
Editorial

With this issue of the *International Journal of Oral History*, I step down as editor-in-chief, and formally turn the reins of direction over to Charles T. Morrissey, former President of the Oral History Association, long-time friend and teacher, and one of the finest scholars working in oral history. It is a delight to be able to place the burden in such capable hands with the assurance that the work we have begun will continue.

When we started the *IJOH*, we stated that the objective would be to remove our discussions from the provincial and limited, and to concentrate upon methodological and theoretical debates. We have done so, and I think this issue will show how far we have come. Herein we present an essay by Louise Tilly, one of the outstanding social historians in the United States and President of the Social Science History Association, which raises important questions about our theory and practice; we have also included a series of replies from some prominent authors in the field discussed by Tilly. The questions raised, the way they are posed, and the answers given all illustrate the growing sophistication of the debates about oral history, of which we would like to think we have been a part. This issue continues our original commitment.

It is customary when stepping down to thank those who have made one's efforts, however modest, worthwhile. I would like to do so. First, I want to thank Alan Meckler, publisher of the journal, for allowing me to have such a bully forum. His trust, faith, and dedication were crucial to its life, and his support is deeply appreciated. John Raimo, our managing editor, is everything one would want an editor to be—understanding, considerate, tolerant, and above all, talented. He made me look good with each and every issue. One of the drawbacks of ending a relationship is losing such friends, but I would hope not their friendship.

I especially want to thank all of the members of the editorial board. Their work and their interest insured the success of our attempts to be truly international, to infuse the *IJOH* with an interdisciplinary perspective, and to develop a theoretical stance.

I think we have accomplished much. I look forward to what others can now do. All institutions need at times fresh ideas, new slants, different

management. That will come, but with it the same commitment to the field, the same wide vision and openness to new ideas and unusual approaches. I wish you all the best of luck.

Ronald J. Grele
Editor-in-Chief

People's History and Social Science History

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A friend and colleague of an author reviews a two-volume history. Acknowledging his relationship with the author, the reviewer calls the books "excellent, full of rich new insights, sparkling with intelligence, the sentiment which underlies our impassioned love for the historian's craft, one of the most beautiful of the disciplines devoted to the study of man." "Yet," the reviewer continues, "it is striking that the individual is almost entirely absent. . . . Psychology, although not totally ignored, is always collective psychology. . . . Is not the author," the review continues, "turning back to the schematic. . . . toward the sociological, a seductive form of the abstract?" (Febvre, 1941b: 177, 128).

Sit back and imagine the time, place, and people involved in this story: a 1980s traditional historian, perhaps a radical people's historian, excoriating a social scientific colleague? Not at all; this is no case of contemporary backlash. The time was 1941, the reviewer Lucien Febvre, the author Marc Bloch, the book, *La société féodale*.

Let us probe a little deeper into this historical context for today's disagreements.¹ Febvre disagreed with Bloch's use of the word "mentality" in the section of the book titled "La mentalité religieuse." "Mentality," Febvre writes impatiently, "but never sensibility [feeling]." Yet when we turn to Febvre's own essay on feeling in history, published in the *Annales d'histoire sociale* earlier the same year, we find that Febvre himself defined a collective psychology of man in the past as appropriate for the historian's concern.

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Emotions Are Contagious

[Emotions] imply relations between men, collective relationships. They are doubtless born within the organic depths specific to a given individual. . . . [B]ut their expression is the result of a series of experiences of common life, of similar and contemporaneous reactions to the shock of identical situations and encounters of the same nature. . . . [L]ittle by little. . . by linking many participants in turn as initiators and followers—[these] end by becoming a system of interindividual motivations that differ according to circumstances and situations. . . . [and] a true system of emotions is built. They become something like an institution [Febvre, 1941a:8].

Earlier still, of course, Febvre had insisted that the group, not the individual, was the appropriate subject for the historian. He wrote in 1922 in *La terre et l'évolution humaine*, "Not the man, never the man, [but] human societies, organized groups." As André Burguière has pointed out, this formula stood at the heart of Febvre's and Bloch's joint enterprise, the *Annales*, from 1929 to the war (Burguière, 1982: 429).

An Old Debate and Contemporary Differences

Thus Febvre and Bloch, in their opening manifesto to the readers of the *Annales d'histoire économique et sociale* in 1929, urged the end to the disciplinary schism, between student of past time and those of contemporary societies and economies, among historians, economists, and sociologists. They called for a free flow of methods and interpretive perspectives among these scholars. Their prescription for breaking down barriers and surmounting schism was not methodological or theoretical discussion, but exemplary practice ("par le fait"). The *Annales* would welcome and publish research in many fields and specialties; research united by a commitment to impartiality. "Our enterprise," concluded Bloch and Febvre, "is an act of faith, in the exemplary virtue of honest work, conscientious and solid" (Bloch and Febvre, 1929: 2).

The tension evident in these programmatic and methodological statements by the founders of *Annales* is a useful corrective context for contemporary social science historians, whether they feel defensive or assertive about their historical practice. As shown by recent surveys of contemporary research and writing, such as that of the *Journal of Interdisciplinary History* in 1981, and that under way in *Social Science History* through a series of reflective review articles, today's historical practice is marked by a multiplicity of approaches. Several claim descent from the *Annales*; other reject this approach and call for a return to other historical traditions; still others call for a politically committed history

in which the individual life and subjective experience are foremost. I leave aside here any consideration of the traditionalists' current polemic,² and focus instead on one of the antinomies in today's historical practice for which the contradictions and hesitations of the founders of *Annales* provide the essential historical context: that between social science history and people's history.³ The main focus of my argument is the alternative conceptualization in these historical approaches to individual experience in the past. Social science history focuses on group experience and individual variations within it, using life history and biography as building blocks; people's history emphasizes individual subjective experience and tends to see it as an identifiable and definable unity.

That the social science and people's history approaches should be in tension—indeed, often in opposition—is ironic. Much of the impulse for social science history came from a wish to understand ordinary people's lives. This led to the search for sources that, when used collectively, compared systematically, and frequently quantitatively analyzed, could reveal aspects of experience for which direct testimony was absent. The great divide is not in the interest of ordinary people's lives, but in the way we conceptualize them, in the questions we ask about them, the sources we use to find out about them, and the way in which we use these sources. Let me illustrate by a comparative look at the self-definitions and practice of social science history and people's history, followed by examples from several European projects that make use of biographical accounts and oral sources in different ways.

Contrasting Approaches

First some definitions. The first issue of *Social Science History* defined work in the field as "research that attempts generalizations of some breadth verified by systematic examination of the relevant evidence and supported by quantitative analysis when appropriate. Research efforts involving comparisons across time between individuals and groups within a single population and between different and properly comparable populations across space and over time will be particularly welcome" (*Social Science History*, 1976: i-ii).

The main purpose of the Social Science History Association, its constitution (also published in the first issue of its journal) begins, "is to improve the quality of historical explanation in every manner possible, but particularly by encouraging the selective use and adaptation in historical teaching and research of relevant theories and methods from related disciplines, particularly the social sciences." Like the *Annales* manifesto and much of the practice that evolved from it, then, the self-definition

of social science history invokes systematic evaluation, comparison, and use and adaptation of theory and method from the social sciences; quantification is not a necessary component, but it is welcome when appropriate. Social science history deals with social groups and relationships. Its central method is collective biography, for it examines individuals in groups by studying variation in their experience and behavior.

We find quite a different emphasis in Samuel's introduction to the collective volume, *People's History and Socialist Theory* (1980). The main thrust of people's history has arrived at the recovery of the essence of individual subjective experience. In oral history, Samuel notes, there is an "overwhelming interest in reconstituting the small details of everyday life; in local history, the shift from 'places,' to 'faces,' from topographical peculiarities to the quality of life; in labour history, the preoccupation [with] the more spontaneous forms of resistance" (Samuel, 1981a: xviii). He continues that people's history, "whatever its particular subject matter, is shaped in the crucible of politics, and penetrated by the influence of ideology on all sides" (Samuel, 1981a: xx). In fact, Samuel believes, all colors of ideologies have produced people's history, but they all share a "yearning for the vanished solidarities of the past, and a belief that modern life is inimical to them."

Thompson (1978) begins his introduction to oral history with a similar insistence on its political possibilities, which, he feels are "intrinsic [to] the oral approach. . . [because it] is about individual lives" (Thompson, 1978: 15). Some forms of women's history could also be called people's history given common concern for the subjective, the dictum that the personal is political, and the method that emphasizes personal documents and micro-description. The distinctive features of people's history, then, are its focus on individuals and their experiences, and its political commitment.

People's history and social science history, despite their different methodological approaches and focuses, share important characteristics. They represent rapidly expanding bodies of historical research. Their advocates practice them with commitment and enthusiasm. They encourage discussion of theory, and it is sometimes central; but theory is not abstracted and removed from practice with social science history. The point is practice, doing history. People's history, then, shares a similar motivation and the common commitment to producing historical data. Nevertheless, they are separate domains. To the extent that people's historians stress subjectivity, individual experience, and evidence internal to individuals and hence neglect both theory and analysis, they do fall short of social science. To the extent that they reject collective biography for an individual focus, they diminish their own capacity to understand variation and resort instead to descriptive detail or ideal types.

Alessandro Portelli, who has done an oral history project with metal-workers of Terni (Italy), turns subjectivity into a virtue:

But the unique and precious element which oral sources force upon the historian and which no other sources possess in equal measure (unless it be literary ones) is the speaker's subjectivity; and therefore, if the research is broad and articulated enough, a cross-section of the subjectivity of a social group or class. They tell us not what people did, but what they wanted to do, what they believed they were doing, what they now think they did. . . . Subjectivity is as much the business of history as the more visible 'facts' [Portelli, 1981: 99-100].

Most social science historians would probably agree that oral sources bring subjectivity to the fore, as does individual testimony in written form, but they would also question whether aggregating individual subjective reports can ever produce a group or class subjectivity. He continues, moreover, that the "researcher must 'accept' the informant and give priority to what he or she wishes to tell, rather than what the researcher wishes to hear" (Portelli, 1981: 103). Again, the social science historian may well ask how the sources can set the problem and its conceptualization.

Portelli paradoxically concludes that workers' oral reports are in no sense the working class speaking for itself: "The control of the historical discourse remains firmly in the hands of the historian; the class does not speak in the abstract, but speaks *to* the historian; and *with* the historian. . . . Things may indeed be more the other way around: the historian speaking through the workers' testimony, ventriloquizing a discourse which is not theirs" (Portelli, 1981: 104-05). The historian, to Portelli, seems to have a contradictory double role as passive listener and ventriloquist, putting words into the mouths of others. Again, he sees this as positive: "Oral history is told from a multitude of 'circumscribed points of view': the impartiality claimed by traditional historians is replaced by the partiality of the narrator. The partiality of oral history is both political *and* narrative: it can never be told without taking sides, since the 'sides' exist inside the account" (Portelli, 1981: 106).

Radical passivity, when faced with subjectivity and unwillingness to see subjectivity and partiality of point of view as a challenge rather than as a given, profoundly separate Portelli's approach from that of the social science historian. Social science historians believe, in general, that the world is knowable, that however feeble our current efforts, it is possible to improve the reliability of our knowledge of the world and even to recognize improvements in that knowledge.

