localities; the socioprofessional breakdown of population; and prices, wages, taxation, and education at all levels. Moreover, my reexamination of the agrarian structure in Mexico on the eve of the revolution generated intense debate among Mexican research scholars.

Why these omissions by Knight? Perhaps they make it easier for him to systematically pass off as ideology and "aprioristic" conclusions what he would otherwise be unable to cope with. To study the real actors of social and political life does not mean neglecting the socioeconomic aspect, but to consider that aspect for what it is: an important but by no means exclusive dimension of the life of these actors. That is why I devoted so much space to the economic crises that preceded the revolution. But in the last analysis, it is the relation between the actors, in all their dimensions, economic, social, cultural, and political, which enables us to reach an understanding of a phenomenon as a whole.

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To the Editor: December 16, 1988

In his riposte, Professor Guerra gives the impression that my review was a wanton academic mugging ("every attempt was made to discredit" the work, he says). This, of course, is nonsense. For one thing, the book has received much rougher treatment at other hands.[1] A substantial part of my review (let me strive for quantitative accuracy: perhaps 35 percent) was very favorable: I referred to the "unusual force, conviction, and erudition" of the work, to its "very valuable" prosopography and to its "masterful" analysis of the political opposition; I called it the "best analysis to date of Porfirian politics," superseding Cosío Villegas. Space limitations forced me to omit certain further comments I could have made, some positive and some negative. Guerra's economic data—specifically, his data on Porfirian agrarian society—are useful, as I have acknowledged elsewhere.[2] But they do not contribute to a rounded socioeconomic interpretation of the old regime and revolution and, indeed, they sit rather uncomfortably within an interpretation which is forthrightly political and which aims to refute the old notion of a popular social revolution.

Before addressing that key interpretative issue, I should first tackle some specific points raised by Guerra's remarks. His allegations of deliberate distortion and virtual lying would be serious if they were not so patently silly. First, as regards chronological labels, my review made it perfectly clear that the bulk of Guerra's analysis concentrated on the Porfiriato. My initial reference to the "long nineteenth century" pointed up the undeniable fact that Guerra's interpretation of Mexican history—specifically, his concept of the "old regime"—transcended the Porfiriato. Indeed, a quick perusal of dates, events, and citations shows that the pre-1876 period gets plenty of attention, as, indeed, it should, given the nature of Guerra's interpretation, which locates the Porfiriato (the focus of the argument) within a much broader historical span. Chevalier recognizes this in his introduction to the book. My reference to the "long nineteenth century," therefore, was neither a distortion nor even a criticism. By stressing the breadth of Guerra's analysis, I sought to underline its importance—and I would do so again. Guerra, seeking to correct my supposed traduction, says that his book is really concerned with the "nature and origins of the Mexican Revolution." Origins,

1. Moisés González Navarro, "La guerra y la paz, o un nuevo refuerzo francés a la derecha mexicana," *Secuencia*, 7 (enero–abril 1987), 57–69.

2. Alan Knight, "Interpretaciones recientes de la Revolución Mexicana," paper given at the Mexican Historiography Conference, Oaxtepec, Morelos, October 1988, publication forthcoming.

