

History as past sociology in the work of Samuel P. Hays:

A review essay

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This tastefully produced collection of sixteen essays, nearly all published previously, but in widely disparate journals, plus a long autobiographical introduction and a brief epilogue, affords an opportunity for evaluating the first two decades of Samuel P. Hays's contributions to American political and social history. Historians excel in different genres. Some are most proficient in the research monograph, some in the popular book or article, some on the editorial chair, some on the lecture platform, some, whose talents remain largely hidden from the professional community, in the small class or tutorial. Hays's *métier* is the provocative, speculative essay. And while it might be feared that this form of scholarly communication would date more quickly than others, in the case of Hays, at least, the essays remain fresh. Indeed, their grouping here encourages the reader to make connections between arguments and to realize the larger significance of points he may have missed or bypassed when he first perused the papers under separate covers. It is a book to muse over, scribble on, steal ideas from, rave at—in short, a book designed to stimulate thought.

All historians' vision—the problems they concentrate on and the facets of those situations they notice, if not so directly and inevitably the solutions they propound—is blindered by personal experience. Born into a family which had moved to extreme southern Indiana early and then largely stayed put for six generations, Sam Hays went off to Swarthmore and Harvard and spent his teaching career in the midwest, but not in the Hoosier State. To someone with such a background and life course, a fascination with genealogy came as naturally as the realization that citified "progressivism" and drives for "moderni-

zation" represented not just objective responses to changed conditions, but value judgments no more deserving of unthinking acceptance than their opponents' resistance to reform was. Robert Merton's contrast between parochials and cosmopolitans was, for Hays, lived experience. A World War II conscientious objector, Hays was no reactionary adopting and adapting Lee Benson's ethnocultural thesis in an attempt to put down economic determinism. But neither was he born to urban liberalism, as were Richard Hofstadter and Arthur Schlesinger, Jr., Hays's intellectual nemises, and their main project—describing and criticizing modern liberalism from the inside, with sympathy—differed profoundly from Hays's no less grandiose task, understanding the conflicts between those who sought to impose what the reformers claimed were universalistic values and methods of organization on others who clung to their own very different beliefs and styles of life.

Hays's method of attack, his informal philosophy of history, was shaped importantly, he remarks (p. 5), by his immersion in the *gestalt* approach as a Swarthmore undergraduate psychology major. Much more inductive and nonmathematical than other schools of social science, but much less wedded to impressionistic methods and to an emphasis on "irrational" explanations for behavior than psychoanalysis is, *gestalt* psychology's most familiar work has explored shifts and differences in perception, pervasive themes in Hays's work. As a historian, he has, in accordance with, if not on account of, his adherence to his early training, emphasized "concepts," simple figures emerging from a ground, rather than more fully articulated, aprioristic "theories," and distrusted en-

thusiasm for complex statistical techniques as much as he has been chary of nonrational approaches to studying human actions.

Hays absorbed the middle-level sociology and political science of group behavior, but neither social psychology, nor the grand theories of Talcott Parsons or Karl Marx, nor the mathematical sociology of Otis Dudley Duncan or Leo Goodman, nor the economics-oriented literature of "positive" political theory, through individual reading. How different social history might have been if Hays had majored in math, economics, or literature, or supped at some other sociological or political scientific tables! Thus, although his intellectual experience is more eclectic than that of conventional historians, Hays's condemnation of "narrow technical training and work" in graduate school, which he thinks may cause historians' minds to become "confined and rigid much too early" (p. 44) is unreflective. Both breadth and narrowness are inevitable; the questions are always which of the many possible subjects to study and how deep to go into any one of them, and in these matters, many will reasonably disagree with Hays's example and prescriptions.