Let us look next at an interchange between Selbourne (1980), a trenchant critic of the subjective approach, and Samuel (1980), a spokes-

person for people's history. Selbourne attacked the British History Workshop group for what he calls their "hallucination of direct encounter with the past, based upon a *resurrectionary* historical mode, itself dependent upon the archival re-peopling of times past" (Selbourne, 1980: 151). Selbourne objects not only to the subjectivity of the evidence – individual testimony – but also to the lack of generalization and explanation that the focus on extreme detail, whether of work or private life, produces. Indeed, many of the articles in the *History Workshop Journal* come close to antiquarian piling up of facts about lives.

In a response to Selbourne, Samuel writes,

It is fair criticism to say that History Workshop has been longer on description than on analysis, or more properly, that it has often tried to embody analysis in description and to collapse the barriers between them. It is also true that we have shown a preference for implicit reasoning, and have allowed little space for analytic abstraction or methodological statements of intent. But we have never pursued description as an end in itself. . . . [Further,] any historical work imposes a false unity on its subject in the very processes by which that subject is defined, whether the framework is provided by laws of development, chronological epochs, or spatial bounds. The themes, whether descriptive or analytical or both, are selected by the historians, and so too are the exclusions which are inevitable both in theoretical and empirical work. Periodisation, however convincing, is always arbitrary; detail, however 'immediate,' is necessarily partial; while the choice of problematic is inevitably trimmed, in greater or less degree, to what the material, or the frame of reference will yield. However elegant the theoretical model, or easy the narrative flow, the totality is, in the final analysis, constructed. . . . [Thus,] there is some justification for Selbourne's charge that the historical representation appears as though it were reality itself, but the explanation is to be sought in the conditions of historical production rather than in [the] alleged naivety [Samuel, 1980: 175].

We could all disagree profoundly with some aspects of Samuel's defense. Nevertheless, his acknowledgment that people's history is constructed by historians – by their questions, their sources, their choices, even by their models or concepts – is far from a celebration of subjectivity. Indeed, it comes close to a validation of social scientific approaches that make these factors explicit.

In his introductory essays on people's history for a volume of collected essays with that title, Samuel (1981a) explicitly gives a social scientific cast to the use of autobiographies. He remarks that

historians are certainly in the habit of using [autobiographies] as though they constituted an unmediated, spontaneous testimony. It is valuable that we should be asked to consider them as exercises in memory, and to inquire

into the invisible conventions they obey. But it does not follow from this . . . that this is the only use to which they can be put, and that historians should not have access to them for the sake of quite other contexts: the reconstruction of a narrative sequence, say, or the corroboration, in subjective experience, of some generalized statement about class relations, family life, or domestic economy [Samuel, 1981b: xlviii].

Indeed, many of us have used autobiographical accounts or fragments of life stories in just this way, and it is perfectly consistent with a systematic, generalizing social scientific approach (for examples, see Hareven, 1982; Moch, 1983).

French Experience in Oral History

Let us turn now to the French debate and practice of history using oral sources and autobiographical accounts, here exemplifying a *genre* of people's history. Jean Peneff, a French sociologist, collected life stories of union militants from Nantes, a large port city in western France. In a report on this study, he explicitly rejects any argument that the "biographical approach" be treated as a methodological revolution or key to deep new understanding. Rather, he sees his biographical evidence as one source, among others: "a document to contextualize and judge, as one must, all evidence." Nevertheless, he seeks in the oral sources both "complexity and an understanding of people's attitudes toward that complexity."

Peneff warns against uncritical use of the autobiographical accounts of ordinary people's lives that have been published in France in the last ten years or so. He believes there is a systematic class variation in individuals' willingness and ability to speak at length about themselves and to accept the notion that their existence is an interesting topic for others. A consequence of this phenomenon is that a large proportion of volunteered autobiographical accounts are success stories about upward mobility. Their use by historians or sociologists, he concludes, is compromised and illegitimate (Peneff, 1979: 56-57).

For his study, Peneff sought out life-long members of the working class, men whose social position and background could be verified, members of a limited set of unions in one place. The interviews did not attempt to force uniform accounts but allowed the interviewees a good deal of autonomy in organizing their responses; at the same time, interviewers were ready to probe for further information or detail in order to clarify points. The goal was to understand the characteristics of militance and how it varied among the major unions. Peneff's findings,

as reported, do not join his questions. The interviewees spoke little about their family background or other subjects, even politics itself. Instead, they gave honor of place to their work lives and to the union struggles that were so central to their lives. Despite its sociological provenance and its methodological caution, the study is unfocused, ready to let the interviewees set the terms of the problem. It is nontheoretical; its findings sound more like traditional historical narrative than like social science.

Another French project, a collaborative effort led by Yves Lequin, produced oral sources as well as collected more common social historical ones such as censuses, police reports, and economic statistics collected by the state. Its overarching mission was an exploration of the interwar period for clues about the conversion to communism, after World War II, of French and Italian workers and their unions. The situation was compared in four southern French cities and in a suburb of Turin, Italy. All were heavy industrial cities, like Nantes. The goal of the study, as Lequin puts it, was to "read, through events, the concrete expression of a social group in a precise and limited situation" (Lequin, 1980: 4).

The historians were not prepared for what they found, coming primarily as they did from study of nineteenth-century working-class formation. The period was characterized by lack of struggle, apparent passivity. Elderly interviewees lingered on family life, the sociability of their residential community, small-scale concrete details about work, and only occasionally local struggles. These French workers seldom remarked on the general political context and were in fact ill-informed on the politics of the period they had lived through. Only four out of fifty life stories mentioned the Popular Front spontaneously. Lequin's laconic conclusion is that "questions about everyday life and those about global issues in no way promote reflection on each other very well" (Lequin, 1980: 195). (He attributes this to an "atomization of political consciousness" due to the tendency of modern industrialism to atomize individuals.) The problem, however, is that his nondirective interviews do not permit him to draw these conclusions. People may not have mentioned some of these matters because they were not asked, or because they were old and had forgotten, as much as because they lacked political consciousness.

The background to the study offered by Lequin and Métral, one of his collaborators, is instructive in this regard. They remark that the part of the study based on oral sources was launched "largely by chance, with no preliminary definition of the specific object of the study or method" (Lequin and Métral, 1980: 149). The project group had already presented its early findings when its members underwent a kind of conversion experience at an academic conference at the opening of the Ecomuseum of Le Creusot, an industrial city in the region they had studied. They were impressed by the valuable information and insights provided in the "sudden

explosion" of direct testimony by worker participants in the conference. Deciding to seek out similar authentic reports, the Lyons historians promptly expanded their data collection from "numbers and the computer to microphone and the interview" (Lequin and Métral, 1980: 150). They also shifted consciously to an effort to understand subjective experience—what they call individual memory, common memory, and collective memory. (This new direction, Lequin and Métral remind us, did not involve abandoning other documentation.)

Although the analysis and writing of reports is still in progress, early reports show that the new focus entailed some costs. In an examination of the meaning of subjectivity, for example, Lequin and Métral seem to retreat to interesting stories—how one man got his nickname, how apprenticeship was an initiation—but avoid addressing their basic problematic. Is this another case of misplaced faith in the immediacy of life history and its potential usefulness for explanation?

Dominique Aron-Schnapper and Daniele Hanet insist that the expectation of seizing the *vécu* (the essence of living) and of finding it in its totality through the oral source is a double illusion (Aron-Schnapper and Hanet, 1980: 193). At a roundtable discussion with Lequin and others, Aron-Schnapper commented that the "statements of interviewees are no more 'pure' facts than are written documents. . . . [T]here is no fundamental difference in how we use them; oral sources must be criticized as written ones always have been." Lequin agreed that from the point of view of interpretation, the two types of document may be the same, but added that there is a fundamental difference between oral sources collected by the historian and other documents: the oral sources are partly the historian's own product. Because of this, he continued, "We've taken the risk, in using oral sources, of never achieving more than indirect representations."

Philippe Joutard, an enthusiast for oral sources, here joined the discussion to make a virtue out of risk. The possible "error" he felt, must be understood: "Oral history leaves institutions far behind; it is a bit the history of the irrational, a discourse of the least ideological type, the least controllable." (All roundtable quotations taken from *Problèmes de méthode en histoire orale*, 1980). Joutard thus again moved the discussion back to acceptance, and even glorification of the subjective nature of the data produced, to a focus on the individual.

It is not clear that Peneff and Lequin and their colleagues would agree with this extreme position; nevertheless, they have fallen into some of the traps of uncritical use of oral sources they have used in nondirective ways. They asked few, and sweeping questions. They started by conceptualizing their outcome as narrative, but when no narrative was possible, they focused on the most individual and subjective factors: motivations,

feelings, sense of complexity, political consciousness. Their historical interpretations are *ad hoc* and, in the end, flat. They permitted too many factors to vary in uncontrolled ways: place, union, age, kind of work, time, background. The number of biographies collected was very small and offers little hope that any representativeness can be attributed to findings based on them. The goals of the studies are descriptive: the findings are unyielding to analysis, even linguistic or cognitive analysis. The questions asked were neither focused nor specified adequately to get useful, as opposed to interesting, answers. There is an irony here in that even Joutard, with his ringing defense of subjectivity, has elsewhere admitted to conceptual and practical difficulties in the analysis of oral sources, difficulties he has apparently not overcome, by his own account (Joutard, 1980: 181).

Social Scientific Oral History and Biography

A social scientific and systematic use of autobiographical studies that goes beyond illustration must try to overcome subjectivity by conceptual and theoretical means. A French sociologist-historian team who have taken steps in this direction are Bertaux and Bertaux-Wiame (1981). They are enthusiasts, and Bertaux has written some hyperbolic exhortations for the biographical approach, as he calls it. Bertaux-Wiame has on occasion surrendered to a "resurrectionary" faith also, as when she refused to analyze certain interviews because she could "say nothing better than my interviewees" (quoted in Aron-Schnapper and Hanet, 1980: 194). Let us, however, look at their practice rather than their program.

Bertaux and Bertaux-Wiame have done a fine study of Parisian bakers. It demonstrates how the common form of recruitment of small shopowner-bakers contributed to their ability to fight off the postwar push toward industrialization of the Parisian bread industry. Their question is general: Why did breadbaking not industrialize? Their focus is specific: How do people become bakers and invested in keeping the industry artisanal? The research method was long interviews, or life histories. These revealed that most owner-bakers in the postwar period had entered the business by contracting a long-term debt to the former owner of the shop that they were obliged to work off. Thus they were committed in an old-fashioned way (a kind of indenture) to keeping the industry old-fashioned.

The ingenuity of artisanal bakers in protecting their interests led to the invention of the "baguette," the small crusty loaf Parisians have come to love so well: a loaf that gets stale in three hours! The bakers thus perpetuated—or at least guaranteed for a time—the need for small-scale, hard-working, long-working bakers—themselves (Bertaux and Bertaux-Wiame, 1981). Although oral sources were central to this study but possi-

bly not indispensable (as the Bertaux claim) conceptualization, questions, and interviews were directive and focused on relationships between and among individuals in the group rather than on individuals' senses of what had happened.

