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Discovering Unheard Voices: Explorations in the History of Education, Childhood, and Juvenile Justice

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What establishes and what ties together a scholar's research agenda—the lines of inquiry along which he or she seeks to make original contributions to knowledge? In my discipline of history, geography and chronology, both defined narrowly (e.g., Massachusetts during the Revolutionary era), set the main boundary lines. Within that boundary, scholars usually probe one or two specific topics or themes, such as the role of the militia and/or gender roles, and seek to explain patterns of change and continuity over time. Most often, except in textbooks, they do so from an identifiable subdisciplinary perspective such as political, cultural, or economic history. They publish their findings in historical monographs and in specialized (e.g., Journal of Social History) or general (e.g., American Historical Review) history journals.

Historians who study education—whether located professionally (as most are) in schools of education or in history departments—generally respect another boundary line that separates studies of colleges, universities, and professional schools, on the one hand, from studies of K-12 education, on the other. Both groups, however, tend to focus on schooling per se, whatever the level or type, and to do "top-down" analysis of the design, organization, funding, staffing, clientele, and especially ideas that underlay the creation of formal educational institutions. Within this conceptual framework— this "prescribed journey"—they seek to identify new questions and methods to illuminate educational experience in the past.

I consider myself to be an educational historian of the United States, primarily of the 19th and 20th centuries, although my research interests extend well beyond schooling to include the legal and cultural history of children and youth, including the operations of the juvenile justice system. Moreover, I have done substantial research that crosses the boundary line between K-12 and higher education; indeed, my empirical work touches all school levels from preschool to professional studies. But what truly motivates and ties together my research is method—and by "method," historians generally do not mean the use of approved, sophisticated analytic techniques that provide a sine qua non of first-rate scholarship, as is true in the field of education. Rather, by the term "method," historians usually mean the framing of novel questions that spur identification of untapped or little-explored databases so as to establish new lines of empirical inquiry into the past (Gaddis, 2002). And the main object of my method has been to discover unheard voices in the history of education, particularly the "bottom-up" voices of children and parents who have been the objects of reform initiatives in education and juvenile justice. To be sure, much of my scholarship inevitably also addresses "top-down" voices such as John Dewey in education and William Healy in juvenile justice. Most often, however, I have sought to identify "top-down" voices that have been largely forgotten and to incorporate them into the mainstream of American educational history.

In short, my main "research problem" has concerned how to expand contemporary understanding of key educational actors and activities in the past—not just the most famous, and not just the reformers, but also the targets of reform, mainly children and parents. This research problem has shaped my historical method, and it links the diverse set of topics that I have explored as an educational historian. In the remainder of this essay, I clarify why I chose this particular methodological approach to expand knowledge in the field of educational history and some of the professional circumstances and decision making that were critical to translating my general research problem into specific projects.

Graduate School and the Establishment of a Research Agenda and Method

I began graduate study in 1968 in a joint law and history program at the University of Wisconsin-Madison, but in 1970, as I became interested in the legal and cultural history of children and youth, I transferred to Columbia University to study with two of the reigning giants in American educational history: Lawrence Cremin and Richard Hofstadter (who died of leukemia during my first semester) (Cremin, 1961; Hofstadter, 1963). However, it was neither Cremin nor Hofstadter, but rather Michael Katz and Anthony Platt, who provided the initial intellectual catalyst for my shift to educational history. In The Irony of Early School Reform (Katz, 1968/

authors employed the tool of case study analysis, which was still novel in historical studies at the time. Katz, in particular, went beyond traditional kinds of "top-down" literary evidence to unearth local data, both qualitative and quantitative, to show how parents and children viewed the origins, processes, and results of educational innovation in their communities. Thus, the challenge that Katz and Platt offered to traditional scholarship was both interpretive and methodological.

At our very first meeting, Cremin raised doubts about the sufficiency or originality of the social control hypothesis to explain the dynamics of educational reform, and he urged me to read Merle Curti, Frank Tracy Carlton, and other scholars of the 1930s to gain perspective on earlier critical traditions in educational historiography (Carlton, 1965; Curti, 1935). But he strongly supported my interest in integrating new "bottom-up" voices into the study of educational reform, and he outlined a broad theoretical framework for reconceiving the entire story of American education that he believed was consistent with my approach. As I soon learned, Cremin was about to publish the first installment of his massive three-volume historical trilogy, American Education. In that volume on the colonial period, and in the two volumes that followed on the national period and the modern (he termed it "metropolitan") period, he radically redefined what the study of educational history was all about (Cremin, 1970, 1976, 1980, 1988, 1989). He also, from a methodological standpoint, embraced the study of individual communities and the inclusion of "bottom-up" voices as essential to recreating lived educational experience in the past. Ultimately, even though I chose a dissertation topic on the juvenile justice system that reflected my intellectual debt to Katz and Platt more than to Cremin, the new conceptual framework to which Cremin introduced me in 1970, and the concrete opportunities he provided for me to serve as both his teaching assistant and his research assistant, profoundly shaped my methods of inquiry throughout my career. And Cremin's analytic framework also reinforced my decision to center my own intellectual energies less on grand synthesis, as Cremin was doing on a scale I could hardly imagine, than on continually seeking to discover new voices in America's educational past.

In truth, for my first 18 months at Columbia, I did not have the time, resources, or good fortune to follow through on these commitments. I had no difficulty in internalizing Cremin's new, unconventional, and disarmingly broad (he liked to call it "latitudinarian") definition of education[1] or the new lingo of "educational configuration"[2] and "educational biography"[3] that he created to transform that definition into an operational research strategy. Indeed, I was intrigued and energized by how much consternation his new approach caused more traditional school-focused scholars in the field, including his distinguished Teachers College mentor (and coauthor with Cremin of a classic textbook in the field), R. Freeman Butts (Butts & Cremin, 1953). But my main energies at the time had to go elsewhere, notably toward mastering the canonical literature so as to pass doctoral qualifying exams.

