NEW APPROACHES TO SOCIAL SCIENCE HISTORY

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Inside the "Iowa School"

Some years ago I was intrigued to discover that an Iowa School of history had existed at the State University of Iowa during the 1950s and 1960s and that I had been involved in it. Several historians have written about the Iowa School, but my recollection of the history program at Iowa during those years differs somewhat from their accounts. Since there was some pioneering perhaps under way in Iowa City, my view may interest some readers. I shall try to describe and explain the individuals, ideas, and institutional context that influenced me, or so I believe influenced me. The word "inside" carries its own warning; others occupying different vantage points may interpret the story quite differently.

After teaching at the university level for three years in Canada, I moved to the State University of Iowa during the summer of 1952. I was hired primarily to teach the history of the American West and the United States survey but also agreed to carry one of three advanced surveys in American history then taught in the History Department: a two-semester survey covering the years 1781 to 1877. This I would alternate every other year with the history of the West. I understood that graduate students who were interested in the West would appear in my graduate seminar as well as others who were concerned primarily with nineteenth-century social and political history. These assignments remained my responsibility at Iowa until, as chairman of the department from 1959 to 1963, I saw the U.S. survey shifted into other hands. Although my own research lay primarily in the field of western economic history during these years, my efforts to develop an advanced course dealing with nineteenth-century America, and my
work with graduate students in political and social history, significantly deepened my interest and involved me with others in the development of what has sometimes been called the “new political history.”

A period of modest institutional growth following World War II, faculty departures, retirements, and deaths allowed the members of the History Department at Iowa to rebuild the staff in a major way. When I arrived in 1952, the chairman, William O. Aydelotte, had only been a member of the faculty since 1947. Five other active members had longer service, but George Mosse had come only in 1944 and Robert Stuart Hoyt in 1946. Of the three professors whose tenure was of long standing, the director of the Historical Society taught only the course in Iowa history and played little direct role in departmental matters, and the specialist in recent American History died suddenly during the course of registration in the year of my arrival, opening the way for another assistant professor. Then in their early forties, Aydelotte and Stow Persons were the departmental elders with clout, and seven of the twelve department members during the fall of 1952 had been born between 1915 and 1923.

The peculiar age distribution in the History Department at the State University of Iowa during the early 1950s was not a simple demographic accident. Nor, despite the low ranking of Iowa in the Big Ten salary scale, did this age profile merely reflect the lack of institutional resources; until 1950 the administration had approved various senior appointments in the department. Rather, the large number of younger scholars in the department reflected Aydelotte’s conviction that Iowa could compete more effectively for staff of high quality at that level of appointment than in the tenured range. In making appointments at UI in these years the dossiers of candidates were scrutinized and discussed with painstaking care; the process, indeed, was literally agonizing. Aydelotte enjoyed phoning graduate advisers in major institutions and forcing them to compare candidates that they recommended with each other or with recent graduates from their programs. Although the salaries were not high for beginners at Iowa, they were competitive and they were sweetened in addition by the proffer of a lighter load during the first semester of teaching and—an unusual gambit—the newcomer was also placed at the top of the rank order for the distribution of the limited number of research assistants available in the department. These tactics attracted a number of able young scholars, as their later careers demonstrated.

For the young scholar at Iowa the environment was an exciting one. All of his colleagues were deeply interested in research and Aydelotte himself set an example in this respect. He was willing to discuss his own work at great length and at almost any time of night or day if an intelligent auditor could be found. When Aydelotte reported that a piece of research was “not very interesting,” it was damned utterly. Nicholas Riasanovsky in Russian history, Frank Gilliam in Ancient History, and Charles Gibson in the field of Latin America were breathtakingly able but never resorted to one-upmanship. Senior colleagues like Aydelotte and Persons demonstrated that they took teaching seriously, although both deplored shallowness or gimmick. The most successful teacher there at the time was George Mosse who administered the Western civilization program in imperial style and taught the major lecture course in it with an exotic flamboyancy that was a revelation to Iowa undergraduates.

The first departmental committee to which I was named had the task of finding a replacement for Professor Harrison John Thornton, the specialist in recent American history who had died at fall registration in 1952. Stow Persons was the senior member of the committee, although Aydelotte considered himself to be an ex officio member of most recruitment committees. We accumulated a set of impressive files and became particularly interested in those of Ray Ginger, now deceased; a Northwestern Ph.D. teaching at Dakota Wesleyan named George McGovern; and a staff member from the University of Illinois with a Ph.D. almost earned from Harvard, Samuel P. Hays. I had met Hays a few months earlier at the meeting of the Mississippi Valley Historical Association and was impressed by his quick effort to throw the conversation into professional channels when he inquired about interesting “ideas” that I might have encountered during the course of the meeting. A pleasantly homespun young man, there was little of the reputed Harvard brashness about Hays, and I liked him. Approximately a year after that meeting, Aydelotte offered the appointment in recent American history at Iowa to him and he accepted, feeling, I believe, that he was somewhat blocked in his favored field at Illinois by the presence of Frank Freidel.

Thus at the beginning of academic year 1953-1954, the personnel in the American history program was set for the next seven years, with Charles Gibson teaching the American colonial period, as well as Latin American history, Stow Persons offering American intellectual history, and C. Ross Livingston administering American foreign relations, in addition to his major field of British Empire history. Hays elected to teach American economic history as his second advanced field, which foreclosed a possibility that had been held out to me when I joined the staff; but I did not find this restrictive at the time. At Hays’s insistence we also made a self-denying agreement to the effect that he would not direct graduate work in the period prior to 1900 and that I would not supervise dissertations within the twentieth
century, although I always brought my undergraduate lectures in western history into the twentieth century. The personnel and teaching obligations sketched here remained in place until Hays decided to leave Iowa and assume the chairmanship at the University of Pittsburgh at the conclusion of academic year 1959-1960.

II

In my own research during the 1950s I concentrated on the social and economic history of the midwestern frontier. In writing a doctoral dissertation on frontier money lending during the Populist Era, I became convinced of the importance of supplementing literary source material with quantitative evidence and had assembled material of this kind from the business records of individual money-lending firms and from the court house records of western counties. After much agonizing I had developed an income or capital flow method of analysis that allowed me to calculate the returns investors had obtained from their foreclosed lands. During the mid-1950s Margaret Beattie Bogue and I were also applying the technique to the business operations of a number of midwestern land speculators. In all this the advice of my dissertation advisor, Paul Wallace Gates, to work with specialists in related disciplines—in my case economic historians and agricultural economists—had proven to be very beneficial and was a practice that I would urge on my own students.

During 1953-1954 I began, as agreed, to develop a course in the social and political history of the American middle period. Much of the monographic literature in American political history to which I turned during this process impressed me unfavorably; it appeared unconvincing and much too impressionistic. It lacked the kind of precision and power that I thought I discerned in the writing of some economic historians whose work I admired. Like most beginning instructors I floundered in the literature and worried about giving advice to graduate students who wished to write theses and dissertations in the area. I was looking for guidelines and fruitful directions in which research might be developed. Both Gates and James C. Malin, with whom I studied during 1948-1949, had strong research interests in the national politics of the 1850s but in my searching I received more assistance from an old graduate school acquaintance, Lee Benson, than from anyone else.