Bertaux-Wiame's study of migration, using life stories, offers another example of successful use of oral sources. As a historian, she started with a distrust for ahistorical demographic categories and a desire to permit her analytic categories to grow out of people's lived experiences. At the same time, however, she was sociologist enough to insist that the failure of previous migration studies lay in their missing the "*social relations* which lie behind emigration, or, to be precise, behind the different currents of emigration" (Bertaux-Wiame, 1981: 251). Her solution was to seek out reports of migrants; not about their motives, but about the people involved in their migration and the steps that brought them to Paris.

To find her population, Bertaux-Wiame went first to the various friendly associations of migrants—the Amicales of the Auvergnats, the Bretons, the Creusois—in Paris. She found that they were composed primarily of men who had succeeded in Paris. They had little to tell her of the mass of migrants, and they were even unwilling to admit that there were others than themselves. She went to old people's homes and clubs, where she primarily found old women. It was these women's accounts of themselves and their male kin which gave her the clues she needed. Gone was the solitary male migrant of the Amicale or romantic novel. Rather, migrants, she found, "are always located in some network of social relations which both guides and sustains them, provides them with opportunities and protects them from threatening risks—in short, which controls them" (Bertaux-Wiame, 1981: 253). As she went on to men, with questions shaped by the knowledge gained from the women migrants, Bertaux-Wiame was told a story with an outcome varied by gender: "While men move through the family to find work, women move through job networks to find a family" (1981: 256). For men, then, family sponsorship was salient to migration to find a job; for women, family-sponsored migration led them to an urban job and a new marriage market.

Her study thus illuminates not only the social meaning of migration but gender differences in this social meaning and other relationships. Another concrete finding is that "there was more in common between the life paths and even the social values of 'proletarians' [people without property] of different regions, than between migrants from the same region but with different resources." Class, not region, was the chief shaper of migration and its outcomes (1981: 263). Bertaux-Wiame's evaluation of her own work brings us back to the question of social science history and subjective evidence. Since she was seeking out social relations (an indirect construct of the facts that historically produced migration) she

rejected conventional surveys that tend to ask about motives. She turned to the biographical approach using life stories, then, not to find complexity, consciousness, motive, or unmediated authenticity, but "to look at actual decisions and actions, and perceive behind these practices the network of social relations which allowed them to take place" (1981: 262).

In her concrete questions, Bertaux-Wiame's practice of the biographical approach moves decisively away from the subjectivity some other exponents of people's history aspire to tap, toward a sociological conceptualization of social relationships as reflected in behavior. She builds from the life stories a record of patterns of behavior in a limited set of decisions and actions; from the patterns, she derives her interpretation, which does not speak directly about individual attitudes but about relationships among people. Bertaux-Wiame's study offers a simple lesson for the social science historian and the people's historian. There can be, and is, a social scientific people's history.

How To, and Why?

This history can proceed in several ways. It can start with theoretical questions and use individual biography in controlled and indirect ways. Or it can do similar analysis of collective biography based on anonymous, incomplete life histories. Both of these methods must focus on social relationships whether the intimate familial ones, such as kin networks or household structure, or public ones, such as political power and the organization of production. Social science, as the study of people in groups, has little to contribute to studies that seek to explain individual states using subjective, internal evidence, or that conversely call on such states to explain collective behavior.

All things considered, we can probably agree with T.K. Rabb's (1981) assessment of the current multiplication of historical approaches as a "flight from materialism" toward feelings, attitudes, unique symbolic acts (Rabb, 1981: 321). Historians who find their problems and questions in these areas are often turning away from social scientific method. But that turning away from social structure and social relationships to subjective accounts or mentalities involves a fundamental error of judgment. To attribute a common mental state to a whole class or an entire people is to ignore their individuality and the very real differences in people's ideas, as well as in their behavior. Jacques Le Goff's brisk dismissal of any tendency to turn away from historical rationality is worth repeating here: "If it is a question of reopening the way to irrationality, I say no. The rationality of history may be *a posteriori*, hazardous, fragmentary, diverse, but it exists. If this were not the case I personally would prefer to write

detective stories" (Le Goff, 1983: 28).

The genius of social science history is twofold. First, its central method—collective biography of one kind or another—preserves individual variability while identifying dominant social patterns. Second, its focus on social relationships rather than psychological states remains our surest guarantee of reconstructing how ordinary people of the past lived out their days and made the choices that cumulate into history. Social science history, properly conceived, is the ultimate people's history.

Notes

1. Discussion in the American social scientific community parallel to the French debate in *Annales* occurred at about the same time; it can be traced to several publications of the Social Science Research Council: Blumer (1949, first published in 1939); Allport (1942); Gottschalk, Kluckhohn, and Angell (1945); Committee on Historiography (1954).

2. See Stone (1979), Hobsbawm (1980), Abrams (1980), and Le Goff (1983).

3. Compare Floud (1982) and Abrams (1982: 318, 327–33). I had not read Abrams' book when I wrote the oral version of this article in November 1982; his choice of quotes and parts of his argument are very similar to mine.

References

- Abrams, P. *Historical Sociology* (Ithaca, N.Y., 1982).
- . "History, Sociology, Historical Sociology," *Past and Present*, 87 (May 1980).
- Allport, G. W. *The Use of Personal Documents in Psychological Science*. Social Science Research Council *Bulletin* 49 (New York, 1942).
- Aron-Schnapper, D., and D. Hanet. "D'Herodote au magnetophone: sources orales et archives orales," *Annales: Economies, Sociétés, Civilisations*, 35 (January–February 1980).
- Bertaux, D., and I. Bertaux-Wiame. "Life Stories in the Baker's Trade," in D. Bertaux, ed., *Biography and Society: The Life History Approach in the Social Sciences* (Beverly Hills, Calif., 1981).
- Bertaux-Wiame, I. "The Life History Approach to the Study of Internal Migration," in D. Bertaux, ed., *Biography and Society: The Life History Approach in the Social Sciences* (Beverly Hills, Calif., 1981).
- Bloch, M., and L. Febvre. "A nos lecteurs," introduction to *Annales d'histoire économique et sociale* (January 1929).
- Blumer, H. *Critiques of Research in the Social Sciences: I, An Appraisal of Thomas and Znaniecki's "The Polish Peasant in Europe and America."* Social Science Research Council *Bulletin* 44 (New York, 1949).
- Burguière, A. "The Fate of the History of Mentalités in the *Annales*," *Comparative Studies in Society and History*, 24 (July 1982).

- Committee on Historiography. *The Social Sciences in Historical Study* (New York, 1954).
- Febvre, L. "La sensibilité et l'histoire. Comment reconstituer la vie affective d'autrefois?" *Annales d'histoire sociale*, III (January-June 1941a).
- _____. "La Société féodale: une synthèse critique," *Annales d'histoire sociale*, III (July-December 1941b).
- _____. *La Terre et l'évolution humaine* (Paris, 1922).
- Floud, R. "Quantitative History and People's History. Two Methods in Conflict?" Prepared for an International Assessment of Quantitative History Conference, Washington, D.C., 1982.
- Gottschalk, L., C. Kluckhohn, and R. Angell. *The Use of Personal Documents in History, Anthropology and Sociology*. Social Science Research Council Bulletin 53 (New York, 1945).
- Hareven, T. K. *Family Time and Industrial Time* (Cambridge, 1982).
- Hobsbawm, E. J. "The Revival of Narrative: Some Comments," *Past and Present*, 86 (February 1980).
- Joutard, P. "Un projet régional de recherche sur les ethnotextes," *Annales: Economies, Sociétés, Civilisations*, 35 (January-February 1980).
- Le Goff, J. "Past and Present: Later History," *Past and Present*, 100 (August 1983).
- Lequin, Y. "Compte rendu de fin d'étude d'une recherche sur structures sociales et représentations collectives: quatre communautés ouvrières de la seconde industrialisation," *Bulletin du Centre Pierre Leon d'histoire économique et sociale de la région lyonnaise*, 4 (1980).
- Lequin, Y., and J. Métral. "A la recherche d'une mémoire collective: les métallurgistes retraités de Givors," *Annales: Economies, Sociétés, Civilisations*, 35 (January-February 1980).
- Moch, L.P. *Paths to the City: Regional Migration in Nineteenth-Century France* (Beverly Hills, Calif., 1983).
- Peneff, J. "Autobiographies de militants ouvriers," *Revue française de science politique*, 29 (February 1979).
- Portelli, A. "The Peculiarities of Oral History," *History Workshop*, 12 (1981).
- Problèmes de méthode en histoire orale: Table Ronde, 20 juin 1980* (Paris, 1981).
- Rabb, T.K. "Coherence, Synthesis and Quality in History," *Journal of Interdisciplinary History*, XII (Autumn 1981).
- Samuel, R. "People's History," pp. xi-xxxix in R. Samuel, ed., *People's History and Socialist Theory* (London, 1981a).
- _____. "History and Theory," pp. xl-lvi in R. Samuel, ed., *People's History and Socialist Theory* (London, 1981b).
- _____. "On the Methods of History Workshop: A Reply," *History Workshop*, 10 (1980).
- Selbourne, D. "Critique: On the Methods of the History Workshop," *History Workshop*, 9 (1980).
- Social Science History*, 1 (Fall 1976). Editors' Foreword and Constitution.
- Stone, L. "The Revival of Narrative: Reflections on a New Old History," *Past and Present*, 85 (November 1979).
- Thompson, P. *The Voice of the Past: Oral History* (New York, 1978).

Between Social Scientists: Responses to Louise A. Tilly

Paul Thompson, Luisa Passerini,
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I wholeheartedly agree with Louise Tilly that the work of Daniel Bertaux and Isabelle Bertaux-Wiame provides an admirable model for oral historians. My quarrel is not with the general direction in which she urges us, so much as with the gross simplifications which she offers in support of her argument.

My first objection is to the overall characterization of people's historians and oral historians as a single, undifferentiated block of "collective behavior" and attitudes. The debates which she cites between Selbourne and Samuel, or others at the conference from which *People's History and Socialist Theory* emerged, show sharp disagreements among History Workshop followers on the role of theory and politics for historians. The hundreds who attend History Workshop regularly include social scientists as well as historians, and amateur enthusiasts as well as professional academics. There is a similar diversity among oral historians, who range from academic sociologists and historians to adult literacy teachers and geriatric social workers. There are also striking differences in the national contexts in which oral history has grown. By seeking to generalize about the entire international oral history movement from a handful of examples, all French (the more so given the relatively recent development of oral history in France), Louise Tilly abandons her own standards of evidence.

It would have been more helpful, I would suggest, to have searched more widely and to have measured her arguments against some of the best examples of recent oral history: to have provided a *critique* of the strengths and weaknesses of, for example, Ronald Fraser's *Blood of Spain* or Jerry White's *Rothschild Buildings* as contributions to political history or urban history respectively. I do not know how selective Louise Tilly's bibliography was intended to be, but I am puzzled that there are no citations of any papers published in the proceedings of the four international European oral history conferences, or from the volumes of the *International Journal of Oral History* or our own *Oral History*. My

own *The Voice of the Past* is the only English oral history publication quoted.

