

The Revivalism of Narrative

A Response to Recent Criticisms of Quantitative History

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In his presidential address to the Social Science History Association Convention in November 1981, Robert William Fogel declared sanguinely that social scientific historians had won their battle for legitimacy within the historical profession in America, and that we should now stop feeling embattled, spend less effort proselytizing, and calmly go on with our substantive work. While his statistics on the occupational advancement of social scientific historians do indicate a degree of acceptance, and while his advice to worry less and pay attention to business will be followed (as that is what nearly all of us were doing anyway), I am less optimistic than Fogel, read the employment trends differently, and see more signs of a reaction against quantitative social scientific history—or what I like to refer to as QUASSH—than he does (Kousser, 1980). Perhaps Professor Fogel and I differ only temperamentally. As a former Marxist, he still retains a bit of faith in the inevitable triumph of progressive forces; as a former Methodist, I am unable to shake off the pessimism that is the psychological residue of the doctrine of original sin. In any case, whereas Fogel seems to think that most recent criticisms of QUASSH are so obviously flawed as to require no answer, I fear

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that some people, especially those with substantial investments in "history-as-it-used-to-be-done," may still be susceptible to false messiahs or, perhaps more precisely, false Jeremiahs.

Unlike other fields, history has always been considered a popular subject, one accessible to any intelligent reader. Philosophy, astronomy, mathematics, and physics have always been arcane. Younger disciplines such as economics became mysterious only under considerable protest, and many of its high priests still double as part-time pop gurus. Yet despite the usually dismal sales figures of anything but military history and biographies of the powerful or picturesque, despite the turgid, pedestrian prose and topical narrowness of the typical monograph, despite the fact that nearly all current "serious history" can be understood only against the background of other works in its field, historians still aspire to general recognition, are quick to damn "jargon," continue to dream of being Michelets, Trevelyans, or Bancrofts, and condemn works they cannot immediately comprehend as "elitist" (Tilly, 1981: 20). At a table in the Big House, Fogel may have missed the rumblings of day-to-day resistance in the slave cabins.

The academic depression in history in this country and in others affected by the baby bust, stagflation, and the reaction against public spending, has, furthermore, dried up the pool of history graduate students, and seems especially to have frightened off those with that combination of mathematical and literary talents so necessary for the practitioners of QUASSH. The remaining puddle appears to abound with people who are either uninterested in moving or unable to move simultaneously in both the humanistic and social scientific cultures. Thus the potential anticliometric audience spans the generations.

Yet if the congregation is large, and in a mood for a revival, what of the content of the sermons? (Tilly, 1981: 37, may be blamed for this metaphor.) What alleged sins do the anti-numerical evangelists criticize QUASSH for and how valid is their castigation? In this counter-revivalist article, I shall attempt to categorize and answer the most common complaints recently made about QUASSH, thereby pointing the way if not toward salvation at least away from purgatory or worse.

The Fashion-Design Analogy. One of the most trivial beefs is that QUASSH is either a passing or an already passed fad (Barzun, 1974: 3; Scheiber, 1981: 349; Stone, 1977: 14). Symbolic anthropology, with its carnivals, magic, and charivaris, is, we are told, now the modish social science. Supply and demand curves and regression coefficients, like the gowns Nancy Reagan wore last month, are fit only for museums. This well-worn but threadbare metaphor dressed up as an argument really needs no rebuttal. While it is often instructive to note what smart people are doing, bargain-basement copies of Edward P. Thompson, Natalie Z. Davis, or Emmanuel Le Roy Ladurie are not necessarily good buys. If historians had always mimicked the style leaders, neither QUASSH nor anthropological history would ever have been cut out and stitched together. Surely the prime practical requisites for intellectual clothes are fit, warmth, and durability, not the dictates of the hautes grands couturiers of Paris or Princeton.

The Failure of Nerve and the Retreat Toward Solipsism. Some critics have become so pessimistic, or so convinced that others are, that they discern, in Lawrence Stone's words, the "end of an attempt to produce a coherent scientific explanation of change in the past" (Stone, 1979: 19; similarly, see Rabb, 1981: 323; Tilly, 1981: 62). Barzun goes further. The "first principle" of the "spirit of history," he tells us, is that "Man has no nature. . . . It is enough that in searching for what has happened by the agency of men everything and its opposite have equal likelihood and 'reason'" (Barzun, 1974: 152).

