

How (and What) Are Historians Doing?

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WHY WE STUDY HISTORY

Why should anyone care what happened in the past? Isn't the present unique and the future unknowable? If so, why not concentrate on the present? Let us leave aside the moral, political, psychic, and aesthetic value of knowing that we now live in only one of many possible worlds and of having some sense of roots; those are valuable reasons for studying history, but they are not essential. The crucial answer is simple and compelling: All reliable knowledge of human affairs rests on events that are already history. To the extent that the social structures and processes we wish to understand endure or take a long time to unfold, historical knowledge becomes increasingly valuable. To the degree that social processes are path-dependent—to the extent that the prior sequence of events constrains what happens at a given point in time—historical knowledge of sequences becomes essential. Historical verification is vital in any analysis of large-scale social change; anyone who wants to understand warmaking, capital accumulation, population growth, international migration, military rule, and any number of other crucial phenomena of the contemporary world had better take history seriously. History provides a key to the present and a guide to the future.

History as a phenomenon and history as a specialized inquiry, however, are two quite different things. History as the set of connections among human activities in time and space certainly concerns specialized historians, but it also plays a significant part in the analyses by geographers, economists, anthropologists, philosophers, and many other skilled observers of human affairs. What sets off the study of history as a specialized discipline?

Any intellectual discipline worth mentioning unites four elements: (1) a set of self-identified practitioners; (2) a series of questions the practitioners regard as important and answerable; (3) a body of evidence they consider relevant to answering the questions; and (4) an ensemble of legitimated

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practices that extract acceptable answers from the evidence. To the extent that they establish an academic base, most disciplines add a fifth element: an institutional structure consisting of associations, meetings, journals, publication series, and incentives to do good work. As pursued in Western countries, the subdisciplines of professional history (e.g., Eastern European diplomatic history, American intellectual history, and modern African history) clearly pass these tests. History in general, over the West as a whole, has more trouble qualifying; salient questions, relevant evidence, and legitimated practices vary significantly from country to country, period to period, and subject to subject. We might best think of history in general as a federation of overlapping disciplines.

Throughout the West, the study of history occupies some common ground. As practiced in Western countries today, history stands out from other organized inquiries by virtue of:

1. Its insistence on time and place as fundamental principles of variation—the prevailing idea being that social processes in, say, contemporary China occur differently than related social processes in medieval Europe
2. The corresponding time-place subdivision of its practitioners, with most historians concentrating on one part of the world, however large, during one historical period, however long
3. The anchoring of most of its dominant questions in national politics, with great attention accorded new answers to old questions or new challenges to old answers, and consequent variation in the major questions being asked by historians of different countries
4. The vagueness of its distinction between professionals and amateurs, with the skilled synthesizer and storyteller who attracts a large public often commanding respect from the specialists
5. Its heavy reliance on documentary evidence and its consequent concentration on the literate world
6. Its emphasis on practices that involve (a) identification of crucial actors, (b) imputation of attitudes and motives to those actors, (c) validation of those imputations by means of texts, and (d) presentation of the outcome as narrative.

I do not claim that every history and every historian in the West exhibits all these characteristics all of the time; some well-established branches of history fail to conform to one or another of these principles. I claim only that they are salient traits of most Western historical practice, that, on the average, they set historians off from other students of human affairs, and that historians whose work does not fit these standards have more trouble making other historians understand what they are about. Let us examine each of these characteristics in turn.

1. TIME AND PLACE AS FUNDAMENTAL VARIABLES

Although they rarely make the assertion explicit, most historians assume that *where* and *when* a social process—the formation of a friendship, the outbreak of revolution, the disintegration of a community, or something else—occurs significantly affects *how* it occurs. All important social processes, in this view, are path-dependent; what happened last year significantly constrains what can happen this year and what will happen next year. Thus Italian industrialization followed a different path from British industrialization in part precisely because Britain started industrializing earlier; Britain both provided a model and shaped the world market for Italy's industrial products. Within Italy, furthermore, the extensive prior development of small-scale industry in the hinterlands of such commercial cities as Milan significantly affected the opportunities for 19th- and 20th-century industrial concentration.

A fortiori, according to standard historical reasoning, urbanization, militarization, and commercialization are not the same processes when they occur within feudal and capitalist regions or periods. Two methodological injunctions follow: First, never interpret an action until you have placed it in its time and place setting; and second, use the greatest caution in making generalizations and comparisons over disparate blocks of time and place.

2. TIME-PLACE SPECIALIZATION

With spectacular exceptions, such as William McNeill, professional historians nearly always specialize in one or two combinations of place and time. Even Fernand Braudel (1979), who defined European history very broadly and roamed easily over five or six centuries, ultimately concentrated his research and writing on southern and western Europe during the 16th to 18th centuries. Most historians content themselves with a much smaller range, arguing that learning the languages, sources, historiography, and social context for the competent study of one or two countries over a century or so taxes human stamina, memory, and ingenuity. A few historical fields, it is true, shrug off time and space limits to deal with specialized phenomena, such as science, population change, coinage, or kinship. In those fields, discussions often move quickly from one time-space division to another. But even there, individual researchers commonly specialize in a single area of the world during a single block of time. And historical fields defined by phenomena rather than by time and place provide the primary identifications of no more than a small minority of practicing historians.

3. QUESTIONS ROOTED IN NATIONAL POLITICS

Even if they point to more exceptions than I have allowed, few historians will dispute my first two statements as broad generalizations about historical practice. Many more will challenge the third, on the grounds that historians pursue their own questions, that much of history does not concern sharply defined questions but efforts to recapture certain situations, mentalities, events, or actions, and that many kinds of history have little or nothing to do with national politics. Nevertheless, I claim that within each major time-place block of historical research, specialists (a) implicitly recognize a few questions as crucial; (b) reward each other for putting new questions on the agenda, for proposing persuasive new answers to established questions, and for challenging established answers to the standard questions; and (c) draw their dominant questions from problems on the national political agenda either of the nation under study or the nation to which they belong, or both.

Historians of the United States, for example, ask recurrently whether a distinctive mentality and social structure was formed in the North American colonies and subsequently guided American life, whether the American war for independence from Great Britain involved a social revolution, whether the Civil War marked the inevitable struggle between two different forms of American civilization, whether slavery and its aftermath made the experience of Blacks entirely different from that of their fellow Americans, whether mass immigration changed the structure of economic opportunity and the possibilities for a militant labor movement, and whether the United States became an exploiter on the European model as it rose to world power. These and perhaps a dozen more questions constitute the general agenda of American history. Teaching, research, and writing center on more concrete versions of these questions. Historians gain recognition by challenging old answers to them, proposing new answers to them, or (best of all) putting new questions on the agenda. Historians, finally, recognize the relevance of new work to the extent that it addresses these questions.

