Time on the Wheel: Cycles of Revisionism and the “New Cultural History”

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Over twenty years ago, the publication of an important quantitative study of African American slavery in the United States caused quite a stir in the historical profession. Coauthored by Robert Fogel, the historian upon whom Stephen Haber most directly relies to introduce his definition of the two epistemological models historians have used to evaluate evidence, this study was entitled Time on the Cross. It made controversial revisionist claims about the efficiency of slavery, the business sense of planters, the solidity of slave families, and the socialization of enslaved people into a shared work ethic with their masters. The methods used to collect and analyze the massive data on which the study was based—including computers and new statistical, mathematical, and economic techniques—were gathered under a single rubric within history and referred to as cliometrics. The methodology, of which Fogel and his coauthor Stanley Engerman were considered particularly prominent practitioners, became known as the New Economic History. Time on the Cross elicited vigorous debate, and one of the more prominent results of this debate was a collaborative volume, Reckoning with Slavery, produced by a combination of economists and historians. This volume took seriously the quantitative and analytical claims made by Fogel and Engerman and,

I am grateful to Gilbert Joseph for suggesting that this forum would be a good idea, and for tracking me down in Chile to get me thinking about the issues. I also wish to thank Eric Van Young, William French, and Mary Kay Vaughan, who shared early drafts of their papers with me to help me get going. Steven Kantrowitz, my colleague at the University of Wisconsin, helped me track down the references and work through some ideas on the debate concerning Time on the Cross.

following the central rule of the scientific method upon which social science history is based, attempted to “reproduce every important statistical manipulation, check every significant citation, reexamine every striking quotation, rethink every critical chain of inference, and question every major conclusion in Fogel and Engerman’s book.” The result, to the authors’ “surprise and dismay,” was a litany of errors. Of course, as the authors of Reckoning with Slavery themselves admit, no book—and especially no book that attempts to cover such a great deal of ground—can hope to be error-free. They found Time on the Cross, however, to be “simply shot through with egregious errors.” And most troubling of all was the “consistent tendency” in these errors: “all seemed to work in favor” of the revisionist position presented by the authors. “When the faults are corrected and the evidence is re-examined, every striking assertion made in Time on the Cross is cast into doubt. The effect in many instances is to restore and reinforce more orthodox conclusions.”

One message that the authors of Reckoning with Slavery wished to communicate to their audience was that the methods of the social sciences need not be devalued by such errors. Indeed, their hope was that a broad cross section of historians “could come to some understanding of the respects in which Time on the Cross transgresses the limits of the behavioral sciences.” For only through a “proper appreciation of the behavioral sciences’ strengths and limitations,” they maintained, would a “fruitful communication” between different epistemological and methodological traditions be possible.

A generation has passed, and the wheel of revisionism, upon which the making of so many of our careers depends, has turned again. The result seems to be, given the nature of the forum in which we are presently engaged, the “new cultural history.” I have cited extensively from the earlier debate because I think that some of the same issues are at play in this particular cycle, particularly the search for a “fruitful communication” between different epistemological and methodological traditions. It is in this spirit that I engage the comments of my colleagues in this forum, and reflect with them on the contributions and limitations of the approaches now under consideration.

One crucial difference between our present debate and the earlier one is, of course, the content of the revisionist claims made. In the earlier cycle, social science historians, inspired by the availability of new quantitative tools, were
convinced that history’s position as a social science had finally been assured: the same standards of evidence and objectivity could now be applied. In the present cycle, those historians influenced by cultural studies and postmodern approaches have moved exactly in the opposite direction. Questioning the legitimacy of all claims to objectivity and to the separation of knowledge from power relations, they have viewed previous truth claims in social science with skepticism and opted for more qualitative approaches.

What does this new revisionist turn actually mean, however? Does it mean, as Stephen Haber fears, that its practitioners do not believe “that there are objective facts that can be established regardless of the subjective beliefs of the observer”?\(^4\) At bottom, the debate is not about the existence of objective facts, nor about the relevance of efforts to determine what they are. By definition, when researchers attempt to verify or ascertain empirical facts, they try to separate these facts from their own belief systems, from the beliefs of the people who produced the documents that provide the facts, and from the relations of power that underlie the production of all sources of evidence. This is not the issue here. Instead, this discussion is about the process through which we go about determining what the facts are. Can we determine them regardless of the subjectivities just described? New cultural historians think not. Unless these subjectivities are foregrounded in the discussion, they are simply driven underground and any attempt to separate them from the facts will not progress very far.

I also think that the discussion is about what kinds of questions can be answered with what methodological approaches, and whether the impossibility of ever really answering a question should be enough to keep us from asking it. Often, as Eric Van Young suggests in his essay, the most interesting questions may indeed be the most intractable. The new cultural history provides us with methods and approaches that facilitate asking intractable questions and suggesting answers. It provides a style and narrative form compatible with asking questions that, while moving the field forward by eliciting new discussions, may themselves always remain unresolved.