In addition, my intention to develop a "bottom-up" dissertation topic in juvenile justice was frustrated by my inability to gain access to original case files from early-20th-century juvenile courts. Cremin's skepticism regarding Platt's (1969) The Child Savers had led me to rethink his methodology; it was only on second reading that I realized that Platt's social control interpretation was seriously compromised by a lack of empirical data on how juvenile courts actually worked on a day-to-day basis. Until I could gain access to such data, I did not think I could seriously improve on Platt's research; therefore, I was reluctant to commit to a dissertation on juvenile justice. In the meantime, I experimented with various other historical topics in Cremin's research seminar. Two of these led to journal publications while I was still a graduate student (Schlossman, 1973,1974), but all involved "top-down" rather than "bottom-up" data, and from my standpoint, none pointed toward the kind of dissertation I wanted to do.

Fortunately, my luck in crafting a "bottom-up" dissertation changed dramatically toward the end of my second year at Columbia following an expedition to the Midwest where, after many failures, I finally managed to gain access to the case files of Milwaukee Juvenile Court for the 1901 to 1920 period.[4] With these unique documents in hand to ground the project empirically, Cremin readily agreed to supervise the dissertation. Indeed, he saw the topic as an exemplar of his new "latitudinarian" definition of education, not only because

Beyond this, however, Cremin—who was already well under way in writing American Education: The National Experience (Cremin, 1980)—was deeply committed to the search for new "bottom-up" data sources that would allow him to bring to life the notions of "educational configuration" and "educational biography" in typical 19th-century communities. Cremin's goal was to show how similarities and differences in community-level educational configurations operated in practice to constrain or expand individual educational opportunities. Thus, my research and Cremin's research were proceeding on similar methodological tracks, and in 1973 Cremin asked me to serve as his research assistant. As I was writing the chapters of my dissertation that analyzed the previously unheard voices of children and parents in Milwaukee Juvenile Court, Cremin asked me to identify three small communities for case study analysis of educational developments in New England, the Midwest, and the South.5 Key in selecting each community would be whether its local (and supporting state) libraries held two different types of historical evidence: (1) data tracking key patterns of institutional growth, such as schools, churches, work environments, benevolent societies, newspapers, libraries, and railroad and telegraph linkages, as well as census and supplementary quantitative and qualitative data outlining the basic structures of family life, and (2) data capturing life histories of individuals raised in that community, whether contained in diaries (published or unpublished), letters, or other personal documents and with particular attention to that person's formal and informal education at home, at school, and in the community. Cremin asked me to locate as much untapped historical data as possible, that is, data dealing with communities and individuals about whom historians had not written much or anything previously. At the same time, at least for his case study of education in a New England community, he made clear that he was leaning toward the early industrial city of Lowell, Massachusetts, because it was the site that had inspired Lucy Larcom's famous reminiscence, A New England Girlhood, which was perfect, in his mind, for purposes of educational biography (Larcom, 1961).

Although my professional commitment to "bottom-up" history was already secure in my ongoing dissertation research, the months that I spent as Cremin's assistant motoring through the American countryside in search of unheard voices in American education refined that commitment and clarified its practical difficulties as a historical methodology. During the New England phase of the research, I learned not to look a gift horse in the mouth. Try though I did to persuade Cremin to focus his case study on lesser-known cities such as Fall River, Holyoke, and Pawtucket, I finally had to admit that none of the new voices I uncovered in these (and other) locales suited Cremin's method of educational biography as well as the writings of Larcom and other female and male mill operatives in Lowell. And once I located rich, previously untapped community-level data on schooling, labor, churches, newspapers, and many other dynamics of community life in Lowell, especially concerning the impact of early Irish immigration on the town's social and economic fabric (including public financing of Irish parochial schools), the choice of Lowell as a case study site had too many advantages to turn down, even though the town was already quite familiar to American historians.

After New England, I traveled south to visit state historical societies and local libraries in Virginia, North Carolina, and South Carolina. Cremin and I had already identified several autobiographies written by former slaves from those states that might serve the purposes of educational biography, but only if they could narrow the search for a case study site that contained ample data on white as well as black educational experiences. Ultimately, what determined the selection of Sumter District, South Carolina, as Cremin's case study site was not so much the family- or community-level data uncovered there for whites—although the southern educational experience generally was richer institutionally than previous literature had indicated—as two other unexpected data resources. First was the availability of manuscript census data by which to calculate how many whites owned slaves in Sumter District and the average number of slaves per farm/plantation—both key for outlining the contours of the district's educational configurations. Second, and more important to Cremin, was the discovery of a second slave autobiography within Sumter District, that of Irving E. Lowery, to compare and contrast with the more widely known slave autobiography of Jacob Stroyer (Lowery, 1911; Stroyer, 1898). This combination of African American data sources made Sumter unique among the several case study sites that I was investigating, and the final choice for Cremin was easy.

was imperative to respect settlement patterns. That is, we would not consider communities in the northern or southern tiers of states such as Ohio, Indiana, and Illinois because they tended to be dominated by settlers from the North or the South, respectively, each of whom had very different prior experiences with state funding of public schooling. Thus, my travels to the Midwest in search of new data centered on the middle geographic range of each state, where the settlement pattern more commonly mixed northerners and southerners. If anything, we believed, it was better to err on the side of southern dominance in selecting a case study site because educational historians had tended to overstate the impact of New England educational traditions in schooling the Midwest.

Cremin's final choice of Macoupin County in Illinois as his midwestern case study site rested in part on the mix of southerners, northerners, and immigrants who migrated there (although southerners predominated), in part on rich data concerning both publicly supported and denominationally supported educational activities (especially by German Lutherans), and in part by my fortuitous discovery (after lengthy residence and largely by accident) of two locally held personal narratives by individuals who had come of age in the county: John McAuley Palmer, a white who achieved considerable success as a politician, and James Henry Magee, a free black who overcame serious childhood illness and struggled during his adult career as a teacher and preacher (Magee, 1873; G. T Palmer, 1941; J. M. Palmer, 1901). Being able to compare and contrast the voices of a little-known white and a free black man in the same county was an opportunity I had never expected to find; it clinched Cremin's choice of Macoupin County for case study analysis.

The Dissertation as a Culmination and New Beginning: Gender and Social Reform During the Progressive Era

My travels as Cremin's research assistant provided a remarkable opportunity to scour the country in search of new voices in educational history. No less important, it offered regular occasions to discuss methodological strategies and choices with him. The experience also increased my appreciation for how unique my "bottom-up" juvenile court data were for analyzing the predicaments of lower-class children and parents during the early 20th century. After completing the dissertation, I won a National Institute of Mental Health postdoctoral fellowship to revise the dissertation into a book, and I completed the book manuscript within several months (Schlossman, 1977).