Entering the Graduate School at Cornell University in the fall of 1946, I had completed all of my requirements for the doctoral disser-

ation in history, with the exception of the dissertation, by the summer of 1948. The History Department at the University of Kansas had kindly allocated a research assistantship to me for the forthcoming year, which would allow me to work under the direction of James C. Malin and use a major manuscript collection at the University of Kansas that bore directly upon the subject of my dissertation. As I was winding up my research in the Collection of Regional History in Boardman Hall, Paul Wallace Gates introduced me to a thin, tall, pleasant young man who was to succeed me as Gates' teaching assistant in the U.S. survey during the coming year. Lee Benson had completed an M.A. at Columbia University and his interests in agrarian radicalism during the late nineteenth century gave us some common ground. Benson completed his dissertation in mid-1951-1952, while at Harvard on a Social Science Research Council Postdoctoral Fellowship studying the economics of regionalism under the general supervision of Professor Walter Isard. During that year he also spent a good deal of time investigating the background and formulation of Turner's frontier hypothesis; and his iconoclastic presentation on that subject to the Henry Adams Club greatly mortified Professor Frederick Merk, who was still carrying the Turnerian banner in the Harvard History Department. The hard times in academia of the early 1950s, plus family concerns, meant a temporary teaching position in New York for Benson, which was supplemented with minor support from the Bureau of Applied Social Research where he had impressed Paul F. Lazarsfeld.

Appalled by the flaccidity of American political history, Lazarsfeld encouraged Benson to try and formulate a more rigorous methodology for American political historians. Although he moved his doctoral dissertation on the background of the Interstate Commerce Law relatively rapidly to publication, I was most intrigued by Benson's work on Turner, his interest in method, and the fact that he was combining the latter with a case study on the background of the Civil War in New York. At the meeting of the American Historical Association in December of 1954, Benson presented a paper, "An Operational Approach to Historiography," which particularly aroused my interest. In general, American historians, he argued, too largely ignored the methodology of history. He maintained that Samuel Eliot Morison's definition of the historian as one who applied "common sense...to circumstances" and Louis Gottschalk's relativist interpretation of historical research were formulas that left much ground for controversy over general conclusions. He contrasted them with "an operational approach," involving a "systematic" approach to the formulation of historical research problems. Follow-
ing Benson’s technique the historian would work by carefully defining a problem, then selecting appropriate data bearing upon it, and finally by choosing the methods of analysis best designed to allow the researcher to obtain the most convincing answer to the questions asked. In illustration, Benson addressed the problem of whether slavery or nativism had most disrupted the political parties during the 1850s, and he used data relating to the State of New York to test the answers given by a number of leading historians. From his analysis of evidence derived from electoral returns and party platforms he concluded that the evidence best fitted the hypothesis that nativism rather than the slavery issue and the Kansas-Nebraska Act underlay the disruption of the national parties in the 1850s. “The codification and improvement of historical procedures,” he maintained, would produce a more successful historical enterprise.  

Benson has explained that his argument for a more systematic methodology at this time largely reflected the ideas of Lazarsfeld and Malin, whose work he cited approvingly in the footnotes. “An Operational Approach” was never published but it stimulated me. The emphasis upon systematic research design and upon the formulation of specific hypotheses that I found was a refreshing contrast to the softness of the monographic literature I had been reading in my course preparation. The illustration of the rewards of relatively simple analysis of voting returns across time impressed me. And the suggestion that party totals changed because the party affiliation of social groups eroded seemed to promise better answers to the questions of why elections turned out as they did than did work in which authors explained party positions in great detail and then took a blind leap toward the ones that they believed had most influenced the electorate. More specifically, Benson’s paper suggested to me that intensive studies of state politics during the 1850s might provide very rewarding dissertation topics.

In 1957 Benson published “Research Problems in American Political Historiography,” an amplification of some of the issues raised in the 1954 paper. However, by this time those of us who had come to know Lee well had heard his major points made and remade in our conversations at the historical association meetings. When, in 1958, he circulated a preliminary version of his New York study that would appear as The Concept of Jacksonian Democracy in 1961, the idea of a politics in which ethnocultural groups were building blocks of party and the agents of political change was already familiar to us through the same channels of communication. Iowa graduate students of the time remember that I talked to them at length about Benson’s ideas.

Aydelotte began an elaborate study of the Corn Laws Parliament during the late 1940s and his plans, procedures, and findings influenced me considerably. He developed his grasp of the methods by which legislative activity could be analyzed by using roll call data as the project proceeded. In the early phases of the study he tried to link the social origins of the members of his parliament to their voting behavior and learned about significance tests and used them to test the strength of the relationships between voting behavior and social background. This phase of the research was somewhat disappointing because Aydelotte was unable to show that the relation between legislative behavior and the background characteristics of the members was any stronger than the bond of party. At about this time Aydelotte met a young visiting professor of sociology, Frederick Waisanen, who explained the scaling technique developed by Louis Gutman and suggested that it might be useful in analyzing his parliament. This proved to be the case and it was clear by at least 1957 that the Gutman scatalog was an analytical tool of formidable power. I wondered whether it could be used to study the U.S. Congress of the nineteenth century.

Keen debate raged during the 1950s about the reliability of Charles A. Beard’s analysis of the motives of the founding fathers. I was fortunate enough to be invited to a seminar of young historians at the University of Kansas during the summer of 1955 where Robert E. Brown presented a series of lectures on Beard’s An Economic Interpretation of the Constitution. He strongly criticized Beard’s methods and findings; the constitution was essentially a democratic document arrived at by democratic procedures, Brown argued, rather than the clandestine product of men defending the economic interests of themselves and their class. In the evenings at a local tavern Forrest McDonald entertained participants with accounts of his adventures in touring the archival depositories of the original states in an effort to do the research that Beard should have done. However, if Beard’s work was challenged, his efforts to prepare a collective biography of the founding fathers and use it as the basis for analysis was not. In 1955 Richard Hofstadter published The Age of Reform and the emphasis that he placed upon the collective profiles of groups of progressive leaders developed by George Mowry and Alfred D. Chandler further encouraged the use of collective biography analysis. And, of course, I was familiar with the Namierite approach and Aydelotte’s efforts to build a comprehensive biographical file for the members of the Corn Laws Parliament. Collective biography seemed to promise much.

During the mid-1950s there were professional and institutional developments underway that were of great significance to young
historians with interests such as mine. These centered in the Social Science Research Council (SSRC) and were at several different levels. Until his retirement Pendleton Herring, the president, regarded history as a discipline entitled to equal time and privileges in the concerns of the board. Although social scientists were becoming increasingly ahistorical in their interests during the 1950s and 1960s, representatives of those disciplines on the board, such as Paul F. Lazarsfeld and V. O. Key, Jr., believed that the historical dimension was of great importance and were willing to defend history as a legitimate object of council concern. Nearing the end of a long career in which he had unerringly argued the importance of political history, Roy F. Nichols had the stature on the council board to see that his discipline would not be forgotten. Young historians benefited particularly from the postdoctoral research training fellowship program of the SSRC and both Lee Benson and I enjoyed such support during these years, as did Forrest McDonald and various others who were interested in cross-disciplinary activity. During the late 1950s and early 1960s the very distinguished Committee on Historical Analysis was at work under the aegis of the SSRC and the chairmanship of Louis Gottschalk. This committee produced a sequel to SSRC Bulletins 54 and 64 dealing with the problems of historical analysis, Generalizations in History. The members included Aydelotte, whose essay in the volume is still approvingly quoted.

