DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES

CALIFORNIA INSTITUTE OF TECHNOLOGY

PASADENA, CALIFORNIA 91125

THE REVIVALISM OF NARRATIVE: A RESPONSE TO RECENT CRITICISMS OF QUANTITATIVE HISTORY

J. Morgan Kousser

This paper was delivered at the First International Conference on Quantitative History, Washington, D.C., March 1982, and at the California State University, Fullerton.

SOCIAL SCIENCE WORKING PAPER 453

November 1982

ABSTRACT

Despite the continued and perhaps even increased productivity of quantitative social scientific historians (QUASSH, for short) and certain evidences of their acceptance by the historical profession, a reaction against QUASSH, first bruited shortly after the initial quantitative work was published, also continues. Calls for a return to the narrative tradition or suggestions that historians are returning to it, recently made by such leaders of the profession as Lawrence Stone and Bernard Bailyn, have begun to percolate down to the popular media.

Rather than dismiss the criticisms of QUASSH out of hand, I attempt in this paper to categorize and answer them. Finding the objections misconceived, illogical, incomplete, or overstated, I examine also a proposal by Theodore Rabb to substitute a criterion of general quality for a consensus on methods. I find it difficult to believe that groups who begin from such different premises as QUASSH and some, but of course not all non-QUASSH historians, will agree on a "quality" criterion. Thus, it is unlikely that the schism will be quickly healed or that a respectful latitudinarianism will soon develop.

"THE REVIVALISM OF NARRATIVE: A RESPONSE TO RECENT CRITICISMS OF QUANTITATIVE HISTORY

*This paper was delivered at the First International Conference on Quantitative History, Washington, D.C., March 1982, and at the California State University, Fullerton.

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 (since that's 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 a faith in the inevitable triumph of progressive forces; while as a former Methodist, I am unable to shake off the pessimism which is the psychological residue of the doctrine of original sin. In any case, whereas Fogel seems to think most recent criticisms of QUASSH so obviously flawed as to require no answer, I fear that some people, especially those with substantial investments in history-as-it-used-to-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." (See Tilly, 1981, 20.) At 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 anti-cliometric audience spans the generations.

Yet if the congregation is large, and in a mood to be revivalized, 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 paper, 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.

Let us begin with <u>The Fashion Design Analogy</u>. 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 Roi Ladurie are not necessarily good buys. If historians had always mimiced 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 haute couturiés 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 non-professionals of the twentieth, that history is "protoscientific." "The physical sciences," White argues, "appear to progress by virtue of the agreements, reached from time to time among members of the established communities of scientists, regarding what will count as 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." Since consensus on such matters, for historians, if not for scientists, rests purely on "moral or aesthetic" grounds, and since 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 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 doesn't add up to an argument, but only to a mound of moribund examples. And 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 Köcka (1982) and Yardley (1982) do, is needless, for this is one particular problem which 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 would, of course, have to build on the "research reports" of others, many of which would be quantitative in nature.[4] Since grand syntheses are few, and monographs, 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 <u>launch</u>, rather than <u>crown</u> 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 a 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]

<u>The Affinity for the Elect</u>. 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). And 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 judgment, 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.)

<u>The Heraclitan Fallacy</u>. Since historians must be able to describe fundamental, massive, continuous changes over long periods of time, and since social scientific theories are rarely "dynamic," another argument goes, 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 satisfice, 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, hasn't fulfilled its promise. It hasn't 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." (Stone, 1979, 13; Bailyn, 1982, 6; Barzun, 1974, 40. The quotation is from Bailyn.) 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.[6] (Blaug, 1981.) In my own field, American political history, the notion of "critical elections," the "ethnocultural" explanation of electoral behavior, and the institutionalism vs. behaviorism debate, all products of QUASSH, have

reshaped the research agenda since 1960. Findings which 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 often represents an important advance. (Fogel, 1960; 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, then 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 put in conclusions and often are considerably more revealing for this purpose than footnotes to "literary" sources. Critics of QUASSH try to have it two ways -- if the technical details are put in, they charge quantifiers with triviality; if they are left out, with mystification. This hardly seems fair, and for myself, I prefer distracting clarity to smoothly presented conclusions whose validity cannot be really ascertained. (Cf. Stearns, 1976, 250-51; Stone, 1979, 21.)

