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HISTORY QUASSHED, 1957-1980*

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SOCIAL SCIENCE WORKING PAPER 309

MARCH 1980

*Forthcoming in
American Behavioral Scientist

ABSTRACT

History QUASSHed, 1957-1980, forthcoming in American Behavioral Scientist.

Social scientists and historians trained in social science began importing quantitative methods and explicit models into history in the late 1950s. At first, many disciplinary leaders stoutly resisted the trend, but in the 1960s and 70s the major historical journals became increasingly receptive to statistical and mathematical pieces and the range and importance of work by quantifiers became impossible for historians to ignore. What defines the new subfield, how healthy is it, and where is it likely to go in the future? A review of recent work on critical elections, geographic mobility, and postbellum Southern economic history suggests that QUAntitative Social Scientific History (QUASSH) has the usual adolescent traumas, but that the diagnosis is favorable and the recommended therapy is an increase in the time spent contemplating the connections between theory and methods and more contact with mainline historians.

History QUASSHed, 1957-1980*

History is different from the other social sciences. Nine contrasting qualities have produced a striking variation between the course of development of quantitative methods in history and that in the rest of the social scientific disciplines. First, historians have framed less compelling research agendas, and feel less constrained by the ones which have been proposed than do professionals in other fields. Historians borrow, rather than invent theories; prize diversity of insight more than coherence; paradoxically, are more attracted by interpretations which claim to overturn or replace older ones entirely, rather than those which stress their continuity with previous structures of understanding.[1] If historians are often classified as belonging to one "school" or another, the underlying educational philosophy is decidedly progressive, the assignments only roughly structured, the discipline very loose.

Second, historians classify themselves by area and period, more than by topic or technique. We call ourselves specialists in the antebellum South, the Renaissance and Reformation, or modern Japan; we very rarely advertise job openings in categories analogous to applied microeconomic theory, deviant behavior, or minority group relations (Tilly, 1979, pp. 28, 45). A historian of one area or period may therefore proceed in blissful ignorance, at least up to a point, of technical advances outside his subfield. Third, and closely related to the second point, we have no widely recognized subdisciplines in theory or methodology. In its Guide to Departments of History, 1979-80, for instance, the American Historical Associations's chart on the specialties of each department includes geographical and period rubrics only (AHA, 1980, pp. 384-415). Thus, there are no niches in which those primarily interested in theory or methods can abide comfortably. They must be specialists in substance, part-timers in everything else.

Fourth, historians are less likely than other social scientists to speed the development of an area by collaborating or even directly competing with each other (Fogel, 1979, p. 35). It appears to be fairly common in other social sciences for complementary collaborators to match up -- a theorist or methodologist with a person more interested in the substance of a problem. Moreover, a challenging problem or appealing topic attracts droves of sociologists, economists, or political scientists, who seem to coexist relatively cheerfully with others working on the same issue; a rumor that another historian is working on a particular subject, on the other hand, is likely to bring a stern letter warning off the territorial intruder, or a frantic competition, often detrimental to the resulting quality of scholarship on both sides, to get published first.

Fifth, the best historians, to a markedly greater extent than the best sociologists or psychologists, still pride themselves on the ability to craft their writings not just clearly, but elegantly. Tables, equations, the use of phrases defined by common convention may foster precision, but they sully prose, and many historians prefer inexactness to stylistic compromise (Barzun, 1974, pp. 40,105, 109; Landes and Tilly, 1971, pp. 11-13). Sixth, many historians claim not merely an

*I want to thank my colleague Lance E. Davis for forcing me to clarify some of my murky thinking and for warning me of some of the gravest of my errors. He is hereby absolved from further responsibility.

aversion to, but an absolute incapacity for mathematics. Curiously, such claims arise perhaps most often from those who deal most in abstractions, the intellectual historians.

If these six traits have impeded the adoption of numerical techniques in history, compared to the progress in other social sciences, three other disciplinary characteristics, two of them facets of the first six, have boosted quantification. For one thing, historians seem even more willing than other social scientists to adopt hypotheses and methods from outside their discipline. Thus, one of the field's leading publications, The Journal of Interdisciplinary History, is something of a crazyquilt of articles which blend history with anthropology, economics, political science, psychology or psychoanalysis, or sociology. For another thing, because it lacks a core of traditional theory or methodology, history tends to be somewhat more subject to fads, to be more malleable and open to innovations than its sister social sciences. The successful application of an approach often inspires widespread emulation -- in slightly different areas or periods, of course. Consequently, a few heralded illustrations of the usefulness of quantitative techniques in history were enough to incite many others to make similar attempts. For a third thing, historians have a fetish for thoroughness, for completeness. A historical reviewer's comment that the author of a book has failed to examine a relevant document collection or ignored another scholar's pertinent work is considered devastating. Historians have therefore found unanswerable the argument that by neglecting quantifiable sources, which could only be fully exploited with complicated statistical techniques, they were failing to exhaust all the resources at hand.