I am quoted, moreover, in a way which is highly misleading, for *The Voice of the Past* is not a primer for anti-scientific history. It contains long discussions of questions such as the verification of evidence, sampling, and the tensions between individual experience and generalization in analysis. I have since taken some of these points further in my contribution to Bertaux's *Biography and Society*, "Life Histories and the Analysis of Social Change" (pp. 289-306). I find Louise Tilly's approach in her 1979 article on "Individual Lives and Family Strategies in the French Proletariat" (*Journal of Family History*, IV) has many points in common with my own. Moreover, I intended books like *The Edwardians* and *Living the Fishing* to be contributions to both sociology and history. After all, I have worked in a sociology department for the last twenty years.

Having said all this about myself, I would want to argue very strongly against the notion of there being only one way forward for oral history. The very diversity of the movement is to my mind a great part of its strength. The constant reminders that historical thinking is part of political thinking, that human experience is lived through individual lives, that there are few facts which are so objective that they cannot be looked at from another perspective, that the very way in which people speak and tell their life stories is shaped by and conveys social consciousness—all these are crucial insights which tend to be swept under the carpet in much social science and history, but are highlighted in oral history work. Some oral historians, quite rightly, have chosen to focus on one problem rather than another. The silences in the stories of those who have lived through fascism, for example, have been a matter of debate among Italian historians, in direct relation to the general debate about the degree of acceptance of fascism by the Italian working class; this has not been the case in Britain, where there is no comparable issue at stake. More broadly, there clearly is an important role for oral history work with no academic focus: for local reminiscence pamphlets and exhibitions and tapes, for helping old people to take an interest and pride in life, and for teaching younger people to read. Community work may not be part of the program of "social science history," but it is of the oral history movement—and I think we are fortunate that it is. I am wholly against a single road: whether my road, the Tilly road, or any other road.

Nevertheless, Louise Tilly raises two very important issues in her discussion, both of which deserve to be pursued. The first is the relationship between political and social commitment and historical work. She has in fact very little to say about this. Is this because the political assumptions of social science historians are so buried that their influence goes unrecognized? One consequence of working with living people as

sources is that oral historians have to face such issues in a direct and personal way: to recognize, for example, the disjuncture between the questions which concern a young feminist women's historian and an older woman informant, and to judge how far the past may be analyzed in terms of present-day thinking.

The second is the place of psychological understanding in social history. Here it seems to me that Louise Tilly's position is dangerously negative. However flat-footed most "psycho-history" may have been so far, it does at least call attention to a fundamental dimension of human *cultural* experience which has been much too neglected by historians. Relationships between parents and children, husbands and wives, sexuality, and the social molding of personality are key historical issues to which oral historians can make major contributions. I agree that we should study individual (subjective) experience within its social context. Still, to leap from that to conclude that we should "focus on social relationships rather than psychological states," as if the two were not intimately bound up with each other, seems to me entirely wrong. It is, I suppose, possible, in studying the nineteenth century to pretend that Freud did not exist, and given the lack of evidence it may not make too much difference. But for the twentieth century—no.

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In the debates over the method and meaning of social history, a cleavage keeps appearing between the advocates of "objectivity" and the supporters of something that has come to be called "subjectivity:" the former claiming that only an objective approach directed toward well defined and measurable objects and behaviors can be said to be scientific; the latter insisting that only the recognition—and indeed the exaltation—of subjectivity allows true objectivity. The relevance of the debate cannot be hidden by the strange and involved tones in which it is conducted; on the contrary, it is its very metaphysical quality that makes it interesting. At times, in fact, the debate turns to arguing whether subjectivity does or does not really exist; that it is good or that it is evil. What a relief! After a long period of empiricism, we are back to a dispute over universals.

The dispute changes subtly as it tackles the problem from different points of view. Divergences always appear enormous, but somebody argues convincingly in favor of positive collaboration, which would mean learning from each other (as does, for instance, Roderick Floud), while others, such as Louise Tilly in the article that appears in this issue, make

a point of fighting error on the other side. (Both authors believe that it is mainly their opponents, *i.e.*, the practitioners of so-called "people's history," who can learn from quantitative history.) An attractive feature of the other side is that—how shall we term them?—people's historians, in spite of being stubborn in their conviction that they are doing something essential, do not pretend to teach much to their opponents, perhaps blinded by enthusiasm for their mission.

Rather curiously, however, the line between the two positions, in favor of objectivity or of subjectivity, cannot really be drawn between two sides. Not only does this line run through each side, but, as is often the case with the struggle between good and evil, dark and light, it divides the individual himself. People's historians, while dealing with the subjective approach, seem to retain some nostalgia for a hard objective frame: such is the meaning of the declaration by Raphael Samuel that construction in history, *i.e.*, the act of choosing and totalizing by the historian, amount to arbitrariness. Quantitative historians, on the other hand, end up praising exquisitely subjective approaches, as does Tilly in the case of the work by Isabelle and Daniel Bertaux. There she juxtaposes two types of subjectivity, when she rules out "complexity, consciousness, motive, or unmediated authenticity" as fit objects of study, in favor of "actual decisions and actions, and . . . the network of social relations." What are the latter but manifestations of subjectivity?

It might be useful at this point to start confronting each other, not on what can be an exhaustive definition of subjectivity, but on what it means for historians to try to study it. I have in mind three major areas—others would doubtless add more—where subjectivity manifests itself in history:

- 1) As a body, not necessarily systematic, of beliefs, myths, reveries, the mixture of "collective representations" to use Emile Durkheim's words, which include all forms of religious experience, but also the "false news" that Marc Bloch recognized as having such an influence in history. Also included in this area are magic as well as the range of mental and emotional attitudes, world-views, and cultural identities embodied in oral and written traditions which act as links between different generations. I am now thinking of the work by Ernesto De Martino on representations of the "end of the world," be they magic beliefs or Marxist concepts or forms of neurosis: all ways of dealing with a major problem in people's lives, all ways of interpreting reality. Can there be a history of the changes and permanence of such subjectivity?
- 2) As the series of choices that individuals and groups such as families make over crucial matters of life. Subjectivity appears here as a pattern of strategies over a life course or several life courses, as

a rationality not to be understood as a totally conscious planning *a priori*, but rather as the result of invention and adjustment to what happens and what is available. Subjectivity manifests itself as the exercise of a limited freedom in carrying out decisions, such as to migrate or not, to control one's fertility and how and to what extent, to buy a house for the family, or to educate one's children – all decisions where individual and collective, conscious and unconscious, external determination and inner decision are interwoven. It is indeed sociologists more than historians who have explored so far the expressions of subjectivity in the life course (Elder), but history too is proving to be a major field for such research (Hareven).

- 3) A further expression of subjectivity is the intertwining of public and private, of personal and political that results in the creation and maintenance of social relations in their many different forms, such as networks, clusters, groups, and subgroups that make up larger entities like nations, classes, parties, and institutions. Here again the actual functioning of social solidarity as well as the development of conflicts on the level of daily life are expressions of an interplay between the objective – *i.e.*, given – and the subjective – *i.e.*, introduced by the individual and his groups. Anthropology has shown the way in this direction of analysis, although some examples are being provided by historians too (for instance, Chaytor and Ross).

In these three areas subjectivity includes not only forms of irrationality, but of rationality, understood as the capacity to cope with reality, and in certain cases as a form of reasoning that might be defined as calculating. Both rational and irrational impinge on the inner domain of individuals as well as on the realm of external facts (such distinctions, like the one between mentalities and behaviors, are relative, but they can still hold for a time as rather widely accepted conventions).

The rational and irrational faces of subjectivity as well as its interior and exterior expressions always show themselves to the historian as mediations. They can never appear as the immediate product of a stream of consciousness or a direct experience. On the contrary, they always exhibit patterns and mechanisms that can be analyzed, compared, and understood. I have grown suspicious of any approach that confines analysis, knowledge, and measure, to exterior quantifiable events and stops short of the interior, the psychic, the spiritual. Thus, materialism is confined to materiality, and whatever pertains to the sphere of subjectivity is treated as impalpable, ineffable, and unknowable.

The reason for this suspicion is that such an attitude, which could be defined as residual positivism, really amounts to a sort of superstition. Superstition does not dare to discuss strange phenomena; it is content

with the fear of black cats crossing a street and hats on the bed as harbingers of bad luck. Historians concerned with subjectivity are not content with its common image as something crazy and unruly, capricious, fallacious, and unforeseeable. The assumption instead is that there might be some pattern, some sense, some history of mental and emotional attitudes, of life strategies and forms of social aggregation; that it is possible to find out their changes, persistence, and permanence, their use and functioning, not only for the Middle Ages, but for our own age.

The subjectivity of the ruling classes has been documented and studied more widely than is the case for the working class and marginal social groups. This is not a sufficient justification for the aura of populism that still lingers among supporters of subjectivity. Subjectivity must be studied exactly in the same way, whether it pertains to the rich or to the poor, to women or to men. The only privilege must be accorded to method and to the recognition of the peculiarities of subjectivity in what is being studied and in who is doing the studying. The procedure is lacunal, the instruments of analysis are yet tentative, the understanding is merely a confused tension, a desire. Fortunately, there is desire in historical research too.

I daresay that at the basis of such desire there is an essential need in social research today. It is not only the quest of the cultural for its right to exist as a realm equal to the economic, or of the qualitative to the quantitative. It is the need of more and more people to find new and larger forms of experience in the symbolic sphere of symbols, which can be shared or understood by other individuals and groups, and to trace their roots in the connection between the present and the past.

References

- Alexander, Sally. *Women, Work, the Family in London in the 1920s and 1930s: Subjectivity and Oral History*, IV Colloque International d'Histoire Orale, Aix-en-Provence, September 24-26, 1982.
- Bloch, Marc. "Réflexions d'un historien sur les fausses nouvelles de la guerre," in *Mélanges historiques* (Paris, 1963).
- Chaytor, Miranda. "Household and Kinship: Ryton in the Late 16th and Early 17th Centuries," *History Workshop*, 10 (Autumn 1980).
- De Martino, Ernesto. *La fine del mondo. Contributo all'analisi delle apocalissi culturali* (Torino, 1977).
- Durkheim, Emile. "De la définition des phénomènes religieux," *L'Année sociologique*, 1897-98, vol. II (Paris, 1899); *Les formes élémentaires de la vie religieuse. Le système totémique en Australie* (Paris, 1942).
- Elder, Glen. "History and the Life Course," in Daniel Bertaux, ed., *Biography and Society* (London, 1981).

- Floud, Roderick. "Quantitative History and People's History: Two Methods in Conflict?," *History Workshop*, 17 (Spring 1984).
- Hareven, Tamara. *Family Time and Industrial Time* (Cambridge, 1982).
- Passerini, Luisa. Letter on the "Debate on Method," *History Workshop*, 11 (Spring 1981).
- Ross, Ellen. "Survival Networks: Women's Neighbourhood Sharing in London before World War One," *History Workshop*, 15 (Spring 1983).
- Samuel, Raphael. "History Workshop Methods," *History Workshop*, 9 (Spring 1980).
- Selbourne, David. "On the Methods of History Workshop," *History Workshop*, 9 (Spring 1980).