The Drunk and the Streetlight. Devotees of QUASSH tend to concentrate on questions for which the available quantifiable data is good. (Stearns, 1976, 250-51; Bailyn, 1982, 9.) Like the nighttime inebriate searching for his misplaced keys near the light, even though he lost them elsewhere, the scholar following such a strategy may be left out in the cold. But 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. And in fact, practitioners of QUASSH have greatly extended the range of usable historical material and have quantified documents which had never been systematically analyzable before. (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 role correctly. And 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 which are not falsifiable and which 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, 131-32.) Thus Hexter contends 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.

I would not go as far as Stephen De Canio (1974, 118-19), who denies, in effect, that one can judge whether one conclusion from purely non-quantifiable 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 hermeneutics 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, since when we enter the jungle of non-quantifiable 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 anymore. (But see Barzun, 1974.) The line is rather that it is one of a variety of useful techniques, that historians should use anything which works, but that only simple statistical methods and verbal theory should be employed. Anything else, anything which would require special training or more than the dilettantish knowledge of a social science which can be absorbed at lunch table conversations or by casual browsing in the library is undesirable, since history must, for unstated reasons, be accessible to all. (Handlin, 1979, 225-26; Hexter, 1971a, 112-16, 142-45; Stone, 1977, 6, 16-17, 33, 36-37; Stone, 1979, 11; Stone, 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 is no doubt a good thing, for, Hexter says elsewhere, undoubtedly echoing the views of many other historians, that 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 which 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, 1971 b, 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, it does represent a significant deviation, for it is a rather catholic subdiscipline, not so tied to time and place as other historical fields are. Students who pay its entry price -- Calculus, Linear Algebra, statistics, theory -- rather than learn different languages, often have more in common than other historians, and can often judge each others' work more competently than is usual for historians whose chronological and geographical 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 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, e.g., Kousser, 1976 and 1979.)

The Messy Data Gambit. While it may well be that printing 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, 143-44.) 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 which does not satisfy the assumptions necessary to employ simpler techniques. This fact implies that the converse of the critics' charge -- that 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, e.g., 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 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 on 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 or 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, lacking 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 or isn't error-free. The machine consumes the user's time and atrophies his mind. Record-linkage problems are insuperable. The data for completed projects is inaccessible, residing as it does on privately-held computer tapes. (Stone, 1977, 26-27, 29, 33, 39; Stone, 1979, 6, 11, 13; Stone, 1981, 63-64; Herlihy, 1981, 126-27.) None of these criticisms will bear close scrutiny, for they either ignore technological advances or fail to acknowledge comparable difficulties in non-quantitative 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 support for training and scholars' research, general underwriting of university budgets, and, most importantly, the value of the projects' research to the scholarly community, on the benefit side. And they must be specific to particular projects. Here, I would stack up two undertakings that I know a bit about, the Philadelphia Social History Project and Time on the Cross, against any combination of the principal non-quantitative endeavors of comparable expense which 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 which 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). Likewise, while much quantified data remains outside depositories, and some cannot be pried out of scholars' hands, the critics have neglected to note that Fogel and Engerman and Stephan Thernstrom have placed theirs in the Michigan machine-readable archives, and that even before that, the authors of Time on the Cross 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 is at least as easily checked for accuracy against the original sources as non-QUASSH historians' notes from manuscripts, newspapers, and published documents, and in fact, procedures for double-checking coding and data entry are much easier to institute, and are no doubt much more widely used in quantitative than in non-quantitative 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 to. 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 non-terminating, 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). Since 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 quality, might agree on the value of particular works. But is it likely that historians will be "indifferen[t] to methods as long as the results are illuminating," as Rabb (1981, 330) asserts they should be? It seems improbable that Hexter and Rabb, for instance, will come to a quick consensus on the value of Rabb's _Enterprise and Empire_. (Hexter, 1971 a, 117-27.) Richard Sennett's denunciation of _Time on the Cross_ as "little more than an intellectual hoax" also seems to bode ill for rational agreement on quality. (Kousser, 1980,890-91.) And Rabb's own acute suggestion that the differences between quantitative and _mentalité_ historians is "a profound epistemological question, not just a matter of technique," that _mentalité_ historians "may regard a question like 'is it true?' as either meaningless or irrelevant" implies that, far from moving toward a potential consensus on quality, historians are becoming, to paraphrase John Dos Passos, "two nations." (Rabb, 1981, 323-24.)