Although these comparisons should not be pushed too far, for all social scientists share the historians' qualities in varying, if somewhat lesser degrees, they do point towards an explanation of both the willingness of groups of historians to try to quantify and the inability of quantifiers to gain dominance in most fields of history.

Quantification came to history late and it came from outside the discipline.[2] In 1957, Lee Benson, a historian schooled in sociology, published a sweeping critique of "impressionistic" treatments of nineteenth-century American elections and called for historians to expand their definition of primary sources beyond newspapers and manuscripts to include quantifiable data. Four years later Benson added practice to preachment, relying heavily on a quantitative analysis of election returns to produce a brilliant and original interpretation of American politics in the 1830s and 40s. In a paper delivered in 1957, two Harvard economists, Alfred H. Conrad and John R. Meyer, reinvigorated the discussion of an old historical problem and initiated the new "econometric history" by demonstrating the profitability both of slavery and of applying modern economic theory and techniques to history. By 1960, the "cliometricians," as they were jibingly labeled, were holding annual conferences at Purdue to coordinate research efforts and criticize each other's papers. A year before, the historian Merle Curti, assisted by several other historians and his psychologist wife Margaret, published a quantitative historical study of community social structure and mobility, which, along with the work of Stephan Thernstrom, inspired legions of students to take up the "new social history"

(Benson, 1972 and 1961; Conrad and Meyer, in Fogel and Engerman, 1971; Curti, 1959).

The response by the historical profession's elite was rapid, but by no means single-minded. To the sometimes strident demands of the devotees of the new history that traditionally-trained historians "retool, rethink, reform, or be plowed under," as one older economic historian caricatured the new program, some historians at first reacted with fright, irrationality, and something close to panic. Arthur Schlesinger, Jr., whose description of Whig and Jacksonian electoral coalitions had failed Benson's systematic numerical tests, retreated behind a hastily-erected wall of dogma. "Almost all important questions," Schlesinger proclaimed, "are important precisely because they are <u>not</u> susceptible to quantitative answers." In a presidential address to the American Historical Association, Carl Bridenbaugh issued a jeremiad against the infiltrating priests of the new religion, warning his fellow historians never to "worship at the shrine of that Bitch-goddess, QUANTIFICATION" (all quoted in Woodward, 1968, pp. 29-30).

Others kept their wits a bit better, declaring the historical faith broad enough to encompass another sect. Reminding his readers that enthusiasm for social science had repeatedly waxed and waned within the American historical profession in the twentieth century, C. Vann Woodward suggested that "rhetorical indignation and the neo-Luddite posture of our conservatives are not effective responses. Smashing computers is not quite the answer." If Woodward seemed to yearn for a revolution which would overthrow the contemporary regime of historical craftsmen who were "even more addicted than those of earlier generations

to over-specialization and narrowness of subject matter," whose "monumental patience" produced such "unimpressive conclusions," he was doubtful of the revolutionaries' prospects for victory and skeptical of their utopian visions (Woodward, 1968, pp. 30, 24).[3]

A third response to the social scientific proselytizers, especially popular among graduate students and younger historians, was fraternization and -- usually timid -- collaboration. Thus, a traditionally-trained historian who found Guttman scaling helpful in his study of the mid-nineteenth century British Parliament, William O. Aydelotte, nevertheless carefully qualified his endorsement of the use of quantitative methods. "Quantification," he remarked in a set of essays advocating its employment in historical study "is merely an ancillary tool, one of several, that can, for certain classes of questions, be of some help." From 1965 to 1970, 120 historians, many of them no less hesitant than Aydelotte, attended summer seminars in historical data analysis at the University of Michigan (Aydelotte, 1971, p. 34; Swierenga, 1970, p. 5).