Two unplanned possibilities for discovering new historical voices had emerged clearly from the dissertation project, and I was fortunate to have several more months as a postdoctoral fellow to pursue them. Both involved issues of gender, a theme of major importance during the mid-1970s as historians provided key intellectual leadership for the rise of women's studies as a legitimate scholarly field.

First, during my studies of Milwaukee Juvenile Court, I was surprised to learn that the National Parent-Teachers Association (PTA)— which was led entirely by women—was the leading advocacy group for juvenile courts during the early 20th century. Why was the PTA so involved in juvenile court if its primary focus, as suggested by its name, was to improve communications between parents and schools? Because historians of education and juvenile justice had never shown much, if any, interest in the PTA, I was optimistic that adding its "top-down" voice to historians' understanding of social reform leadership during the Progressive Era would be a notable contribution. Second, as I completed my book, I realized that boys and girls had followed fundamentally different pathways into juvenile court. Boys were charged mainly with property crimes, and girls were charged mainly with sexual offenses—actually, not so much "offenses" per se, such as prostitution, as simply sexual activity itself (for which their male partners were not held accountable). Moreover, my Milwaukee data suggested that girls were incarcerated for their "crimes" at higher rates than were boys. I noted these points briefly and tentatively in the book, but I sensed that a significant next step in my own research would be to give full voice to the distinctive experiences of girls in court and thereby to introduce gender as a key variable in historical studies of juvenile justice.

education based on G. Stanley Hall's ideas about child development (Schlossman, 1976). I also tracked radical changes that occurred in the organization and leadership of child development research and parent education during the post-World War I era. In particular, I highlighted major initiatives in parent education led by unheralded women's organizations such as the Child Study Association of America, the American Association of University Women, and the Merrill Palmer School as well as numerous contributions to child development research and practice initiated by women such as Edna White, Helen Thompson Woolley, Patty Smith Hill, Sidonie Gruenberg, and Lois Meek Stolz, all of whom educational historians had previously ignored. What began as an offshoot of my juvenile justice research ended up opening a new subfield within educational history and engaging me for nearly a decade in professional association with scholars and practitioners affiliated with the Society for Research in Child Development (Schlossman, 1978b, 1981, 1983a, 1983b, 1986a, 1986b).

In "The Crime of Precocious Sexuality," extending the little I knew about female delinquency from my study of Milwaukee Juvenile Court, I analyzed a large body of obscure early-20th-century writings on the sexual behavior of adolescent females, venereal disease, and eugenics of which historians were almost entirely unaware (Schlossman & Wallach, 1978). This "top-down," quasi-scientific literature clearly explained why girls, but not boys, were regularly charged with sex offenses in juvenile court during the early 20th century. The literature invoked the traditional Victorian double standard on sex but did so in the context of a real epidemic of venereal disease and the growing popularity of eugenic ideas that aimed to eliminate inherited mental and moral deficiency via incarceration and sterilization of promiscuous girls. In addition to these voluminous writings, I located select but revealing statistical data from juvenile courts and correctional institutions that reinforced my preliminary findings from Milwaukee. Girls throughout the nation were incarcerated in public and private reform schools at roughly twice the rate of boys, even though the girls' "offenses" were almost entirely sexual rather than criminal in nature. As with my research on parent education, this offshoot of my doctoral dissertation identified fascinating new voices that enlarged historians' understanding of the impact of gender on social reform during the early 20th century. Over the past three decades, the historical study of female delinquency has become a major subfield in women's history, the history of childhood, and the history of juvenile justice (Odem & Schlossman, 1991; Schlossman & Cairns, 1993).

A Historian at Rand: Linking Bottom-Up Social History to Policy Research on Children and Parents

Naturally, I was pleased with the directions in which my postdissertation research was moving, but I was also concerned that for all the new data I had located, I was not finding true "bottom-up" voices that enabled me to present children and parents as central actors—as agents in history—as the case files of Milwaukee Juvenile Court had allowed me to do. Thus, my research on parent education mainly broadened historical understanding of the middle-class and academic innovators in the field, not of the parents and children who were the targets of parent education initiatives. Similarly, my research on female delinquency and several other juvenile justice topics that I studied during the late 1970s offered new information about courts, clinics, correctional institutions, and delinquency prevention efforts in public schools but contained very few insights on the children and parents who were the clients of these institutional initiatives. Unexpectedly, it was only after I joined the RAND Corporation in 1979— ostensibly to apply historical perspective more directly toward public policy—that I found several exciting opportunities to contribute not only new historical voices but also unique "bottom-up" voices into historical scholarship on education, childhood, and juvenile justice.6

During my first year at RAND, I received grants from the Ford Foundation and the National Institute of Education (NIE) to conduct two historical research projects on bilingual education and delinquency prevention, respectively. Both projects were closely linked to contemporary policy issues, but each one gave me considerable methodological latitude. For parts of each project, I explored relatively familiar topics data sources in both projects and to offer new perspectives on children and parents as actors in 19th- and early-20th-century education and juvenile justice reform.

In the Ford-sponsored study of bilingual education, I focused on two principal topics, neither of which educational historians knew much about and both of which held potential to enlighten the divisively ideological debates that were then taking place on the subject (these were the early years of the Reagan administration, which was strongly hostile to federal subsidy of bilingual education). First, what was the attitude of professional educators to bilingual education during the first half of the 20th century? Second, did German immigrants to the 19th-century frontier attend bilingual schools that were publicly funded? After reading widely on the first topic, I concluded that my most original contribution as a historian would be to call attention to the little-known but insightful scholarship of George Sanchez, a noted Hispanic American educator of the early 20th century, and of Herbert Manuel, Sanchez's mentor at the University of Texas (and who, despite his last name, was not of Hispanic heritage). Archival data were available for both men that shed light on the entire history of schooling in the Southwest, perhaps the least researched region in all of American educational history. I was especially intrigued that neither Sanchez nor Manuel viewed bilingual education as a "self-evident remedy" to school failures among native Spanish-speaking children. Personally, I was an advocate of bilingual education; thus, I was doubly fascinated by the doubts that both Manuel and Sanchez raised about its limitations and potential misuses in the classroom. My goal as a historian was to use new knowledge about educational controversy in the past to complicate current public debate and to demonstrate the continued relevance of Manuel's and Sanchez's views for everyone in the field (Schlossman, 1983c). (Some bilingual education advocates, it should be noted, interpreted my work as hostile to both Sanchez and bilingual education, although it was not intended to be.)