The substantive essays contain hardly a sniff of the monograph. Traditional empirical historians, to exaggerate a bit, proceed by picking a topic, immersing themselves in the relevant collections of manuscripts, official documents, and newspapers, and then and only then consciously putting what they've found into patterns; most "new" empirical historians' procedures are much the same, except that quantifiable data supplements conventional sources. Hays's habit, at least in the production of these papers, is quite different. He reads some secondary works, thinks out their implications, applies the resulting ideas to small case studies himself or assigns the projects to students, and then generalizes the findings rather broadly, if usually tentatively. Thus, having noted that municipal reformers often took the business corporation as a model and boasted of the support of chambers of commerce, and that James Weinstein had shown that businessmen were behind many of the moves to adopt commission and city manager government, Hays looked at the struggles over the adoption of the city commission in three Iowa cities and suggested that similar clashes between upper-class, often prohibitionist centralizers and lower class, often "wet" and immigrant subcommunities probably lay behind conflicts over municipal reform throughout the country (pp. 61-62, 205-32). The empiricists' procedure is to research first, ask questions later; its virtue is solidity, and its fault, lack of clear direction. Hays's technique is to contemplate first, throw out suggestions, and hope that the research gets done sometime by somebody; its strength is stimulation and its shortcoming, which he recognizes (p. 50), is incompleteness. In a particularly provocative passage, for instance, Hays asserts that "Too complete immersion in evidence may well dull the historical imagination so as to obscure other possible ways of looking at the past, and may require a complete shift from evidence into more abstract concepts

in order to free one's imagination for a fresh set of observations" (p. 110).

If free, Hays's imagination is certainly not undisciplined. Somewhat surprisingly, this book reveals that his point of view bears a striking resemblance to that of the older *Annales* school, the sociological, or perhaps geographical, approach of Braudel rather than the currently trendy "anthropological" bent of Le Roy Ladurie. Without citing any of their works, even in his discussions of family reconstitution, presumably because he came to his conclusions independently, Hays shares their distaste for what both he and Braudel call "event history" (p. 116), static social science, and complex quantification (pp. 128-29). Although somewhat more concerned with explanation than the French are, Hays is equally skeptical of large or highly abstract theories. "Concepts without reference to concrete cases lead to irrelevant abstractions; data development without conceptual guides produces a *mélange* of unrelated and insignificant facts" (p. 180). Analytical history, not economics, is for Hays, as for the *Sixieme Section*, the preeminent discipline in the study of society.

Like the *Annalistes*, Hays sees history as an "attempt to reconstruct the *process* by which societies change over time. The emphasis is on *society as a whole*, not isolated segments of it, and broad changes over time, not episodes" (p. 129, my italics). Despite the fact that his own major published monographic research treats only the period from 1880 to 1920, Hays seems to aspire to uncover the *longue durée*: "The historian is uniquely concerned with *long-run social change*, change over not just two or three years but over decades and centuries" (pp. 145-46, his italics). If he implicitly rejects the French dogma that politics is too transitory and superficial to be worth attention, Hays considers political history interesting and useful chiefly for the light it throws on social values, structure, and processes.

It is from this nearly *Annaliste* platform that Hays launches his telling critique of the old political and social history. Ironically like Charles A. Beard debunking the Founding Fathers, Hays castigates what he terms the "liberal" or "reform" view of American politics, in which "the people" confront "the interests" as normative, rather than objective, and as misleading, because it fails to cut through rhetoric to expose the clashing social values of competing groups (pp. 68-70). Conservation and municipal reform, for example, were not struggles between good guys and bad guys, but between sets of people with markedly different *mentalités* (pp. 215-16). This failure of "liberal history" to identify the opposing groups' outlooks correctly, Hays contends, paradoxically makes the chief scholarly proponents of a "conflict" view of American history into "consensus" historians, since they assume that all "the people" shared an ideology, which "the interests" opposed not because of disagreements over ultimate goals, but on account of immediate, naked self-interest (pp. 68, 148).

Adopting Thomas Cochran's influential censure of the

"presidential synthesis," Hays also finds fault with previous historians for ignoring politics at the "grass roots" (pp. 53, 66-67). Concentrating only on who won each election, rather than what the votes reveal about the distribution of popular attitudes; overly concerned with single events, rather than with larger structures and processes; basing their analyses too much on words and therefore putting too much stress on such national issues as trusts and tariffs, rather than founding their conclusions on analyses of voting behavior, which, Hays believes, show that voters were much more worried about such long-term local conflicts as those over prohibition, the "reform" historians have, according to Hays, fundamentally distorted what actually happened (pp. 54-56, 63, 78, 88-92, 115-16, 137, 149, 365). Had they looked more closely at local scenes, he contends, historians would have put much greater emphasis on continual ethno-cultural conflicts and less on changes in control at the national level, which he thinks were less important to the electorate (p. 85). Furthermore, the "reformers" (as well as some of Hays's students, a fact that he fails to point out) have erred in treating the extension or decrease of governmental activity as a single phenomenon, instead of realizing that a particular group may want state control expanded in some areas at the same time that it desires government to leave other behavior alone (pp. 56-59).