Nonetheless, by 1970 there seemed to be a lull in the <u>Methodenstreit</u>. By that date it was virtually impossible for traditionalists to deny the significance of the research published by the "new" political and social historians; while in economic history, the cliometricians had swept all before them. The econometric historians were powerful enough to take over the strongest disciplinary journal, the <u>Journal of Economic History</u>; their social and political counterparts started new ones -- the <u>Journal of Social History</u> (1967), <u>Historical Methods</u> (1967), and the <u>Journal of Interdisciplinary History</u> (1970). In both range and depth, the body of work based on the analysis of quantitative data was impressive. In political history, the "ethnocultural thesis" rested on examinations of patterns of voting returns in ethnically and religiously homogeneous geographic areas; the theory of "critical elections," on correlations of election returns by area across time; and various hypotheses about the behavior of particular legislative bodies, on Guttman scaling and factor analyses of roll calls (Benson, 1961; Kleppner, 1970; Holt, 1969; Formisano, 1971; Burnham, 1970; Aydelotte, 1963; Silbey, 1967; Alexander, 1967). In social history, scholars tabulated the extent to which individual family heads remained in the same area or the same occupational rank over time; demographers charted changes in marriage, birth, and death rates, as well as in family size and type; while other social historians graphed patterns of wealth and landholdings and alterations in those patterns (Thernstrom, 1964; Demos, 1969; Greven, 1970; Lockridge, 1970; Main, 1965). Economic historians used statistical techniques and neoclassical theory in their often strikingly novel treatments of economic growth, slavery, human and nonhuman capital formation, demographic and technological change, and fiscal and monetary policy (Fogel and Engerman, 1967). More selfconsciously theoretical than the others, the new economic historians developed the explicit counterfactual model. Usually trained as economists, they sprinkled their work liberally with regression equations and complex supply and demand curves (Fogel, 1967; Davis, in Taylor and Ellsworth, 1971). By contrast, scholars in the other two fields typically identified themselves with the concerns and more literary style of history, in which most of them had received their degrees. By the end of the 1960s, then, a growing band of quantifiers had moved

beyond propagandizing and built a scholarly edifice which was grand enough to inspire a new review article industry (Bogue, 1968; Clubb and Allen, 1967; Rothstein, 1970; Fogel and Engerman, 1967).

During the 1970s, quantification became almost respectable in the discipline. Several books which relied largely on numerical evidence won major prizes (Hackney, 1969; Alexander and Beringer, 1972; Lemon, 1972; Thernstrom, 1973; Fogel and Engerman, 1974; Scott, 1974). A new organization, the Social Science History Association, was launched, attracted members, held conventions, produced a journal. Mainstream organs of the profession added tables to the usual prose, and increasingly published not just counts, but regressions, correlations, even factor analyses. The number of tables per page in five leading historical journals increased five-fold from the early 1960s to the late 1970s, and the proportion of these tables which were based on more than simple counts rose from none in the 1960s to 27 percent in one journal and 11 percent in all five periodicals. In the more specialized subdisciplinary organs of social scientific history, the trend was similar and the analogous figures markedly higher. Mammoth historical data banks and large samples of historical statistics were drawn and made available to the general scholarly community. Indeed, machine-readable data from elections, legislative roll calls, censuses, city directories, wills, family reconstitution studies, and other sources has come on-line, as it were, far faster than historians can analyze it. At least several hundred historians have gone through introductory statistics courses in graduate schools or in special summer institutes. Most departments which train doctoral students now have at least a token "quantifier," and a few encourage their students to take more advanced theory and methods courses in other departments (Kousser, 1980).

It would be wrong, however, to represent the 1970s as a time of triumph for cliometrics, or to project these apparent trends confidently into the future. Historians expect Thermidorian reactions, and some have already begun to dismiss quantification as not much more than a passing fad or to speak of such alleged dangers as "the quantitative trap" (Stone, 1977, pp. 14, 29; Genovese and Genovese, 1976, pp. 210-11; Stearns, 1976, p. 250). Defections from the ranks, often proclaimed with considerable fervor, as well as healthy if sometimes vituperative self-criticism within the community of quantitative historians have been seized on by long-time opponents as evidence of the imminent demise of the subdiscipline. Thus, two years of criticism by cliometricians led one historian to refer to <u>Time on the Cross</u>, the best known quantitative historical work of the decade, as "little more than an intellectual hoax" (Richard Sennett, quoted in Shapiro, 1976, p. 202). "Softer" social scientific approaches -- psychohistory, vaguely "Marxist" analyses of culture, "anthropological" studies of <u>mentalitiés</u> -- are currently voguish (Stout, 1975; Stone, 1977, p. 14). The academic recession, with its attendant deep cuts in graduate programs and new faculty slots, has especially hurt newer specialties, social scientific history among them.

But impressionistic surveys of scholarly public opinion are inadequate tests of the value of such a bundle of approaches as social

scientific history, and an uncertain guide to its fate. One might project three alternative futures for social science history: relative autonomy from the rest of the history profession, but with sufficient strength to maintain a viable research tradition; integration with more traditional history; and decline and disappearance, probably involving the absorption of each cliometric subculture into its adjacent social scientific discipline. On the basis of current definitions of the field and the quality of recent work, what are the auguries?