Perhaps all this is too pessimistic and reflects my sectarian,

"chapel" upbringing. Perhaps the critics and their potential and actual parishioners will recognize their venial and more serious sins and convert or at least cease to condemn. But an examination of their recent preachments convinces me that rumors of the growth of a healthy latitudinarianism have been rather exaggerated.

FOOTNOTES

1. On this point, in contrast to others to come, I am happy to array myself with Hexter. See Hexter, 1971b, 286.

2. Although Hobsbawm's 1980 challenge to the accuracy of Stone's description has attracted less attention than Stone's original article, as yet no one has performed a quantitative or any other test in order to determine which of them is more nearly correct.

3. Wood (1982, 8) has pointed out that Middlekauff's (1982) narrative of the American Revolution, wallowing in battle sketches, almost wholly slights many of the major developments in political thought and institutions during the period, not to mention the principal trends in economic and social history. What Wood calls "old-fashioned narrative history with a vengeance," even by one of our finest traditional historians, therefore, is, almost of necessity, blindered. The situation in Renaissance history is similar. See Honour, 1982.

4. Bailyn, 1982, asserting that "No effective historian of the future can be innocent of statistics . . ." makes this point repeatedly.

5. For a similar, more eloquent statement by a non-QUASSH historian, see Wood, 1982.

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 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, is, to my mind, only further evidence of the superiority of the QUASSH approach.

7. I plead not guilty to Jürgen Köcka'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. . . ." (Köcka, 1982.) The sentence in my text, above, was in the version of my paper which was distributed at the Washington Conference, and Köcka had it available to him when he wrote up his comments, prepared for this volume, which he had presented orally in Washington. Moreover, much of my own work is based on non-quantified 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 theorums of plane geometry made sense. The sense of the theorems, that is, has been understood

in common, in this as well as many other areas of mathematics, by people who could agree on little if anything else. There are, of course, abstruse philosophical conundrums about what constitutes a proof and similar problems, but these typically concern much higher branches of mathematics than are necessary to understand basic statistics, and 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.

BIBLIOGRAPHY

Aries, Philippe (1962) Centuries of Childhood: A Social History of Family Life. New York: Vintage Books.

Bailyn, Bernard (1982) "The Challenge of Modern Historiography," American Historical Review, 87: 1-24.

Barzun, Jacques (1974) Clio and the Doctors: Psycho-History, Quanto-History, and History. Chicago: University of Chicago Press.

Beach, William W. (1980) "A Second Look: 'The Agenda for "Social Science History,"'" Social Science History, 4: 357-364.

Blaug, Mark (1981) The Methodology of Economics, Or How Economists Explain. Cambridge, England: Cambridge University Press.

Burton, O. Vernon (forthcoming) In My Father's House Are Many Mansions: Family and Community in Edgefield, South Carolina. Chapel Hill, N. C.: University of North Carolina Press.

Cox, Gary W. and J. Morgan Kousser (1981) "Turnout and Rural Corruption: New York as a Test Case," American Journal of Political Science, 25: 646-663.

Davis, Lance E. (1971) "Specification, Quantification and Analysis in Economic History," in George Rogers Taylor & Lucius F. Ellsworth, eds., Approaches to American Economic History. Charlottesville, Va.: University Press of Virginia: 106-120.

DeCanio, Stephen J. (1974) Agriculture in the Postbellum South: The Economics of Production and Supply. Cambridge, Mass. & London: The MIT Press.

Fogel, Robert William (1971) "The New Economic History: Its Findings and Methods," in ___ and Stanley L. Engerman, The Reinterpretation of American Economic History. New York: Harper & Row: 1-12.