As I have summarized them, to be sure, the questions are all too broad and vague for precise answers; they require explication, refinement, subdivision, and translation into terms of more or less, when and where, under what conditions. Yet they all remain on the agenda of national politics, shaping debate, identifying relevant analogies to contemporary problems, and suggesting solutions to national ills. Historians of one nationality who study the history of another nation thus become ambivalent, sometimes responding to the agenda set by the object country's nationals and sometimes interpreting that country's history in terms that their own compatriots will understand.

4. AMATEURS AND PROFESSIONALS

Although the Western world contains 40,000 or 50,000 professional historians—people who not only have doctoral degrees in history or their equivalents but also spend the major part of their time teaching, writing, or doing research on history—a number of nonprofessionals make significant contributions to historical research. Some are novelists, essayists, and other kinds of writers who occasionally undertake historical writing, some are public figures who write memoirs or reflections, while others are people who make their livings in other ways but spend time digging in old books, newspaper files, private papers, and local archives for material that will appear in lectures, films, pamphlets, books, and articles for specialized historical periodicals. Without much hesitation, professionals use the best of those nonprofessional works for reference. They do not assume that only the anointed can do valid history.

Professionals and nonprofessionals alike value good historical writing that appeals to the general reading public. As compared with most other academic fields, historians do not make an especially sharp distinction between the contributions of professionals and nonprofessionals. Let me not exaggerate: Publications in internationally esteemed journals and by well-known presses clearly command greater respect among historians than do articles in local historical journals. The historical works that attract the largest lay audiences often do not meet professional standards, and many professionals feel envious ambivalence when nonprofessionals, however expert, write widely selling historical works. Nevertheless, history stands out from other social science disciplines in the relative interpenetration of professional and amateur efforts.

5. DOCUMENTARY EVIDENCE

Written material provides the vast majority of recognized historical evidence. For very recent history, interviews, films, and tapes begin to supply important evidence. At the far reaches of history, nonwritten artifacts start to matter seriously as evidence. But between those limits, written documents constitute the historian's stock in trade, the ability to locate and read relevant documents makes up a significant part of the trade's secrets, and members of the trade recognize the skillful deployment of documents as good craftsmanship. Historians share with linguists and literary critics a great concern for texts, but the historian's texts often include such dull, routine documents as tax rolls and administrative correspondence. Indeed, in many kinds of history (certainly in those I practice), one of the active researcher's primary

qualifications is the ability to sit still and stay awake while going through mounds of papers having little intrinsic interest, and either accumulating bits and pieces of information that will eventually fit into a larger design or searching for the one text that will make a big difference.

6. ACTORS, MOTIVES, AND NARRATIVES

Any student of human behavior balances between treating people as objects of external forces or as motivated actors. By and large, Western historians assume that they are describing the actions of motivated actors—individuals, families, classes, nations, or others—and that they can therefore reasonably arrange those actions in narratives—coherent sequences of motivated actions. Historians justify the imputation of attitudes and motives to actors by means of texts that presumably reflect those attitudes and motives. The narrative mode is by no means the only possible way to present history. One could, for instance, trace simultaneous connections among many actors and show how they changed, or follow the unfolding of complex processes, such as proletarianization and capital formation. Historians sometimes do these other things, of course. But on the whole, they do not recognize the enterprise as history unless it eventually yields, or at least informs, motivated narratives. Most historical writing, furthermore, consists of creating motivated narratives from documents that do not contain narratives and provide only sketchy indications of motives.

The education of professional historians reflects these six characteristics. Speaking very generally, a historical graduate education in Western countries falls into four phases. First comes a general synthetic survey of the histories of different areas and periods, spiced with occasional looks at exemplary or controversial works. Next, closer examination of current historiography is conducted, with particular attention to substantive and methodological controversies. Third, the student is initiated in the use of documents, often in the form of a master's thesis or its equivalent. Finally, one or two doctoral dissertations establish the initiate's ability make original contributions to knowledge in some particular field of history. (Where the full state-recognized doctoral dissertation makes the scholar a candidate for major professorships, as in most Germanic countries, the second dissertation is supposed to be a major work and typically appears after years and years of teaching and research.) The dissertation sets the standard for the historical monograph: a focused problem, a well identified set of primary sources, exhaustive coverage of the existing literature and available sources, and a careful statement of the ways in which the research alters previous understanding of the

problem. In general, professional historians feel that only mature scholars who have already crafted a monograph or two can (or should) bring off broader syntheses.

The six traits of Western history-writing mentioned earlier mark out a distinctive enterprise. Whether they are advantages or disadvantages depends on the task at hand. A discipline organized in this way is unlikely to discover principles that apply across large ranges of space and time, to make much headway analyzing processes that leave few written traces, or to have great success dealing with social changes that operate through the cumulation of diverse actions by millions of actors. But it is likely to do very well in helping literate people appreciate the problems of their counterparts in distant places and times. For many historians, that establishment of sympathetic understanding is the hallmark of well-crafted history. For some, indeed, it constitutes the only valid ground of historical knowledge.

History as an organized discipline shares a number of traits with folk history, the ways that ordinary people reconstruct the past. In the West, for the most part, people take history as a set of stories about individuals who act for well-defined motives with clear consequences. At a scale larger than the storyteller's own milieu, powerful and famous individuals occupy a large part of the story, just as their motives, actions, and consequences provide a major basis for moral and political reasoning; Stalin, Churchill, de Gaulle, and Roosevelt become emblems and explanations of a whole era. Folk history rarely concerns superhuman forces, complex social processes, or ordinary people—except as objects or distant causes of history, or at the point of contact between the teller's own life and certifiably great events or persons. History written by specialists gains popular appeal to the extent that it conforms to these standards.