All the participants in the present forum are concerned with what types of methodologies help us answer what types of questions. On one extreme, perhaps, are Haber’s discussions of workers’ income, estimations of marginal product, and the price of bread in New York and Philadelphia: given certain definitions and assumptions, as well as a way to calculate values and prices, it seems quite easy and transparent to decide if a particular worker is exploited or

if a regional market exists. On the other extreme, one might argue, is Eric Van Young's discussion of culture as text and text as culture. If contemporary anthropologists observe a complex cultural performance and shrink it into a text, at least the living performers can serve as a check on the anthropological imagination. What happens, on the other hand, when historians attempt to expand or “rehydrate” a text and make it into a culture once its practitioners are long dead? What gets lost along the way? And is it possible to control for the effect the water has when we rehydrate?

It is tempting simply to divide up inquiry into those questions best answered by easily quantifiable and verifiable methodologies—“hard” questions like markets, population, wages, prices—and the “soft” questions that involve the effects of power, morality, perceptions, values, memories, emotions. Once we do so, we can then choose sides and each score points against the other. To the discussions of exploitation and regional markets we can say, “Sure, so you have an index of exploitation or a definition of a regional market. But what about the really interesting questions, such as how the workers of that time defined exploitation, and what the relationship was between this moral or political definition and the quantitative index established?” Or alternatively, might there have been other factors that explained the change in the price of bread, factors that happened to work in similar ways in New York and Philadelphia at a particular point in time? And to Van Young’s concerns we could say, “It took you this long to find out that we can never really know what people felt or perceived in the past, at least those who did not leave their own memoirs or records! Let’s just stick to the methods and facts about which we can have some confidence and not pretend to do things we cannot do.”

If we are searching for a “fruitful communication” between different epistemological and methodological traditions, however, this does not seem a satisfying way to go. There are times when establishing indexes of exploitation or the existence of regional markets is extremely important for all historians, though a good dose of skepticism about the transparency of quantitative sources—and their seeming “objectivity”—can also be healthy. One valuable

5. This is precisely what Haber argues when he writes that questions like “how common people perceived, accommodated, and resisted capitalism and the formation of the nation-state” might be interesting, but “it is not clear that there are objective facts that can be established to answer them,” ibid. As I discuss in more detail below, Haber unfortunately does not consider the empirical evidence provided by new cultural historians when he critiques them for not having an evidentiary base. For this reason, exactly what he considers an “objective fact,” at least in theory different from the data marshaled by new cultural historians, remains unclear.
example of this is John Coatsworth’s article on the production of maize during the Porfiriato. Based on a set of government statistics that saw the production of maize taking a major downturn during the second half of the Porfiriato, scholars had posited a direct economic causation for the Mexican Revolution in falling maize supplies and increasing hunger and poverty. However, in re-examining the statistical sources themselves and the way they were produced—local political authorities often had little access to reliable data in their districts, but needed to present figures to their superiors—Coatsworth became convinced that the production of maize at the beginning of the Porfiriato had been substantially overestimated. When this early overestimation was corrected, the production curve for maize remained fairly flat, neither increasing nor falling significantly. With this revision Coatsworth challenged historians to search for different and more variegated explanations for the Mexican Revolution, and especially for explanations that incorporated political and cultural reasons for the unrest that exploded between 1910 and 1920.6

There are times when taking seriously the beliefs and emotions of historical actors—even when we will never have direct access to them—is also crucial to our attempts at understanding. In his 1978 article on Pancho Villa’s attack on Columbus, New Mexico, Friedrich Katz took seriously Villa’s analysis of the relationship between the United States government and the revolutionary faction of Venustiano Carranza, and he provided substantial indirect evidence to suggest that Villa invaded United States territory not out of some visceral desire for revenge, but because he was convinced that Carranza had made a secret deal with the United States. By forcing a diplomatic incident, Katz suggests, Villa hoped to bring this deal out into the open. In this case, of course, “proof” of Villa’s thoughts and beliefs cannot be forthcoming; but a careful consideration and comparison of existing documents allowed Katz to open up a new dimension to Villa as a political figure and, by so doing, revise existing judgements of Villa as a “bandit.”7


If we all agree that the importance of revisionism in history is precisely to open up new questions that force us to see familiar processes or relationships in a new light, then it seems to me that the most important and productive historical discussion will always go on at the margins—between different epistemological and methodological traditions, in the interstices between different fields and approaches. If this is the case, then we may often be debating rules of evidence and argumentation, standards of proof, rules of causation, and the legitimacy of different analytical categories. I believe we should welcome these debates, but in order to make them as fruitful as possible, I would propose adding one more set of rules to the discussion: those governing the debate itself.

I propose that we apply consistent rules of evidence and argumentation and clear standards of proof to our debates about epistemology and methodology. This means that, when discussing the implications and potential contributions of a particular work, we should take the whole of it seriously rather than pick and choose to support our own position or opinion. It is here that Haber, in his various published critiques of the new cultural history, does not live up to the standard he himself wishes to set for the historical profession.