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It is always gratifying to see one's work publicly praised. The honor is all the greater when the praise comes from such a serious scholar as Louise Tilly, whose research is well known and held in high esteem among French historians.

I am, however, a bit puzzled by the strong contrast Professor Tilly draws between the work of Daniel Bertaux and myself and that of colleagues we respect and admire, who have done much more than we have to develop oral or people's history. Our embarrassment reaches a peak when their work is slighted, while ours is lauded.

There are two main orientations in using oral sources to develop social history: one looks for "facts," and the other focuses on mentalities. I believe, however, that these approaches complement each other to a considerable degree. When asking about their respective contributions to social-historical knowledge, Professor Tilly raises a fundamental question, but the answer she proposes seems much too trenchant. Personally, I would not dare to affirm that one approach is "scientific" and the other is not. In order to make such claims, one would have to stand on rather firm epistemological ground; one would need to assume a clear and undisputed meaning for such expressions as "the science of history" or "the science of society." Such is not the case, and although I basically agree with the intellectual project called "social science history," I must add that the very practice of collecting evidence from living memories leads one to a certain kind of epistemological relativism. Let me try to develop this point further.

* * *

One may observe, to begin with, that the prevailing feeling among "oral historians" is one of participation in a collective adventure; differences in topic, approach, or style are perceived as individual differences rather than as contrasting orientations.

There are several reasons for this. One is that "oral historians," whether they come from history, sociology, anthropology, political science, or other disciplines, share the common experience of having had their work submitted to harsh criticism by their "more scientific" colleagues. (Notwithstanding my personal orientation toward "facts," I was not spared this criticism.) This shared experience may account for the feeling of solidarity alluded to earlier.

More significant in the long run, however, is that all scholars collecting life stories or similar oral evidence have experienced the interactionist nature of such data: being producers as well as analysts of data, they cannot help developing a relativistic attitude concerning its accuracy. This relativism is easily transferred, rightly or wrongly, to *any* other kind of data and to theories too closely based on them. Thus, one is led to believe more in interpretation than in explanation.

Third, life stories possess a specific and disturbing feature. By their very nature, being at the same time descriptions of *situations* and descriptions of *reactions to situations*, of structures and *praxis*, they induce doubts about the attempts to conceive of social-historical reality either as the result of pure "objective" structures or, conversely, as the product of pure "subjective" human action. Situated actors and social dynamics quickly move to the center of the stage – and who could claim to be able to seize and imprison in concepts such moving and unpredictable entities?

It is nevertheless quite true, as Tilly points out, that there are several ways to use oral sources. Differences in approach result in assigning a different *status* to life stories, for example. Scholars focusing on mentalities, consciousness, and the like will consider them as *texts* and proceed to analyze them as such, using techniques which may be derived from linguistics or hermeneutics. The literal accuracy of life accounts is not their chief interest. It is, however, a central question for those who use life stories as means to gather information about the *social* features of situations, with the expectation that structural characteristics will appear through a series of case studies.

In this latter case, interviewees are considered as informants and the factual meaning of what they say will usually be taken at its face value, until it is questioned by another testimony or other data. This led me once to state boldly that such life stories speak by themselves and that I had nothing to add to them – certainly an imprudent statement, which was cited as indicating a lack of scientific spirit. What I meant to say was that

to comment upon an isolated, fact-oriented, well-told life story was bound to end up in paraphrasing. The analyst should instead focus on what lies *between* the narrated lives: recurrent descriptions or anecdotes should be indicative of the existence of patterns of structural relationships or processes. In this approach, the focus is hardly ever on individual cases but *beyond* them, on hypothetical collective processes.

However, an interest in subjective meanings hidden in life stories may also develop into an objective analysis of, say, shared beliefs or values ("symbolic structures") for a social group, an epoch, or a generation.

Thus the usual dichotomies: objective/subjective, material/mental, scientific/literary become blurred along with the strict associations between their respective poles: objective-material-scientific *versus* subjective-mental-literary.

If, furthermore, one brings into the picture the disruptive presence of *praxis*, which is subjectivity objectifying itself, the reasons behind our epistemological agnosticism begin to appear. They will become clearer through concrete examples drawn from our research.

* * *

Within the past two decades a number of French scholars believed that they had found in structuralism the path to a truly scientific approach. Daniel Bertaux and I were among that number, and this structuralist style of theorizing is probably one of the elements of our work that Louise Tilly found interesting.

Being a structuralist was not very original. What was more unexpected was the choice of a technique, the life story, which had a long-established reputation of being heavily loaded with subjectivity. This choice seemed at odds with our basically materialist orientation. In fact, it was made for contingent reasons, but as it proved valid and quickly yielded interesting results, we stayed with it. Our materialist outlook led us to orient the life stories we were collecting toward accounts of concrete situations, and reactions to these situations, rather than toward meanings, values, or attitudes. However, as such uninvited entities as life goals or life projects kept creeping into our field of vision as stowaways on the life story boat, we began to feel the need to try and move beyond our structuralist framework in order to account for individual *praxis* and collective pressure for or against change. Thus, our initial choice of a method proved to have long-term consequences, although we discovered it only along the way.

Even today most of our work consists in unraveling and documenting the existence of structural relationships and processes which, acting as constraints (and sometimes as resources) upon the situations and

opportunity structures of a given category of people, contribute to the shaping of their life trajectories. It is by finding recurrent patterns throughout a series of life trajectories that we infer the existence of sociostructural processes. These we search for neither at the individual (microsocial) level nor at the societal (macrosocial) one. Instead, we look to an intermediate level, as for instance a branch of production, the situation of young migrants on the markets of labor and housing, or the type of relationships within a family that are bound to arise in a given class situation.

It is at this middle or *mesosocial* level that we have so far discussed most of our findings. This is the level of what Sartre called "les médiations," through which large societal structures become tangible constraints for individuals and families, and through which, conversely, the aggregated practices of isolated actors may eventually influence macrosocial processes. Universal abstractions like class, market, power, or family take specified, concrete forms which differ according to social situations and historical time. Which particular form they take here or there in everyday life is one of the main things we are looking for through our life story interviews.

People tell us about the kind of situations in which they find themselves or have found themselves; they usually understand quite a lot about them. It is out of this "native knowledge," as it might be called, cross-checked and confirmed by other types of sources whenever possible, that we build up a representation of social contexts.

Nevertheless, even if we consciously orient the story-telling toward factual accounts, it does not mean that the life stories we collect are wholly "objective," as if they were made up of a mere succession of "facts." To imagine such a life story is to perceive at once its impossibility. A life story is a narrative, constructed along the way by a person (in a setting of interaction) who at the same time is recalling events and experiences *and* giving them meanings.

The autobiographical discourse cannot help being meaning-oriented. We were at first reluctant to admit this, since we were only interested in "facts" (as defined above, *i.e.*, from a structuralist point of view). However, we soon learned that we could get at facts only through the mediation of the meanings that were conferred on them. This is so because to tell one's life story is not equivalent to the unwinding of a prerecorded tape. To recall—and to forget—is to work on one's past from the point of view of the present.

But life accounts are not pure fiction. They are based on events, scenes, lived experiences. We found it was possible, by taking into account the meaning-construction activity instead of ignoring it, to reach out across the present point of view into the past itself.

Let me give an example. During the course of our research on the

artisanal bakery in France, I did a particular inquiry into the conditions of apprenticeship during the 1920s and 1930s.* For this I interviewed fifteen aging bakers and bakery workers. All of them came from the same broad social background, *i.e.*, poor, rural families. (None was a baker's son.) Each of them had been apprenticed to a small rural baker at age thirteen or fourteen. It could, therefore, be assumed that they had all passed through roughly the same experience. Indeed, they all mentioned (more or less spontaneously) the very long hours of work—twelve to sixteen hours every single day of the week. Each had also experienced quarrels with the master baker, a struggle to catch his "secrets," and poor housing conditions in the attic of the master's house. Still, I soon noticed how strikingly different were the accounts of what I assumed to be the *same* conditions of life, depending on whether the interviewee was still a bakery worker at the time of the interview or whether he had become a self-employed baker along the way.

With old bakery workers, either still working or retired, the feelings of injustice and exploitation that had filled their hearts while an apprentice forty or fifty years earlier were still alive and burning, underlying all their recollections. Subsequent work experiences had only strengthened and crystallized these feelings. Recalling their masters, they would describe them as harsh and brutal, giving examples and bitter descriptions of scenes from their youths. They usually made no attempt to distance themselves from the young apprentice they had been: he was still living inside them.

On the contrary, for those of my interviewees who had eventually become self-employed, and especially for those who had in turn trained apprentices, the recollections of their own apprenticeship took on a very different coloring. While they remembered the harsh conditions of work and life, they always took care to add that "there is no other way to learn the trade." If recalling a quarrel with the master, they would put the blame on themselves, describe laughingly the mistake they had made and how, as a natural reaction, the master had corrected at once the mistake and the person who had done it. It became obvious that they were identifying not with the young apprentice they had once been, but with the master

*Isabelle Bertaux-Wiame, "L'apprentissage dans la boulangerie artisanale entre les deux guerres. Une enquête d'histoire orale" (Paris, Groupe de Sociologie du Travail, offset, 1976). See also Daniel Bertaux and Isabelle Bertaux-Wiame, "The Artisanal Bakery in France. How It Lives and Why It Survives," in Frank Bechhofer and Brian Elliott, eds., *The Petit Bourgeoisie: Comparative Studies of an Uneasy Stratum* (London, 1979); Daniel Bertaux and Isabelle Bertaux-Wiame, "Life Stories in the Baker's Trade," in Daniel Bertaux, ed., *Biography and Society* (Beverly Hills, Calif., 1981); Daniel Bertaux, "Stories as Clues to Sociological Understanding," in Paul Thompson, ed., *Our Common History: The Transformation of Europe* (London and Atlantic Highlands, N.J., 1982).

baker they had become. "It was rough all right; but if one wants to become a master baker some day one has to go through it."

I found several examples of this phenomenon. Take, for instance, the problem of getting up in the very middle of the night, when it is time to begin the work. The baker is used to it, but what about the young boy who, only the month before, was living with his mother and going to primary school? He punches the alarm clock and goes back to sleep. The baker climbs the stairs and wakes him up again, but as soon as he is out, the boy falls back on his pillow, already asleep. The next time the baker comes up it is with a pan of icy water, and . . . here we go!

A seventy-year-old retired bakery worker, recalling such a scene, told me that "afterwards, I was so frightened I almost lost sleep altogether." Fifty-five years later he had not yet forgiven this master for his roughness toward the helpless child he was at the time. The years when he learned the trade were also those when he had his first experiences of exploitation and injustice.

Now take the same scene (it obviously describes a very common practice), as described by a master baker recalling his own apprenticeship. The story is factually similar, but is given a different meaning: "Of course he had to find a way to get me on my feet!". One of the interviewees went on at length explaining how, instead of going to bed early as he "should" have done, he sometimes stayed up "late" (meaning past eight o'clock in the evening) with friends of his own age. "I was getting only what I deserved." Seen in this light, through the eyes of a master baker, exploitation of apprentices and roughing them up are redefined as "long hours" and "learning" according to the norms of the trade and taken for granted. Scenes which one may assume have been lived with repressed anger and resentment are now reconstructed from the master's point of view. While any attempt at resurrecting forgotten feelings would probably prove hopeless, the facts can be brought to life again, provided one probes for them.