PECULIARITIES OF SOCIAL AND ECONOMIC HISTORY

Within such a discipline, the sorts of social and economic history that have taken shape since World War II occupy a peculiar position. On one hand, they became auxiliaries to the pursuit of the standard big questions: What accounts for the rise and fall of ancient empires? To what extent did the growth of large-scale industry mark off a new stage of world history? What caused the great revolutions of our era? Did the major world religions shape distinctively different ways of political, economic, and social life? On the other hand, their practitioners soon began to identify actors who did not appear in the standard playbill, turned away from the construction of motivated narratives, borrowed extensively from the adjacent social sciences, and started to ask

eccentric questions such as: Under what conditions have sustained declines in fertility occurred? Have family forms and sentiments changed fundamentally in the era of capitalism? When and how do industrial economies stagnate? These deviations generated plenty of excitement but made it more difficult to integrate the analyses of social and economic history into attempts to answer the grand old questions.

Consider the case of European social history, the historical field that I know best. Some of postwar history's greatest achievements occurred in European social history: the revision of our ideas concerning population change, the discovery of human faces in revolutionary crowds, the charting of historical variants in family life, and the identification of mobility, complexity, and variety in what had been considered a vast, immobile, and undifferentiated European peasantry. Consequently, some of the discipline's sharper controversies also broke out on the terrain of European social history: whether the typical concerns of European social historians actually blinded them to politics, whether classes form in direct response to changes in the organization of production, whether the old extended family is a myth, whether cottage industry marked a (or *the*) standard path to capital-concentrated production, and so on. The controversies have drawn even more attention to the difficulties of integrating conclusions from European social history into general histories of Europe.

On the whole, European social history, as practiced in Western countries since 1945, has centered on one enterprise: reconstructing ordinary people's experience of large structural changes. In general, that has meant tracing the impact of capitalism (however defined) and changes in the character of national states on day-to-day behavior. Studies of migration, urbanization, family life, standards of living, social movements, and most other old reliables of European social history fit the description. Disputes within the field, by and large, concern (a) the means of detecting ordinary people's experience and of describing large structural changes, (b) the actual assessment of that experience, and (c) the identity, character, and causal priority of the relevant structural changes. Social historians contend rather little about whether they ought to be linking big changes and small-scale experiences.

Much of recent European social history emits a populist tone. Its writers rail against histories of kings and generals, insist on the intrinsic value of knowing how relatively powerless people lived in the past, claim that synthetic histories commonly misconstrue the character of the masses, and argue for a significant cumulative effect of ordinary people's action on national events, such as revolutions and onsets of economic growth. "History from below" is the cry.

Populism complements the central method of social history: collective biography. The painstaking accumulation of uniformly described individual events or lives into collective portraits, as in political prosopography, family reconstitution, and analyses of social mobility, takes its justification from the belief that the aggregates so constructed will provide a more telling portrayal of popular experience than would the recapitulation of general impressions, observers' commentaries, or convenient examples. It also establishes much of the common ground between social history and sociology, political science, and economics. In those disciplines, researchers likewise often build up evidence about aggregates from uniform observations of many individual units.

Within the area occupied by collective biography, social historians are most likely to adopt formal methods of measurement and analysis: fragmentation of individual characteristics into variables, quantification of those variables, formal modeling of the processes and structures under study, and rigorous comparison of observations with the models, frequently by means of statistical procedures. Where observations are uniform, instances numerous, models complex but explicit, and characteristics of the instances meaningfully quantifiable, formal methods permit social historians to wring more reliable information from their evidence than they could possibly manage by informal means.

There, however, acute controversy begins. First, despite the readily available example of survey research, historians have not been nearly as assiduous and successful at measuring attitudes, orientations, and mentalities as they have at quantifying births, deaths, and marriages. A major object of study and a major mode of explanation in history therefore remain relatively inaccessible to formalization. Second, historians tend to ground their pressing questions in times and places whereas social scientists tend to root them in structures and processes; to the extent that social historians adopt social scientific approaches to their material, they separate themselves from the questions that animate other historical work. Which set of questions should take priority? Third, the models and arguments that social historians borrow from adjacent social sciences often fit their historical applications badly—assuming independence of observations, being indifferent to the order in which events occur, calling for the recurrence of identical sequences, and so on.

Alas, historians could not deal with these disparities between social history and other histories by shrugging them off as simply another way of learning about human action. For the social-scientific approaches, if valid, challenged the very means by which conventional historians moved from elites to masses, from leaders to followers, from kings to their kingdoms: by treating the larger body as a more or less unitary actor or set of actors and

imputing to the actor(s) coherent motives, attitudes, or mentalities. If collective effects occur chiefly not through the aggregation of individual mentalities but through the compounding of social relations and resources—which is the premise of most social-scientific work—then historians who want to move validly beyond this level of the single individual have no choice but to analyze that compounding. If they do so, they are undertaking a version of social science.

Lest anyone take me for a social-science imperialist, let me state clearly that my hope for the social sciences is that they will all become more historical and that sociology, in particular, will dissolve into history. But that is not the issue here. We are examining the choices that confront present-day Western historians as they now practice their craft.

ALTERNATIVE HISTORIES

The division between social-scientific and other kinds of history reflects a much broader division within Western historical thinking. The division ultimately depends on philosophical choices which we might define provisionally as a series of alternatives:

1. History's dominant phenomena are (a) large social processes or (b) individual experiences.
2. Historical analysis centers on (a) systematic observation of human action or (b) interpretation of motives and meanings.
3. History and the social sciences are (a) the same enterprise or (b) quite distinct.
4. Historical writing should stress (a) explanation or (b) narrative.

Beneath these choices lie deep questions of ontology and epistemology: Is the social world orderly? To what degree and in what ways is it knowable? Does the capacity to reflect and react to reflection distinguish humans from all other animals and thereby render the assumptions and procedures of the natural sciences inapplicable to human history?

Rather than a strict dichotomy, to be sure, each of these pairs represents the poles of a continuum; the many historians who say "Let's look at the intersection between individual experiences and large social processes" or "Let's combine explanation with narrative" aim at the middle of those continua. Very few historians station themselves precisely at either pole of any continuum.

Nevertheless, the choice of a position within any continuum entails (however unconsciously) profound philosophical choices. In general, histo-

rians choose similar positions within each of the continua, and on the whole historians place themselves closer to the second choice in each continuum—closer to interpretation, individual experience, distinctness, and narrative—than do social scientists, psychologists, biologists, and other students of human behavior. Without too much violence to the complexity of historical practice, we might therefore combine the four continua into one, whose extremes bear the labels “social-scientific” and “humanistic.”