In a similarly depressing vein, Hayden White has concluded on the basis of an examination of professional historians of the nineteenth century and nonprofessionals of the twentieth that history is "protoscientific." White argues that

the physical sciences appear to progress by virtue of the agreements, reached from time to time among members of the established communities of scientists, regarding what will constitute a scientific problem, the form that a scientific explanation must take, and the kinds of data that will be permitted to count as

evidence in a properly scientific account of reality. Among historians no such agreement exists, or has ever existed [1973].

As consensus on such matters—for historians, if not for scientists—rests purely on “moral or aesthetic” grounds, and considering that historians are unlikely to agree on what is right and beautiful, “historiography has remained prey to the creation of mutually exclusive, though *equally legitimate*, interpretations of the same set of historical events or the same segment of the historical process” (White, 1973: 12-13, 428, italics mine.)

But Barzun’s dogmatic assertion undoes more than what he sneeringly dubs “quanto-history”; Stone’s assertion tends in the same direction; and White’s reduces history to a brand of literary criticism in which it is apparently not possible to specify grounds for preferring one interpretation to another. If systematic knowledge of the past or present of human beings is unobtainable, why study any kind of history?¹

The Back to Macaulay Suggestion. Bernard Bailyn has recently called for historians to produce “synthetic works, narrative in structure,” to rise above the merely local and the monographic “research reports.” “The great challenge of modern historical scholarship” is, he asserts, to produce “essential narratives” for a lay audience (Bailyn, 1982: 7). Others echo what Bailyn admits is his value-laden judgment about the “goal of history” (Barzun, 1974: 93; Supple, 1981: 205) or adopt a more critical stance, but believe the scholarly trend is already taking the narrative direction Bailyn favors (Stone, 1979).²

Yet there seem to be no a priori reasons why histories should take the form of narratives rather than analyses, or why historians should be more concerned to make their findings available to the public than say, physicists or sociologists—and neither Bailyn nor others whom I have read have made much of an effort to provide any such reasons. Citing all the precedents for history as a chronological account from Herodotus to Nevins does not add up to an argument, only to a mound of moribund examples. While few of us would eschew the fame—and almost none, the royalties—to be gained from writing a really popular work, self-

interest and social duty are hardly the same thing. (Worrying about history becoming too narrowly professional, as Kocka, 1982, and Yardley, 1982, do, is needless, for this is one particular problem that the free market can solve.)

A really comprehensive synthesis, moreover, one which attempted to cover most aspects of a society, could hardly be narrative in structure for there would be too many different, only feebly connected stories to tell. In the past, grand narratives were narrowly political or military. Is it any longer possible to pretend that such views are synthetic?³ And even if someone found a way to construct a grand synthesis of social, political, economic, and intellectual history, he or she would, of course, have to build on the "research reports" of others, many of which would be quantitative in nature.⁴ As grand syntheses are few, and monographs are necessarily many, there would still, even in this scheme, be much need for QUASSH.

It also seems to me that synthesis retreats before knowledge, that coherent accounts of very large topics *launch* rather than *crown* what Imre Lakatos called "scientific research programs," and that new syntheses in mature subjects can be produced (if at all) only by people who consciously or unconsciously blind themselves to the clutter of burgeoning scholarship. Thus, synthesis for synthesis' sake may reduce rather than add to the sum of knowledge. Narrative, with its hidden assumptions, buried causal structures, and lack of falsifiability is too obviously an inferior good to run QUASSH out of the marketplace.⁵

The Affinity for the Elect. If, as often noted, one of the appeals of QUASSH is that it enables one to say something about the masses, then those who doubt the importance or the possibility of learning much about anyone except elites may renounce QUASSH. Those who hold with Barzun that "history is about the active minorities to which majorities yield or consent" (1974: 111) can perhaps survive, depending on how large the minorities are, with only prosopography, factor or scaling analysis, and a theory of the state (if anybody ever develops a good one). If one believes that "no amount of counting . . . is going to reach into the heart of

the matter" of his field's most important subject, and that what he admits is "the hoary method of argument by example, and elite example at that" is a surer means, then QUASSH may be useless to him (Stone, 1981: 72, 76). Anyone who doubts Barzun's value judgement, or Stone's empirical judgment, or who thinks the conditions of the compelled or consenting majority or the interrelationships between elites and masses be of interest, however, will need a larger tool kit (see Kousser, 1982).