* * *

Thus, we cannot reject subjectivity, if only because the native knowledge that we need so desperately is embedded into it. Where else could we find it? But the dialectics between social relationships (in the preceding example, relations of production of the artisanal form of production) and subjectivity does not end here. Native knowledge is not developed for the sake of contemplation, but learned the hard way and synthesized *in view of action*. This leads again to the question of *praxis* (by which we mean

not only occasional participation in social movement, but also the acts of everyday life inasmuch as they imply an effort to transcend one's condition, however modest the means—for instance, teaching one's children to expect a better life and fight for it).

This ability to *act* in a creative way, and not only to enact prescribed roles as structuralism would have it, has to be taken into account if one wants to understand social and cultural change, a large part of which (for example, the sharp drop in natality) cannot be explained away by influences received "from above." But who will tell us how to grasp scientifically such "living objects" that are at the same time embodiments of structures (through the enactment of roles and the internalization of collective values), meaning-constructing persons carrying the weight of their own unique history, and individuals endowed with the miraculous capacity of *praxis*?

One would have to possess not only multiple, but contradictory skills to follow simultaneously all the roads to conceptualization sketched above, which open together in front of anyone who starts collecting life stories. One would have to become sensitive to the meanings hidden in each narrative, while remaining strictly materialist; to focus on one single life story and explore its depths, and to multiply them in order to cross-check findings; to understand lived experience from within, and to examine it objectively from the outside; to see individuals in their social contexts, and social contexts as the result of aggregated practices; to conceive of human beings as free-willing creatures, and to submit them to sociohistorical determinations.

It is impossible for a normal person to assume at once such widely divergent points of view. Still, the challenge can be met collectively, if the various approaches (and the specific skills required by each of them) are taken up separately by various scholars. Conviviality will then become a necessary, if not a sufficient condition for confrontation and possible synthesis.

Thus, it is out of necessity that one finds differing approaches within the field of oral history; diversity does not mean deviation from the scientific model, but complementarity. Each approach works on one part of the mosaic, and it is the totality that will ultimately make sense. Provided the same goal is shared (I would define it as the development of useful knowledge) the plurality of approaches is not a weakness, but an asset of the field.

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"In Mr. James Hopkin," writes Frederick Douglass (*Life and Times*, ch. 6), "the succeeding overseer, we had a different and a better man, as good perhaps as any man could be in the position of slave overseer. Though he sometimes wielded the lash, it was evident that he took no pleasure in it and did it with much reluctance."

"Sources . . . used collectively, compared systematically, and frequently quantitatively analyzed" (to use Dr. Tilly's apt summation) have made it possible for us to know a great deal about slave-whipping. A well-known quantitative analysis (that of Robert Fogel and Stanley D. Engerman in *Time on the Cross*) informs us that each slave was whipped an average of 0.7 times per year: one of those hard facts which we have learned to appreciate greatly because (in Tilly's words again) they reassure us that, indeed, "the world is knowable."

There are other facts of which we can be certain. It is self-evident, for one, that no slave ever was, or could have been, whipped 0.7 times; it is also a fact that, while we can count the strokes, we have no precise way of weighing them. Although we cannot precisely quantify the actual scattering of strokes among individual slaves or the circumstances which determined the actual wielding of the whip, yet this does not mean that weight and distribution do not exist and do not count. While some facts are hard, others are irremediably "soft." However, objective scientific research cannot be founded on ignoring whole categories of events simply because our current equipment is too gross to countenance their shape and texture. It is, on the contrary, the duty of the researcher to deal with these facts as best he can, either by devising adequate techniques or by openly declaring that there are gaps in his/her knowledge.

The very distinction between hard and soft facts is actually often blurred and questionable. Responsible historians have disagreed with Fogel and Engerman's methods and results—leaving us with the feeling that the promised certainty has failed to materialize and what we have is, at best, an approximation or a hypothesis. We are not sure of the objective fact—how many times slaves were whipped; on the other hand, there is a strange hard core to "soft facts," which we can perceive if we consider that we know with absolute certainty what Frederick Douglass remembers thinking about concerning the overseer's feelings while whipping the slaves.

Hard facts and figures also do not protect us from ideology and manipulation. I have another example in mind, which I would like to preface with my favorite joke. It is about an American Indian chief who dispatches scouts to find out how many palefaces are in the stockade. They return and tell him there are 2,016. How do they know there are exactly 2,016? "Easy: the stockade has four sides; there are four sentries on each side, which makes sixteen; inside, there's a whole bunch of people, gotta

be at least 2,000." Let us now turn to one of my favorite history books: Emmanuel Le Roy Ladurie's *Le Carnaval de Romans*. On the second page, we are informed that in 1357 the town of Romans had "about 6,013" inhabitants. This startlingly approximate exact figure is the result of a calculation which Ladurie describes in a footnote at the end of the chapter. His authority is an unpublished thesis, which calculates the population by multiplying the number of taxpayers by five (average number of members per family unit), and adding a 0.67 coefficient to allow for non-taxpayers. However, says Ladurie, "this seemed too high," so he lowers the five to 4.5, while leaving the 0.67 unaltered. He thus obtains a coefficient of 5.17, which he then multiplies by the number of taxpayers.

The combination of an unpublished source with a subjective evaluation of what "seems" the right coefficient is hard to swallow as it is. It becomes harder when we stop to wonder why it should be 4.5 and not, say, 4.58. We discover then that the mathematical process is actually shaped by aesthetic and mythical factors. One aesthetic element is the preference for round figures, which explains the 4.50, or four and a half, coefficient. The mythical one is the belief in the *ipso facto* precision of non-round figures, which is why Ladurie does not tamper with the 0.67. It is all tied together by the fact that 0.67 is actually a round figure in disguise: the decimal translation of the fraction two-thirds. Thus, the rigorously scientific odd figure, 5.17, turns out to be the sum of two impressionistic round figures: four and a half plus two-thirds. We do not know precisely and certainly how many people lived in Romans in 1357 any more than we know how many palefaces were in the stockade.

On the other hand, all that Sitting Bull needed to stage his attack was a rough estimate: 2,016 or 2,050 made little difference. The same applies to Ladurie's research: "it is a matter of orders of greatness anyway," he writes at the end of the footnote. Then why this display of numbers exact to the last digit, to impart a knowledge we don't *really* need anyway? Because the information Ladurie is conveying is not at all about Romans, but about himself. It is a self-presentation (at the beginning of the book) where he lets us know that he is one of those historians who deal with figures and hard facts, a scientist who will countenance no approximation in his results—no matter how approximate his data is.

We may draw two conclusions from the discussion so far. One is, as shown by Frederick Douglass, that subjectivity is a *fact*. What the overseer felt while whipping him, or what he thought the overseer felt, is important enough for Douglass to record it several years later; and in fact it does tell us a great deal about possible forms of the relationship between slaves and the plantation hierarchy. The second conclusion is that historians, like everybody else, are endowed with subjectivity. As the example from Ladurie shows, the best historians project themselves, their self-

perception, their values, in their work even when they pretend they don't (and this pretense is of course part of their subjectivity too).

So, there is no question of "turn[ing] subjectivity into a virtue," as Dr. Tilly criticizes me for allegedly doing. Subjectivity is no more a virtue than breathing is: it is just something inherent in human beings. It is indeed very useful and necessary to study non-breathing bodies; but, to my knowledge, no anatomist would condemn as unscientific his colleagues who chose to concern themselves with breathing ones.

It seems to be generally accepted that oral sources are peculiarly suited for the study of this particular aspect of human beings. I would not recommend that one rely primarily on oral sources if one were trying to reconstruct planters' budgets; I would not (and neither would any of my colleagues) rely only on oral sources for any project whatsoever. But I would not recommend that one rely on planters' diaries or company records if one is trying to find out what it meant to be whipped or what it feels like to be fired. Or to strike back.

This also goes for my other statement with which Tilly finds fault: the one about giving "priority" to what the informant wishes to say and "accepting" the source. I admit my wording is confused and slightly extreme, although the context of the paper amply clarifies what I had in mind. No awkwardness of expression on my side can justify Dr. Tilly's discussing my paper as if I had said "exclusivity" rather than "priority," and as if "accepting" the source could indeed be understood to mean an attitude (Tilly's words) of "radical passivity." What I was trying to describe, what I think the whole text of the paper describes, and what I do in practice anyway, is exactly what Dr. Tilly lauds in the work of Jean Peneff: "allow[ing] interviewees a good deal of autonomy in organizing their responses; at the same time, [being] ready to probe for further information or detail in order to clarify points." That I envision no radical passivity at all could have been assumed from my suggestion that interviews be published with the questions asked by the researcher, rather than as monologues as they often are.

At one level, then, there would seem to be only a verbal misunderstanding between Dr. Tilly and me. However, I am intrigued at the kind of logic that is behind this misunderstanding. In fact, I think it reveals another case of subjectivity showing through a veneer of objectivity.

Let us go back to the question of "accepting" the source and why Dr. Tilly thinks it means that the historian effaces himself altogether. "Accepting" is, in fact, always a necessary part of any relationship and dialogue: even in a love affair, "accepting" the partner does not mean that one is passive, but it is often a requisite for being accepted in turn. I wonder, in fact, how we are going to have the informants accept us, and give us reliable information, if we are so shocked at the idea of accepting

them. In my view, the inter/view is a situation where there are two people inter/acting. Apparently, for Dr. Tilly this is impossible: you can only have one person acting and the other submitting, and we'd better be on the right side of this struggle for power.

This turns up again in the question of giving "priority" to what the sources wish to say. "The social science historian," writes Dr. Tilly, "may well ask how the sources can set the problem and its conceptualization," if we let them talk first, and take what they are and what they feel as our starting point. As is often the case with outside views of oral history, Dr. Tilly seems to lose sight of the fact that in our case the "sources" are human beings. Human beings have—granted, to varying degrees and with varying ability—a tendency to set problems and conceptualize, especially when they are discussing their own lives. That they may conceptualize and set problems in ways that are different from what we expected and planned is indeed an asset: we always stand to learn more from the unexpected than from a repetition of our processes of thought. Hence, incidentally, Peneff's very correct insight that the informants' answers don't match his questions—and his even more laudable practice of *accepting* them anyway and using them to change his own conceptualizations. Indeed, we might easily ask the reverse question: how is the social science historian going to set the problems and conceptualize—about other people's lives, at that—without letting them talk about them first? And are we really sure it would make much more sense to "give priority to what the historian wishes to hear," rather than to "what the source wishes to tell?"

As I already noted, however, here again is not a question of who talks *first*, but of who talks. Consistent with the approach that envisions only one active partner in the interview, Dr. Tilly takes my "priority" for exclusivity; my respect for the source's need to express him/herself for "radical passivity." Only one person talks, only one person conceptualizes, only one person sets the problems. This *may* be applicable when dealing with archival sources. It is not just disrespectful, but wrong methodologically, when dealing with people.