A second division comes immediately to mind. Historians vary enormously in the scales at which they typically work: from the individual person to the whole human race. Although logically independent, the continua small-scale/large-scale and social-scientific/humanistic correlate weakly; to a certain degree, historians who choose the humanistic end of the one range also tend to choose the individual end of the other. Still, those relatively humanistic historians who emphasize mentalities and culture—their number has increased in recent years—frequently work at the scale of the region, the nation, or even the continent. Many relatively social-scientific historians, moreover, work by aggregating individual observations into distributions and then adopt quite individualistic explanations of the distributions they find. Thus a rough two-dimensional representation of variations in historical approaches looks like this:

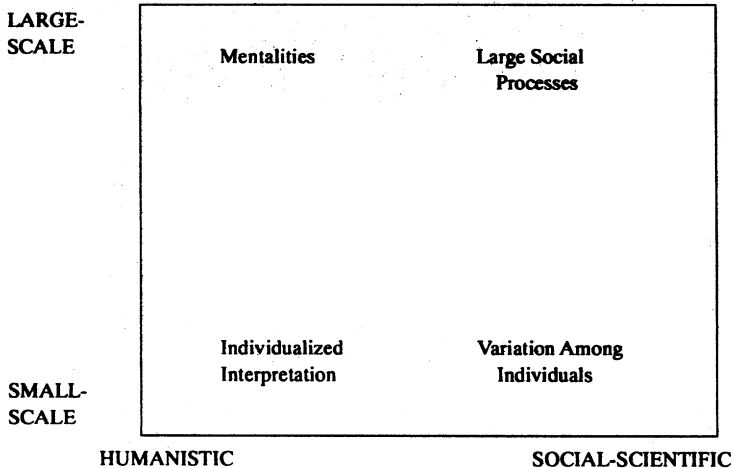


Figure A.

The distinctions make a difference. Gertrude Himmelfarb, English historian and astringent critic of what she calls the “new history,” draws her sharpest line between the diagram’s lower left-hand corner and all the rest:

Thus the new history tends to be analytic rather than narrative, thematic rather than chronological. It relies more upon statistical tables, oral interviews, sociological models, and psychoanalytic theories than upon constitutions, treaties, parliamentary debates, or party manifestoes. Where the old history concerned itself with regimes and administrations, legislation and politics, diplomacy and foreign policy, wars and revolutions, the new focuses on social groups and social problems, factories and farms, cities and villages, work and play, family and sex, birth and death, childhood and old age, crime and insanity. Where the old features kings, presidents, politicians, leaders, political theorists, the new takes as its subjects the “anonymous masses.” The old is “history from above,” “elitist history,” as is now said, the new is “history from below,” “populist history.” (Himmelfarb, 1987, p. 14)

Himmelfarb argues that these new histories have no capacity to deal with politics and suggests that without politics history has no coherent frame. By *politics*, she appears to mean national politics, the politics of states rather than of such groupings as local communities, lineages, or ethnic blocs. “After several decades of the new history,” she continues,

we can better appreciate what we are in danger of losing if we abandon the old. We will lose not only the unifying theme that has given coherence to history, not only the notable events, individuals, and institutions that have constituted our historical memory and our heritage, not only the narrative that has made history readable and memorable—not only, in short, a meaningful past—but also a conception of man as a rational, political animal. And that loss will be even more difficult to sustain, for it involves a radical redefinition of human nature. (p. 25)

She regrets, it seems, the loss of the liaison between professional history and folk history.

Furthermore, in the lower left-hand corner of the diagram, Himmelfarb is not delighted with all the company she finds. Some historians who work humanistically at the small scale—as we shall see—show a disinterest in national politics and have a weakness for the interpretation of ordinary people’s experiences. Himmelfarb wants politics, especially national and international politics, to retain its priority throughout history. As for the other corners, she regards the histories of mentalities, of large-scale processes, and of individual-to-individual variation, to the extent that they become the dominant historical concerns, as threats to the very enterprise of history.

A few years earlier, E.J. Hobsbawm (1980) replied to a similar indictment, less shrilly stated, from Lawrence Stone, a pioneer of the “new history” who had become disillusioned with what he regarded as its excesses (not fast

enough, however, to escape the wrath of Himmelfarb, who holds Stone's work up as a salient example of history's decay). "In short," declared Hobsbawn,

those historians who continue to believe in the possibility of generalizing about human societies and their development, continue to be interested in "the big *why* questions", though they may sometimes focus on different ones from those on which they concentrated twenty or thirty years ago. There is really no evidence that such historians . . . have abandoned "the attempt to produce a coherent . . . explanation of change in the past." (p. 4; internal quotations from Stone, 1979).

The contrasting positions make it clear what is at issue: not only taste and political preference (although both have their weight) but the very explanatory schemes and central questions of historical research.

In case it is not already obvious, perhaps I should declare frankly that my own preferences and most of my own work lie on the right-hand side of the diagram. But I greatly enjoy, and profit from, the best contributions on the diagram's left-hand side; the following discussion, whatever else it does, should prove that. The point of this essay, in any case, is not to argue for the superiority of one kind of history or another but to identify alternative forms of historical practice, discuss their requirements, assumptions and consequences, and clarify the choices that Western historians are actually making.

AROUND THE FOUR CORNERS

Let us explore the two-dimensional variation by reviewing some exemplary historical works—books that almost all historians will agree are excellent but that take very different approaches to their subjects. To see historical craftsmanship at work, let us concentrate on monographs rather than syntheses. To increase comparability and keep me on relatively certain ground, let us examine four outstanding works in western European and North American history: books by Carlo Ginzburg, E. P. Thompson, E. A. Wrigley and R. S. Schofield, and finally Olivier Zunz. The four do not constitute a representative sample of recent historical work—what four could? But they do provide relatively pure examples of monographs in each of the diagram's four corners, and thus mark out the space within which most historical work goes on.

GINZBURG'S SIXTEENTH-CENTURY MILLER

Carlo Ginzburg's (1980) *The Cheese and the Worms* places itself firmly in the lower left-hand corner of our diagram: small-scale and humanistic,

seeking to interpret an individual's experience. In 1584 and again in 1599, the Roman Inquisition tried and convicted Domenico Scandella, a miller from Montereale in northeastern Italy, for heresy; the first time he went to jail, the second time to the stake for burning. From the trial records, a few other local sources, and an enormous knowledge of 16th-century Italian popular culture, Ginzburg constructs a credible account of both an extraordinary person and of the cultural world in which he lived.