When discussing the various works he sees as emblematic of this new historiographical trend, Haber does not address the broader purposes, arguments, or narratives of any of them. Instead, the examples criticized are consistently taken out of context and not assessed against the whole project of the work. In his discussion of my book Peasant and Nation, for instance, which he sees as a prime example of the retreat from “empirical verification” in history, he cites the book in 13 different footnotes, containing 19 references to 13 pages out of a 472-page book. Of those, 13 references (or 68 percent) are to the preface and introduction, which set out theoretical issues and approaches but present no empirical evidence. The remaining 6 citations are all to specific abstract phrases or moral arguments taken out of their evidentiary context. So where is the possibility to assess truth claims, if the examples criticized are taken out of context, the empirical evidence excised, and the author then faulted for lack of empirical verification? Indeed, can we engage in “fruitful communication” among methodologies and epistemological approaches in such a manner?

I am still convinced, however, that discussion and debate within, between, and among disciplines—about the use of distinct methodologies and episte-
mologies, about rules of evidence and argumentation, standards of proof, rules of causation, and the legitimacy of different analytical categories—is not only possible, but utterly crucial. It is in this sense that I agree with Haber that when historians influenced by cultural studies and postmodern approaches use language, categories, and standards of evidence and argumentation not suited to “persuade scholars not already predisposed to believe,” we choose to limit the range of our audience and influence in ways that may not always be advisable. As I wrote in the preface to *Peasant and Nation*, postmodern and poststructural approaches have a language and jargon that is prohibitive, “not only in the reading but, more important, in the writing.” In addition, many methods associated with these approaches are “distressingly ahistorical. Language and discourse seem to exist outside time and outside the people and the sociopolitical struggles that used, transformed, and produced them.” So what is a historian to do?

I propose we cultivate a more systematic awareness of when and why we might wish to use the analytical, methodological, and linguistic accoutrements of a particular approach. In the case of postmodernism or poststructuralism, for example, prohibitive language, abstract categories, and broad-ranging claims initially served as battering rams against the fortress of perceived positivism. As had been the case with earlier attempts at radical revision, they were useful in bashing down the first impediments to a radical rethinking of strongly held assumptions. Yet once the first line of defense was down, the same weapons or tools were not as effective with what lay behind. Battle cries are not subtle communication tools; quite to the contrary, they elicit new postures of defense in the audience and prevent authors from rendering the quieter, more delicate shades necessary for a full description. It is at that point in a cycle of revisionism that scholars have always begun to look for more finely honed instruments.

The new cultural history has, in my opinion, been struggling precisely with these issues. As the contributions to this forum by Eric Van Young, William French, and Mary Kay Vaughan make clear, the works published within this broad tradition in the past seven to eight years have opened up important new questions and approaches in the study of Mexican history. They have made a difference not because they lack empirical evidence, but because they have struggled to devise and employ new tools in the analysis of this evidence, and in the formulation of narrative. If we are to keep moving the discussion

9. Ibid., 364.
forward, we need both to evaluate the successes and failures of these efforts
and to consider where to go from here. Since these two goals are interrelated,
I will discuss both of them in relation to three big issues raised by my col-
leagues: the concept of hegemony; the concept of gender; and the dialogue
between anthropology and history that stands at the core of both fields’ engage-
ments with cultural studies.

In many of the discussions about the new cultural history, the concept of
hegemony has emerged as an important yet highly contested concept. First
coined by Antonio Gramsci as part of a Marxist theory of politics, it was meant
to help him understand why Europe had not undergone socialist revolution
after the First World War. In this context, it emerged as a concept to explain
domination, and especially to understand how the oppressed could come to
believe in some of the cultural and political notions espoused by their oppres-
sors. Yet at the same time, by also coining the term counterhegemony, Gram-
sci suggested that such forms of cultural and political domination could be
counteracted and someday overcome.11

In the more recent work of James Scott that posits the existence of a
relatively autonomous, “hidden” sphere of lower-class resistance, hegemony
becomes more specifically a synonym for the system of political and ideological
domination that lower-class people only appear to accept. Scott conceptual-
izes two essentially parallel systems of political belief and action, which meet
only in moments of crisis or uprising. In this context, the more interactive or
flexible meanings of the term hegemony are less useful.12

Other scholars have chosen to take the concept in a different direction.
Beginning with the work of Raymond Williams, hegemony and counterhege-
mony began to be seen as interdependent concepts that help us analyze specific
cases of political or cultural struggle.13 Along with several other Gramscian
concepts, hegemony and counterhegemony were also embraced in a similar
way by the India-based school of Subaltern Studies.14 In the mid-1980s, Argen-