The Heraclitan Fallacy. In light of the fact that historians must be able to describe fundamental, massive, continuous changes over long periods of time, and considering that social scientific theories are rarely "dynamic," another argument says that the models and methods of QUASSH are of limited and limiting use (Hexter, 1971a: 111; Beach, 1980). Even to the extent that historians are normally concerned with such matters—and nearly all of us have much more modest goals—they need to measure changes systematically, which may require the use of statistics, and they need some way of determining what changes are really important and hints on what sorts of variables might explain the changes. (For an example of one such immodest study, see North, 1981.) For these purposes, social science provides a reservoir of ideas with which to supplement more vaguely formulated "common sense." For most purposes in historical study, comparative static models will suffice, and the dreams of grandiose, truly dynamic models are both utopian and usually unnecessary (Kousser, 1981).

The Disappointed Lourdes Pilgrim, the "What's New?" Riposte, and the Disdain for Details Stance. QUASSH, it is often charged, has not fulfilled its promise. It has not answered the big questions, has with a few exceptions merely confirmed what was already known, and has become mired in tedious local studies and "vision-limiting . . . technical problem-solving" (Bailyn, 1982: 2; Stone, 1979: 13; Barzun, 1974: 40). Such comments seem to me to be based on fundamental misconceptions of the research enterprise. New modes of analysis such as QUASSH raise rather than

settle questions (answers are always only provisionally accepted anyway) and in this, QUASSH has been quite fertile (Blaug, 1981).⁶ In my own field, American political history, the notion of "critical elections," the "ethnocultural" explanation of electoral behavior, and the institutionalism versus behaviorism debate—all products of QUASSH—have reshaped the research agenda since 1960. Findings that may seem obvious in retrospect were often less apparent before the research was carried out; and in any case, replacing a good hunch with firm support for a proposition can represent an important advance (Fogel, 1980; Tilly, 1981: 61).

Patronizing remarks about narrow foci or artisanry also mislead. Although local history may be merely antiquarian, community studies are often essential in the testing of larger hypotheses. If small areas are treated as quasi-experiments, they become the true and sometimes the only possible laboratories for systematic empirical history (see, e.g., Burton, forthcoming).

The details of equations or estimation, furthermore, are no less important for judging the degree of faith one should have in conclusions and often are considerably more revealing for this purpose than are footnotes to "literary" sources. Critics of QUASSH try to have it both ways: If the technical details are put in, they charge quantifiers with triviality; if left out, the charge is mystification. This hardly seems fair; and for myself, I prefer distracting clarity to smoothly presented conclusions whose validity cannot really be ascertained (cf. Stearns, 1976: 250-251; Stone, 1979: 21).

The Drunk and the Streetlight. Devotees of QUASSH tend to concentrate on questions for which the available quantifiable data are good (Stearns, 1976: 250-51; Bailyn, 1982: 9). Like the nighttime inebriate searching for his misplaced keys near the light even though they were lost elsewhere, the scholar following such a strategy may be left out in the cold. Of course, all historians are limited by the available surviving data—those who rely on letters and diaries fully as much as those who use census records. In fact, practitioners of QUASSH have greatly extended the range of usable historical material and have quantified documents that had

never been systematically analyzable (see e.g., Fogel and Engerman, 1974; Cox and Kousser, 1981).