Scandella did not join some existing heretical sect but fashioned his own cosmogony from experience, inclination, and fragments of oral and written tradition. He believed, for example, that the world, including God, had emerged from a primitive chaos; the image of worms generating spontaneously in cheese—whence the book's title—often served him as a metaphor for that original creation. He denied many Catholic orthodoxies in favor of his own view that Christ was human and the Catholic Church a tool of greedy priests and monks. He not only believed these things but told many other people about them. The church's hierarchy could not forgive Scandella for teaching others such heresies, even after he had gone to prison for them.

Ginzburg places the text of Scandella's interrogation at the center of his book, but uses it as a prism. He looks through the prism at different angles, seeking to ask how a 16th-century village miller could have arrived at his astonishing worldview. In the search for sources of Scandella's beliefs, Ginzburg undertook a close reading of all the books with which the miller's testimony reveals him to have been familiar. The same method took Ginzburg to books and booklets that Scandella probably did not know to see whether his heterodox ideas were circulating more widely in the Italy of his time. "Naturally," says Ginzburg in one instance,

there's no reason to suppose that [Scandella] was familiar with the *Ragioni del perdonare*. In sixteenth-century Italy, however, in the most heterogeneous circles a tendency existed . . . to reduce religion to nothing more than worldly reality—to a moral or political bond. This tendency found different modes of expression, based on very different premises. However, even in this instance, it may be possible to discern a partial convergence between the most progressive circles among the educated classes and popular groups with radical leanings. (p. 41)

Then, like a detective tracking his suspect, Ginzburg begins searching for the traces of an oral tradition on which Scandella might have drawn. First he shows that the heretic systematically recast the texts he mentioned as his sources in favor of consistent ideas about the nature of God and man. Then Ginzburg assembles fragments of evidence, including the existence of another heretical miller, for the activity of a loosely connected network through

which independent, rural people might have circulated the radical egalitarian ideas for which Scandella died.

That search leads Ginzburg to his more general argument. Little by little, he raises doubts that the rural heresies radiated downward from elite thinkers, such as Martin Luther, and tenders, ever so delicately, the counterhypothesis that both peasant heresies and literary heterodoxies drew on a widely circulated, constantly evolving, popular oral tradition. "It is this tradition, deeply rooted in the European countryside," Ginzburg writes at one point, "that explains the tenacious persistence of a peasant religion intolerant of dogma and ritual, tied to the cycles of nature, and fundamentally pre-Christian" (p. 112).

Ginzburg proves neither the existence of such a coherent tradition nor the derivation of Scandella's extraordinary beliefs from it; although Ginzburg's scholarly notes establishes his wide awareness of parallels and connections, his method excludes the possibility of proof in any strong sense of the word. But his patient, subtle glossing of the texts concerning the village miller eventually expands a reader's awareness of popular creativity and of intellectual traditions that moved in partial independence of elite culture.

More conventional biographies belong in the same small-scale interpretive category as Ginzburg's essay; some of them (for example, Harvey Goldberg's [1962] still-stunning appreciation of Jean Jaurès) likewise help us understand their subject and the subject's milieu simultaneously. Richard Cobb's (1986) incomparable blends of reminiscence, biography, and *pointilliste* history belong in the class as well. Alain Lottin's (1968) portrait of 17th-century Lille through the journal of an artisan offers an example of a less daring, but very rich, approach to the subject. At a slightly larger scale, Emmanuel Le Roy Ladurie's (1975) reconstruction of the life of a 14th-century Pyrenean village from another set of Inquisition proceedings, Franco Ramella's (1984) treatment of the struggles of Biella's 19th-century wool workers, and Dirk Hoerder's (1977) portrayal of popular involvement in Massachusetts' portion of the American Revolution all display the power of small-scale interpretive studies to recapture the actual terms in which ordinary people experienced great issues and events.

Thinking small, then, does not necessarily mean thinking unambitiously. As Lucette Valensi and Nathan Wachtel (1976) said of Le Roy Ladurie's *Montaillou*:

Individual destinies are situated where they intersect with each other: the *domus*, the region, the intellectual universe—the environment, the "mental equipment" of the time: but while Lucien Febvre did portraits of illustrious persons, Le Roy Ladurie reconstructs obscure lives and plunges us into the

everyday life of the past. The attempt to totalize history encounters history's traditional calling, the study of those things that only happened once: the particular touches the general, reappearing in all its inexhaustible richness. (p. 8)

The general, in this view, is ineffably complex; interpretation of life at the small scale provides the principal path to historical knowledge.

THOMPSON'S ENGLISH WORKING CLASS

Those who work in the upper left-hand corner—the large-scale and humanistic corner—of our diagram agree on the complexity of collective life but argue that a historian can nevertheless identify and interpret patterns concerning whole peoples. In that corner, E. P. Thompson's work has been a beacon to historians. Thompson's *The Making of the English Working Class*, published in 1963, immediately stimulated the greatest tribute to a historical work: a combination of delighted praise, angry criticism, and eager emulation. Soon after the book's appearance, many bright young scholars of different lands had formed the ambition to write "The History of the ——— Working Class" on the model of Thompson's classic.

No one who reads the book will have trouble understanding why. *The Making* combines scintillating history with vigorous polemic. It stalks two different preys: the capitalist interpretation of economic history and economic Marxism. "In this tradition," says Thompson of the latter,

the very simplified notion of the creation of the working class was that of a determined process: steam power plus the factory system equals the working class. Some kind of raw material, like peasants "flocking to factories," was then processed into so many yards of class-conscious proletarians. I was polemicizing against this notion in order to show the existing plebeian consciousness refracted by new experiences in social being, which experiences were handled in cultural ways by the people, thus giving rise to a transformed consciousness. (In Abelow et al., 1983, p. 7)

Assuming, rather than establishing, a common experience throughout England, Thompson traces transformations in class action and consciousness between 1790 and 1832. "This book," says Thompson (1963, p. 11), "can be seen as a biography of the English working class from its adolescence until its early manhood. In the years between 1780 and 1832 most English working people came to feel an identity of interests as between themselves, and as against their rulers and employers." Thompson insists on this sense of class not as a thing or a position but as a dynamic relationship to antagonists. The *making* of the English working class, in his account, consisted of bringing to full consciousness that dynamic relationship of workers to employers and

rulers, with the accompanying realization that workers had the power to act against their exploiters.