11. Antonio Gramsci, Selections from the Prison Notebooks, eds. and trans. Quintin Hoare
Haven: Yale Univ. Press, 1985), and Domination and the Arts of Resistance: Hidden Transcripts
14. For a fuller treatment of this issue, see Florencia E. Mallon, “The Promise and
Dilemma of Subaltern Studies: Perspectives from Latin American History,” American
Historical Review 99 (1994).
tine sociologist Ernesto Laclau coauthored a book with French philosopher Chantal Mouffe that attempted an extremely adventurous theoretical fusion of Gramscian theory of hegemony with postmodern discourse theory; while they did not cite Williams directly, Laclau and Mouffe’s use of hegemony and counterhegemony was within the same general vein.\textsuperscript{15} I have treated the promise and the problems associated with some of these trends in detail elsewhere; but for the purposes of this discussion I would like to highlight two distinct analytical perspectives that have emerged from these theoretical conversations.\textsuperscript{16}

The first emerges from the legacy of Raymond Williams. When attempting to conceptualize politics, pairing hegemony and counterhegemony allows us a flexible understanding of political culture by helping us see change in a dynamic, multivocal way. If political institutions and systems of domination rest on complex combinations of violence and legitimation, then understanding the process of legitimation necessitates an ability to analyze how alternative political and cultural notions are generated and then negotiated in relation to the dominant system. The pairing of hegemony and counterhegemony allows us to do this, and helps us generate hypotheses about cultural and political change that permit us to be sensitive to what is going on “off-stage” and to the long-term impact of these events on systems of political rule.

A second direction in which these conversations can take us emerges more directly from the work of Laclau and Mouffe, as well as Subaltern Studies. Here an incorporation of linguistic and discourse analysis allows for a finer grained consideration of the actual processes through which power and meaning are negotiated. Here the concept of hegemony is used, as William Roseberry put it, “\textit{not} to understand consent but to understand struggle; . . . What hegemony constructs, then, is not a shared ideology but a common material and meaningful framework for living through, talking about, and acting upon social orders characterized by domination.”\textsuperscript{17} In this context, the process of negotiation and struggle is highlighted and analytically magnified.


\textsuperscript{16} When focussing on the various uses of hegemony in “Promise and Dilemma of Subaltern Studies,” I emphasized that the combination of terms from Marxist political theory with methods of linguistic analysis has yielded creative tensions that scholars needed to analyze carefully, both for the positive and the negative, when using these approaches in their own work.

For me in my work, the concept of hegemony has been especially useful because it has these multiple dimensions. In the specific context of Latin American history, it has allowed me to seek an explanation for why different countries developed differentially stable and legitimate sociopolitical orders out of the historical interaction of dominant and subordinate political groups. Successful and unsuccessful efforts at achieving stability and legitimacy could thus be explained within the same context: hegemony as a pattern or framework of struggle (what I have called hegemony as process) sheds light on both. At the same time, hegemony in the original Gramscian sense—in which counterhegemonic notions are partially incorporated into a system of rule in order to legitimate or stabilize it—could help us distinguish those cases in which a relatively lasting pact emerged between rulers and ruled. This is what I called hegemony as end point.

If we think of hegemony only as an end point to a struggle, rather than as the struggle itself, it is a less widely useful concept. We are often in danger, as well, of confusing one meaning or use with another. Thus, when William French discusses the various uses of “culture” and suggests that my use of the term “communal hegemony” leads back in the more traditional direction of defining culture as a relatively coherent system of norms and values that guide all forms of conduct in a society, he seems to be understanding hegemony as an end point rather than as a process of struggle. If the very multivocality of the concept also makes it potentially contradictory, does this make it less effective?

This confusion is more than compensated, in my opinion, by what the double meaning and use of hegemony allows us to conceptualize. Not only can we analyze politics close up as an interaction between rulers and ruled, but we can also more easily discern divisions and interactions among factions of rulers and among factions of ruled, examining the internal power relations at the top and bottom, and even rethinking the interaction between dominant and subaltern as sometimes involving only a sector of each. This fine-grained use of hegemony as process then permits us to understand the contingency of any hegemonic end point: since any pact will likely be negotiated between a sector of the rulers and a sector of the ruled, it will be subject to renegotiation, contestation, or breakdown at any stage.

An equally multivocal category, with analogous opportunities for confusion, is gender. An example of such confusion occurs in Haber’s essay, when in commenting on Peasant and Nation he faults me for both making gender a crucial part of my story and, at the same time, for not having direct access to data
about what women did.\textsuperscript{18} While all of this is true, an important link in the story is also missing. In the same paragraph from which he cites, I define gender as “the social construction of sexual difference and its use in the organization of power relations,” and suggest that it “organizes hierarchies among men, and between young and old, as much as between women and men.”\textsuperscript{19}