Franz Boas taught anthropologists to "beware of intelligent Indians" who "may have formed a theory," *i.e.*, conceptualized, about what they are talking about. His disciple Margaret Mead warned field workers that we are not in the field to tell the informant our theories and concepts, but only to listen. We will do our conceptualizing at home, in our easy chair. Dennis Tedlock, from whom I take both quotes, goes on to say that, together, they mean that we are supposed "to play dumb with smart informants and play smart with dumb ones." In other words: in order to avoid "muddling" the supposedly objective data flowing from the informants' mouth, we are requested to reject their subjectivity and hide away our own. Such a natural, and in many ways interesting, fact as

subjectivity becomes something that must be suppressed at all costs—a blemish, a danger, a vice. Which is implicit in Dr. Tilly's choice of words: one can "turn into a virtue" only something that is the opposite of one. In fact, isn't subjectivity the apparent reverse of the scientists' one and only idol, to be worshipped blindly right or wrong—Objectivity? I would submit that it is not; that the only true objectivity is the one that is able to include subjectivity in its horizon. But I am willing to admit that the view of subjectivity as a positive evil to be expelled from the garden of science is also plausible and respectable—albeit, like most distinctions between virtue and evil, a highly subjective one.

The same implicit appeal to the reader's subjective emotional responses and unacknowledged value system returns also in what is the most relevant and problematic question raised in Dr. Tilly's essay: "whether *aggregating* subjective reports can ever produce a group or class subjectivity." (my italics) It is indeed a serious question, although I believe readers might react differently if Dr. Tilly were to write "comparing" instead of "aggregating." It is true that life histories (and individual human beings) are never one hundred percent comparable, but neither are whip strokes (and census data). Yet, with appropriate techniques and limited expectations, we may do some useful work. Though we have no general formula applicable to all cases and situations, I believe we possess some techniques that allow us to form trans-individual concepts out of individual testimonies. I also believe that we ought to strive for better techniques, rather than deny that the thing can be done at all.

Let me start with an example. Dr. Tilly generalizes about a whole field and discipline, that of oral history, by looking at the work of a few individuals who are neither "average" nor can be objectively proved to be "representative." Yet her descriptions of her object have a high degree of accuracy. How does she manage this feat which, according to her own paper, would seem impossible? I suppose she went through all the literature in the field; found recurrent themes and ideas; traced the authors who originated them or who appeared to express them more fully; took these authors' writings and analyzed them; and reassembled her analytic data into a set of conclusions. What she used was a criterion of *quality* (in sociological terms, of course), which allowed her to credit a few individual texts with a more general relevance (though *how much* more I guess we'd all be at a loss to quantify. Soft facts, again).

There is one respectable academic discipline that uses this method all the time, because it deals with quality and with extremely idiosyncratic, individual testimonies: literary history, literary theory, and literary criticism. Although each work of art is supposed to be incomparable with any other, literary scholars manage to discover enough common, shared traits to allow them to speak of *genre*, style, school, period, and traditions.

Tzvetan Todorov says that each work of art creates its own *genre* – which doesn't prevent him from outlining a general theory of prose, and a definition and theory of a specific literary *genre* ("fantastic" literature). All these constructs (Todorov's emphatically so) are debatable and open to revision, but so are the number of whip strokes and the number of palefaces in the stockade. Or the interpretation of voting patterns in New Jersey.

Literary scholarship builds its samples through a procedure which is different from that of sociology or statistics. Rather than building an advance, ideal sample, based on statistical data (so many novels, so many sonnets, so many plays; so many by men, so many by women; and so on according to age, trade, and geographical origin), literary scholars are expected to go through the whole universe (*partly* relying on previous surveys, of course), and then build their sample *a posteriori*, relying on quality and on internal characters of the text (the fact that one cannot actually read all the books, or interview all the people who live in a given town is what makes these disciplines permanent works in progress). The interplay of *genre* and discourse, tradition and innovation, *clichés* and experiments in a given text reveals a great deal about the text itself, about the culture which it helps shape, and about their relationship.

Now, one thing that I am sure of is that oral history (but perhaps all of history) does not deal directly with people and events, but rather with verbal constructs, stories, and texts. Working on that level, recognizing the verbal nature of our material, we can use many techniques that already exist in order to analyze and compare texts, rather than people. We may thus go on and construct transtextual categories that take us, if not all the way to the mythical "collective" consciousness, at least a few steps beyond the mere individual.

Another important fact is that literary scholarship does not normally deal with the *average* but foregrounds (or at least includes) the exceptional, the unique. I believe that this is due to the fact that literary scholarship openly deals not just with *fact*, but with *meaning* as well. Now, history deals with meaning too: however, the cult of objectivity leads many historians to downplay this aspect, in the false belief perhaps that "facts" or "figures" speak for themselves.

They don't. Let us go back to the 0.7 lashes in *Time on the Cross*. Once we have this data, what do we do with it – what does it *mean*? Fogel and Engerman seem to think that it means that whipping was a very secondary problem in the slave experience; it rarely occurred, certainly not enough to make a great deal of difference. This of course is consistent with what they are trying to prove: that slavery wasn't so bad after all, at least when compared with the experience of other working classes of the time. However, as the Italian critic Beniamino Placido immediately pointed out,

the difference between slaves and factory workers was not in *how many* times they were whipped, but in how many times they *could* be whipped; it was not a matter of averages, but of *possibilities*. Here the question of distribution becomes relevant again: if ninety-nine slaves were never whipped, and one received seventy lashes, the latter's exceptional experience was also a message that weighed on the expectations and behavior of all the others. The factory worker knew that he could never be whipped, no matter what he did; the slave, even if he never was, knew that it could happen to him, and he'd better be careful. So, if we had the biographies of all those 100 slaves, the story of the one who got whipped could not be quantified and averaged out with all the others, because it is, in fact, a shaping factor in the lives of those who didn't have this experience. To put it briefly, it is an objective part of their subjectivities. (We could take a less gruesome example. Very few American boys in the Gilded Age grew up to become millionaires or presidents, but *no* Italian boys did. This difference—slight in actuality, infinite in possibility—has a great deal of influence on the people's histories of these two countries).

What we learn from literary scholarship is that an exceptional work is not representative of the average, run-of-the-mill book production of a given time and place. But it represents a possibility; it opens a road, it points a direction; it sets a yardstick against which everything else will be measured. The same applies—with necessary adaptations—to oral testimony. More and more, I have grown to view oral history as providing us not so much a pattern of common experience as one of shared possibilities, real or imaginary. That they are hard to unify under any precise, rigorous pattern, suggests the different shapes of destiny that are at any given time held before people's eyes and that find a place in their minds. It also helps us visualize the pattern of society not so much as a grid of geometric squares (as it is often—for many respectable and useful reasons—represented), but rather as a mosaic in which every piece, though it may resemble the others, yet differs slightly from all of them. I am convinced, incidentally, that this image is much closer to objective reality.

I am sure that this does not exhaust the range of what can be done with life histories and oral sources: I only submit it as an example, to show that we are not utterly helpless when we work with this type of material and with the problems it raises. I would not urge all historians to learn literary criticism, any more than I would urge literary scholars to learn history. (Which I do all the time. In fact, might it not help historians to be better equipped to deal with words, which is what they use most of the time, and what they write in all their works?). History can benefit from a whole range of other sciences, and all other sciences can benefit from history. That is why I really don't know what to make of Dr. Tilly's reiterated charge that my work "fall[s] short of social science history."

In a way, it's as if she were criticizing Carl Lewis for falling short of the world high jump record. That's not what he's dealing with; he's dealing with the broad jump, and is pretty good at it. Why should he have to measure up to the standards of some other discipline? As Rhett Butler said to Scarlett O'Hara, "frankly, my dear . . ." The world is richer for having both a high and a broad jump record: taken together, they point to different directions that are available for human talent and effort. I don't believe that everybody needs to play the same game. Therefore, though I gladly and wholeheartedly support Dr. Tilly's basic contention – that there is no contradiction between social science history and people's history (whatever *that* is) – yet I am in utter disagreement with her final statement, that "social science history . . . is the ultimate people's history." There is no such thing as the ultimate anything. True, as Dr. Tilly says, "the world is knowable" – but it doesn't end when we know it, it keeps spinning, and presenting different faces to the sun. We don't need any ultimate history, but a lot of different and partial and temporary histories, each redefining and undermining the others, each "falling short" of the others and therefore needing them. Some brands of history may look to others like treachery and betrayal. Yet, as someone said (I believe at the start of a revolution), if this be treason, let us make the most of it.

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Louise A. Tilly's Response to Thompson, Passerini, Bertaux-Wiame, and Portelli, with a Concluding Comment by Ronald J. Grele

How disappointing that a discussion about oral history should involve so much talking, and so little listening! The paper that began the discussion insisted on the common ground of social science history and people's history. Then it made the distinction:

To the extent that people's historians stress subjectivity, individual experience, and evidence internal to individuals, and hence neglect both theory and analysis, they do fall short of social science. To the extent that they reject collective biography for an individual focus, they diminish their own capacity to understand variation and resort instead to descriptive detail or ideal types.

The point, then, was not that the search for individual testimonies was inferior, useless, or wrong-headed; far from it. That paper insisted on the value of assembling evidence on individual experiences, indeed argued that a certain way of assembling the evidence constituted the central advantage of social science history. The point was that making the reconstitution of individual consciousness the central activity of historical analysis places history on weak ground, and cuts it off from the solid and accessible terrain of collective biography.

What do our commentators have to say about that choice?

Their chief theme is that there is no one best approach to history in general, or oral history specifically. What is needed is diversity and methodological openness. I disagree; a *systematic* history offers more promise for understanding the past, our goal as historians.

I appreciate Isabelle Bertaux-Wiame's interpretation of the origins of solidarity among oral historians as the result of harsh criticism directed at them. Nevertheless, I insist that there is a very large difference between her goals and methods (and those of Thompson, and to a large degree, Passerini) and those of Portelli. Bertaux-Wiame speaks of seeking patterns throughout a series of life trajectories, not at the individual or societal, but at the middle level. That is consonant with my position. Paul Thompson insists that his *Voices of the Past* is not a primer of anti-scientific history,

and indeed, I never wrote that it was. In fact, I find that his effort to make oral history systematic and sociological congenial also. Ironically, on the one point where he finds me "dangerously negative" (*i.e.*, my warning about the difficulty and dangers of studying individual psychological states in the past), Thompson offers potential research topics, like the molding of personality or the relationships between husbands and wives, that are not individual but social and relational, and hence within my definition rather than making his point.

With delicious ironic relief, Luisa Passerini welcomes the return of a dispute over universals—subjectivity *vs.* objectivity. My argument was much more concrete than such abstractions. I gave concrete examples, and the principles on which they were based, of problems and methods that offer more promise than others. There is some overlap between my examples and hers; see her #2, patterns of strategies over a life course, and #3, the intertwining of public and private in the creation of social relations—networks, groups, clusters. Both of these conceptualizations attempt to screen out subjectivity of sources by moving one step away from individual perceptions to the strategies and relations of historical actors, as evident in observed patterns of behavior (or reports of behavior). Both provide concepts and principles for screening out subjectivity of interpretation by the individual historian. Subjectivity is understood here as the condition of viewing things through the medium of one's own mind or individuality; it can occur in either the historian's mind or that of historical actors, and, therefore, in their testimony. Moving away from the minds of actors and looking at collective patterns of action that suggest strategies, regularities, and systematic variation of behavior, is moving away also from trying to plumb meaning in individual minds, and toward a possibility of objective interpretation.