"Method" Acting, Mentalité and Mysticism. Mentions of Wilhelm Dilthey's *verstehen* technique have always reminded me of Marlon Brando trying to become the character he was playing. As good an actor as he is, he is always Brando. With actual historical persons, the problem is even more difficult, for, unless the available information on a character's inner life is extraordinarily rich, there is often no way to tell whether the historian-actor has played his or her role correctly. In contradistinction to a fictive drama, the historian must maintain that his portrayal is not only true to life in general but true in regard to a particular life. Can claims that are not falsifiable and that entail no precise falsifiable conclusions, even in principle, really be part of scholarship? Some apparently believe so, and contend in addition that historians are somehow uniquely qualified to make them (Hexter, 1971a: 131-132). Thus Hexter declares that historians who free themselves from the shackles of analytical history, who discard as futile the notion of demonstrating sufficient causes, win "a chance to sense the force of the togetherness of events" (Hexter, 1971b: 118). While I enjoy speculation and Zen conundrums as much as the next person, I have never been able to understand the basis for the assertion that *verstehen* amounts to more than informed guesses, or to determine clear standards for deciding which historian has "the Force" with him or her.

I would not go so far as Stephen De Canio (1974: 118-119) who denies, in effect, that one can judge whether one conclusion from purely nonquantifiable evidence is better warranted than another—at least, in the case of postbellum southern sharecropping. I also believe that in many cases such evidence is necessary and desirable.⁷ Nonetheless, I am always a bit wary of calls for historians to be especially attentive to questions of hermetics and *mentalité* because of my fear that these slogans mask a desire to relax standards of proof. Hexter (1971a: 68) and Barzun (1974: 91, 96) explicitly endorse imprecision in this context. In fact, because when we enter the jungle of nonquantifiable evidence we must leave such weapons as significance tests and sensitivity

analysis behind, we should be particularly on guard—more anxious than at other times to make our assumptions, reasoning processes, and evidence explicit. In these endeavors, practice in QUASSH may provide valuable training, even when its theories and techniques may not be applicable.

The Populist Pose, the "Some of My Best Friends Are Quantifiers" Dodge, and the Fingers and Toes and Anything Goes Routines. Few critics openly reject QUASSH out of hand any more (but see Barzun, 1974). The line is rather that it is one of a variety of useful techniques, that historians should use anything that works, but that only simple statistical methods and verbal theory should be employed. Anything else—anything that would require special training or more than the dilettantish knowledge of social science that can be absorbed at lunch table conversations or by casual browsing in the library—is undesirable because history must, for unstated reasons, be accessible to all (Handlin, 1979: 225-226; Hexter, 1971a: 112-116; 142-145; Stone, 1977: 6, 16-17; 33, 36-37; 1979: 11; 1981: 86). Fortunately, according to Hexter (1971a: 142) the "arithmetical procedures" most useful to historians often require only "a level of sophistication usually attained in the fifth grade of primary school." That such statistics are sufficient, Hexter feels, is a good thing, for, as he says elsewhere (probably echoing the views of many other historians) mathematics is "the least commonsensical of all human intellectual activities" (Hexter, 1971b: 49).⁸

Coming as it does from scholars known for their good works, high professional standards, and obvious devotion to the Protestant ethic, this gospel of easy salvation seems anomalous. The inconsistency between their treatment of QUASSH and their practices and pronouncements on other topics is apparently due to misunderstanding. QUASSH is at once similar to and different from other kinds of history. To write or professionally evaluate Greek, Chinese, Byzantine, or nearly any other kind of history requires language skills, training, and a knowledge of the historiography that neither laymen nor historians in other fields usually possess. "Everyman" may be able to read a historical

work in a field of which he is ignorant, and he may be able to determine whether the account is logical and well written, but he cannot determine whether it is less false than other explanations, to state the proposition in Popperian terms, or, to be more blunt if less precise, whether or not the work is true (cf. Hexter, 1971b: 53-55). A deep understanding of serious history is simply not open to the average intelligent reader. In that sense, history has long since ceased to be a popular subject and QUASSH marks no change of direction.