Rather than a chronological narrative, Thompson's 800-page book contains 16 closely linked essays grouped into four sets: 18th-century traditions bearing on England's Jacobin movement of the 1790s; workers' experiences with industrialization; popular radicalism from the early 19th century to the 1830s; and class and politics in the 1820s and 1830s. Within individual chapters, however, Thompson blends narratives of particular struggles and movements with analyses of the ideas that informed them: Jacobinism, working-class religious movements, Luddism, agricultural laborers' revolts, strikes, and demands for Parliamentary reform. He takes his account up through the mobilization that brought the Parliamentary Reform of 1832 without offering a sustained analysis of that mobilization—or of its aftermath, when workers who had joined with artisans, shopkeepers, and capitalists to demand broadened representation faced the fact that many of their allies had gained the franchise while they had not. At that moment, Thompson suggests the English working class came close to shared consciousness and revolutionary intent.

Thompson's pages overflow with stories, argumentative asides, and quotations from relevant texts—especially the texts. In the vein of literary history, Thompson made two great innovations. The first was to broaden the notion of texts from written books and pamphlets to include not only poems, songs, and broadsheets but orations, utterances, rallying cries, visual symbols, and ritual acts. He sees a few great texts—especially those of John Bunyan, Thomas Paine, William Cobbett, and Robert Owen—as fundamental sources and expressions of working-class ideas. But he regards the more fragmentary and less literary sources as crucial for establishing how workers actually articulated the great ideas and (like Ginzburg) holds open the possibility that the great authors actually crystallized well-established popular traditions.

Thompson's second innovation was in piecing together the whole range of texts as a literary historian might, grouping them into families identified by similar themes, matching working-class shouts and threatening letters with well-known essays, interpreting the fragments in terms of the master texts and the master texts in the light of the fragments. He uses this method, among other things, to determine which thinkers and activists came closest to the genuine temper of workers; thus he argues that the great organizer Francis Place, for all his effectiveness in creating associations and lobbying Parliament, represented working-class views far less well than Thomas Hodgskin or John Gast (Thompson, 1963, p. 521).

By this weaving together of diverse texts, Thompson arrives at an interpretation of changes in working-class consciousness over the course of successive struggles from the 1780s to the 1830s. The basic transformation in that period, declares Thompson,

is the formation of "the working class." This is revealed, first, in the growth of class-consciousness: the consciousness of an identity of interests as between all these diverse groups of working people and as against the interests of other classes. And, second, in the growth of corresponding forms of political and industrial organisation. By 1832 there were strongly-based and self-conscious working-class institutions—trade unions, friendly societies, educational and religious movements, political organisations, periodicals—working-class intellectual traditions, working-class community patterns, and a working-class structure of feeling. (p. 194)

The same transformation, in Thompson's account, took English workers from John Bunyan to Bronterre O'Brien, from defense of the old moral economy to demands for power in the industrial economy, from scattered attacks on local enemies to mass movements, and from Luddism to Chartism.

E. P. Thompson's analysis shares with Carlo Ginzburg's the effort to construct a worldview from the incomplete evidence supplied by texts. But Thompson operates on a much larger scale and looks much more deliberately for signs of change; his subject includes all English workers (not to mention their allies and enemies) over half a century. He preserves the unity of his subject and holds to an interpretive mode, by taking all fragments as variations on the same theme: the emergence of a widely shared consciousness, a strongly connected organization, and intimate links between organization and consciousness.

Although Thompson wrote one of the most influential historical works of the past 30 years, he does not stand alone in his corner. John Brewer (1976) used similar materials and methods (and a different theoretical perspective) to examine British popular politics in the decades before the beginning of Thompson's book. Natalie Zemon Davis (1975) used small events to illuminate large themes of popular culture in 16th-century France, and Eugene Genovese (1974) inquired into the lives and beliefs of Black American slaves. More than anything else, the idea of partly autonomous, widely shared popular orientations—whether called mentalities, culture, or something else—has animated work in the upper left-hand corner of our diagram.

WRIGLEY AND SCHOFIELD'S ENGLISH POPULATION

Demographic history locates chiefly in the upper right-hand corner of our diagram, stressing social science and the large scale. That certainly applies

to E. A. Wrigley and R. S. Schofield's (1981) *The Population History of England, 1541-1871: A Reconstruction*. In the 1960s, both French and English demographers began to realize that the registers of baptisms, burials, and marriages long maintained by Christian churches would, under some conditions, yield reliable estimates of changes in the fertility, mortality, and nuptiality of the populations attached to those churches. In different ways, French and English research groups began the massive task of using those sources systematically to reconstruct vital trends before the age of regular national censuses, which began at the outset of the 19th century. The Cambridge Group for the History of Population and Social Structure took a threefold approach: extensive studies of household composition and other characteristics of local populations using whatever sources were available; derivation of refined estimates of vital rates by means of genealogies compiled from parish registers and similar records; and estimates of national vital rates by aggregation of births, deaths, and marriages from a sample of parish registers.

The Population History of England draws on the first two but concentrates on the third. Bulky and technical, its style stands about as far from *The Cheese and the Worms* and *The Making of the English Working Class* as one could imagine. More than half of the book's nearly 800 pages go into methodological discussions. The pages swarm with numbers, tables, and graphs. Yet the book generates excitement in its own way. For the Cambridge Group's research transforms our understanding of population change in England—and, by extension, in other parts of Europe—before 1800.

Wrigley, Schofield, and their collaborators wrought their revolution by means of wide-ranging organization and a series of technical innovations. Their organization included the recruitment of volunteers throughout England who abstracted information about baptisms, burials, and marriages from more than 400 sets of local registers from as early as continuous series existed, and then shipped the information to Cambridge in standard format for computerization, tests for reliability, and aggregation into national estimates of annual numbers of births, deaths, and marriages. The central technical innovation was "back projection," the use of birth and death series to move back, 5 years at a time, from the sizes and age structures of populations enumerated in 19th-century censuses to best estimates of population sizes and age structures before that time. After making allowances for immigration and emigration, they essentially subtracted the children born in a given 5-year interval from the population in the previous interval, added the persons who died in the same 5-year interval to the population in the previous interval, then cycled through the series again and again until they had consistent demographic histories of the 5-year cohorts that entered the

English population from 1541 onward. After years of compilation, testing, and refinement, the estimates of total population made it possible to compute birth, death, and marriage rates back to 1541.

The results are remarkable. They reveal 16th- and 17th-century populations (a) in which large numbers of people never married; (b) which never suffered the great waves of death once believed to be the inevitable consequences of periodic harvest failures under preindustrial conditions; (c) which recovered very rapidly from the losses that were brought on by subsistence crises because marriage and marital fertility rose rapidly; (d) in which illegitimate births and marriages rose and fell together instead of varying in opposite directions; and (e) which experienced a substantial rise in fertility (much more important than the conventionally expected decline in mortality) during the rapid population growth of the 18th century.