Hays also indicts political historians on three counts of formalism. Mesmerized by government, they have neglected to extend the study of power relationships to nonofficial activities, as, for instance, those in educational, social, and economic groups and institutions (pp. 67, 88-91, 98). Transfixed by laws, they have failed to step back and consider the larger trends which complexes of laws represented, for example, in twentieth century America, tendencies toward centralization, systematization, the imposition of cosmopolitan values, and the use of scientific or technical criteria in management and problem-solving (pp. 76, 81, 85). Wont to speak of such "broad forces" as "progressivism," they have played down "the peculiarities of the human situation," therefore depersonalizing and distorting their histories and giving them the character of teleology, rather than of group struggle (pp. 51, 110-11).

Yet when they have looked at groups or individuals, Hays faults them for other errors. Recognizing that the study of politics may properly involve treatment of elites, he takes to task some previous historians who have carried out collective biographies, such as Alfred Chandler, Richard Hofstadter, and George Mowry, for generalizing on the basis of flawed research designs (pp. 71-73, 206-8). Moreover, he charges that biographies of single figures, which at least used to be the prime genre in political history, almost inevitably beg the question of their subjects' importance and of the relationship between the life and the times (pp. 92-93). About the only hope for political history, as Hays sees it, is to die and be born again as social history.

Not that he is uncritical of social history, either. An "amorphous" bundle of topics which did not fit under the traditional political, economic, or diplomatic rubrics, social history has lacked "conceptual framework[s]." "While giving rise to new content, social history has failed to develop its promise of bringing order into the subject matter of history as a whole" (p. 133). Shackled to a "progressive" point of view dating from its beginnings in the 1920s and 30s, social history has "absorbed the 'problem-policy' perspective of social reform," treating members of disadvantaged groups as "problems," but rarely analyzing the problem-solvers themselves, taking the explanations of reformers, which are actually quited colored, at face value (pp. 138-41, 205-6). The same ideological bent has led social historians to misclassify the forces of change and those of resistance. For, Hays asserts, while the upper class, often through the private business corporation, has taken the lead in "sweeping away old institutions and creating new," and the lower strata have been more conservative, more resistant to change, social historians have usually seen things the other way around (p. 159). Partaking of some of the "formalist" traits of political historians, moreover, social historians have devoted too much attention to organized groups (for instance, labor unions), and too little to less structured ones which left poor records.

Nor does even the "new social history" escape criticism. Studies of geographical mobility have overstated the flux and chaos of American society because they have given too little weight to the fact that movement took place within a set of relatively fixed social structures and according to a patterned life cycle process (p. 192). Young men moved, but settled down by middle age, and even when they traveled, the social structures in the cities, towns, and rural areas in which they relocated closely resembled these from which they had come.

But Hays is not merely a critic. Implicit in his critiques and explicit in other essays is a sketch of a general "conceptual framework," modernization, which he has developed to take the place of the "reform" orientation's description of American life over the last century (p. 431). Conservation and municipal reform are Hays's chief examples of the process by which America has become more organized and integrated, and more subject to the control of a scientific and professional elite, an elite which composes a new upper class and is allied with people whose privileged positions derive from wealth. Functional organizations of kindred workers and professionals, planners and economic interest groups have replaced geographically based political parties and local business concerns as the prime movers in the public and private policy arenas. The greater complexity of the economy and the society and the increased geographical scope of decision-making units have heightened the importance of control over the means of communication; conversely, those with expertise in or access to wide-scale communications have sought to extend their influence by transferring authoritative power from lower

to higher levels of government. Thus organizations have a built-in drive both to expand and to centralize, and their dynamics give rise to a series of continual conflicts between cosmopolitan experts and those parochial forces who can exert their maximum influence only if decision-making authority remains at lower, more geographically restricted levels of government (pp. 247-61). By attending to and largely accepting the rhetoric of the intelligentsia, historians have both distorted the nature of the conflicts and overlooked these broader and more important trends.