On the second topic, I was particularly intrigued by how politics had shaped both advocates' and critics' views of the history of language instruction in the United States. Critics of bilingual programs confidently asserted that publicly sponsored vernacular instruction had always been rejected as un-American, backward-looking, and pedagogically impractical because, in a pluralist society, it was impossible to accommodate the language traditions of multiple immigrant groups. Advocates of bilingual programs, on the other hand, countered that politically powerful immigrant groups, notably Germans during the 19th century, had regularly been able to force local public schools to offer subject matter instruction in German. Neither advocates nor critics, alas, seemed much interested in verifying the historical claims that grounded their contemporary politics. What, then, was the "American historical tradition" in bilingual education? I simply did not know, and my review of the relevant historical literature persuaded me that no one else really knew either (but see Fishman, 1966; Kloss, 1977).

In developing a methodology, I wanted most of all to establish a solid empirical base for challenging or confirming the contrasting political claims about the educational tactics of 19th-century Germans. If, during their heyday of political power, German parents did indeed gain public subsidy for extensive vernacular instruction, this would establish a notable historical precedent and undermine the critics' charge that bilingual programs were antithetical to American public school traditions. If, on the other hand, Germans did not try to gain, or tried but did not succeed in gaining, public funding, then advocates for bilingual programs would be forced to acknowledge that their views marked a sharp departure from educational policy and experience in the past.

Because I did not read German, I was seriously handicapped in the types of historical data that I could readily access to judge German public opinion on the subject. That is, I could not read German-language newspapers, diaries, letters, reminiscences, and the like that were potentially ideal to tap into ground-level, 19th-century German opinion. But I had neither sufficient funding nor time to invest in large-scale translation or in a German-speaking research assistant, so I decided to see whether I could develop a viable methodology based solely on English-language sources. Thankfully, from the extensive community-level research that I had done for Cremin on education in the Midwest, I already knew that 19th-century rural schools. Nonetheless, it seemed clear that public funding of German-language instruction was fairly widespread, at least in states where Germans were thickly settled and state authority over education was minimal. But my methodology worked out much better in four important centers of German urban settlement: Cincinnati, St. Louis, Milwaukee, and Indianapolis. In each of those cities, I was able to locate annual school reports that supplied sufficient data, both qualitative and quantitative, to develop a substantial new empirical base for understanding the role of parent advocacy in promoting bilingual education on the urban frontier. Public funding of German instructional programs was, in fact, commonplace in each community, and German parents were remarkably active in fighting politically for the programs' establishment and maintenance. But despite the political activism of German parents, the design and longevity of specific programs varied markedly from city to city; moreover, political battles were often bitter and prolonged, with outcomes varying from year to year. Staffing, supplying, and implementing bilingual programs were administrative nightmares; programs that seemed to be effective one year came apart the next year (Schlossman, 1983e).

Thus, no single or simple conclusions emerged from my analysis that indisputably reinforced or challenged the opposing historical claims of bilingual education protagonists during the early 1980s. But the historical data did introduce the voices of 19th-century parents into current debate, and the analysis also generated two conclusions that, in my judgment, spoke equally to both sides. First, even in communities where bilingual programs were sustained for many decades, no program remained politically uncontested for long. Second, in every city, daunting and recurring implementation problems were common, especially with regard to the development of curricular materials, the selection of teachers, and the refinement of age- or grade-appropriate teaching methods in the classroom. There was, in short, no golden era in 19th-century America where conflict was absent or programs were secure—no single road or final solution for diffusing the political volatility of bilingual education in a pluralist, immigrant-receiving society that saw public schooling as key to cultural assimilation. Only patience, an experimental attitude, and an open recognition of difficulties and failures enabled bilingual programs to survive in individual communities as long as they did.

My NIE-sponsored study of delinquency prevention sought to recreate the rise and demise of innovations in the field during the first half of the 20th century. The study was funded largely because policymakers had little readily available information about the past to draw on in trying to invent new programs for current application. In short, they were genuinely concerned about reinventing the wheel. The project presented many and varied opportunities for both synthetic and original research. As in my bilingual education project, my research strategy was to dedicate as many resources as possible to identify new historical voices that might contribute something unexpected to how policymakers sorted out their options in the present (Cohen & Schlossman, 1977). During the early 1980s, policy evaluations in delinquency prevention often stressed the gap between design and implementation that undermined field experiments before they could be fully tested. My research strategy was to identify new historical data that would speak directly to this current concern about program implementation as well as program design. (The pioneering studies of implementation in education by my RAND colleagues, Paul Berman and Milbrey McLaughlin, were vital in shaping my historical research strategies for analyzing both delinquency prevention and bilingual education [McLaughlin, 1989].)

Not all of my forays were successful in uncovering useful historical data. In several parts of the project, I had little choice but to focus on program design rather than implementation and to synthesize, in somewhat original ways, data that were already known to specialists in the field (Schlossman, 1978a, 1983d; Sedlak & Schlossman, 1985). In two instances, however, my methodological approach achieved more than I initially thought was possible.

First, in Berkeley, California, I learned of an innovative delinquency prevention program— the community coordinating council—that was co-invented during the 1920s by the chief of police, August Vollmer, and the superintendent of schools, Virgil Dickson (a former doctoral student of Lewis Terman's at Stanford University). In addition to several targeted programs that were run exclusively by the police, the coordinating council brought representatives from the schools and the police, mental health, and business communities together considerable statewide influence in California (especially in the Los Angeles area) during the 1930s and 1940s. Luckily for me, in addition to a small archive of Dickson's administration as school superintendent, both Vollmer and the police department had established separate voluminous archives that enabled me to examine the implementation, as well as the design, of the coordinating council experiment (Liss & Schlossman, 1984; Zellman & Schlossman, 1986).