From another viewpoint, however, QUASSH does represent a significant deviation because it is a rather catholic subdiscipline, not so tied to time and place as are other historical fields. Students who pay its entry price—calculus, linear algebra, statistics, theory—rather than learn different languages often have more in common than do other historians, and can often judge each others' work more competently than is usual for historians whose chronological and geographic specialties differ. To parody a bad poem, a regression coefficient is a regression coefficient is a regression coefficient. But just because the techniques of QUASSH can be applied in a very wide variety of cases, its study is more of a common necessity for historians than is that of languages or other research tools whose usefulness is restricted to a particular time or place. And proficiency in QUASSH is (perhaps even more than proficiency in languages) not easily attained. To be able to follow the journals in two or more disciplines, appraise the value of new works, and read up on novel or previously unexplored aspects of theory or methods requires a great deal more training and work than Hexter and Stone believe desirable. Employing inappropriately simple methods or inadequately understood theory produces only flawed history (see Kousser, 1976, 1979).

The Messy Data Gambit. While it may well be that carrying parameter estimates to six decimal places reflects exhibitionism rather than a desire for exactitude, some critics put forth the quite different contention that historical information is too imprecise to require the use of sophisticated methods or theory (Handlin, 1979: 11-14; Hexter, 1971a: 143-144). In fact, much quantifiable

historical evidence is at least as accurate as a great deal of current data with which economists and other social scientists have to deal. Indeed, a considerable portion of modern econometrics—for example, generalized least-squares, logit and probit analysis, and unobservable variables techniques—has been developed in order to deal with data that does not satisfy the assumptions necessary to employ simpler techniques. This fact implies that the converse of the critics' charge—the messier the data, the *greater* the necessity to use sophisticated analytical techniques—is closer to the truth than is the original statement (Fogel, 1971: 8).

The "Mythical" Misconception. Ignoring the clear and convincing explanations offered, for example, by Davis (1971), some erstwhile friends of QUASSH continue to misconstrue the nature of the explicit counterfactuals often employed in economic history (Herlihy, 1981: 123). To restate the case briefly: In any causal argument, a statement that X produces Y implies that if X were not present, either some different outcome would have occurred (in the case of a necessary condition) or perhaps that some other X would have caused Y to occur (in the case of a sufficient, but not always necessary, condition). In either case, counterfactuals are *always* logically implicit; and the cliometricians' innovation was just to spell them out as an aid to understanding both the reader's and the writer's parts. Thus, to criticize their use in principle, although not, of course, in particular instances, is to take either a logical stance in favor of muddledness, a stylistic position in favor of obscurity, or to assume that outcomes in the period succeeding every past event are never predictable enough to allow specification of a counterfactual (or a limited number of counterfactuals), which is simply another version of the view that if we lack absolute certainty we can say nothing. (Martin, 1979: 57, seems to take the latter position.)

Terminal Confusion. A last set of criticisms relates not to QUASSH in general, but to its familiar handmaiden, the data-processing machine: Computer-based projects cost too much. Coding loses data and is not error-free. The machine consumes the user's time and atrophies his or her mind. Record-linked

problems are insuperable. The data for completed projects are inaccessible, residing as they do on privately held computer tapes (Stone, 1977: 26-27, 29, 33, 39; 1979: 6, 11, 13; 1981: 63-64; Herlihy, 1981: 126-127). None of these criticisms will bear close scrutiny, for they either ignore technological advances or fail to acknowledge comparable difficulties in nonquantitative research.

Comparative cost-benefit analysis must take account of all factors—book acquisition expenses for libraries and opportunity costs for scholars' and students' time as well as grant sizes on the cost side, and on the benefit side support for training and scholars' research, general underwriting of university budgets, and, most important, the value of the projects' research to the scholarly community. They must also be specific to particular projects. Here, I would stack up two undertakings that I know a bit about—the Philadelphia Social History Project (PSHP) and *Time on the Cross (TOTC)*—against any combination of the principal nonquantitative endeavors of comparable expense that come to mind, the papers of American presidents and other notables. Whether all of their conclusions are ultimately accepted or not, both PSHP and *TOTC* have altered the research agendas and issued novel and elaborately documented challenges to many of the reigning dogmas in their fields. Can anyone make higher claims for the paper projects?