yes; nature, no. A book which stops in 1911 and which offers no real analysis of the successive regimes and factions of 1911–20, still less of the post-1920 revolutionary settlement, cannot make that claim. If Guerra is prepared to settle for "a history of the Porfiriato," that would be fine with me, but he would actually be selling himself short. Second, regarding Guerra's neglect of English-language sources, it is hardly adequate to plead that his book is a 1982 thesis, published unchanged in 1985 (he says, in his letter, that this "is clearly stated on the opening page": I can't find this statement). I worked on the reasonable assumption that a book published in 1985 could certainly accommodate research published in 1976 (Anderson: a "crucial omission," I suggested, to which Guerra makes no reply), or 1981 (Vanderwood), or even 1982 (Joseph and Vaughan). As for the colonial omissions, Guerra says that I failed to signal their relevance. *Au contraire*, I stated that the works of Farriss, Taylor, Lockhart, and Van Young would have helped Guerra better understand the nature of debt peonage and avoid "some of the fanciful romanticization which informs his picture of traditional colonial society"—namely, his picture of "traditional," seigneurial, "holistic" haciendas, governed by paternalism and characterized by cosy interpersonal relationships. I leave it to his readers, especially colonialists, to decide whether there is any validity to my criticism. Third, concerning sources, I did *not* blame Guerra for failing to use *local* archives (as he wrongly complains); nor for failing to breach the formidable ramparts of the Defensa Nacional archives. These omissions may be regrettable, but they are not culpable, and I didn't say they were. My point was that Guerra fails to use archives *tout court*, and this, I argued, leads him to certain misconceptions. A final specific point concerns the freemasons: Guerra says that "it is little short of lying to assert, as Knight does, that the only proof I provide of their importance is an analogy with the Spanish Cortes in 1931." Again, let me quote what I said: "the influence of masonic lodges . . . is asserted (the assertion is crucial to Guerra's argument) with a corroborative footnote pointing out that 39 percent of the Spanish Constituent Congress of 1931 were freemasons." A "corroborative footnote" means a citation designed to bolster an argument; nowhere did I say that this was the *only* proof advanced. However, the rest of the proof is (as Guerra himself admits) pretty thin, for which reason, I assume, he feels obliged to resort to this bizarre argument by analogy.

This gets us to the more central question of methodology. I am struck by Guerra's commitment to self-evident truths. This may be fine for framers of eighteenth-century constitutions, but it is risky for twentieth-century historians. First, note the sweeping assumption that a form of "modern" politics, analogous to "modern" science, appeared with the French Revolution (at least—and this is a whopping qualification—"as far as Latin countries are concerned"). The French Revolution marks a watershed between the old regime and the "revolutionary age," at least for Latin Europe and Latin America. However, Guerra states, the toilers in the vineyard of Latin American history—backs bent on primary archival work, perhaps—have missed this obvious point; as he chooses to put it, "any serious observer of Latin American historiography can only be struck by the extraordinary lack of sociocultural and political variables." Guerra offers enlightenment to the purblind toilers. In this, his self-appointed task parallels that of the critics of the "social interpretation" of the French Revolution whom he lists for our benefit and who, he asserts, have confounded ("usefully overcome") the old "socioeconomic standpoint." There are several problems here. First, it would be easy (but otiose) to produce a counterlist of weighty historical authorities who subscribe to the "socioeconomic standpoint" on the French Revolution. Fashion is no guarantee of truth, and rival bibliographies are easily traded to and fro.

Second, the novelty of the French Revolution may be less striking to those nurtured in a culture other than the French (or "Latin," though I dislike and distrust this crude cultural label). The English and American revolutions involved ideas and patterns which were also globally influential and innovative. It is also a commonplace that analysis of the industrial revolution and its impact on preindustrial society became a staple of nineteenth-century thought—both "Latin" (e.g., Durkheim) and "non-Latin" (e.g., Weber). This in turn gave rise to social theories which are integral to any consideration of "modernization," which must therefore be seen as a socioeconomic as much as a sociocultural or strictly political process. It was for this reason that I pointed out Guerra's neglect of Porfirian and revolutionary "developmentalism" (roughly, "Puritanical" efforts to educate, moralize, and clean up the wayward Mexican masses). Guerra replies that he did not aim to describe "social life" in Mexico, which shows he has missed the point. We are talking here of a developmental—or "modernizing"—*ideology* at least as important as the narrower political ideology which obsesses Guerra. This importance is not surprising, since many Porfirian and revolutionary elites were far more concerned with economic development than with political modernization—not least because formal political modernization had already occurred through the Independence and Reform movements. Their project, in other words, was strongly economic in content and strongly influenced by global examples of economic development (in the main, "non-Latin"). In this respect, the Mexican Revolution went beyond the French Revolution (which possessed no comparable conscious economic project) and bore comparison with other "modernizing" or "developmental" revolutions/regimes which have affected the Third World. I would not go so far as John Hart in qualifying the Mexican Revolution as a movement of national liberation. But I would maintain that any analysis which focuses on the alleged "modern," *afrancesado* politics of Mexican revolutionary ideology/praxis to the complete exclusion of its economic developmentalism and would-be social engineering is misleadingly one-sided and palpably Eurocentric.