There is no consensus at this time on a description or even a name for the object of our concern. Donald McCloskey has posed perhaps the most crisp and pugnacious definition: "A cliometrician is an economist applying economic theory (usually simple) to historical facts (not always quantitative) in the interest of history (not economics)" (McCloskey, 1978, p. 15).

There are only five things wrong with this definition. First, since it is often difficult to distinguish economic from political or social historians in topics, methods, or the invocation of theory, why should any of them escape the chiding monicker of "cliometrician?" Second, the question of which segment of economic theory to apply to a particular historical issue is sometimes unclear or contested (Ransom and Sutch, 1979, pp. 65-7), some economic historians use theories from other disciplines (Easterlin, in Tilly, 1978), and noneconomic cliometricians often draw upon social sciences other than economics. Third, for some of the most important topics in economics, for example, growth, the prime data source is historical, and economists in such fields often examine historical sources in the interests of economics as much as of history (e.g., Kuznets, 1968). Likewise, historical sociology or political science is often conducted in the interests of both history and the other discipline (e.g., Wallerstein, 1974; Burnham, 1970). Fourth, there just may not be any part of neoclassical economic theory which is applicable to certain earlier periods, since, according to North, that theory gives slight attention to ideology, the formation of and changes in tastes, and alterations in the underlying economic constraints which determine the structure of economies (North, 1978). In other words, in changed and changing times, theory may misguide. Fifth, if the concept of theory is broadened to include abstractions from other social scientific disciplines or from other economic churches (Marxist, institutionalist), how can a theory-based definition distinguish economic history from, say, psychohistory or the casual use of "theories" by persons whom nearly everyone would call mainstream historians?[4] Thus, a definition tied solely to "theory" is either too narrow or too broad. Neither "neoclassical economic history" nor "social science history" will quite do as a title for the field.

What, then, about a concentration on method?[5] After all, one of the first tags for post-1960 economic history was "econometric history" (Fogel, in Fogel and Engerman, 1971, p. 2), and many refer to the larger field as "quantitative history."[6] There are two chief difficulties with this title. First, historians, particularly economic historians, have always counted things and have often presented data in tabular form or subjected it to at least crude statistical tests. The contemporary French "serial history" emphasizes counting, but seems

vastly different from American cliometrics (Furet, 1971; Forster, 1978). The adjective "quantitative," therefore, fails to distinguish the new from the old economic history and confuses the use of numerical data merely to describe from its employment in testing explicit models. Second, some important work in cliometrics involves sketching out, refining, and criticizing theories and their applicability to particular topics or areas (e.g., Swanson and Williamson, in Schnore, 1975; Goldin, 1979; Reid, 1979; Temin, 1979; Wright, 1979). Such exercises may be expressed wholly in prose or may involve mathematics, but they seldom contain much empirical data or statistical analysis. Yet they remain too integral a part of the process of the field's development to be excluded from it by definition.

Should one, then, draw the line between "scientific" or "analytical" and "traditional" history, as Fogel does in a recent paper (Fogel, 1979b), thereby thrusting himself into the unlikely company of Jacques Barzun (1974)? I think not, for, first, the attributes of "scientific" history, at least as Fogel lists them, describe a much broader spectrum of historians and historical works than most would include under any other of the proposed rubrics. For instance, the "Jim Crow thesis" of C. Vann Woodward, one of the historians Fogel labels "traditional," seems to me to meet nearly all of Fogel's tests for "scientific" history (Woodward, 1968). It is based on an explicit theoretical proposition (summed up in the contradiction to William Graham Sumner's slogan "folkways make stateways"); focuses on collectivities of people and, at least potentially, on recurring events (the ebbs and flows of racism); may draw on quantitative as well as "literary" evidence (see Kousser, 1974); can be verified or disconfirmed (see Woodward, 1971, chapter 9); and has certainly been controversial. If Woodward writes well enough to command an extensive lay audience, that is hardly a grave enough sin for which to exclude him from the band of "scientists." Second, the proposed tags are hardly value-free -- the modern world's distaste for traditional ways and its respect for science or for analytical minds cannot escape the most obtuse observer -- and, in fact, the names may express a standard to rally around or a set of likes and dislikes as much as they do a definition.