Hays asserts that the control of municipal governments in the United States, for example, has gone through three stages over the last century and a half. In the pedestrian city, local notables easily dominated ward and town meetings. The initial stages of suburbanization, which encouraged middle-class flight from the urban core, increased the city's class and ethnic homogeneity and left many city wards in the hands of small businessmen and "machine" politicians. But continued population growth put a premium on influence over the technology of mass communications, and this development, coupled with the widespread switch from ward to at-large electoral systems, restored the privileged to urban dominance, a process which did not go unnoticed or unchallenged by those whom the new system displaced (p. 345).

While his criticisms have alerted historians to unexamined assumptions, illogical arguments, and counterproductive limitations on data sources which have flawed previous work, and while his often fresh and cogent suggestions have helped to shape both the new political and the new social history, Hays's program for historical studies seems to me to contain fundamental deficiencies, many of them similar to those which he has pointed out in the work of other historians. "Concept" is one of Hays's favorite words. It or its variants occur, for instance, six times on page 433, and probably several hundred times in the book as a whole. Unlike "theories" or "hypotheses," which invite tests, suggest falsifiability, are inherently explanatory statements, "concepts" or "conceptual frameworks" are purely descriptive, are mere rubrics, organizing devices, interpretive stances. Calling Gifford Pinchot a cosmopolitan, or a branch of evangelical religion "modern" or another "fundamentalist" merely affixes labels to them. To name is not to explain. Hays's preferred type of history leaves the causes out, and that is, to me at least, unacceptable.

Several of the specific concepts which Hays employs, moreover, seem teleological, even Hegelian. Hays does not use "modernization" normatively, as others have, but he does see it as linear, unitary, and probably irreversible, and he reifies it. On page 286, for instance, we read that "The educational system, therefore, provided an instrument for modernizing forces to work their way out. . . ." On pages 246-47, we learn that "In the modern technical system, however, there is a dynamic, *self-generating and self-sustaining*, which embodies

the *spirit* of science, empirical inquiry, and planned environmental manipulation," and on page 249 that ". . . the long-run tendencies in system organization did not lean *naturally* toward decentralization and smaller units of organization, but, on the contrary, toward larger and larger units of control . . ." (my italics). But systems don't act; only people do. To attribute will or tendencies or spirits to disembodied notions is simply nonsensical.

Sometimes like Hegel, Hays at other times resembles Marx upending Hegel. If he does not reduce ideology to the status of a mere superstructure, Hays does believe that a political movement's stated intellectual positions reveal little about the nature of that movement. "The peculiarities of a political movement are rarely observable from the political ideology. . . . They can be determined, however, from the way in which the people involved in that movement differ from others in the political structure" (p. 100). Prohibitionism and temperance, for instance, which he believes were strongest in the upper class during the 1840s and 50s, the middle class from 1865 to 1890, and rural dwellers afterwards, were merely "instruments of social control," attempts to impose one group's general cultural predispositions on another set of people (p. 90). Dry rhetoric about alcohol abuse offers no more of a key to the cold-water movement than progressive pronouncements do to that crusade. Similarly, in designing undergraduate history curricula, Hays opposes organizing courses around ideas and would delete "art, literature, and philosophy" from history courses because students tend to divorce them from life and to consider them as "artifacts to be studied for their own sake" (p. 404).

Yet to subtract the study of culture from the study of cultures is surely to render each less interesting, and, more important, incomplete. That history students often believe ideas have lives of their own or at least that thoughts have consequences suggests that people in the past may have felt so, too, and that they may have acted, at times, in response to ideas. Furthermore, Hays's crude sociology of knowledge approach mistakes ideologies for mantras. He apparently believes that if ideas mean anything, they don't mean at all what they say. He rejects, for instance, the conservationists' self-conception that they were interested in controlling development in order to preserve forests, wilderness, and other nonreplenishable resources in favor of the view that the movement was "a phase of the impulse in modern science for precision, efficiency, order, and system" (p. 235). Yet even if he were right, his analysis wouldn't answer the question of why systematic thought bubbled up when and where it did and why it took the form of the conservation movement. Further, if there were any way to decide that his position was more correct—and he presents none—it would not follow that the leaders' expressed ideas were irrelevant to understanding the movement, for their followers may have been attracted by the overt, not the allegedly latent ideology. Likewise, white ribboners may have just

wanted to save themselves and others from the horrors and consequences of drunkenness. Conclusions on such matters require a more sophisticated research design than Hays offers. The place of ideas in politics is too complex a question to be settled merely by assertion or assumption.