Finally, we must return to the old, vague, and contentious tradition/modernity dichotomy. If it is to be used (and I don't deny its heuristic utility for *some* purposes), it must be accompanied by precise and comprehensive definitions, not circular assertions or brief invocations of authority. The same goes for another recurrent and related concept in Guerra's book, "holism." Diligently rereading the book, I still find repeated references to, but no adequate definitions or discussions of, these key concepts; and the author's failure to supply them—indeed, his dogged belief that he has supplied them when he clearly hasn't—is evident in both book and riposte. In the latter, Guerra asserts that "tradition and modernity are not vague intuitions. They refer to clearly definable codes of social organization." If so, they should be clearly defined; then we might understand their "operationalization" rather better. As it is, tradition becomes a *passe partout* to analogical anarchy. For example, Guerra sees "traditional ties" as diagnostic features of traditional society. He happily compares Mexican caudillos with medieval lords, Mexican clienteles with their classical Roman counterparts. "Analogical does not mean identical," he admits, and we can all agree on that obvious point. But, it needs stressing, Guerra is not introducing simple *analogies*, but also *causal derivations* (however long-term and indirect). Mexico is a "prolongation" of Spain, and Spanish analogies are repeatedly given priority; more generally, Mexico is a product—mutated and peculiar in some respects—of "Latin" culture and civilization. "The Latin world is one in which personal ties have an extraordinary intensity," Guerra avers (another self-evident truth?); Mexico must therefore be interpreted on the basis of its presumed membership of a distinct cultural club, of Latin/European origin. Yet Mexico was a subjected colony, rather than a "pacted" peninsular kingdom; more important still, the revolutionary movement in late eighteenth-century Europe (and not just *Latin* Europe) was directed against a monarchical and aristocratic old regime, whereas the revolutionary movement in late nineteenth- and early twentieth-century Mexico was directed against a neoliberal, or "order and progress," dictatorship. In other words, Mexico's (Porfirian) old regime was quite different from the Bourbon old regime(s). Guerra recognizes the ambiguity of the Díaz regime—its blending of (in his terms) tradition and modernity. But if the "old regime" is such a confused hybrid, suffused with the genes of "modernity," how can the "origin and nature" of the revolution (Guerra's alleged explananda) be conceived in these dichotomous terms? It would be truer and clearer to see the revolution as—primarily though not solely—a movement of aggrieved popular, especially peasant, groups, a movement premised on socioeconomic resentments and strong collective, even class sentiments. Yet neither Guerra's economic analysis nor his sociopolitical assumptions (tradition/modernity, holism, etc.) provide an adequate conceptual framework for the revolution, not even for the initial (Maderista) revolution (recall that the book stops in 1911). That revolution is presented as a bewildering collage of movements, events, and actors: "the Maderista revolt appeared as a revolution of society in all its diversity and according to the motives and positions of each actor within the strategic field which is his" (II, 299; also II, 286). No one argues for a monolithic revolution; but Guerra's haphazard collage is final proof of the inadequacy of his methodology. Neither the supposedly self-evident truths of tradition, modernity, and holism, nor the Francocentric preoccupation with "new" politics, à la Augustin Cochin, provide adequate bases for understanding why Mexicans rebelled in 1910–20.

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