In retrospect a minor venture of the Committee on Historical Analysis, the Conference on Early American Political Behavior, held at Rutgers University, June 12-14, 1957, was more important to some of us than the committee’s volume on generalizations. Richard P. McCormick and Lee Benson promoted this meeting and McCormick’s former teacher Roy F. Nichols was probably the key individual in mobilizing support on the council board for it. McCormick’s report on the conference, prepared in collaboration with Benson, appeared in SSRC Items in December 1957 and is a key document for anyone wishing to assess the efforts to reorient political history that were under way at the time. The report is even more revealing if it is read in its original manuscript form because the SSRC editor removed illustrative and explanatory material from it as well as a number of recommendations.

McCormick began the report with a criticism of the narrowness of the political history of the day. Scholars did not view political institutions within their environmental context and they did not relate the national scene to developments at the state level. Political biographies were unduly laudatory and omitted important aspects of political reality. McCormick (and Benson) proclaimed that political history should be viewed as cultural history “in the largest meaning of that term.” They suggested that “the most attractive opportunity, in this field of study ... is for the development and application of concepts that will relate political behavior to the whole body of American culture.” They noted the sadly dated character of much of the literature in political history and concluded that new “studies of ... typical features of the political scene” were essential.

The remainder of the report strikes receptive chord after chord in minds of those familiar with the future development of social science or behavioral history during the next twenty-five years. Historians should “attempt to locate the focus of power within political organizations.” There should be “intensive case studies” of the role of issues in American politics. Scholars should “get behind the election returns” to understand why individuals voted as they did and did not vote. “How did the voter take on a group or party identification?” McCormick asked. “All such investigations should be carried forward with a broad recognition of the total cultural setting.” McCormick reported that in discussing methods the conference considered the possibilities involved in using “detailed election statistics and supplementary data” to carry out “systematic analysis of the behavior of groups of voters.” They also discussed the feasibility of using “comparative analysis” so that “political behavior can be examined operating under different conditions in the several states.”

Whether the SSRC editor responded to shortage of space or whether circumspection, tact, or conservatism guided the editorial blue pencil, we cannot know, but certain passages in the original manuscript were deleted. These included the assertion that “the study of political behavior can provide a basic synthesis for the history of American culture” and the observation that excessive emphasis on the validation of the Turner and Beard theses had unduly narrowed professional perspective. Eliminated also was a statement urging historians to arm themselves with new concepts and to master new analytical techniques. “The rigorous historian will be alert to use whatever tools he can to exploit his materials.” The editor also deleted references to Louis Hartz, Daniel Boorstin, and Richard Hofstadter who were lauded in the original report for introducing important new analytical concepts.

Also struck from the published report was the admonition that “we must constantly be seeking new hypotheses to test, and the current ferment of thought in the field of behavioral or social analysis should provide fruitful ideas to guide historical inquiry.” In discussing the “astonishing variety of behavior from state to state and even from county to county,” McCormick also suggested in the initial draft that a “judicious balancing of studies to reveal all the dimensions of
American politics” was in order, and urged that these studies should be “developmental and dynamic.” From a passage detailing the need for studies of voters, the editor deleted the amplification that such studies “should enable us to perceive what considerations produce political group-consciousness. They should also make apparent cyclical trends in voting behavior and reveal those critical periods when stability was disrupted by readjustment.” Here is perhaps the earliest statement by historians of realignment theory. References to “panel analysis” also disappeared from the manuscript as did comments on the possible usefulness of collective biography and content analysis.12

The editor of Items also deleted a number of suggestions for institutional development that foreshadowed much that was to come. The conference participants recommended that more conferences of the same type be held; they stressed the value of developing a clearinghouse through which interested political historians might obtain knowledge of studies in progress in the field; and they noted that it would be beneficial to establish a center for the collection and processing of certain types of source materials of broad usefulness, particularly voting statistics and party platforms. Finally, they urged that individuals should be encouraged to begin parallel studies within broad cooperative projects. With the exception of the latter suggestion, all of these suppressed recommendations were ultimately implemented in one way or another.13

Even today, SSRC Items is probably read by only a minority of the country’s political historians. As a former SSRC fellowship holder I received the newsletter and no doubt read the conference report with considerable interest. However, the ideas in it would not have been new to me at the time either; they were in effect in the air, discussed in conversations with Benson particularly, at the professional meetings. Of those who attended the Rutgers Conference, Benson, McCormick, and Charles G. Sellers made notable contributions to the new political history. McCormick expressed reservations about quantitative methods at an early date and Sellers’ romance with behaviorism was relatively short-lived, but in later years Nichols reflected the changing currents in political history by writing two books that were much more theoretical and conceptual than his earlier publications.14

My approach to the study of history was eclectic—one of my doctoral candidates of those years called it ecumenical.15 I became interested in problems, issues, or questions in American history and then looked for evidence that would help me find answers and convince me that I understood what had happened insofar as I could. Coached by Gates and Malin, I was prepared to cross disciplinary boundaries in search of either data or methods that allowed me to extract meaning from evidence. Sometimes that search was frustrating, such as when I sat in on statistics courses at Iowa State during a year as a postdoctoral fellow and listened to the exposition of statistical concepts by instructors who illustrated their lectures with examples featuring seed-bed lattices, the use of twin calves in controlled experiments, and the establishment of randomness in tests of the effectiveness of ingredients in lemon chiffon cake. Despite the occasional dead end, such searching, I believe, was usually productive and even the statistics courses paid dividends in the long run when I encountered researchers who were successfully applying statistical methods to historical problems. I was always much more at home in my research when I stayed close to the evidence and I found hypotheses or generalizations of the middle range much more congenial than explanations in which one or two factors were assumed to shape a wide variety of developments. For some years after my arrival members of the department often lunched at the Mad Hatter restaurant adjacent to the campus, and I remember a lunch at which Stow Persons, Hays, and Paul Sharp (then at Iowa State) tossed around generalizations with a casual ease that left me admiring, but also slightly frustrated and envious.

During the middle and late 1950s I was much influenced by Aydelotte’s example, but I was much more in contact with Sam Hays than with any other departmental colleague. Doctoral advisees typically prepared fields with both of us and one of them has recalled the willingness of the Americanists to discuss programs and problems with them irrespective of their declared major field.16

My natural bent tended to complement rather than duplicate Hays’s approach and predilections. None certainly can deny the solid foundation of manuscript research that underlay his emerging book on the conservation movement of the early twentieth century. But I remember an early conversation in which he joked fun at manuscript grubbers who prided themselves on exhausting every manuscript collection appropriate to their research and who enjoyed getting together with others of like mind to compare juicy tidbits, relevant and irrelevant, that they had discovered. Hays was at his best as a synthesizer and he had a great talent for spotting parallel drifts in secondary works and for drawing new and broader meaning from the research findings of others. These he could expound with high enthusiasm. Hays was a serious person. A highly effective lecturer, somewhat clerical in style, he apparently felt occasionally that he did not include enough humorous material in his lectures. At one such moment during 1955 he read me an amusing extract that he was cutting from the Des Moines Register and he explained that it was to be the beginning of a file of such material that he would use to
emphasized his lectures; he placed it in a manila folder labeled “Humor.” In making ready for his successor as departmental chairman five years later, I inspected the office, which had been cleaned after his departure. The filing cabinet still stood beside the desk and I opened the drawers to see that all had been cleared. One file remained; it was headed “Humor” and its sole content was the newspaper clipping of 1955. No doubt, if pressed, Hays could recall similar foibles of his Iowa colleagues as well. But, in general, during the middle and late 1950s, Persons, Hays, and Bogue made a reasonably good team; although the occasional minor abrasion of feelings occurred, these were never serious enough to erode the general sense of respect that we held for each other. In retrospect this state of affairs perhaps reflected the common sense and tolerance of Persons to a greater extent than we realized. Ten to fifteen years our senior in professional experience, he was more tolerant of the naivete and rough edges of his younger colleagues than others might have been.