Indeed, in a variety of regional subfields of history it has become increasingly common to suggest that when we explore the meaning of categories that denote hierarchy—such as gender, race, ethnicity, and class—we need to see how these categories apply to, and are lived by, people on both sides of any particular divide. Not only do women have gender, then, so do men, and we need to explore how the meanings of femininity and masculinity are constructed and argued over in specific societies and historical contexts. Not only do people of color have race, so do whites; and the historical construction of whiteness is getting increasing attention from historians. We should also consider these reciprocal interactions in the case of class identities, and explore how particular forms of class dominance emerge in specific societies, often with distinct regional, gender, and racial referents or identities attached.\textsuperscript{20}

The move away from equating gender with women has been especially productive in helping cultural historians understand power relations within popular culture as well as between dominant and subaltern groups. In his discussion of recent books by Ana María Alonso and Steve Stern, William French notes the value of such developments. At the same time, he notes that they have emerged in relation to an already existing and important literature in the history of women, even as gender and women’s history may sometimes have different or even conflicting agendas. Though Mary Kay Vaughan does not systematically address the issue of gender in her essay on the twentieth century, notions of paternalism and male power within popular culture are mentioned for several subregions of the country and, if considered along with works such as Ilene O’Malley’s book on the gendered constructions of revolutionary heroes, would also allow us to move forward in understanding

\textsuperscript{18} Haber, “Worst of Both Worlds,” 367–68.
\textsuperscript{19} Mallon, \textit{Peasant and Nation}, xix.
\textsuperscript{20} Among recent works using gender analysis to understand femininity and masculinity as they relate to issues of color, class, and region are Steve J. Stern, \textit{The Secret History of Gender: Women, Men, and Power in Late Colonial Mexico} (Chapel Hill: Univ. of North Carolina Press, 1995); and Nancy Appelbaum, “Remembering Riosucio: Race, Region, and Community in Colombia, 1850–1950” (Ph.D. diss., Univ. of Wisconsin, Madison, 1997).
the gendered dimension of twentieth-century negotiations around state-making.\(^{21}\)

As Vaughan rightly emphasizes in her essay, new cultural historians must be careful to trace how their subjects, along with the cultures, discourses, identities and power relations they construct and contest, change across time and space. It is in this sense that, from the very beginning, this revisionist trend has rested on a collaboration and conversation between anthropology and history. Sometimes this conversation has remained more implicit or between the lines; other times it has dramatically come out into the open. But it informs the concerns of the essays by Van Young, French, and Vaughan, as well as Lomnitz’s work and mine. What does this interdisciplinary conversation entail, and how can it help us to continue in our process of rethinking and revision?

For new cultural historians, as well as the new social historians who came before, a conversation with anthropology began in the use of methods and sources through which we hoped to gain access to the actions and beliefs of common folk, those whom traditional historians had considered voiceless. This entailed, in some cases, the use of oral history as a key source; in others, the use of methods of ethnohistory or ethnography. Of course, such methods were easier to use when researching twentieth-century history; but the culturally sensitive reading of written sources, especially court or Church records, was also used by ethnohistorians on both sides of the disciplinary divide who worked on earlier time periods.\(^{22}\)

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One of the problems with oral history that historians have repeatedly discussed is that the source is not deposited in an archive and is thus not easily verifiable. In some areas of Latin America, there have been attempts to address this issue through the creation of oral history archives that hold tapes and transcriptions of interviews carried out by a combination of different researchers. While considerations of privacy need to be taken into account in the compiling of these collections, in the long run they will help us expand and rethink our use of oral sources. Still, as Van Young points out in his essay, the ability to access oral testimony is temporally quite limited. When working on societies several hundred years in the past, he points out, the documents that survived have lost their “soft tissue” of cultural meaning and representation long ago, leaving only the hard bones of institutions behind. As a consequence, Van Young wonders, do anthropologists somehow have it a little easier because they are dealing with people, ideas, and performances in the “ethnographic present,” where the subjects can talk back and therefore multiply the layers of meaning available to the researcher? Though I certainly agree that live, talking subjects give scholars a definite advantage in the analysis of cultural meaning and representation, I do not see this as a disciplinary question. In other words, I do not think that anthropologists are privileged in the struggle to gain access to the voiceless.

When anthropologists also began going through their crisis about the meaning of their discipline, one of the first concepts to be heavily criticized was ethnographic authority: the construction of expert status through the participation in and observation of a culture. In much cultural anthropology the main informants were not named nor listed; one did not get a sense for who they were, what their place in society was, nor what their relationship with the ethnographer had actually been. That an individual could travel to a different society and culture, set down roots for a while and meet people, and then come back and present expert testimony to the academy, came into question. How was that authority constructed?23