Since no one name seems entirely satisfactory, and since arguments about definitions pall rapidly, perhaps the best solution is to combine them. Quantification is certainly an important distinguishing feature: nearly every important set of ideas in the subdiscipline can be expressed in mathematical symbols or tested with statistics, and an emphasis on quantification excludes psychohistory and a great deal of conventional history. But it is numbers and symbols wedded to explicit, logically consistent, and fairly broad hypotheses or full-blown theories which divides the older and newer approaches.[7] That is to say, it is the combination of quantification and theory which is central. And nearly all the theory and statistical methods have been and will no doubt continue to be imported from other social sciences. So unwieldy a composite definition is perhaps best expressed in an acronym, QUAntitative Social Scientific History (QUASSH), which also reflects the rather argumentative style of its practitioners.[8]

Judging the health of an adolescent is no easy task. We expect to see growth, a deepening of experience, signs of maturity, but the youth may still be awkward, somewhat raw, his development uneven. A review of three areas drawn respectively from political, social, and economic history confirms that QUASSH is a normally developing adolescent. I shall focus on the so-called theory of critical elections, recent analyses of past geographic mobility, and treatments of the postbellum Southern U.S. economy.

The notion of critical elections had two sources. V. O. Key, Jr., a political scientist, noticed patterns of stability broken by sharp and steep discontinuities in New England election returns, and, a few years later, Lee Benson applied the concept of business cycles to New York elections (Key, 1955; Benson, 1961). The idea which both shared was that voters usually went along following ingrained patterns of behavior, nearly always voting for candidates of the same party in successive contests, until either a growing pattern of strains produced a political earthquake or an extra-political cataclysm such as an economic depression forced electors from their habitual political abodes. American political history was sliced into five normal periods by the presidential elections of 1800, 1832, 1860, 1896, and 1932 -- that is, by the coalescence of national parties and the triumph of the Jeffersonians, the emergence of mass politics inspired by Jackson, the sectional split over slavery, and two depressions (Chambers and Burnham, 1967).

As a way to organize data and inspire research, the concept of critical elections has been of considerable importance to political history. It has focused much closer attention on election returns than historians previously gave those invaluable records of mass behavior and political attitudes, and encouraged students to learn and employ more sensitive statistical techniques in their analysis and to think more deeply about long-run changes in the electoral process (see, e.g., Burnham, 1965). Yet it is severely flawed on five counts. In the first place, it contains no well worked-out microtheoretical mechanism, though some could be proposed. Why should an individual elector continue to vote the same way in successive "normal" elections, what causes voters to shift massively at one time rather than another, and when many do shift, why don't all move uniformly? In other words, what, in this theory, motivates various groups of individual voters to act as they do at various times? In the second place, the theory is operationally vague. How large a shift has to occur to make an election "critical"? How much backing and filing can go on before a "stable" period has to be ruled "unstable"? Suppose successive elections produce "large" shifts, but for apparently different reasons, or suppose there is no trend in the party balance in two or more upheavals. Does calling these sets of elections a "critical period" do more than disguise our ignorance? In the third place, there are difficulties in the choice of statistical methods. Most investigators have used correlations, regressions, or the analysis of variance to uncover trends and break points in a time series of elections. Even ignoring the "ecological fallacy," which haunts the correlation of returns from different areas, the techniques have led to several problems: one index may lead to different conclusions than another; since areal election returns are usually highly correlated from year to year, the

criterion of statistical significance is of little help in determining differences in the level of association; and none of the techniques, at least as employed by historians so far, allows the observer simultaneously to discover the extent of shifts and the make-up of the groups who change their behavior. In the fourth place, the connection between critical elections and policy, which is, after all, presumably the chief end of electoral activity, is vague (Burnham, 1970, pp. 175-93; Lichtman, 1976, pp. 339-41). How soon after a critical election should policy be expected to be altered? What determines the extent and direction of the policy realignment? How and why does policy in areas of major concern to the electorate move about during eras of electoral stability? In the fifth place, the data increasingly seems not to fit the theory. Returns from many states do not follow national trends (Benson and Silbey, 1978) or vary widely from state to state (Fischer, 1964). "Normal" periods appear a good deal less constant than seems consistent with the notion of a neat stable-unstable dichotomy (Benson, Silbey, and Field, 1978, p. 88). "Critical elections" get strung out into "critical periods" as long as twelve years, and even then, close observers find that alignments on either side of a critical period resemble each other more than they do elections within the critical period (Shover, 1974; Lichtman, 1979, pp. 206-07). Clearly, the notion of critical elections by now raises more problems than it settles, and historians need a new synthetic idea.