The second and more notable product of the NIE study dealt with delinquency prevention experiments in Depression-era Chicago, especially the Chicago Area Project (CAP). An offspring of research conducted by the world-famous Chicago School of Sociology (W. I. Thomas, Robert Park, Ernest Burgess, Frederick Thrasher, Clifford Shaw, Henry McKay, and many others), the CAP, unlike the Berkeley program, was already legendary in the field of delinquency prevention. In fact, I initially shied away from studying it for precisely that reason, thinking that I could draw what was useful to my project from prior historical and sociological scholarship. As I read the relevant secondary literature, however, it became clear that scholars had devoted their intellectual energies mainly (and repetitively) to articulating the CAP's theoretical premises, not to exploring how it came into being and actually worked on a daily basis—in other words, to program design, not program implementation or maintenance.

When I visited Chicago initially, I did not expect to find an archival base to support the kind of implementation research that I ideally wanted to do; after all, if such data were readily available, wouldn't the numerous previous scholars who had studied the CAP have used them? For whatever reasons, the answer was no. I found a sprawling, minimally organized but extraordinarily rich archive that contained in-depth implementation data for several of the high-delinquency neighborhoods of Chicago where the CAP had operated between the 1930s and the 1950s. Moreover, the archive contained many interviews by the CAP field-workers with youth who belonged to competing neighborhood gangs. It did not take me long to realize that this archive contained the best "bottom-up" historical data I had encountered for studying juvenile delinquency since I had completed my research on Milwaukee Juvenile Court.

Because of time and budget constraints in my overall NIE project, I was compelled to select a single neighborhood (South Chicago) to study both program design and implementation issues in-depth. While still counting my good fortune in locating such data, I was startled several weeks later to learn that another archive that contained the personal papers of the CAP's chief field-worker in South Chicago, Stephen Bubacz, had literally just opened. With these two excellent archives to draw on, I soon completed the first historical study of the CAP in action and even dared to conclude (based on statistical data that had been compiled but never brought to light) that the program may have been successful in reducing rates of juvenile delinquency in South Chicago. The analysis not only shed new empirical light on principles of implementation in the field of delinquency prevention, it also painted a vivid "bottom-up" portrait of the everyday lives of working-class children and parents struggling for individual and community survival in Depression-era Chicago (Schlossman & Sedlak, 1983; Wolcott & Schlossman, in press). Furthermore, several months after the historical research was completed, I teamed with an interdisciplinary team of RAND colleagues to investigate the logic, operations, and effectiveness of delinquency prevention programs currently in operation (early 1980s) in the same community of South Chicago. The result, Delinquency Prevention in South Chicago, was a uniquely integrated study of a pioneer delinquency prevention program in a single neighborhood, past and present, using both qualitative and quantitative analytic techniques. And the program, interestingly enough, appeared to be as successful in the present as in the past in reducing rates of juvenile delinquency (Schlossman, Shavelson, & Zellman, 1984).

New Subjects, New Voices in the History of American education

During the mid-1980s, two unanticipated projects emerged, one supported by RAND and the other by a separate consulting arrangement, enabling me once again to advance my own methodological agenda and discover two sets of fascinating new voices in American educational history. The RAND project was an underserved children and attack the origins of dental disease. As the study progressed, a surprising finding emerged: The incidence of caries among children was already declining precipitously and independently of the experimental interventions. The main explanation appeared to be the dissemination of fluoride throughout American society during the 1970s via municipal water supplies and the food chain as a whole. From a cost-benefit standpoint, the RAND analysts concluded that the new school-based interventions, in addition to being more difficult and expensive to implement than their supporters had anticipated, seemed largely superfluous given the rapid general decline in children's incidence of dental caries (Bell et al., 1984).

In light of these emerging findings, Klein asked me to investigate whether schools had ever been used successfully in the past as sites to improve children's health. I already knew that during the Progressive Era, schools had employed doctors and nurses to identify and treat disease among immigrant children and that schools had cooperated with boards of health to persuade immigrant parents to approve vaccination of their children against various infectious and potentially deadly diseases. Might it not also have been possible, I wondered, that Progressive Era schools took steps to curtail dental disease? Klein doubted this given that a central premise of the recent school-based initiatives was that they were historically unprecedented in reconceiving the role of schools as a delivery site for dental services to children. But he agreed to provide a week of funding to test out my hunch.

Suffice it to say that only a few days of intensive research in school reports and dental journals from the early 20th century were necessary to establish, first, that children's oral health was a major public health concern at the time and, second, that educators and dentists had collaborated to extend school social services to include dentistry. During the Progressive Era, both public and private funds supported school-based and free-standing dental clinics to deliver preventive and reparative services to children who could not otherwise afford them. These preliminary discoveries, Klein believed, justified further inquiry and funding. Over the next several months, I located large amounts of previously unused historical data on children's dentistry in various libraries and archives across the country. The historical research established conclusively that dentistry was part and parcel of the creation of school-based social services during the Progressive Era. Indeed, until the 1950s—with the Depression era as the high point—both preventive services delivered by hygienists and reparative treatments delivered by dentists to children in both schools and free clinics provided the majority of dental services that American children received altogether. This entire set of historical voices had been totally forgotten. The school-based dental experiments of the 1970s were obviously not as original in design or execution as their advocates believed (Schlossman, 2004; Schlossman, Brown, & Sedlak, 1986).

A second unanticipated and fruitful opportunity to identify new voices in educational history came from the Graduate Management Admission Council (GMAC, sponsor of the Graduate Management Admission Test [GMAT]), which during the mid-1980s was conducting research on long-term student demand for the MBA degree for its core constituency of leading American business schools. After several efforts at statistical modeling, the GMAC researchers raised a sobering question: How reliably could they predict future demand for graduate business degrees if they had little understanding of how current demand had originated, and more generally, how and why had graduate business education changed over the course of the 20th century? Although I had no expertise on this topic, the GMAC asked me to assist the council in assessing how much was already known about patterns of change and continuity in MBA programs, which time periods had seen the greatest expansion of demand, and what factors had driven the change process inside and outside of the business and educational communities. The focus, I was told, should be as much as possible on major business schools, both public and private, and every effort should be made to understand both the policy decision-making process and curricular and pedagogical developments. These kinds of information would interest current business school deans and admissions officers, whose responsibility was (particularly as national ranking schemes gained popularity during the 1980s) to keep their institutions ahead of the demand curve.