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23. Among the works I found most helpful along this line are James Clifford, The Predicament of Culture: Twentieth-Century Ethnography, Literature, and Art (Cambridge:
Some anthropologists, when faced with this challenge, stopped doing ethnography altogether. They began to read cultures as texts and to reread texts, whether archival or published. Some adopted the criteria used by literary critics to “read” cultures and cultural meaning. Other anthropologists, however, objected strenuously to this new disciplinary turn, arguing that literary methods, by emphasizing insight and empathy rather than scientific validation, stripped anthropology of its capacity to generalize and compare across cultures. As Melford Spiro put it in a 1986 article also cited by Eric Van Young, even if in the human sciences subjective methods must of necessity be used “as techniques of inquiry” or in “the formulation of interpretations and explanations,” the linguistic turn, or—as Spiro himself called it—the “hermeneutic agenda,” has confused technique with method. “If it is pluralistic in technique,” Spiro insisted, “scientific inquiry is not, however, pluralistic in method. If scientific techniques consist of the empirical procedures employed for obtaining or eliciting data, the scientific method consists of the logic or rationale according to which those data are judged to be evidentially relevant, adequate, or sufficient for the acceptance or rejection of hypotheses.”24

Spiro’s main concern, as a sophisticated critic of what he called the “hermeneutic circle,” was that anthropologists not use the same subjective criteria to gather, interpret, and validate data. “If ethnographic interpretations are processed through all those subjective filters,” he continued, “an objective—a public and replicable—method is required for deciding whether an interpretation should be accepted or rejected. The scientific method, which assesses the validity of interpretations by the logical procedure of testing their predictive or retrodictive consequences, constitutes such a method.”25 Unless some form of verification is possible, Spiro reasoned, all sense of intellectual responsibility seems to fall away. And he concluded:

It might be argued that in large measure that is the way that, in fact, it has usually been. But to adopt the hermeneutic method is to ensure that, in principle, it is also the way it must always be.

For while the boundary in all of the social sciences between theory


25. Ibid., 140.
and ideology, fact and value, objective and subjective, evidential constraint and personal preference is often blurred, the hermeneutic method guarantees that those boundaries will never be clear.26

Whether or not those boundaries can ever be clear—and I think this is indeed an important part of the debate surrounding the usefulness of linguistic or postmodern approaches—I do agree with Spiro that our intellectual responsibility continues to reside in the way we assess the value and usefulness of different interpretations. But I do not agree with him that what he calls the hermeneutic method somehow takes us into a conceptual or intellectual abyss. To the contrary, I think it challenges us to rethink, and actually to expand, the methods through which we compare, verify, and generalize our interpretations. Even as we abandon the claim to science, and accept the inevitably blurry line between objective and subjective, we do not have to abandon our never-ending efforts to distinguish between fact and fiction. Indeed, I believe that the interdisciplinary dialogues enriched by recent postmodern approaches can in fact enrich and deepen these efforts. Let me give some specific examples from the dialogue between history and anthropology.

The image of anthropologists and historians as ships passing in the night has had an unfortunate ring of truth. While historians, unhappy with the limited access documents gave them to the voices of the common people, journeyed to the field to work in ethnographic methods, many anthropologists, unhappy with the limits of ethnographic methods, were journeying back to make use of archival materials. At times it seemed that the practitioners of the two fields had just switched places, each yearning for the better situation they saw on the other side of the disciplinary fence. But occasionally the resulting dialogue has opened up exciting new venues in the interdisciplinary study of culture and power, resistance and domination.

French addresses some of these questions in his comparison of both historical and anthropological works. One of these, he notes, is a reconceptualization of culture. Most directly addressed in Stern’s The Secret History of Gender, French points out, culture emerges—in dialogue with the work of David Warren Sabean—as a series of arguments over values and standards of behavior, rather than as a discrete set of norms and values that guide all kinds of conduct in society. Another key reconceptualization involves the concept of community. Out of a dialogue between anthropology and history, and of both with Foucauldian social theory, French observes that subaltern communities emerge

26. Ibid., 141.
as sites of conflict and internal struggles for power, no longer the privileged repositories of heroic agency that they often were in the social history of a decade ago. And finally, French points out that the “agency of the historian,” and the position that the scholar takes vis-à-vis the text, is also of great interest and concern in this interdisciplinary dialogue; he juxtaposes Alonso’s *Thread of Blood* and my *Peasant and Nation* when discussing this particular question. The implications of this last point, especially in defining narrative strategies, are something I will return to below. But first I would like to consider the new directions that a history-anthropology dialogue has suggested in the use and analysis of sources.

What many of us have begun to learn—and perhaps most importantly, begun to make methodologically operational—is that the combination of anthropological and historical sources, critically and dialogically read, provides the most satisfying window on culture, community, and politics. In my own work, perhaps most clearly and intensely in my recent research on twentieth-century Mapuche indigenous communities and the Chilean state, this combination has been fruitful and exciting. When oral history itself becomes an ongoing dialogue, its very lack of transparency becomes its greatest strength. In southern Chile, in several Mapuche communities where I did research between 1996 and 1997, oral history became a way of collectively reconstructing and debating the past. Once I had made contact with people at the local level, I carried out interviews in which I shared archival materials I had found. We discussed the documents as part of our conversation, trying out different meanings and interpretations of them, using them as starting points for a conversation about the community’s past. I offered copies of the sources that interested people, as part of an ongoing exchange. When interviews were long and taped, the next time I visited I brought back a transcript and invited the interviewee to read it over and suggest changes or correct errors. Often these transcripts then served as starting points for the next conversation. And over time, as I interviewed a number of people within a single community, I used their different and often conflicting versions of what had happened before to animate subsequent discussions.27