If a perusal of voting returns led Key and Benson to the concept of critical elections, it was Stephan Thernstrom's conning of individual census and city directory entries which opened up the problem of geographic mobility. Engaged in a study of historical social mobility in Newburyport, Massachusetts, Thernstrom (1964, pp. 85-90, 96) found that he lost a considerable portion of his subjects from decadal census to census. Plausible death rates could account for only a small portion of them; name changes were minimized, since he traced only male heads of households; misrecording of names might explain a few more, but the town was small and Thernstrom was careful and thorough. The conclusion became inescapable that many people -- perhaps as many as two-thirds of the working-class in ten years -- had simply moved away.

Thernstrom's striking finding led immediately to a rash of similar studies organized around the questions of the extent of geographic mobility and the reasons for moving (Thernstrom, 1973, pp. 222-26). Were residents of Newburyport singularly mobile? Did the extent of geographic mobility vary with size of place, region, length of settlement of the area by Europeans, prosperity of the place of residency, or chronological time? Within each area, did the proportion of movers and stayers vary by race, ethnicity, wealth, education, or occupation? Could computer programs be developed which would accurately link entries in different years, even of those persons with common names? Often collaborating, nearly always employing a computer to simplify tasks and avoid boredom, certainly aware of each other's work, a large number of historians attacked these issues. In general, they found decadal mobility consistently high -- usually from 40 to 60 percent -- in all types of areas and throughout the nineteenth and twentieth centuries in America. Stability varied positively with social status in the nineteenth century, and somewhat negatively, though perhaps not linearly, in the era since 1945. Adequate record linkage programs were written.

Yet advances in the field have slowed in recent years, and several major problems remain. First, there has been no attempt to construct or to import from other disciplines a rigorous and extensive model of why individuals move. Second, no one to my knowledge has solved the mystery of how to determine where they went. Was there, as Thernstrom has suggested, a "permanent floating proletariat" in the nineteenth century (1973, p. 42)? Third, despite the application of sophisticated multivariate statistical techniques to the analysis of social mobility in sociology (Duncan, 1979; Goodman, 1979, and earlier works cited there), there have been few attempts by historians to disentangle the correlates of mobility in order to determine whether the relation of ethnicity, say, to geographic mobility was real or spurious (but see Weber and Boardman, 1977). Fourth, it has been suggested that the extent of mobility has been overestimated because the areas studied have been so small, that if the territory investigated were extended to include outlying suburbs, adjoining counties, perhaps even states, the measured rate of mobility would diminish considerably. Scholars who make such an argument, of course, believe implicitly that relatively short moves shouldn't be treated as equivalent to longer moves. Yet such an extension would make the study of mobility incredibly time-consuming, even using indexed census material. Furthermore, the difficulty of linking records would become increasingly severe as the area expanded, and it would become much more difficult to explain these phenomena satisfactorily, for researchers would need a theory and a set of operational definitions which not only accounted for the decision to move or stay, but also for the decision to relocate at different distances and in different types of surroundings. Perhaps this theoretical challenge is implicit even in the current formulation of the research task, but attacking the full-blown problem does seem to require an unreasonably large step forward. Fifth, the implications of the reported degree of spatial mobility for social history haven't been entirely elucidated, and for political and economic history, haven't been faced. In a "humanistic" sense, what did this apparently massive churning of humanity mean in an individual's or family's life?

The unfortunate consequences of the compartmentalization of the sub-fields of QUASSH can be illustrated by a brief view of recent work on the postbellum Southern economy, centering on the most ambitious and provocative study, Ransom and Sutch's <u>One Kind of Freedom</u> (1977). There are two main questions in the economic history of the South from 1865 to the end of the nineteenth century: What accounts for the regional growth rate, compared to some ideal; and what explains the distribution of income and wealth, again in relation to some hypothetical state? Focusing on a five-state "Cotton South," Ransom and Sutch (hereafter "R & S") claim that the rate of growth was slow, relative to the states outside of the South, because flawed Southern financial institutions "locked in" tenant farmers to the "overproduction" of cotton. While recognizing that emancipation increased black incomes and welfare -- that is, income plus time spent not raising crops -- R & S contend that the increase was less than it would have been had the South not "uniformly" adopted the institution of sharecropping, and had not farmers purchased some portion of their food and other supplies on credit from small-time

merchants who held local territorial monopolies. Southern economic lethargy, they assert, did not result from Civil War destruction, the effects of which were quickly repaired, nor was the racial difference in income received the product of governmental action, of pure racial discrimination, or of the forces of competitive capitalism. Both in a micro and in a macroeconomic sense Southern farmers were "locked in" to too much cotton and too little prosperity until the arrival of that anomalous savior, the boll weevil.