Certainly in the years of our joint tenancy at Iowa, both Hays and I came to owe each other intellectual debts. During the academic year 1954-1955 we shared an office and in the course of the spring semester I asked one of my doctoral candidates to read the paper that Benson had presented at the AHA meeting in December. When we later discussed the paper Hays was working at his desk and after the student had departed, my office mate swiveled around in his chair and remarked that the paper was “interesting stuff.” Benson’s stress on the ethnic factor in the paper was congenial to Hays who had done some work with Oscar Handlin at Harvard and who deeply admired the latter’s books, *Boston’s Immigrants* and *The Uprooted.* After a little discussion of Benson’s ideas he asked me to introduce him to Benson if that should be convenient at a historical meeting, and shortly thereafter I did so. This was to begin a decade in which relations between the two were cordial and supportive. Both Hays and I were to be conduits through which Benson’s ideas flowed to the graduate students in American history at the State University of Iowa. And in the year following our office conversation Hays set his research assistant to work collecting data on the ethnic and electoral characteristics of Iowa townships.

Hays was impressed also by my arguments in behalf of establishing a manuscript collecting program at the university, analogous to the efforts of the Regional Collection of History at Cornell University, and he became an energetic supporter of such activity. But I owed much to him also. It was he, standing at the new book shelf of the University Library, who inquired whether I had yet seen Manning J. Dauer’s study of the Adams’ Federalists, including as it did detailed charts showing the votes of all congressmen and explanations for their political behavior. Dauer’s work was almost inspirational to me in its suggestion that we could indeed explain congressional behavior with great precision. He also brought V. O. Key, Jr.’s *Statistical Primer* to my attention, and that little book helped me understand some of the applications of statistical methods to political history that I had failed to find in the classroom at Iowa State. And there was much more in the way of intellectual give and take with Hays in the course of graduate examinations and hallways and social conversations that benefited me.

Stow Persons’ meticulous and slightly abstract approach to the issues of intellectual history certainly stimulated me, although the impact in retrospect was more general than specific. However, stimulating colleagues were also to be found in other departments. Alex C. Kern, John G. Gerber, and Charles Miller supervised an active graduate program in American civilization in the English Department and frequently requested the Americanists to serve on doctoral committees. The emergence of such programs during these years had generated a good deal of intellectual elan and I never read one of these dissertations without finding something of substantive or theoretical interest. During the 1950s several members of the School of Journalism were encouraging students to explore the techniques of content analysis and some dissertations with this methodological focus reached my desk as well. The members of the Department of Political Science shared the upper floors of Schaeffer Hall with the historians and this department also was changing the guard during the 1950s. Donald Johnson, Robert Boynton, Arnold A. Rogow, Lane Davis, John R. Schmidhaeuser, and Dell S. Wright were all energetic young professionals with whom the historians had frequent contact. In those days also the campus Humanities Society was a very broad-gauged organization that presented a wide spectrum of speakers from the humanities and social sciences. I heard a good many exciting discussions in the Senate Chamber of the Old Capitol Building.

III

All of the department members in American history at the State University of Iowa enjoyed teaching undergraduates. However, it was from the graduate students that I learned the most. There was never a large graduate program at Iowa during my stay; our total number of students in residence each year probably varied from the high thirties into the fifties. Normally the Western civilization course used some fifteen to twenty assistants and the graduate school
provided us with half-a-dozen research assistants and a few fellowships. After 1960 we also had a few fellowships to distribute under Title IV of the National Defense Education Act. (I remember that Hays, to my surprise, opposed the initial application vigorously when I brought the question of applying for this aid before the department during my first year as chairman, 1959-1960.)

If the graduate program was small, it also attracted some extremely promising young people during the 1950s and 1960s. There was no dominant reason for this; the Iowa program did not have the national reputation of many others. Although we did not encourage the practice, some good undergraduates stayed for advanced work and we were all gratified when a young man whom we had encouraged to go to Stanford, returned to take the Ph.D. in the department because he felt that the Americanists exceeded their West Coast peers in enthusiasm, supportiveness, and, perhaps, even grasp of subject matter. At Brooklyn College, Arthur C. Cole and Madeline Robinton encouraged several able young men to come to Iowa as a result of ties with Persons and Aydelotte, and because they believed that young New York intellectuals needed to learn something of the world beyond the metropolis on the Hudson. Others arrived from colleges in Iowa or adjacent states because they understood vaguely that the State University of Iowa was a good school and knew that it was close to home; one bright student in medieval history from southern Iowa did not cross the Mississippi until he was well into his doctoral program, venturing daringly into Chicago on his first foray into "the East." In its search for teaching assistants in Western civilization, the department sometimes selected students who had completed their M.A.s in midwestern institutions that offered little or no doctoral work. Such appointments did not always work out well but the policy also brought a number of fine students into the department. Although some students in American history enrolled at Iowa during the 1950s specifically to work with Persons, the rest of us in American history did not have national reputations of his stature. It was not until 1959-1960 that Rodney O. Davis arrived from the University of Kansas, steered in my direction by James C. Malin.

I inherited two graduate students from my predecessor: John Clifford and Robert Boyd. The latter did not remain in the program beyond the M.A., but Clifford was already launched on a study of "The Literature of Ranch and Range," a topic that he had selected under the guidance of George Mowry, who was a member of the Iowa faculty from 1945 to 1950.19 Clifford was older than I, already well into his thirties; he had seen war service in the Seabees and had a good deal of other miscellaneous occupational experience including work as an oil field roustabout, he still retained an interest in a garage in his home community in southern Illinois. Aydelotte had offered him a full-time assistantship, supported by foundation funds obtained for his study of the Corn Laws Parliament, and Clifford continued with that project until he submitted his dissertation. He was short in stature and spoke a mixture of southern Illinois and other midwestern border-state patois; he was as down to earth as an old shoe but he was a highly intelligent person who could write well and he was full of pungent wisdom. From him I learned more about the problems of public opinion research and comparative literary analysis than he obtained from me on any subject. He was also a mine of information about the Iowa graduate program, although in this respect his information was less trustworthy, I suspect, than I assumed at the time.