The resulting portrait of English population dynamics shows how much Malthus underestimated the effectiveness of the "preventive check" (abstinence from marriage and sex) in his own country and how much England escaped from "Malthusian" vulnerability to harvest fluctuations during the commercialization, proletarianization, and agricultural expansion of the 18th century. Explanations of these changes remain controversial (see Goldstone, 1986; Levine, 1984, 1987; Lindert, 1983; Weir, 1984a, 1984b; Wrigley, 1987). Still, the Cambridge Group's research has set what has to be explained on an entirely new plane.

As the nearly instantaneous response to the Wrigley and Schofield findings indicates, they made a difference far outside the zone of strictly demographic concerns. The total population figures supply denominators for a whole series of crucial per capita measures, such as personal income and agricultural productivity, and thus affect both the periodizing and overall characterization of British economic growth. The rising nuptiality and fertility of the 18th-century call for a much more active account of people's involvement in rapid population growth than did the old notion of declining death rates and "population pressure." And the high rates of celibacy in earlier centuries—long suspected but now confirmed—help explain the large role of unmarried "servants" in English agriculture and manufacturing before the era of capital concentration.

Wrigley, Schofield, and the Cambridge Group carried out one of the largest enterprises in the upper right-hand corner of our diagram, but not the only one. Philip Curtin's (1984) studies of the slave trade and of long-distance exchange in general, Robert Fogel and Stanley Engerman's (1974) econometric analyses of production under slavery in the United States, Jan de Vries' (1984) portrayal of European urbanization, Peter Lindert and Jeffery

Williamson's (1983) analyses of changes in income and labor force during English industrialization, and Michael Schwartz's (1976) examination of smallholders' politics in the United States all exemplify the use of social scientific approaches to investigate history on the large scale.

ZUNZ'S DETROIT

Olivier Zunz's (1982) study of Detroit's changing social geography from 1880 to 1920 does not operate on the small scale—a single individual and his environment—of *The Cheese and the Worms*, but it does use evidence on individuals and households to build up a picture of alterations in the city as a whole. Over the 40-year period that Zunz studies, Detroit went from a city of small machine shops and mixed trades to the factory-dominated metropolis of the automobile industry. But it remained a fairly low-density city with much of its housing stock in buildings lodging one, two, or three families rather than dozens or hundreds. To trace alterations in the city's fine spatial structure, Zunz sampled the household-by-household manuscript records of the 1880, 1900, and 1920 United States censuses, using the block cluster (four sides of one block and the adjacent sides of two blocks across the street) as his sampling unit:

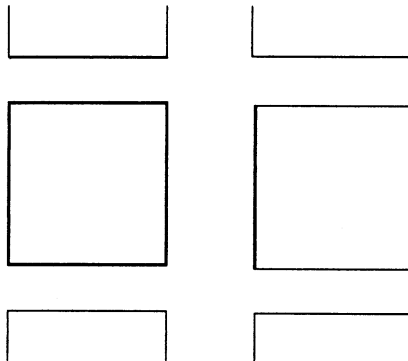


Figure B.

(It helped, of course, that most of Detroit was laid out in rectangular blocks between rectilinear streets). This meticulous effort gave him observations on the households who were likely to see and interact with each other from day to day. (Research in Detroit's local records confirmed that plenty of social life did proceed at the scale of the block front.) As a complement to the large file on households, Zunz compiled evidence on land use and building type in each block cluster. Thus he had extraordinarily fine evidence concerning who lived where, in whose company, and in what physical surroundings.

In the Detroit of 1880, Zunz discovered well-defined patterns of clustering by national origin, but at this small scale rather than in the form of major segments of the city settled by Germans, Irish, Blacks, Yankees, or others. In that city of fragmented capital, migrants clustered near their places of work, which were often ethnic enterprises. They created ethnic neighborhoods by helping each other find housing close at hand, by sharing dwellings, and by establishing local stores that catered to their own countrymen. The Detroit of 1900 displayed similar patterns, although the arrival of many Poles and Russian Jews altered significantly who lived and worked where.

The year 1920, however, marked a fundamental change. During the previous two decades, the automobile industry had exploded and become the city's dominant economic activity. By then, automobile manufacturers, especially Henry Ford, had installed assembly lines in large factories employing hundreds of workers. The reorganization of production transformed Detroit's labor force, especially by expanding the number of machine tenders working under relatively strict time-discipline. In the process, thousands of migrants, Black and White, came to Detroit from the American South.

The city's residential geography shifted accordingly. From a city of small-scale clustering by national origin, with pockets of high-income housing, Detroit became an exemplary case of large-scale segregation by class and race, with national origin operating chiefly within the limits set by class divisions. Two factors converged to produce that result: the change in employment, which made it impossible for workers' households to cluster around ethnically defined workplaces; and developers' deliberate construction of housing for separate markets defined by class and income. Thus the concentration of capital promoted the concentration of social classes.

Ever prudent, Zunz states the results of his study more cautiously than I have. As Zunz summarizes his findings:

In nineteenth century Detroit . . . industrial geography allowed the immigrant working classes access to jobs without disrupting their neighborhoods. The factories progressively encircled the city, and immigrant neighborhoods found

themselves at the focal point of industries. The location of these communities, near no particular factory but not too distant from any, was an asset to family economy; men and women, parents and children went off to work in different directions. It was not until 1920 that cohesive socio-ethnic neighborhoods, well adapted to the new urban and suburban subdivisions of residences and large factories, replaced the nineteenth-century ethnic neighborhoods. (p. 343)

For some decades, then, Detroit hosted what Zunz calls a “dual opportunity structure,” one set of channels feeding people of a given national origin into firms run by people from the same background and the other set taking them into the bureaucratized world of industrial employment. Eventually, the first set shriveled as the second expanded, and ethnic firms survived only in enclaves. As a result, daily routines, everyday social relations, opportunities for social mobility, and the quality of life changed drastically. Zunz’s study shows us the making of a class-based world built around jobs in big industry.