As I quickly learned, very little was known about the history of American business education, especially at otherwise, appeared to be minimal. Despite these obstacles, the GMAC agreed to fund an initial inquiry designed to illuminate the change process in graduate business education, especially during the post-World War II period. I agreed to lead the project but made my long-term involvement contingent on whether I could add genuinely new historical voices to what was already known about Harvard and Wharton.

At first, I lacked a driving question to motivate the study as a whole or to direct my methodology. But the project took off quickly once I realized that MBA programs began to expand enormously during the 1950s, that curricula and pedagogical methods were often radically transformed during the 1960s, and that not Harvard or Wharton but rather Carnegie Mellon University; the University of Chicago; Northwestern University; Stanford University; the University of California, Berkeley (UC Berkeley); and the University of California, Los Angeles (UCLA), were generally viewed as the new leaders in the field during the postwar era. Furthermore, I learned, most changes in educational philosophy and practice were somehow linked to two widely read, highly critical assessments of business education sponsored by the Ford Foundation and the Carnegie Corporation in 1959. The involvement of foundations in business education reform caught me by surprise, but it was very promising for the development of a research methodology. My earlier studies of parent education had shown how the Rockefeller Foundation, through generous and strategically aimed grants, had virtually recreated the entire field of child development during the 1920s and 1930s at institutions such as the University of Iowa, the University of Minnesota, UC Berkeley, Teachers College at Columbia University, and the University of Toronto (Schlossman, 1981). Had Ford and Carnegie perhaps played a similar change agent role in postwar business education? If so, then why?

I inquired whether the Ford and Carnegie archives (like those at Rockefeller) might be made available for my research. At Carnegie the answer was no, but Ford, which was near the end of a major effort to organize its archives for potential scholarly use, invited me to visit. Suffice it to say that the files of several grant officers and trustees of the Ford Foundation introduced me to some of the most intriguing "insider" voices that I had ever encountered in studying educational history. As early as the 1940s, and very much part of cold war economic strategies aimed against the Soviet Union, Ford (and Carnegie) had begun planning a multiple-pronged grants strategy to shake up American business schools and infuse them with mathematical methods and theoretical insights pioneered by avant-garde social scientists such as Hebert Simon and Robert Merton. Both foundations launched their grants programs (and their critical assessments of current teaching in business schools) during the early 1950s. Within two decades, they had spearheaded a major revolution in the professorial ranks of business schools and in the subject matter, pedagogical methods, and clientele they taught, especially at the graduate level. Thus, the change process had been purposeful, centrally directed, generously funded, quick to take root, and remarkably successful in expanding the popularity of the MBA degree—findings that the sponsors of my research, quite understandably, found intriguing (Schlossman, Sedlak, & Wechsler, 1987b, 1988).

In addition to providing detailed insight into the foundation's role as a change agent, the Ford records contained pointed commentary and considerable documentation (especially correspondence but also curricular materials) gathered from several of the best-known business schools in the country. This information facilitated a next-stage research strategy, the selection of specific university sites for conducting in-depth case studies of the change process in business education during the quarter century following World War II. The methodological emphasis, to be sure, would be almost entirely on "top-down" voices at the Ford Foundation and the individual universities (e.g., presidents, provosts, deans, department chairs). Nonetheless, these were the voices of key players in American universities who historians had marginalized from the mainstream of educational history. Because the growth of MBA programs in size and prestige was one of the most dramatic developments in postwar higher education, I had no doubt that this line of inquiry would significantly expand the boundaries of traditional scholarship in higher and professional education. The contribution would be even greater if I could go beyond Harvard and Wharton and analyze the change process in other influential public and private business schools that had entirely escaped historians' attention.

problem was that no one could remember where the records were currently located. After several days of fruitless search in faculty offices and file cabinets, a secretary retrieved a set of keys to open various doors and closets, the contents of which no one knew. After several failures, she opened a large closet in a student dining area; quite literally, the records I had been seeking began to fall out from the shelves of their jam-packed hiding space of many years. It took several days for me to transport and organize the records, but they were in fact exactly what I needed to begin identifying who pushed for and against change at UCLA during the 1950s and 1960s and to account for pressures inside and outside of the university in shaping the change process (grants from Ford mattered, but competition with archrival UC Berkeley was also key). This awkward uncertain hunt for fugitive administrative records in closets, file cabinets desk drawers, basements, and the like was repeated with numerous variations over the next decade as I enlarged the range of case study sites (e.g., Michigan State University, University of Washington, Ohio State University, University of Michigan, University of Pittsburgh, Northwestern University, Stanford University, Harvard University, Carnegie Mellon University, Dartmouth University, University of Western Ontario) and began the long-term process, which is still under way, of understanding how the modern American business school came into being (Gleeson & Schlossman, 1992a, 1992b, 1995; Gleeson, Schlossman, & Allen, 1993, 1994; Schlossman & Sedlak, 1985, 1988; Schlossman, Sedlak, & Wechsler, 1987a, 1989a, 1989b; Sedlak & Schlossman, 1991).

Personal experience and Research Problems: Homework and the Parent—School Interface

Since the mid-1990s, I have extended my research agenda and method to cover one main new topic in educational history, namely, the place of homework in American schooling. In this instance, and for the only time in my career, personal experience suggested the topic. As the parent of two school-age children, I found myself deeply perplexed (in both public and private schools) by teachers' inconsistent use of homework at all grade levels—from no homework at all in the middle grades, to large assignments in the lower grades, to massive assignments equivalent to an adult work week in upper grades. When I inquired about the logic behind these assignments, I was politely told by principals and teachers that I was an overbearing parent, that my child was not "gifted," or that I was a confused "progressive" who needed a strong dose of William Bennett as an antidote to sentimentalism in education. This frustrating experience prompted two realizations I had never considered before my children attended school: first, that on academic matters (especially in the early grades), homework usually provides the main interface between teachers and parents; and second, that the experience of homework is vital in shaping the spirit of parent-school relations as a whole, for better or for worse (Gill & Schlossman, 1995).