My graduate seminar began in the second semester of 1952-1953 with two students enrolled. It continued, except for periods of research leave, through the spring semester of 1964. Like most of the instructors at Iowa I mixed together M.A. candidates, doctoral candidates who were working on their dissertations, and the occasional student from other departments. By the mid-1950s the group normally numbered between six and ten and stayed at that level with some tendency to grow a bit during the early 1960s. I have always envied graduate advisors who direct narrowly focused seminars on areas or topics that they are themselves researching and I agree that participation in a seminar where all the members are working on closely related topics can sometimes produce more sophisticated and exciting challenges than classes where disparate topics are under discussion. However, I also believe that the highly integrated research seminar is often achieved by restraining the interests and imaginations of individual members and sometimes turns into pure exploitation. I did not find it uncongenial therefore to bring students with primary interests in the West and the middle period together and to give them wide latitude in their choice of research topics, so long as they could demonstrate that appropriate research resources were available for their work.

Imitating Paul W. Gates, I normally suggested a number of areas that I thought would reward research to those students in my seminar who were unprepared to develop subjects of their own choice. Relatively few simply borrowed from my topic list. I did influence some to investigate and develop subjects of their own choosing within general areas that I suggested might have considerable potential. The first M.A. candidate to complete his degree solely under my direction was Richard N. Kottman who combined both his own and my interests by developing an M.A. thesis on the Populist contingent in the U.S. Congress during the 1890s. This study forecast a continuing interest in the seminar in affairs at the congressional level. When one of our
students from Brooklyn College, Joel H. Silbey, reacted negatively to a paper of Norman Graebner's that advanced an extreme view of the importance of sectional attitudes in national politics in the years after 1846, we agreed that Guttman scaling might well provide a useful tool in testing this hypothesis in congressional politics and policy making. The first scales that he developed and brought into the seminar generated great interest within the group. Silbey's impressive study involved the development of comprehensive voting scales in the Congress from 1840 to 1852 by hand computation, a phenomenal expenditure of labor in precomputer days.20

Discussion of Silbey's research encouraged other seminar students to use the same technique in looking at congressional activity in other eras during the nineteenth century and I visualized a series of such studies that would cover most of the nineteenth century. During the early 1960s George Nielsen attacked the Era of Good Feelings and Gerald Wolff the mid-1850s, centering on the passage of the Kansas-Nebraska Act. I decided that I would investigate the Congress during the Civil War after my book on the development of the commercial Corn Belt was completed, and Edward Gambill expressed interest in working up the reconstruction congresses. Impressed by Benson's case study of New York, Rodney O. Davis decided to examine Jacksonian politics from a western perspective and used Guttman scaling in studying the Age of Jackson in Illinois.21

Malin's study of population mobility in selected Kansas townships had impressed me greatly and I had included township case studies of credit processes in western townships in my dissertation, in part as a result of his advice. I collected comparative census, mortgage, and land transfer data for a number of Iowa townships and for Bureau County, Illinois, during the research for the study of the evolution of the commercial Corn Belt in Illinois and Iowa that I had begun in 1954. Although one of my research assistants referred to her work derisively as "counting cows for Mr. Bogue," I believed that "grass-roots history" (the phrase had already been used by Dean Blegen of the University of Minnesota) was important and encouraged various members of the seminar to work on such matters. Within my seminar William L. Bowers and Rodney O. Davis investigated population mobility in midwestern townships and Mildred Throne, the gracious and supportive research associate at the Historical Society, independently assembled and analyzed census data for Wapello County, Iowa, part of the southern Iowa region that she had described in her doctoral dissertation directed by Louis Pelzer during the 1940s.22

The mobility studies were interesting but they left much unanswered about community processes in general. As a SSRC Fellow at Iowa State College during 1955-1956, I sat in on various of the rural sociology classes and did considerable reading in the community studies that sociologists had done both in the current and earlier generations. I tried in that year to formulate the social processes in frontier communities by developing a model based on social science theory. It was good to find sympathetic reaction to such interests in the seminar and George Boeck essayed a comparative study of the early development of the lower Iowa river towns. Unfortunately the amount of research involved in developing a comparative study, using three or four centers, proved to be a good deal more intimidating than either Boeck or I had imagined. Facing the challenges of supporting a family and beginning his college teaching career, he decided to restrict his study to Burlington alone. The comparative dimension could, however, be developed by others, we believed, if they were prepared to study comparable midwestern centers using somewhat similar techniques. Faye E. Harris essayed a study of Keokuk with something of this motivation but her dissertation was not completed until after I had moved to Madison.23

The student who was most successful in developing a comparative framework for research in community processes was Robert Dykstra. He had proven himself to be a good and thoughtful student in my survey course as an undergraduate shortly after my arrival at Iowa, but I would not have forecast an outstanding record in graduate school for him at that time. He returned to graduate school after several years in the armed forces, however, with his career objectives firmly set and he demonstrated intellectual growth with a vengeance. But initially I had some doubts. He entered the seminar suggesting that he wanted to study western violence and justified the importance of the topic by noting Dwight D. Eisenhower's references to the western method of settling differences in face-to-face confrontation and the president's apparent belief that Bill Hickock was a representative problem-solving westerner. To Joel Silbey and Stanley Parsons and the seminar director, this agenda seemed to promise a foray into old-fashioned triggerometry and we pressed him hard. Why not, we suggested, examine the institutional context from which frontier violence grew by preparing a comparative study of the early cattle towns. Bob agreed and the resulting M.A. thesis provided an article that won the Edwards Prize of the Agricultural History Society. He expanded the topic into a doctoral dissertation that Alfred A. Knopf published and the book was awarded the Spur Award. But Bob was as stubborn as a good scholar should be and developed his interest in Hickock beyond that required for his dissertation; ultimately he published a fascinating article on the Hickock myth. But the other members of the seminar perhaps denied him the opportunity to emerge
as an academic expert on the subject of American violence during the late 1960s.24

Electoral studies attracted a number of students in the seminar. After reading Benson’s 1954 paper, Morton Rosenberg, the first doctoral candidate who chose his dissertation topic under my direction, elected to study the emergence of the Republican Party in Iowa during the 1850s. One of Stow Person’s advisees, George Daniels, examined the behavior of German-dominated townships in Iowa during the election of 1860 and made an interesting contribution to the scholarly debate on the political behavior of the American-German electorate in the election of Lincoln. Stanley Parsons used both comparative county-level voting studies and Guttmann scaling in analyzing the local dimensions of Populist behavior in Nebraska.25

Robert Swierenga’s dissertation was a somewhat special case. During the early 1960s a member of a Chicago law firm contacted me. He and his colleagues were representing the Sac and Fox Indians in a suit for reimbursement for underpayment for lands relinquished by the tribe to the federal government in a treaty of 1843. There was considerable money at stake in the case since the cession involved some twelve million acres of rich Iowa land covering all or part of more than thirty counties in east central Iowa. The Iowa Indians were also parties in the suit and four law firms were involved as counsel. The lawyer’s understood that there had been considerable speculative activity in the cession after it had been placed in the open market by the federal government and they believed that a study of this speculation might provide them with useful material in their efforts to prove that the fair market value of the lands at the time exceeded that provided in the treaty.