While R & S conducted prodigious and innovative research in quantified census and merchantile records, they largely ignored recent work in political and social history, failed to validate satisfactorily their controversial choices between economic models, and could have benefitted from a good dose of the concerns of traditional historians. Thus, they claim that blacks were unable to use politics to influence postwar economic arrangements because "soon" after enfranchisement, blacks were "effectively disenfranchised" (1977, p. 1). Yet studies by political historians demonstrate that blacks had a good deal of influence during Reconstruction and that they continued to participate in Southern politics in most states through the 1880s and, in several, until the turn of the century (Kousser, 1974). Furthermore, crucial parts of their schema rest on assumptions about sharecropper geographic mobility, but R & S seem to be unaware of the social historians' studies of mobility and they make no attempt to chart the degree of mobility themselves. They contend, for one thing, that the tenure of sharecroppers was too insecure to provide them with an incentive to make farm improvements, a condition which led logically to the continual deterioration of fixed agricultural capital (p. 101). A finding that the rate of tenant farm-to-farm mobility was low, however, would imply that tenants had de facto tenure, and would severely undercut this contention. For another thing, they assert that one reason farmers couldn't switch from one furnishing merchant to another who offered better terms is that the farmer would have to be concerned about being refused credit or even the right to make cash purchases from the first merchant if the second went out of business or was too far away to get to in an emergency (p. 128). A finding that tenant geographic mobility was high, so that 'croppers need not have been very concerned about next year's buying arrangements, would cast doubt on this argument. Most fundamentally, R & S's central proposition -- that local merchant monopolies locked farmers into "debt peonage" and an overconcentration on cotton -- would be undermined if it were found that tenants moved from area to area often, for then tenants could seek out places where merchants or landlords would allow them to grow as much food and as little cotton as they wished to. If there was some entry into the ranks of the merchants, and R & S don't deny that there was, newly established merchants would bid for mobile customers, farmers would not be bound to particular merchants, and competitive, not monopoly prices would prevail.[9] Note that while a finding of substantial farmer mobility would damage R & S's argument, a discovery of immobility would not necessarily prove it, for it would be just as logical to assume that tenants or renters who stayed put were satisfied with their arrangements as it would to assume they were unhappy, but compelled to stay because of debts. The empirical and

theoretical questions thus come down to the same ones as in the rest of the geographic mobility literature -- what was the rate of movement and why did some people move and others stay?

R & S assert that the merchant-farmer relationship should be modeled as one of monopoly, instead of as monopolistic competition, free market competition, or the market plus racial discrimination, as other economic historians have contended (DeCanio, 1979; Higgs, 1977).[10] Further, they postulate an objective function for every individual farmer which would lead to a crop mix which is not "socially optional" -- a condition difficult to square with conventional arguments from neoclassical theory (Goldin, 1979; R & S, 1979). R & S believe that the typical applied economist's procedure -- posit a model, apply it to data, see how well the results fit -- may mislead when there are no better apriori reasons to employ one model than another, and that scholars must choose between models by examining non-quantifiable evidence (Sutch, 1977, pp. 402-03). They therefore seek to justify their preferred models by surveying "contemporary testimony." The problem is that their survey is, perhaps unavoidably, incomplete -- they draw heavily on one non-random sample from North Carolina in 1887 to generalize about the whole South for 45 years. It is also probably unrepresentative of the opinions of the poorest farmers, for tenants were apparently not sought out by the North Carolina reporters and seldom wrote to farm journals to express themselves on implicit interest rates and the terms of their contracts with merchants. The opinions R & S quote are therefore chiefly those of landholders and agricultural reformers. Moreover, the quotations sometimes do not show what R & S wish to prove. For instance, many of the statements decrying the decline in farmer self-sufficiency appear to be as much concerned with regional as with the individual farmers' self-sufficiency, while others seem to reflect little more than a nostalgic desire for independence from the market, or the extraordinary conditions of the depression of the 1890s (R & S, 1977, pp. 151, 161-64). Perhaps a systematic treatment of the "literary" evidence would support the R & S models, but they have not performed it.

Application of the traditional historian's mode of thinking might also undercut R & S's basically static model, which assumes, in effect, uniform fixed parameters from about 1870 to about 1900. While historians revel in variations (Landes, 1978), for instance in different tenure arrangements in sharecropping contracts, economists, tied to simple models, tend to assume uniformity, or at least R & S do (e.g., R & S, 1977, pp. 101-02). If the historians' standard procedure makes generalization less likely and focuses too much on deviant cases, the economists' may prove incorrect generalizations by assumption and put too little emphasis on empirical evidence.[11] In the instance, sharecropping may well have been more allocationally efficient in both the long and short run than R & S claim, and 'cropping contracts may have varied considerably over both space and time. The existence of possible variations is an empirical question, not one to be dodged by supposition, as they tend to do. In sum, although R & S have raised the study of postbellum Southern economic history to new levels, there is plenty of work which needs to be done on the subject by all types of historians before any particular picture of that economy can be fully accepted.