Politics for Hays is merely a corollary to society's theorem, a logical straightforward derivative of differences in subgroup cultures. Rhetoric is largely symbolic, the act of voting, predominantly an expression of a locally oriented, group-fostered *mentalité* rather than an instrumental or rational choice among candidates who take different positions on issues (p. 157). This behaviorist view of politics, which has come under increasing attack by the "rational choice" school of political scientists, who have access to surveys of the twentieth-century electorate, may more closely fit the nineteenth century, but it certainly deserves closer scrutiny than Hays or any of the other ethnoculturalists have given it. A glance at "rational actor" models in political science suggests, too, that Hays places too little stress on the role of elected officeholders and office-seekers in shaping the voters' choice sets and tastes and in organizing them into blocs. While his studies do, commendably, span both the electoral and the policy areas, he neglects courts and legislatures and consequently gives too little notice in his general remarks on politics to the differences which variations in institutional structures have made. Pictures of politics which blot out its societal background are surely defective, as the thrust of Hays's work clearly demonstrates. But portraits which leave out major elements of the foreground are also disfigured.

Hays's attack on what he terms "hard quantifiers" reflects a position sufficiently common today as to deserve special attention. HQs, he thinks, have chosen problems not on the basis of inherent interest, but on the ground of data availability; have sought to apply the "precise theories" of static social science to the messy dynamics of long-run change; and have underemphasized research design and the inductive logic necessary to link data to concepts and overemphasized formal statistical training. No such formal training is necessary, he claims; a few "ad hoc 'minicourses'" have done the job for the graduate students at Pitt. A "scientific" approach to history is insufficient; it must be joined to Diltheyan "imaginative reconstruction" to form the true historical art of "informed speculation" (pp. 36-43, 376-79).

While some of these criticisms are partially correct, none is so well taken as to preclude a response. To be sure, HQs are as attracted by a sweet data set as more traditional historians are by an especially informative manuscript collection. But, lacking readily available figures on subjects of interest, HQs have also often developed them in ways and from sources that no one had thought of using systematically before. The reconstitution of European families from parish registers springs to mind, but no less innovative have been Richard

Easterlin's estimates of regional income or Robert Fogel and Stanley Engerman's use of New Orleans slave market records and plantation birth registers as evidence about young slave women's sex lives. In such cases at least, a concern with quantification, far from restricting inquiry, has spurred innovation.

It is true that most social scientific theories lend themselves to comparative statics, but are not inherently dynamic, that neoclassical economic models, for instance, are not designed to accommodate major shifts in tastes or institutions. But the problems with which historians are concerned are most often of exactly the comparative static variety, and, if other things can be assumed to be constant, available structures of understanding can readily accommodate discrete changes in tastes or institutions. Consider some examples from Hays's own work. Based on stable local group loyalties reinforced by the repetition of similar issues in campaign after campaign, voter behavior during most of the nineteenth century, he asserts, was nearly unchanging. The rearrangement of the 1890s involved no alterations in group mentalities, but merely changes in the actions of elites. Nothing in this scene requires moving pictures; a sequence of snapshots with the same focus would capture it well enough. Or consider what economists would call the "stylized facts" he offers regarding the adoptions of at-large voting schemes and city commission and manager governments. In this instance, Hays postulates a shift in institutions, but none in ideologies, and his model is of the comparative statics variety. The goals of ward politicians and lower-class ethnic groups on the one hand and upper-class cosmopolitans on the other remained constant, he believes, and to test whether the institutional shift differentially disadvantaged the former party, he tries to measure the power of each at two points in time, before and after the adoption of the new electoral arrangement. Finally, let us look at the early twentieth-century conservation movement. Conflicts occurred because a new elite of engineers and scientific managers arose to challenge the previously unassailed power of local exploiters of the environment, and the new elite propagandized effectively enough to create a new body of conservationist opinion in the populus. The outcome of each struggle was determined primarily by the institutional level at which it was fought. Now, while static models cannot easily account for the rise of a new elite or the growth of conservationist opinion, neither can any hypotheses Hays proposes. Given these changes, pictures of decision-making which postulate maximizing behavior on the part of the actors can model the clashes and predict the outcomes of the conservation battles. In general, so long as some elements of the sets of actors, tastes, and institutions can be assumed to be roughly fixed, which is nearly always true in historical investigations, static models can shed a great deal of light on the matters to be explained. The search for "dynamic" models, usually a fruitless quest, is also usually an unnecessary one for historians.¹