Naturally, I set out to see what educational scholars had to say about homework and its place in American schooling today. I was pleased to learn not only that there was exemplary research on the subject but also that prominent scholars, such as Harris Cooper and Joyce Epstein, had similarly emphasized the key role of homework in parent-school relations (Cooper, 1989a, 1989b, 1994; Epstein, 1983, 1988; Epstein & Pinkow, 1988). From a historical standpoint, however, there was no scholarship whatsoever; homework might just as well have been absent from the 19th- and 20th-century American child's school experience.

Obviously, homework had a history; the problem was that scholars had ignored it. This did not surprise me because, unfortunately, educational historians have ignored many matters concerning the content and methods of day-today instruction in both urban and rural schools. I decided to probe whether the history of homework could be studied not solely as a tool for helping children to learn subject matter but also—and a topic that had interested me since my earliest studies of parent education—as a vehicle for examining relations between parents and schools. If the latter, the research would be especially important to pursue. Despite pioneering research by Carl Kaestle, Geraldine Joncich Clifford, and William Reese on parent-school relations during the 19th and early 20th centuries, and despite Cremin's attempt to interweave family and school experience via the concepts of "educational configuration" and "educational biography," historians homework was one of the most contested school practices in American educational history and that it had long provided parents with a regular outlet to criticize or praise teachers and to express strong views about what went on in school.

At first—and this stage, rather embarrassingly, lasted for nearly a full year of active research—I thought that the seminal period for studying homework was the 1930s. This was consistent with how previous scholars had tracked trends in attitudes toward homework during the previous half century, and I had no reason to challenge their periodization (What are the proper chronological boundaries for studying homework over time?) (Cooper, 1989a; Otto, 1941). I did, however, decide to spend several weeks using the Reader's Guide to Periodical Literature to confirm what I was confident I already knew. As expected, I found both plentiful discourse on homework during the 1930s and a relative absence of such discourse during the 1920s. Publications about homework abounded during the 1930s in both education journals and popular periodicals. Anti-homework sentiment was the dominant theme and a central component of "progressive" educational thought during this period. Numerous communities eliminated homework in the early grades and sharply limited its use in the junior high and high school years—a pattern that would not be substantially altered until after the launching of Sputnik during the late 1950s. Because the documentary evidence introduced such compelling new voices into educational history, I was already preparing an article explaining why sustained dialogue about the place of homework in American schooling had first emerged during the 1930s. It simply did not occur to me to probe further back in time, even though the Readers' Guide to Periodical Literature and other bibliographic tools were readily available to do so. Only nagging doubts about the meaning of two obscure references I had seen to court cases involving homework in Texas and Mississippi in 1887 and 1909, respectively, delayed my completing this article (Balding v. State, 1887; Hobbs v. Germany, 1909).

Retrieving the two court cases spared me from one of the more serious mistakes a historian can make, that is, the misdating of a significant historical movement. Something was clearly happening with regard to homework during the late 19th and early 20th centuries about which I knew nothing. Therefore, I pushed my research effort backward in time and was eventually able to determine conclusively that it was during the late 19th century, not the 1930s, that Americans first began to seriously debate the pros and cons of homework. The relative lack of controversy about homework during the 1920s was the exception, not the rule. And although the views of progressive educators about homework during the 1930s remained fascinating, they could best be understood as refinements of the views of turn-of-the-century commentators such as the psychologist G. Stanley Hall, the physician-reformer Joseph Mayer Rice, and the magazine (Ladies' Home Journal) publisher Edward J. Bok.

Nonetheless, despite my initial misunderstanding of the chronology, my main interpretive point about the discourse on homework held up: During the first half of the 20th century, most educational scholars were sharply critical of teachers' reliance on heavy, repetitive, memory-driven homework assignments of the sort that had shaped the modal school experience since the early 19th century. As one of the educators put it, homework (especially before the fifth grade) was widely viewed as "a sin against childhood." This conclusion inevitably raised new questions about the meaning and impact of the "progressive" movement in American education during its heyday (Gill & Schlossman, 1996).

But what about parents' views of homework? Did parents mainly agree or disagree with the anti-homework sentiments of the educational experts? After all, it was the voices of parents— my own voice, in particular—that had provided the initial incentive for me to open this topic to historical analysis. My prior data collection from educational journals, popular periodicals, school board reports, and newspapers during the first half of the 20th century had left me uncertain whether parents mainly agreed or disagreed with the anti-homework position of the professional educators. Thus, after completing a follow-up article that extended analysis of educators' views on homework from the 1930s to the 1960s (Gill & Schlossman, 2000), I began to probe whether a "bottom-up" line of inquiry was possible to determine whether educators and parents mainly agreed or disagreed about homework.

sentiment? Eventually I concluded that it was not. After many months of research, I was able to locate more disinterested data—usually contained in locally administered polls or surveys of parents' views, in doctoral dissertations about educational practice and attitudes toward schooling, and in incidental comments by parents and school officials reported in annual school reports and in local newspapers— that revealed parents' voices unmediated by the views of homework opponents. Overall, these data showed that parents mainly disagreed with educators. In all regions of the country, parents supported substantial homework for their children, not only to improve academic performance but also to build character, train work habits, fill otherwise idle time, and provide a concrete starting point for facilitating parent-teacher communication. From a methodological standpoint, then, it was clearly vital to locate "bottom-up" historical data before one could reach a fair overall assessment of what parents thought about homework (Gill & Schlossman, 2003b).

But could this methodology be carried a step further to identify not only parents' but also students' views on homework during the first half of the 20th century? In other words, might it be possible—on this topic as in some of my research on juvenile delinquency—to introduce children as active historical agents with their own points of view? Alas, after several years of data collection, it became obvious that I could not produce a parallel study on children's views. The available empirical data were simply inadequate to provide a reliable answer. But what about children's homework behaviors? If their views were unascertainable, could their actual conduct be charted by obtaining data on how much homework they actually did? For some time, this prospect did seem alive because more than a dozen quantitative and qualitative studies were conducted during the first half of the 20th century on how much time children spent doing homework. On closer inspection, however, the studies were too local, too diverse in grade levels, too small in scale, and too inconsistent in research methods to yield reliable measures on students' homework at any one point in time, much less to chart change over time. Thus, this line of historical inquiry also had to be abandoned.