History is, indeed, different from the other social sciences, but it is less different than it used to be. By this time, QUASSH has produced so many fresh studies with important implications for existing historical interpretations -- only a fraction of which are reviewed here -- that the subdiscipline can no longer be ignored by the profession, nor is it being ignored (see, e.g., Fogel, 1975; Woodman, 1977; Kousser, 1980). At the same time, devotees of QUASSH, I hope I have shown, neglect mainstream historical skills and the works of other QUASSH and non-QUASSH historians only to the detriment of their own work. While they must retain their interest and keep up with the latest literature in allied non-historical fields, QUASSH historians cannot profitably segregate themselves into ghettoes labeled "economic," "social," or "political" history, or even into a consolidated subdivision combining quantitative workers in all three. They cannot return to their individual social scientific homes and retain their effectiveness as students of the past. The only choice which will maximize welfare is for the immigrants to retain the ethnic customs of their social scientific homelands, yet at the same time fully integrate into history.

FOOTNOTES

1. Compare the initial popular and scholarly reaction to Fogel and Engerman's Time on the Cross (1974) with that to the "social savings controversy" (see Fogel, 1979a) or to Ransom and Sutch's One Kind of Freedom (1977). All three aroused controversy among economic historians, but only TOTC, whose conclusions ran counter to current opinion on many points, was much noticed by the general historical community. See Temin, 1979, p. 57.

2. Of course, historians, especially economic historians, have always counted or used implicitly quantitative phrases such as "more," "less," "most," etc. But the rapid development in social science theory and statistical methods in the postwar era and the continuing revolution in data processing technology have given a qualitatively different cast to quantitative history in the last two decades.

3. For a similar response, see Woodman, (1972). The "sectarian" epithet is in widespread use. See, e.g., Hexter, (1972), p. 386.

4. It is possible that the addition of the modifiers "explicit" or "self-conscious" to "theory" might save a theory-based definition. (Davis, in Taylor and Ellsworth, 1971, pp. 106-07; Landes and

Tilly, 1971, page 10). I see three difficulties with this approach: First, the "theory" may be so vague as not to yield any very precise predictions. Second, a theory may be advertised, but not really be integral to the attached analysis. Third, a theory or generalization may be used self-consciously, but implicitly -- I suspect that nearly every scholar does so from time to time to avoid tedious repetitions or needless distraction of his audience.

5. Obviously the "new" names ("new economic," "new political," "new social" history) are unsatisfactory, for first, the initial innovations occurred a generation ago and second, the titles convey little information.

6. Two of the first introductions to the sub-field emphasized the word "quantitative" in their subtitles. Those titles did not contain the words "theory," "social science," or any variants. See Price and Lorwin, 1972; Aydelotte, Bogue, and Fogel, 1972. Similarly, the titles and subject-matter of two major articles in The American Historical Review emphasize the "quantitative" theme at the expense of "Theory." Aydelotte, 1971, chapter 2; Erickson, 1975.

7. For fuller treatments of the links between methods and theory and the necessity for political historians to form more fully adumbrated and logical theories, see Kousser, 1976 and Kousser, 1979.

8. The acronym is the invention of my colleague Daniel J. Kevles, at the end of a long day.

9. No economist could seriously contend that the 8000 country store owners of 1880 could collude to exploit tenants. If one merchant were profiting above the market rate, it would always be in the interest of another merchant, perhaps a new entrant in the area, to shave prices or contract terms a bit in order to draw in customers. Once this happened, and the fact became known -- and surely such valuable information would travel rapidly on market or court day, when farmers thronged to country towns -- other merchants would have to match the first merchant's bid, and so on and on until profits were driven to "normal" levels, unless the farmers were hopelessly tied to particular merchants. If there was local merchant collusion in one place, then poor tenants with few worldly possessions could easily move to areas without such cartels.

10. This situation reflects on McCloskey's definition of cliometrics as the application of economic theory, for here it is precisely the question of which of several competing economic theories applies best which is at issue. The choice between theories which plausibly apply is an empirical matter.

11. I do not mean to imply that economists always avoid dealing with messy cases or that historians never openly generalize, or that either tendency is necessarily bad. I do believe that each group has a different tendency, and that in this work R & S strike the balance too far toward the side of generalization by assumption.

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