27. I don’t presume to be the first person to use these methods, since they come out of a deep interdisciplinary dialogue in progress for about a decade. Among the studies from the other side of the disciplinary divide that take on historical issues with sophistication and complexity are Marisol de la Cadena, “Race, Ethnicity, and the Struggle for Indigenous Self-Representation: De-Indianization in Cuzco-Peru (1919–1991)” (Ph.D. diss., Univ. of Wisconsin, Madison, 1995); Joanne Rappaport, *The Politics of Memory: Native Historical Interpretation in the Colombian Andes* (Cambridge: Cambridge Univ. Press, 1990);
Just as documents served as entry points into local memory during my research in southern Chile, versions of local history retrieved through interviews served as points of departure for further archival research, much of it in the centralized archives in Santiago. Often, oral versions of events were vague on dates, or unclear as to the names of specific people, even when their actions could be remembered in great detail. As a result, making links to the archival record could prove frustrating at best. Still, several key events would not have emerged nor been made clear from documents alone had I not gotten my first lead from oral history. And often, following up on the gaps or unanswered questions in the documents allowed me, through oral history interviews, to round off important stories that had emerged first in the written record.

At times conflicting oral history versions allowed me to press for new or different information from a key interviewee; or copies of documents about events we had discussed encouraged a person to tell me the whole story during a second conversation. Such an approach facilitated the exploration of divisions within communities, and thus the identification of conflicting versions of history with particular political, cultural, social, or economic factions. This made real the broader statement about community and culture being arenas of argumentation rather than seamless and unified wholes.

In one particular community, where leaders publicly accepted me as the community’s historian and agreed to cooperate in exchange for receiving a report about the community’s history at the end of my research year, I interviewed people from different local factions and wrote an 80-page initial report. Before I left I presented it to key leaders and informants, who promised that they would read it and comment. Two months later I received a phone call disagreeing with some of my interpretations. The discussion, as well as the collective construction of history, goes on.28

But what does all this mean for the researcher’s position within the narrative? My preference, as French points out in his essay, is to place myself within the struggles over power and meaning, while Alonso uses irony and paradox to distance herself from her subjects/friends/informants. Citing Bernard Cohn, and Mark Thurner, From Two Republics to One Divided: Contradictions of Postcolonial Nationmaking in Andean Peru (Durham: Duke Univ. Press, 1997).

Alonso insists that anthropologists must in effect take responsibility for understanding and feeling the consequences that events and processes have had on their subjects. “These consequences are not just cognized,” she writes, “they are felt. I have largely effaced emotion from my prose,” she continues, “because I cannot envision a form of writing that would allow me to capture the pain of these consequences. A style that is distanced from felt experience has characterized much anthropological writing.” Yet she concludes: “This is a form of distance that I think we should try to overcome.”29

Distance is, at times and for certain purposes, an important and useful tool. If we place ourselves within the struggles over power and meaning, if we put the lives and emotions of the people we study first, do we not tend to recreate an official story, even if it is theirs—and, inevitably, partially ours—rather than the story told by the dominant classes? This is the second reason Alonso gives for her distanced style. Citing Stuart Hall, she calls for an end to “that ‘heroic’ view of popular discourses and practices which presumes an autonomous subaltern domain, which represents subordinated groups and classes as the victims but never the accomplices of effects of power that are construed as wholly external.”30 Yet at the same time, what if the effects of power we wish to study are largely internal to the subaltern domain or community? In Alonso’s case, how many Namiquipan stories are there to tell, and to how many of these does Alonso’s narrative give us access? Unless Alonso stays inside, how will we gain entry to a more contradictory gamut of the versions and explanations of history the Namiquipans possess? Is it Alonso’s ironic distance that highlights the versions given by elders from originario families, and foregrounds their contradictory yet privileged relationship to hegemonic forms of power?

When, how, and if to distance ourselves from our subjects are not questions with a single or permanent answer. To begin with, none of us can ever hope to gain access to the entire spectrum of stories, conflicts, and wounds contained in a single case. The questions we ask, the people we befriend, even the quietest gesture we make will open some doors and close others. Since of necessity we must make gestures and befriend specific individuals when we initiate a research process, often we are only partially aware of what we have done to cause a particular opening or closure. If the events and discourses we reconstruct are always partial, our ways of telling must be flexible. I believe we

30. Ibid.
should strive, depending on each particular case, for a narrative and analytical style that facilitates the respectful and empathetic telling of the partial truths we are able to collect. Sometimes that means staying inside, maybe even allowing the tearstains to remain on the page. Sometimes it means gently using distance to chronicle the coexistence of conflicting and mutually hostile stories. For those of us committed to social and cultural history, it means bearing witness to the ways in which abstract historical processes—markets, states, capitalism, war, colonialism, imperialism, authoritarianism, and military repression—leave direct imprints on the bodies and memories of human beings. Transcribing these imprints into narrative will of necessity require different rules of evidence and argumentation, standards of proof, and rules of causation than a study of the price of wheat or of mortality rates. Even asking the questions that will yield access to such imprints will necessitate the legitimation of different analytical categories.