Passerini's first example, "collective representations" or cultural identities, brings her closer to Alessandro Portelli, whose position I continue to find ahistorical and unsystematic. He offers literary criticism as a model for analysis, a model I believe not even the most "traditional" historian would accept, even in biographies of individuals. Here I agree with Eric Wolf (*New York Times Book Review*, January 20, 1984, p. 31), that cultural analysis of the type Passerini and Portelli are discussing may be enjoyed "as a type of appreciation—the way you listen to music or enjoy painting." History is something else: historians evaluate sources and use them to illuminate the past in a specific time and place, or a set of times and places, not to understand human nature or the human condition in a universal sense. The latter forces the enormous differences and vitality of individuals into reconstructed abstractions. Focusing unsystematically on individual experiences and feelings is "deconstructed" history, a retroactive psychology. Examining patterns and relations in the past

makes it possible both to generalize and to appreciate variation. That is what the diversity of history is about.

Our common effort to relive the past will not gain if the discussion of oral history becomes a dialogue of the deaf.

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Creating and using oral histories makes one very sensitive to the ways in which documents are the creations of the moment; the way in which the performatory circumstance of their origins influences the content, style, form, and tone of the presentation. In short, we are always thrown back into a concern for context. Thus, before discussing Professor Tilly's essay and the responses to it, I think it would be useful to note its origins, assess its "outsider's" view of oral history, and then cull from it what is of genuine concern and what is not.

It must be remembered that the essay was originally Professor Tilly's presidential address to the Social Science History Association on the moment of her ascendancy to its chief office. It consequently contains a certain horatory tone expected of such addresses, and a certain imperialism whose chief goal is, one supposes, to spur the convinced and committed on to further efforts in the field. For that reason, I don't think it is necessary for us to dwell at length on propositions that relate to that purpose and that purpose alone—such as the claim that the field of social science history is to be defined by the Association and its journal, or the concluding sentence that "Social science history, properly conceived, is the ultimate people's history." I am sure that, even within the Association, there are those who would question whether the central method of social science history is collective biography. The concluding sentence we leave to those who are more directly involved in people's history to ponder, especially whether or not it deprives the term of any meaning at all.

Rather, I think it would be most useful to address Tilly's concerns about what it is oral historians do and how they think about what they do, then note the major issue raised about "objectivity" and "subjectivity," and finally move on to what I believe to be the most important challenge presented to us about our work. Although it contains some naïveté and a certain conflation of differences, like most outsider's views, this essay also finds in our work a tension which most of us have not yet addressed, a tension in our varying attitudes and approaches to the interpretation of our interviews.

As most of us know, in conducting oral history we have two separate but linked sets of problems—those arising from the creation of the documents, and those arising from the analysis. The two cannot be fully divorced because, as Portelli and others have long noted, the historian is involved in the creation of the documents produced, and there is bound to be evidence of the involvement (and therefore the analysis) of the historian in the interview. For present purposes, however, we must keep the two stages somewhat separate. This is necessary because Professor Tilly's critique of the work of Portelli and the History Workshop oral history efforts deals with the analysis of documents, while that directed at our French colleagues (Bertaux-Wiame excepted) concerns the collection. In the latter case, Tilly is quite correct in pointing out how the methods of collection, selection, interviewing technique, etc. influenced the outcome of the process and were partially responsible for the eventual problems of analysis. Here it will only be noted that the indirect method of interviewing, and the view that oral sources are the least ideological type of sources, are in direct contrast to the methods and views of Portelli and Samuel, both of whom emphasize the role of the historian, politics and, by inference, ideology, in the creation and analysis of oral histories.

While questions of method are of tremendous importance, the current debate is really about larger issues. It would be a disservice to Tilly and our own respondents to talk at length on this theme, since the real questions concern what we do with our data after we have collected it, and what it is that our approach tells us about the past. Is there a special role for oral histories in our common effort to know as much as we can about what happened, and why it happened as it did? And what kind of analysis can we develop, given the special nature of our sources, to allow us to speak with some certainty about those questions?

In this case, the discussion is not helped much by the conflation of different approaches and historical stances. First, there has to be some question about the close connection drawn between people's history and oral history. People's history is more than oral history. Interviewing people is just one technique used by people's historians to recreate the past lives of the heretofore unnoticed classes, sexes, and races of the world. On the other hand, oral history is a method, available to people's historians but also to others who do not have the same interests, ambitions, or political concerns. I personally welcome the close connection, but we must all be aware of the very real differences, just as Tilly is when she discusses the relationship of quantification and social science history, being careful to note that quantification is one of the many tools available to the social science historian. Subjectivity is inherent in the oral history method, while it is a political and intellectual choice for the people's historian.

Second, it is important to note the differences in approach between

Portelli, Passerini, myself, and others, and the approach of the History Workshop articulated by Samuel in his response to Selbourne quoted by Tilly. I think a review of the literature in this journal, Britain's *Oral History*, the American *Oral History Review*, and other publications would show that Portelli and others have never neglected theory and analysis in favor of deep description. If anything, they have been accused of being too theoretical and analytical. Tilly may not like the particular set of theories used by Portelli (literary analysis), but it is hardly the same type of work as that of the History Workshop. The lack of generalization and explanation, and the focus on detail of the History Workshop, comes from the tradition within which our British colleagues work. It owes more to the work of E.P. Thompson than to a conscious attempt to avoid issues. I doubt that Tilly would claim that *The Making of the English Working Class*, with its detailed description and use of vernacular sources such as folklore, is not a work of science. The clue to the difference is Portelli's claim that what makes oral history different is that it tells us less about events than about their meaning. Oral history, of course, does both.

My own impression of the work in the History Workshop, other British works, and *The Voice of the Past*, is that their major concern is with what happened rather than the meaning of what happened, the tradition of British empiricism. Thus, oral history is used in very different ways by different people working in different traditions. In this respect, the work of Bertaux and Bertaux-Wiame has more in common with the aims of Portelli, since its real brilliance is in demonstrating the thesis of Nicos Poulantzas that aspects of one mode of production can exist within the structure of a newer mode of production, and the use of interviews to show the tremendous psychological cost for Parisian bakers who sought to maintain themselves in the face of the pressures for the new. As Samuel suggests, the explanation is in the conditions of historical production rather than any "naïveté." Portelli, Bertaux, and Bertaux-Wiame had no hesitation in moving from individual experience to theory because they had at hand, in the one case literary and in the other sociological theory, which made the leap possible.

But what then *does* unite these efforts? Why is Bertaux-Wiame so on target when she recognizes a community of interest? It is, of course, the focus on individuals and their experiences, but it is also more. It is the expansion of the definition of interests to include the life of the mind, thought, ideology, attitudes, psychology, if you will. It is the conviction that behavior alone will not explain social relationships, that how people see the world is as important in understanding how they act as the action itself. Indeed, thought itself is sometimes a form of action. All of this is closely connected to the belief that people are actors in history, and in many cases they tell us about their world with a precision and insight that

we, as outsiders, lack. (Thus, Bertaux-Wiame's claim that certain of her interviewees said it better than she could have.) Some of this is new, some of it is nothing more than what anthropologists and folklorists have been doing for years.

There is also the assumption that people live in culture and share ideologies, modes of expression, visions, and psychologies. If they did not, social organization would be impossible. Also, as we all know, social relations involve more than just working alongside one another, living in a household. They involve common expressions, common histories—or in the case of the Parisian migrants, women sharing one history and men another, which certainly tells us something about the subjective aspects of that relationship. It is conceivable that, as Portelli claims, we can discover a system by linking (not aggregating) many participants.

The problem here is the use of terms such as "objective" and "subjective" and their various meanings. On the one hand, those who study subjectivity (feelings, expressions, etc.) must realize that the study itself is often a situation in which the phenomena under investigation are objectified. The literary analysis of a subjectively-produced document is exactly that—an attempt to bring to bear a method of analysis outside of the thing itself. On the other hand, those who use non-literary documents must also understand that in the creation of even the most hard data, certain subjective elements are present, and the selection of which facts to present, use, and discuss depends upon many factors, not all of them similar to the factors deemed important by other "scientists." The creators of the United States Census were no less blind to the role of women in migration than Bertaux-Wiame's migrants to Paris. Luisa Passerini's point about these terms is, therefore, a bit more than an abstraction. It goes to the heart of the matter—what it is we do as historians. We all wish our data were better and our theories more powerful, yet we exist at a particular time and place in the history of history. Perhaps if we just dismissed these terms we might be able to discover, in a more precise manner, exactly what it is that Tilly has isolated in her review of our work.

What she has seen so clearly is the emergence of two differing approaches to the analysis of oral histories—what Daniel Bertaux, in a seminar here at Columbia, termed the "hermeneutic" approach and the "ethnographic" approach. The hermeneutic approach, in essence, brings to bear upon the individual document all the latest techniques of literary criticism and communications theory to discover the interpretative power of the text, its levels of discourse, its hidden meanings, and its ability to transmit a message. Based firmly in phenomenology, the concerns of those who use this approach are concentrated upon the moment of creation, the nature of the dialogue, the form it takes, and the formal relations between

the various elements of that dialogue. Through this analysis, we find how people manipulate truth, make history, tell stories, and carry on the collective memory. The assumption is that it tells us something about mind in action, something that is shaped by many in the culture either on a formal or an experiential level.

The ethnographic approach seeks to locate the testimony in the society, to study it for what it tells about people and their relations to other people, the world of production. There is, however, also an attempt to relate the levels of discourse, and the use of language, to social domains. Thus, it is much more concerned with questions of representation of both data and population, and more closely linked to similar efforts in sociology, ethnography, and some branches of anthropology.

To Tilly, one is science, the other not. Yet both move beyond illustration and seek to devise concepts and theories which link individual testimonies to tell us about the past. The concerns are different, the approaches often in conflict (though it is interesting to observe how many people using oral history seek to blend the two, as an examination of back issues of this journal would show). They are, however, rarely discussed. Perhaps, this is where we are not listening to one another, and the arena in which the view of an outsider is most useful. Although, as Bertaux-Wiame notes, we have assumed a common enterprise, there may be some very real distinctions that we must make, if for no other reason than to be more self-conscious about our practice. If we bear in mind the distinction noted above, the work of Portelli and the theories he elaborates are quite comprehensible. So too are those of our other authors, and so too is Tilly's. I am not sure it is a matter of science, and one quote from Eric Wolf does not convince me that a science of human behavior is not interested in human nature or the human condition, or that that is what Portelli and Passerini are really talking about. But I do sense that both approaches offer us ways to be more precise about what we are looking to discover in the past, and why it is important.

There are many specific questions addressed by Tilly and by our respondents which I have not discussed here. I urge our readers to offer their own comments, their own insights. We will all be helped by the debate.

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