Zunz’s *Changing Face of Inequality* presents the standard technique of social history, collective biography, in unfamiliar garb. Although the conventional individuals and households appear in his analysis, Zunz focuses his effort on arbitrarily defined block clusters, whose virtue is that they are arbitrary but uniform, and therefore allows him to make comparable observations in different parts of the city and at different points in time—comparable, that is, from the viewpoint of a systematic observer. A whole generation of American urban historians (e.g., Katz, 1975, Thernstrom, 1964) assumed the possibility of systematic observation without agonizing over exactly what meaning their subjects attached to class position, mobility, or work experience. Historians who do collective biographies of officeholders, political conflicts, or organizations likewise sidestep the problem of subjective comparability by assuming partial equivalence of uniformly observed events. So far, neither they nor their many critics have clarified the grounds for justifying or denying the validity of their assumptions.

COMPARISONS AND CONCLUSIONS

The monographs by Ginzburg, Thompson, Wrigley and Schofield, and Zunz fall far short of representing the full variety of Western historical work. Nevertheless, they provide relatively sharp examples of four distinctly different genres of historical research. Since none of the authors stays strictly in the corner assigned to him, we might represent the location of each this way:

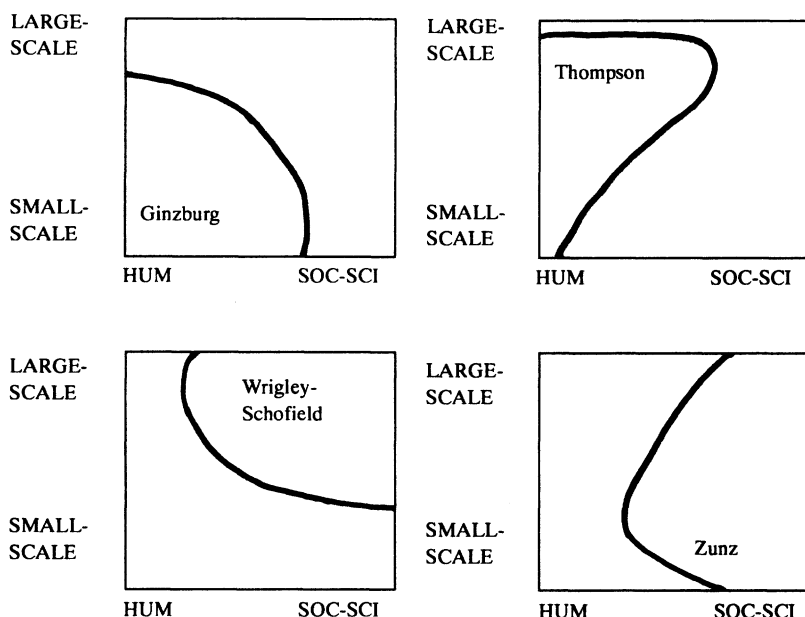


Figure C.

Ginzburg aims primarily at the smallest scale possible—a single individual—but moves repeatedly up the scale to offer interpretations of 16th-century mentalities in general. Wrigley and Schofield, in sharp contrast, reach down to the level of parishes for important parts of their evidence but concentrate the bulk of their effort on a national population; they do so, furthermore, in impeccably social-scientific terms. Zunz alternates between the levels of his block clusters and the city as a whole but occasionally deals with individuals, and in his more synthetic moments associates changes in Detroit with transformations of the whole American economy. He, like Wrigley and Schofield, conceives of his task as the systematic explanation of variation, although his variation occurs in both time and space. Thompson frequently turns to observations of individuals and small groups on his way to building an interpretation of changing outlooks at a national scale.

Other historians occupy more of a middle ground. Rudolf Braun (1960) combines simple demographic descriptions with a close reading of sermons and other direct testimonies as he reconstructs change in the hinterland of Zurich during the rise and fall of cottage industry. Keith Wrightson and David

Levine (1981) take one Essex village as their object, using detailed evidence of long-term demographic change as a base for identifying transformations of social structure, but turn quickly to information bearing on the texture of local life. Tamara Hareven (1982) confronts interviews of former mill workers with materials drawn from censuses and similar sources on the way to recapturing experience in a New England textile town. Herbert Gutman (1976) blends a poetic sense of 19th-century Black experience with robust numbers describing shifts in Black household composition. The remarkable contributions of Emmanuel Le Roy Ladurie (1966, 1975), Catharina Lis (1986), Ewa Morawska (1985), Jean-Claude Perrot (1975), Michelle Perrot (1974), Jane and Peter Schneider (1976), Rebecca Scott (1985), Laurence Stone (1977), William Taylor (1979), and Katherine Verdery (1983) all demonstrate the practical possibility of combining interpretation and systematic analysis of variation.

Nevertheless, the choices are real and pressing. Suppose we accept in full the premises of interpretive history at the small scale, thereby making Ginzburg's *modus operandi* (if not his subject matter) the center of historical practice. Then the claims of social-scientific history at the large scale will seem foolish and distasteful. Suppose, on the other hand, we surmount the epistemological and ontological barriers to believing large-scale social-scientific history feasible. Then interpretation will recede from the center to the close periphery of historical practice.

All in all, the choice of scales appears to be less daunting than the choice of historical philosophies. As the authors we have reviewed demonstrate, a skilled historian can move gracefully from the individual to the group to the nation without losing grip on a historical problem. Despite Gertrude Himmelfarb's fears, a political focus and a concern with mentalities can complement each other very nicely; E. P. Thompson's histories display that complementarity from beginning to end. The hard choices separate the endeavors which I have, all too simply, labeled "humanistic" and "social-scientific."

It is tempting to take a flatly pragmatic view of the choices: Let's do all kinds of history, and see which of them yield the best results. But the debate about which is "best" ultimately goes beyond taste or practical experience to questions about the character and accessibility of social reality. The philosophical problems will not wait forever.

Are historians, like migrant birds, condemned forever to oscillate between two poles? Is any synthesis of humanistic and social-scientific approaches to history possible in principle? Yes, it is. Let us leave aside uneasy compromises: a gesture to each side, including Ginzburgian glosses of Zunzian

findings and Wrigfield-style “verifications” of Thompsonian interpretations. A resolution to the difficulty will arrive under one of four conditions:

1. A discovery that reliable knowledge of human action is impossible, in which case both enterprises collapse
2. Proof that individual experiences are coherent and intelligible but large social processes are not, which condemns social science
3. Contrary proof that subjectivity is never reliably accessible but recurrent patterns of human action are, which scuttles humanistic history
4. Successful aggregation of reliably known individual experiences into collective action and durable social relations—which, if accomplished, will transform all the social sciences, as well as history.

I never said the task was modest—or easy.

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