That some HQs have computed first and analyzed only

later and then rather mindlessly, just as nonquantifiers have sometimes read first, explained only afterwards, and generalized faultily, is unfortunately correct. But although there is no substitute for a well-thought-out plan of study and often no way to salvage a poorly laid-out project, a ritualized call for more concentration on research design may well only distract historians from deeper difficulties. The questions are how to go about setting up a problem properly, whether attention to technique and to design can be complementary, or, as Hays seems to assume, they are necessarily competitive, and whether a hypothesis can be solidly established through the use of overly simple or imperfectly understood statistical methods. On the first, surely the most desirable way of proceeding is to begin by borrowing or constructing the clearest and most completely specified explanation sketch possible which is relevant to the particular topic of concern, modifying the theory if necessary in light of the results of the investigation. To use the language of Bayesian statistics, historians should concentrate more than they often do on reducing the diffuseness of their prior beliefs. It is deductive logic which is most crucial here, not inductive logic, as Hays believes. In regard to the second question, just having to think about statistical techniques which are based inherently on causal reasoning, such as regression or logit analysis, can often help researchers clarify their hypotheses. To yield unbiased results, for instance, OLS regression equations must be properly specified; to be interpretable, the independent variables and their inherent relationship to each other must be clearly understood. What better way would there be to learn research design than to be required to puzzle out the logic of variations in the form of a complex predictive equation, tracing the arguments for each form back to their theoretical roots! To pose a choice between an emphasis on training in statistics and training in research design, as Hays does, is at best misleading and at worst seriously muddled. The third question answers itself much more resoundingly than Hays seems to believe. Simple eyeball comparisons of a time series of votes in a few townships or wards just will not establish some propositions which Hays asserts are true, such as that nineteenth-century electors responded primarily to local, rather than to national, issues, and that their choices were very stable over time (pp. 157, 298-

99, 308, 364). Nor will data from nonrandomly chosen homogeneous areas prove the ethnocultural thesis, as I have argued elsewhere.² Historical craftsmen who work with quantitative data need to acquire a well-stocked tool kit of techniques, and the period of apprenticeship must certainly last longer than a mini-course.

Finally, it is hard to know what to make of Hays's insistence on retaining "imaginative reconstruction" as a proper mode of historical thought. If he means only that it and "informed speculation" are inevitable concomitants of the process of historical research, then few would quarrel with him. Every student of the past muses about what "his" characters must have done or thought in situations for which there is no direct evidence. If Hays is saying that his own informed speculations in this and his other volumes are stimulating and often suggest fruitful paths for research to follow, then I would certainly not disagree. But if he is suggesting that imagination and speculation alone are sufficient, without firmer evidence, to warrant conclusions in history or other empirical fields, then I must regretfully leave Hays's often-delightful company and rank myself with the HQs.

NOTES

1. Cf. William W. Beach, "A Second Look: 'The Agenda for 'Social Science History,'" *Social Science History* 4 (1980): 357-64. Economic theorists have been working hard over the last decade to develop dynamic models, and they have had some notable successes, of which historians should be more aware. For starters, see, e.g., Burton H. Klein, *Dynamic Economics* (Cambridge: Harvard University Press, 1977); David Cass and Karl Shell, *The Hamiltonian Approach to Dynamic Economics* (New York: Academic Press, 1976); Akira Takayama, *Mathematical Economics* (Hinsdale, Ill.: The Dryden Press, 1974), chapters 5-7. In fact, dynamic arguments have a long tradition in economics. See, e.g., Paul A. Samuelson, *Foundations of Economic Analysis* (Cambridge: Harvard University Press, 1961), chapters 9-11, and references cited therein.
2. "The 'New Political History': A Methodological Critique," *Reviews in American History* 4 (1976): 1-14.