While coding and record linkage are, indeed, time-consuming tasks, the critics have ignored advances in hardware and software that virtually eliminate the necessity for making restrictive coding decisions (see Tilly, 1981: 53-83) and have greatly increased the reliability of record linkage (see Hershberg et al., 1976). They have also neglected to note that while much quantified data remain outside depositories and some cannot be pried out of scholars' hands, Fogel and Engerman and Stephan Thernstrom have placed their data with the ICPSR. Even before that, the authors of *TOTC* allowed their harshest challengers free access to their data. This act, the approximate equivalent of Stone's sending Trevor-Roper all his note cards for "The Anatomy," has no parallels outside QUASSH so far as I know (Stone, 1948; Trevor-Roper, 1951). Furthermore, machine-readable data are at

least as easily checked for accuracy against the original sources as are non-QUASSH historians' notes from manuscripts, newspapers, and published documents. In fact, procedures for the double checking of coding and data entry are much easier to institute, and are no doubt much more widely used in quantitative than in nonquantitative projects. How many non-QUASSH scholars regularly employ associates to make independent readings of manuscripts to confirm their own glosses, or wade through extensive manuscript collections twice to be sure every quotation is precisely recorded?

In regard to wasted time and mental numbness, tastes will perhaps differ. For myself, I would rate hours before a microfilm machine or reading somebody's letters, bills, and laundry lists—especially those in handwritten form—as at least as brain-curdling as trying to figure out how to make a computer do what I want it do. And both, in my experience, seem to obey the universal law of completion times: Make your largest reasonable estimate of the time it will take to do something, double it, then apply the exponential function to the result.⁹

Defining Hardcore History, or I Know It When I See It. It may be that some aspects of the disputes between QUASSH and non- or anti-QUASSH historians are nonterminating, not so much, I think, because each side is equally correct, but because the entrenched interests of each may so color their responses to particular arguments or their willingness to acknowledge the existence of certain points that even the most acute minds on each side will never meet. As children of Kuhn (1970), we should not be very surprised at this.

One thoughtful proposal to circumvent this dilemma has been offered by Theodore K. Rabb (1981). Considering that everyone except Barzun adheres rhetorically to the position that QUASSH is at least sometimes useful, the leaders of the profession, while they cannot agree on general standards of equality, might agree on the value of particular works. But is it likely that historians will be "indifferent to methods as long as the results are illuminating," as Rabb (1981: 330) asserts they should be? It seems improbable

6. In this respect, it seems to me, Stone (1981: 63-64) is excessively uncharitable toward the Cambridge population history group. Through works, such as Laslett (1965), Wrigley (1971), and Wrigley and Schofield (1981), they have done and continue to do more to inspire interest in demography and family history and to shape the field's questions in a precise, scientific direction than have the works of any other historians. That the specificity of the Cambridge group's positions and the openness with which their evidence is arrayed allows others to criticize them effectively, while the vagueness of the statements of historians such as Aries (1962) makes them hard to disagree with, to my mind is only further evidence of the superiority of the QUASSH approach.

7. I plead not guilty to Jürgen Kocka's charge that I wish to restrict history entirely to "those subject areas and problems which can be handled in a quantitative social-scientific way" (Kocka, 1982). The sentence in my text, above, was in the version of my paper that was distributed at the Washington Conference, and Kocka had it available to him when he wrote up his comments, prepared for this issue, that had been presented orally in Washington. Moreover, much of my own work is based on nonquantified sources.

8. Read disingenuously, Hexter's statement is obviously false, for people from the fifth century onwards, from the most diverse cultures and speaking the most diverse languages, have agreed that the typical proof of the basic theorems of plane geometry made sense. The sense of the theorems, that is, has been understood *in common*, in this as well as in many areas of mathematics, by people who could agree on little if anything else. There are, of course, abstruse philosophical conundrums about what constitutes proof and similar problems, but these typically concern much higher branches of mathematics than are necessary to understand basic statistics. In any case, even the most skeptical generally admit that they can understand, if not wholly accept, the reasoning of their opponents. Probably, however, Hexter is simply saying that *he* has a hard time with math. As with all of Hexter's writings, the reader should allow, in deciding how to interpret this statement, for Hexter's puckishly hyperbolic style.

9. I owe this law to my Caltech colleague Aron Kuppermann.

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