At this time my second book was almost finished and I had decided that I would not serve a second full term as departmental chairman but I was eager to investigate the legislative background of the great Republican laws relating to western development passed in the U.S. Congress during 1862. So I responded to George Pletsch that I could not take on the job. However, after some discussion I agreed to serve as an advisor to the project if we could find a suitable advanced doctoral candidate who would assume oversight of the project and make the subject his doctoral dissertation. Initially I had some doubts about the propriety of such an arrangement, since the Indians would pay the doctoral student during his work on the subject in lieu of support that he would otherwise have obtained by working as a teaching assistant in the department. But our Graduate Dean believed that there was no ethical problem involved, provided that we obtained adequate assurance that the lawyers would not try to influence the nature or shape of the research findings. This counsel was willing to provide.

There were several among my doctoral advisees who were sufficiently advanced in their programs to take on such a project. Each of them had qualities that suggested that he could do the job satisfactorily, but I turned first to Robert P. Swierenga because he had demonstrated a remarkable capacity for completing major assignments on time. Bob accepted the position and we met with Mr. Pletsch to discuss the shape that the study would probably take. Since Bob had been planning to develop a dissertation on the political behavior of the Dutch settlers in Iowa, he knew no more about the operation of the federal land disposal system in Iowa than the normal doctoral candidate who had prepared a field in western history. As a result I played a larger role in developing the initial outline than I preferred, but, as the project developed and Bob settled down to work on it, he truly made it his own although he had resources at his disposal of a magnitude that would have made most doctoral candidates envious.

A firm of Detroit lawyers supervised the abstracting of federal land sale data at the county level in the Sac and Fox cession, and this group also supervised the initial computer sorting necessary for making lists of speculators and totaling their purchases after being informed of our needs. This computer printout arrived in Iowa City in 1963. Swierenga computerized the technique of calculating speculative profits that Margaret Bogue and I had used in earlier studies and applied it with minor changes to the speculative data derived from the entry data. He also ploughed through masses of related sources, gathered county tax data, and had microfilm copies made of the great collection of business manuscripts left by a firm of Virginia bankers and land speculators, Easley and Willingham. Since the demands of the claims case placed the research on a more demanding schedule than that of doctoral projects in general, Swierenga also received permission to use a couple of assistants for a time. As a result various jokes circulated about the Sac and Fox Benevolent Society. Ultimately the Indian claims lawyers submitted the dissertation as an exhibit at the hearings before the Indian Claims Commission and the major government expert on Iowa land disposal ultimately recommended to his University Press that the manuscript be published there.26 It was the first piece of computer research completed in the history doctoral program at Iowa, although Dykstra was using the computer independently in a study of Iowa electoral behavior before the speculative project was completed.

The objectives of the Iowa graduate students mentioned here were much the same as those of doctoral candidates in American history
generally. In most cases they hoped to answer questions or illuminate problems that had interested others before them. But the completed work did differ from that being done in most American research seminars of the time in its use of hitherto ignored or underutilized evidence drawn particularly from the manuscript federal and state censuses of the late nineteenth century, precinct and county level voting returns, and federal and state legislative roll calls. In utilizing such data students in the seminar were engaged in real methodological pioneering within the discipline. Silbey, for example, was the first specialist in American history to use the Guttmann scale in a major way; I am not aware that any other American graduate student in history had incorporated the results of multiple regression in published electoral analysis before Stanley Parsons did so in 1963. Dykstra’s *Cattle Towns* was illuminated by his grasp of social theory derived from reading in the work of social scientists.

In directing my seminar I tried not to scare off students who were temperamentally or intellectually committed to more traditional methods of historical analysis. Indeed, I was aware that in a sense they laid out a more difficult path for themselves than did those who took the behavioralist road. Their work would not have the novelty appeal of those who were experimenting with new methodologies. And the suggestion that the new methods somehow invalidated more conventional ones was, I believed, totally misguided; one should use all appropriate evidence. I was happy therefore to have a hand in the development of dissertations like those of George W. Sieber (the *W. J. Young Lumber Company*), Leonard F. Ralston (the *Iowa railroad land grants*), George Schultz (a biography of the Indian missionary, Isaac McCoy), Willard I. Toussaint (a biography of the Copperhead Iowan, Charles Mason), and Hubert H. Wubben’s study of the *Iowa Copperheads.*

During the early 1960s the Iowa department, John Snell informed us, was rated somewhere between twentieth and twenty-fifth in the country and we were all aware that our graduate students were at a disadvantage in competing for jobs with more prestigious departments. I encouraged students, therefore, to counter this fact by publishing journal articles as soon as possible, preferably before they entered the job market. But a good many of the editors and editorial boards were conventional in attitude and much that was being done in my seminar was unconventional in varying degrees. Smaller and state-level journals appeared to provide the best opportunities for acceptance. At the Iowa State Historical Society, Mildred Throne looked to the department for material for the *Iowa Journal of History* and from 1955 to 1960 published one or more papers submitted by Rosenberg, Silbey, Ralston, Boeck, Wubben, and Bowers.

Following Dr. Throne’s death in 1960, the superintendent of the historical society suspended the publication of the *Iowa Journal of History*. We in the American program had lost an able colleague and a good friend and the students particularly now found their most promising publication outlet closed. Although its professional standards were less exacting, the *Annals of Iowa* remained as an avenue of publication and Rosenberg, Davis, Sieber, Swierenga, and Toussaint all used it during the early or mid-1960s. But Father Jerome V. Jacobson, the editor of *Mid-America*, displayed an interest in behavioral history, and Boeck, Daniels, Silbey, and Davis were able to publish work in his journal. The editor of *Agricultural History* accepted a fine article from Dykstra and later also approved a manuscript from Wolff. Dykstra published in the *Midcontinent American Studies Journal* as well, and when he became the editor of *Civil War History*, begun initially by Clyde C. Walton in the University Library, he accepted articles from Silbey and Swierenga. Meanwhile, Stanley Parsons published a study of the sources of Populist voting strength in *Nebraska History*. An insightful critique of Walter P. Webb’s work by Joseph Cash ultimately appeared in the *Rocky Mountain Review*. Although these articles were in general modest endeavors, they gave the young authors the satisfaction of seeing work in print, contributed to professional *esprit de corps* within the group of graduate students and recent graduates, and enriched placement dossiers.

As a young instructor I found another aspect of the graduate program at Iowa to be particularly stimulating. The program was too small to allow the use of graduate proseminars, courses intended primarily to prepare doctoral candidates for their comprehensive examination. Therefore, we allowed graduate students to enroll for independent reading after they had taken appropriate undergraduate survey courses. Theoretically such registration might involve little or much personal contact between the student and the instructor, depending on the arrangement. But I fell into the practice of requiring graduate students to work through a list of twelve books or equivalent sets of articles under a three-credit registration and accepting oral reports on all but two or three that served as the basis for a comparative written critique.

Although it was often possible to combine the reports of two, three, or even four students who were preparing the same field, some of my independent study registrants were working up the middle period and others the American West and combination into one or two groups was sometimes impossible. Often during the early 1960s I devoted an afternoon or evening each week to independent study conferences. If this was a considerable investment of time, there was a payoff. To be prepared for the conference, I had to read or refresh my memory of the
book under discussion. The graduate students were highly intelligent people and the questions that they raised and the discussions that flowed from their initial reports educated me no less than the students involved. Once when I pressed Cash to explain the methods that Lee Benson had used in selecting his sample units for examination in the preparation of The Concept of Jacksonian Democracy, he stuck by his guns and maintained that there was no discussion of this subject in the book. He was right; I had derived my knowledge from talking to Benson. When I pointed out the oversight to the latter he was astounded and remedied matters somewhat by raising the issue in the introduction to the paperback edition that was then in process. If other reports did no more than help prepare the students for their doctoral field exams, they also left an instructor who had reviewed, probed, and questioned the ideas of leading historians time and again in discussion with perceptive people. Direction of independent study, although avoided by many instructors, is a great way to establish control over subject matter.