__________ (1980) "'Scientific' History and Traditional History," in L. J. Cohen et al., eds., Logic, Methodology, and the Philosophy of Science, 6, Amsterdam:

__________ and Stanley L. Engerman (1974) Time on the Cross. Boston: Little, Brown and Company.

Handlin, Oscar (1979) Truth in History. Cambridge, Mass.: Harvard University Press.

Herlihy, David (1981) "Numerical and Formal Analysis in European History," Journal of Interdisciplinary History, 12: 115-135.

Hershberg, Theodore, Alan Burstein and Robert Dockhorn (1976) "Record Linkage," Historical Methods Newsletter, 9: 137-63.

Hexter, Jack H. (1971 a) Doing History. Bloomington and London: Indiana University Press.

__________(1971 b) The History Primer. New York: Basic Books.

Hobsbawm, E. J. (1980) "The Revival of Narrative: Some Comments," Past and Present, 86:3-8.

Honour, Hugh (1982) "From Alberti to Zoppo," New York Review of Books, 29, #13 (Aug. 12): 23-24.

Köcka, Jürgen (1982) "Theories and Quantification in History," paper presented at First International Conference on Quantitative History, Washington, D.C., March 5, 1982.

Kousser, J. Morgan (1976) "The `New Political History′: A Methodological Critique," Reviews in American History, 4: 1-14.

__________ (1979) "History - Theory = ?," Reviews in American History, 7: 157-162.

__________ (1980) "History QUASSHed: Quantitative Social Scientific History in Perspective," American Behavioral Scientist, 23: 885-904.

__________ (1981) "History as Past Sociology in the Work of Samuel P. Hays: A Review Essay," Historical Methods, 14: 181-186.

__________ (1982) "Restoring Politics to Political History," Journal of Interdisciplinary History, 12, 569-95.

Kuhn, Thomas S. (1970) The Structure of Scientific Revolutions. Chicago: University of Chicago Press.

Laslett, Peter (1965) The World We Have Lost. New York: Charles Scribner′s and Sons.

Martin, Albro (1979) "And It Will Never Be History, Either, Unless . . ." Essays in Economic and Business History: 53-59.

Middlekauff, Robert (1982) The Glorious Cause: The American Revolution, 1763-1789. New York: Oxford University Press.

North, Douglass C. (1981) Structure and Change in Economic History. New York: W. W. Norton and Company.

Rabb, Theodore K. (1981) "Toward the Future: Coherence, Synthesis, and Quality in History," Journal of Interdisciplinary History, 12: 315-332.

Scheiber, Harry (1981) "American Constitutional History and the New Legal History: Complimentary Themes in Two Modes," Journal of American History, 68: 337-50.

Stearns, Peter N. (1976) "Coming of Age," Journal of Social History, 10: 246-55.

Stone, Lawrence (1948) "The Anatomy of the Elizabethan Aristocracy," Economic History Review, 18: 1-53.

__________ (1977) "History and the Social Sciences in the Twentieth Century," in Charles Delzell, The Future of History. Nashville: Vanderbilt University Press: 3-42.

__________ (1979) "The Revival of Narrative: Reflections on a New Old History," Past and Present, 89: 3-24.

__________ (1981) "Family History in the 1980s: Past Achievements and Future Trends," Journal of Interdisciplinary History, 12: 51-88.

Supple, Barry (1981) "Old Problems and New Directions," Journal of Interdisciplinary History, 12: 199-205.

Tilly, Charles (1981) As Sociology Meets History. New York: Academic Press.

Trevor-Roper, Hugh R. (1951) "The Elizabethan Aristocracy: An Anatomy Anatomized," Economic History Review, 2nd series, 3: 279-298.

Wood, Gordon S. (1982) "Star-Spangled History," New York Review of Books, 29, #13 (Aug. 12): 4-9.

Wrigley, E. A. (1971) Population and History. New York: Mc-Graw Hill.

__________ and Roger S. Schofield (1981) The Population History of England, 1541-1871. Cambridge, Mass.: Harvard University Press.

Yardley, Jonathan (1982) "The Narrowing World of the Historian," A.H.A. Perspectives, 20, #6 (September), 21-22.