Moving the new cultural history forward within the field of Latin American history necessitates deep reflection and debate around these complex and vexing methodological questions. As has already been the case, many of the most helpful discussions will be interdisciplinary: between historians and anthropologists; and among those who engage in cultural studies from the perspectives of literature, history, anthropology, sociology, and political science. Other times, as in the past, productive dialogue will occur across regional boundaries: with French historians of culture, politics, and memory; with the subalternists of India; with Africanists who study ethnic politics, resistance, and the construction of tradition; with Latin American historians of their own societies who, over the past generation, have lived intellectual, social, political, and economic processes starkly different from those of us who mainly make our lives in Europe, Canada, or the United States. For these interdisciplinary and cross-regional exchanges to be fruitful within our own field of Latin America, however, they need to be heard and appreciated in all their dimensions. And it is here that the kind of narrow and short-sighted attack encouraged by the work of Haber does more damage than good.

By focusing on red-herring issues like whether or not cultural historians believe in facts, Haber and others in effect flatten out debate. They pretend to defend the field against an assault on objectivity, and in so doing aim to hide from view the deeper debates about documentary analysis, narrative voice and strategy, and the responsibility of scholars vis-à-vis their sources and “subjects.” Rather than pretend that cultural studies, and alongside it cultural history, are seamless, top-heavy juggernauts of antifact, it seems more useful to look a bit more closely at the textures, seams, and fissures of debate within
them. While some of the debates within cultural studies and cultural history are not terribly useful, in others scholars are struggling to hammer out new strategies to help us deal with our recurring questions about access to the so-called “voiceless” or, as Eric Wolf once put it (referring to Marx), the “people without history.”

It is particularly interesting, in this context, that while Haber uses my essay in the *American Historical Review* and the “Founding Statement” of the Latin American Subaltern Studies Group interchangeably as evidence of a retreat from objective history, two of those who signed that statement have recently cited my same article as an example of exactly the opposite trend. According to José Rabasa and Javier Sanjinés, my article shows signs of a “turf battle” between anthropologists and historians, such as myself, who still hope to glimpse “a subaltern subject” through the layers of documentary camouflage, and “those employing postmodern approaches who cannot go beyond seeing documents as textual constructs.” In the opinion of Rabasa and Sanjinés, therefore, I would occupy the position of the empiricist who claims the high moral ground because I am willing to get my hands dirty in the evidentiary mud, or because I proclaim, in their words, “a privileged access to materiality.”

So which is it? Am I the morally and politically motivated radical who is eschewing all forms of objective or fact-based history in order to further my own political agenda, or am I the turf-conscious historian interested in defending empirically based history from the attacks of postmodern radicals whom I see as stepping on my disciplinary toes? It seems to me that I am neither, precisely because the purpose of the new cultural history is to take seriously the kinds of challenges and new perspectives offered by cultural studies to the kind of disciplinary work historians do. I am not “non-disciplinary,” as Rabasa and Sanjinés suggest the Latin American subalternists are, precisely because I do believe, as does Spiro, that unless some method of verification is possible we cannot responsibly assess the intellectual value and usefulness of different interpretations, and, at the same time, because I do not believe, as do Spiro and Haber, that the much-vaunted scientific method is the best way to assess most interpretations. I am convinced instead that serious and collegial inter-


33. Ibid., vii.
disciplinary conversation—which seeks to understand the distinct methods, criteria, and value systems applied by different disciplines to the process of collecting and presenting evidence—is crucial to the intellectual work in which we are all engaged.

I do agree with Haber that we Latin American historians face an important set of choices. If, as he suggests, we “choose to adopt rules of evidence and argumentation that will make [our] arguments persuasive” to what he defines as “a broad audience of historians and social scientists,” these rules may also close off many of our least traditional sources, innovative narrative strategies, revisionist analytical categories, and complex methodological discussions and debates. I propose we choose differently and, with astounding temerity, turn the choice on its head. For in my mind, our “relevance as a field of scholarly inquiry” rests not on requiring everyone to submit to the party discipline of a single set of rules, but instead on adopting rules of discussion and debate that will allow for serious dialogue among contradictory methodological and epistemological traditions. The wheel of revisionism grinds on and our grip on historical reality is tenuous enough as it is, without further limiting the tools with which we gain access to it. Perhaps the most fruitful discipline to which we can all submit is that of mutual respect. Promoting and practicing forms of debate that are honest and collegial will not, in my opinion, stifle disagreement. To the contrary, it will foster deeper, more candid, and substantial intellectual exchange.
Scholars at loggerheads