IV

In early November of 1959 Hays presented a paper to the Iowa secondary school teachers of history and social studies at the annual meeting of the Iowa State Education Association. It appeared in the July 1960 issue of the Iowa Journal of History after he had resigned from his position at the State University of Iowa to accept the chairmanship of the department at the University of Pittsburgh. To one analyst of the Iowa School this article appeared to be an important statement of radically new views and it was indeed the first in a series of presentations by Hays exhorting the historical profession to adopt a different approach to the study and writing of American political history. Relatively short and undocumented, though making reference in the text to a number of works, this piece was a plea for history that emphasized the “vital human quality of the past” by “systematically studying human experience and behavior so that solid and concrete generalizations emerge.” He attacked “presidential history” as illustrating a “formal approach,” and cited in contrast Handlin’s The Uprooted, as a study of the immigrant experience that saw “history from the inside out.”

Hays suggested that the new emphasis could be achieved by shifting attention from national politics to grassroots happenings. If one looked away from the trust-busting of traditional accounts to the local level in Iowa during the years 1877 to 1914, one discovered that the central concerns of politics were cultural in nature. These issues reflected the clashing interest of Puritan-derived pietistic values with those who came from a different cultural background, whose religion was more formal and ritualistic, and whose view of sabbath observance and the use of alcohol were much more permissive. Hays used township voting statistics from Carroll County (“typical of most Iowa counties”) to illustrate the way in which the cultural backgrounds of the residents in various townships were linked to extremely strong preferences for either the Democratic or Republican parties.

In another section of this article, Hays argued that history should be “organized around the goals of human action, not the techniques, around the ends rather than the means.” This was illustrated by discussion of the Farmers Grain Dealers Association of Iowa and the Corn Belt Meat Producers Association, groups that had supported both the enhanced regulation of railroads under the Hepburn Act and the return of the railroads to corporate control in 1920, showing that their action was consistent with their needs despite the seeming contradiction in policy involved. “The understanding of any particular government function must rest upon an analysis of the circumstances which give rise to that function,” he argued, as well as “the groups which demand it and the ends which will be served through it.” In a fourth substantive section Hays argued that the analysis of election statistics provided “an excellent opportunity for undertaking a grassroots approach” and illustrated his argument with discussion of statewide and local changes in party voting patterns during the late nineteenth and early twentieth centuries. Each of Hays’ examples, “the importance of cultural issues as opposed to the trust question, the analysis of the role of government as a means to an end, and the possibility of using election data to define problems in history” involved a “refocusing of attention from the outward formal aspects of history toward the level of human behavior.”

Hays’s paper deserved to be well received. It was clearly written in terms that the school teachers should have found both congenial and stimulating. Its place in the history of the development of the new political history has, however, been somewhat misunderstood. In a final section in which Hays discussed the problems that teachers might encounter in trying to develop such an approach, Hays referred to helpful reading material, citing Handlin’s Uprooted for the second time, and also mentioned Lubell’s The Future of American Politics and works by V. O. Key, Jr., Gordon Baker, Stephen Kemp Bailey, and Lee Benson’s first book, Merchants, Farmers and Railroads, an examination of the interest group background of the Interstate Commerce Act. In fact the paper was highly derivative and should be
viewed as an effort on the part of Hays to sift and order the ideas circulating at the time among a substantial group of young Americanists and illustrate them with examples derived from preliminary research that he or his students had conducted in Iowa. In criticizing presidential history he followed the road broken in Thomas C. Cochran's famous article on the "presidential synthesis." The influence of Lubell is clearly evident. Benson had already circulated a preliminary version of The Concept of Jacksonian Democracy in which he had devoted much of a chapter to the importance of ethnocultural determinants in American politics and he had generalized such distinctions into Puritan and non-Puritan political behavior. Benson had illustrated the significance of interest group objectives in Merchants, Farmers and Railroads and he was a one-man gang in advocating the use of electoral returns from at least 1954 onward. In a chapter of The Concept of Jacksonian Democracy Lee Benson pictured individual political action as goal seeking in a theoretical model of electoral behavior; given the processes of university press publishing, he could hardly have derived it from Hays's article. And, as we have seen, the emphasis on political activity as behavior was found in the report of the 1957 conference at Rutgers and the original draft of that report used the term "social analysis," a phrase selected by Hays in the mid-1960s to typify his approach to history.31

Hays characterized his illustrations as attempts "to categorize history in terms of types of human experience, types of human understanding of the world, types of human values, and types of resulting human action. This is a group analysis of society in which one sorts out events in history in terms of social organization and behavior." Perhaps Hays's greatest personal contribution to the general pool of ideas floating through the minds of a substantial number of teaching peers and students was his stress on the human part of the phrase human behavior. But such emphasis was explicit or implicit in the thinking of others as well. Benson classified political behavior in terms of the individual human goals that it reflected and the same stress surfaced in my own advice to students that they must make people the subject of their narratives if they wanted to develop an interesting writing style. In fact a network of some magnitude developed during the 1950s; through its communication channels flowed most of the ideas fundamental to the new histories developing during the 1950s and 1960s.32

The ideas or example of some older historians provided impetus to the thinking of network members as in Paul W. Gate's admonitions to his graduate students to work with social scientists or James C. Malin's various methodological innovations. Some members of the emerging network were especially shaped by particular influences: Benson's ties to the Institute for Applied Social Research at Columbia University were, in retrospect, absolutely crucial to the later developments in American political history; Hays' contacts with Handlin at Harvard certainly conditioned his contributions to the interchange at professional meetings; my thinking was greatly influenced by reading the articles and research bulletins of agricultural economists and rural sociologists. But Richard P. McCormick, Charles Sellers, Thomas Pressly, Robert Berkhofer, Leslie Decker, Forrest McDonald, and others fed their contributions into the communication channels, each drawing upon his own background, each conditioned by his own particular interests.

If anyone could be said to have dominated this informal system of professional contact during the late 1950s it was Lee Benson. His influence in changing the research emphasis in American political history during the 1950s and 1960s was far more important than that of anyone else. However, one should not underestimate the importance of the encouragement and monetary support provided by the SSRC to young historians who displayed interdisciplinary or behavioral inclinations during these years.

From the standpoint of the young instructor, conditions for intellectual growth were close to ideal in the Iowa History Department during the 1950s. We were given our heads to a degree that was impossible in most history graduate programs of equal or greater prestige. Departmental governance was democratic, although not without judicious management, as Aydelotte sometimes admitted. Younger people, therefore, had a sense of self-worth that was seldom found in programs in which an older group firmly controlled the decision-making processes. But more important still, none, whether senior or junior, took colleagues or graduate students aside to read them lessons on the perils of experimentation or misled them into believing that the only good history was narrative history.

Is it possible, however, that the broth of new methodology available in Iowa City may have been less stimulating and useful to the graduate students than to their teachers? Unquestionably the young Americanists encouraged students to experiment with techniques that they did not fully understand themselves. One can argue that this is the essence of higher education—teacher and student learning together—but I suspect that a few students believed that they were being used as guinea pigs. I remember one of my doctoral advisees who was studying electoral behavior in a midwestern state. We agreed that he should investigate the ethnic composition of the electorate but the published federal census of 1850 did not tabulate the place of birth of county residents. Such information could, however, be developed by examining the manuscript census rolls and I agreed with this man
that he should consider the possibility of doing this before I left for a year of research and study. In my absence Hays served as this student’s adviser and enthusiastically argued that his analysis should be extended to every county in Iowa in 1850. Assembling these data was a very considerable task that took most of the year while I was on leave. I expect that I would have settled for a sample. But with the data collected, neither Hays nor I really understood the analytical techniques required to extract full meaning from them. Ultimately my own study carried me much further in that direction but this was after a dissertation had been accepted that was less than fully satisfactory to either the candidate or myself.

There were perhaps other unfortunate side effects. In some cases graduate students may have had more difficulty in finding a publisher for their work than if they had devoted comparable energy in preparing a narrative study. Some press readers were intolerant of an analytical approach and quantitative methods. One case in particular frustrated me, when a young instructor with a Ph.D. from Iowa obtained a split reading at several presses; despite some strongly supportive readings each press board refused the work because the readers had not approved it unanimously. Yet the manuscript was initially well done and developed an important thesis that other scholars exploited to their profit some years later. Students in the Iowa program also suffered when Hays left after seven years and I accepted a position at Wisconsin in 1964 after twelve years in Iowa City. Christopher Lasch and Malcolm Rohrbough who replaced us were highly able scholars but they had very different research interests and approaches than we did. Despite their earnest efforts to carry forward the supervision of students they inherited, they could not have been expected to understand fully the potential of either the methods or the subjects that were laid before them, or to exercise the kind of supervision that adequately guarded the student from wrong turnings or misapprehensions.

Despite these qualifications my later contacts with the students of that era at Iowa suggest that they were in general as happy as graduate students are likely to be and that student morale was a good deal higher there than in some prestigious programs. Most of the Iowa students of those years believe that the program prepared them well for their careers. I know that I never insisted that graduate students in my seminar must use social science methods in their work and I made it clear that I respected good narrative history. I regret that I was sometimes less prepared to advise students in the intricacies of particular techniques than was later true, but I did urge advisees in such cases to seek help from specialists in related disciplines, as I had done myself while a graduate student. It is, I believe, a more defensible practice than that found in programs where students are insulated from the related disciplines. And since there were five Americanists in the Iowa Department at the time, there were stimulating alternatives to working with Hays or Bogue. Indeed Persons and Gibson were more eminent in their fields than the younger Americanists. We did not exploit a captive clientele.

Was there indeed an “Iowa school” of American history at work during the 1950s and 1960s? That phrase is much too grand, I suspect, to convey the meaning of the Iowa situation. But the program there was one in which fresh approaches and methods were encouraged and this attitude was not confined solely to quantitative methodology or to the specific fields of American and British political history. The graduate research in nineteenth- and twentieth-century American history at Iowa did display different emphases than those found in most other graduate programs of the period. Perhaps two-thirds of the thirty or so M.A. and doctoral advisees whose work I directed between 1952 and 1964 prepared studies that clearly showed interdisciplinary or behavioral influences. Some of these students completed their work after my departure. My group of advisees swelled somewhat as a result of Hays’s resignation, because some incoming students considered his successor to be a cultural rather than a political historian, and I chaired the M.A. exams of several of Hays’s M.A. candidates as well as the doctoral defense of Don S. Kirschner who had developed a fine doctoral dissertation on rural-urban conflict in the Midwest under Hays’s guidance. There is of course a kind of gathering momentum in doctoral advising and Hays left the program at the point where his doctoral group was achieving the size necessary for good interaction among its members. No doctoral candidate began work in Hays’s seminar and completed it while he was still at Iowa. Samuel P. McSevney, surely one of his most outstanding students, defended his dissertation under the chairmanship of Christopher Lasch.

Quite accidentally and in large part independently, three members of the History Department at the State University of Iowa during the 1950s were intrigued by the challenge of adopting a behavioral approach to political history. With their students they constituted by far the largest nucleus of such activity at the time. Of the three, Aydelotte surely made the most original contributions during those years and his willingness to discuss the methods and theory involved
in his research and his contacts in eastern universities and abroad ensured that his approach and work would have a respectful hearing beyond the Iowa campus. His role as a member of the SSRC Committee on Historical Analysis contributed to the same end. When compared with the American areas his graduate student group was small but several members of his seminar prepared extremely interesting work. Each of us was tremendously influenced by developments in related disciplines and each of us reacted somewhat differently, but we did share a great many common assumptions and aspirations. In these years neither Hays nor I in any way matched the virtuoso efforts of Lee Benson to meld the social science methods and theory of the time with historical practice to produce a different brand of political history. On the other hand, time would show that both Aydelotte and I were more willing to apply quantitative techniques rigorously than Benson. Hays was even less dedicated to the use of quantitative methodology than the latter; he never produced a substantive article or monograph in which there was a major dimension of quantitative analysis, although he encouraged others to do so.

In comparison with some European illustrations of academic schools of thought or practice, the Iowa case was much too short-lived and involved too few faculty-level individuals to qualify. There was among the principals somewhat less agreement on a broad range of conceptual matters than I, at least, read in the meaning of "school." But if the number of faculty and graduate students involved together are considered a measure of the definition, Iowa probably outdistanced the Owsley, or Vanderbilt, school by a good margin. Although useful, labels are not after all as important as many believe. There was excitement at Iowa during the 1950s and early 1960s and I have always been glad that I was part of it. The program certainly suffered when Hays left in 1960 to try and reshape the Pittsburgh department in ways that strongly reflected his experience in Iowa City. But students at Iowa continued to declare an interest in behavioral history and my students during the early 1960s continued to be an exciting group. Nor was the social science approach to history abandoned in the department when I succumbed to the lure of the research libraries at the University of Wisconsin in 1964. Aydelotte still pursued his research in Iowa City and scholars like Alan B. Spitzer, Henry G. Horowitz, and Robert W. Dykstra maintained an interest in interdisciplinary research there during the 1970s. By this time a second and even a third generation of behavioralists were at work in American political history and the landscape of American political history looked very different than it had during the early 1950s.

NOTES


2. This paper was not published in its original form. I have a manuscript copy in my possession in which pp. 4, 7-10, 14-15, and 38 are particularly relevant to this summary paragraph.


9. Ibid., pp. 2, 4-5.
10. Ibid., pp. 5-8.
11. Ibid., pp. 21.
12. Ibid., pp. 4, 6-8.
13. Ibid., pp. 8-10.


33. During 1982 I corresponded on these matters with most of the Iowa students whose names appear in this chapter, as well as with Don S. Kirschen, Samuel T. McSeveney, William Cumberland, and David Tucker who did their dissertations at Iowa during the 1950s and 1960s in areas other than mine. I much appreciated their willingness to discuss their days at Iowa with me.