But what about the more recent past—the second half of the 20th century? At the suggestion of my Carnegie Mellon colleague, John Modell, I had already acquired some unique statistical data on homework during the 1940s, 1950s, and 1960s from the little-known Purdue Opinion Panel, a nationally representative sample of 9th- to 12th-grade students' views on a wide variety of topics related to schooling. The panel's creator, H. H. Remmers, periodically asked students about their attitudes and behaviors regarding homework and how much of it they did on a regular basis. If comparable data could be located for the period between the 1970s and 1990s, it might be possible to assess changes in students' homework behaviors over this lengthy time period and to compare behaviors with shifting currents of thought and policy regarding homework.

Fortunately, with a number of minor adjustments, it was indeed possible to link homework data from the Purdue Opinion Panel with several data sets covering more recent time periods to track homework trends among high school students for the entire half century.7 Analysis of these data resulted in the publication in 2003 of "A Nation at Rest" (Gill & Schlossman, 2003a). Not only were high school students at the turn of the 21st century not doing more homework than they had done during the early 1980s when the modern academic "excellence" movement began, but their homework behaviors actually had not changed much (except for a brief post-Sputnik burst) since the end of World War II. In other words, the "voice" of students' behaviors—to the extent that statistical data could capture that voice—was clear: Doing a minimal amount of homework was the American tradition.

Not surprisingly, "A Nation at Rest" garnered considerable publicity in the educational and popular press because it confounded conventional wisdom about how much homework students were actually doing during the late 1990s (Gill & Schlossman, 2003a). From my perspective, however, the article's chief contribution lay more in the novel historical questions that preceded and motivated it, the use it made of history to illuminate and complicate current educational discourse, and the example it provided of why methodological invention is so important to discover new voices and integrate them into the tapestry of educational history (Gill & Schlossman, 2004). In these ways, despite its heavy reliance on quantification, the article was consistent with the pathway I had long followed to guide my research in the histories of education, childhood, and juvenile

I left RAND during the late 1980s to teach in the Department of History at Carnegie Mellon, and in the main I have continued to elaborate the methodologically driven research agenda that I set in graduate school during the early 1970s. My one main deviation occurred in 1993 when I returned to RAND as a full-time consultant to conduct a series of historical inquiries about blacks, women, and gays in the military. This research was part of a multidisciplinary study (prompted by President Clinton's directive to end discrimination in the military on the basis of sexual orientation) designed to provide the secretary of defense with information and analysis relevant to the development of policies toward openly homosexual military personnel. From the start, it was clear that there would be hardly any time to conduct original historical research. Instead, my goal would be to synthesize and interpret readily available knowledge (i.e., secondary sources and published collections of primary documents) in ways that intersected with the findings of other RAND analysts working on the same subject from different social science, legal, and medical perspectives. Assisted by an extraordinary team of Carnegie Mellon graduate students, I shuttled regularly between Pittsburgh and Santa Monica and worked long days to accommodate a tight political timetable. After several months, we produced several hundred pages of well-grounded, albeit mainly derivative, historical scholarship on each of our three topics.8 Afterward, in collaboration with one of the graduate students, I expanded the research on African Americans into a book about the desegregation of the U.S. armed forces (Mershon & Schlossman, 1998).

Why did I agree to deviate so sharply from my own research agenda to participate in this unrelated RAND project? There were three main reasons. First, the opportunity to put history to direct policy use at the federal level amid swirling political and cultural controversies was simply too novel and challenging to turn down. Second was the undeniable attraction for a historian to work as an integral part of a research team—a collaborative role that historians are rarely asked to play in the modern world of social science research (except for the occasional historian, including myself, who is asked to participate in National Research Council studies). Third, although no time was available to identify new historical voices, I believed that the project held potential to enable history itself, as a discipline, to be heard as a distinct and equal voice in a large-scale, multidisciplinary, policy research project. As it turned out, this confidence was well placed; under the cosmopolitan intellectual leadership of Bernard Rostker, an economist, and Scott Harris, a political scientist, our voice as historians was privileged no more and no less than that of other disciplines in generating a coherent analytic perspective and specifying policy recommendations (National Defense Research Institute, 1993).

Although this venture into applied, synthetic historical research was as exciting as any I had ever tackled, the methodological challenge of discovering new voices in the history of education, childhood, and juvenile justice is still what motivates my career.

Periodically, for lack of alternative documents to evoke their voices, I have found it necessary to collect quantitative data to write about certain groups of children whose life experiences would otherwise remain lost to history. So, for example, I have analyzed statistics from 19th- and early-20th-century reform schools to demonstrate the regular incarceration of non-criminal children, from adult prisons to determine how often individuals ages 17 years or younger were incarcerated there (most were minor offenders), and from courts to analyze how race and nationality influenced judicial decision making about whether to incarcerate a delinquent child and in what type of institution to incarcerate the child. I am quick to admit that I find the collection and analysis of quantitative data intrinsically dull compared with the chock-full-of-life "top-down" and "bottom-up" documents that animate most of my historical research. Still, when such documents are unavailable, I find myself turning increasingly to quantitative analysis, especially for writing about arrested and incarcerated children whose very existence would remain forgotten if not for the collective statistical portraits that historians can draw of them from police, court, and correctional records (Schlossman, 1989, 1995; Schlossman & Cairns, 1993; Schlossman & Pisciotta, 1986; Schlossman et al., 1984; Schlossman & Spillane, 1995; Schlossman & Turner, 1990, 1993; Wolcott & Schlossman, 2004). Whatever the data sources, the prime challenge for me as a historian remains to use method imaginatively so as to generate new knowledge about otherwise anonymous children, parents, and educators in the past and to integrate that knowledge into how we design policies to educate and safeguard the well-being of children and youth today.

learning that results from that effort, direct or indirect, intended or unintended. The definition is intended to call attention to the wide range of situations and institutions in which education has gone forward over the years" (p. x).

2. On "educational configurations," Cremin (1977) observed "the tendency of educative institutions at particular times and places to relate to one another in configurations of education. ... Each of the institutions within a given configuration interacts with the others and with the larger society that sustains it and that is in turn affected by it. Configurations of education also interact, as configurations, with the society of which they are part" (p. 142).

3. On "educational biography," Cremin (1976) wrote, "An educational biography is an account or portrayal of an individual life, focusing on the experience of education. ... Individual Americans came to educational opportunities with their own purposes and their own agenda and moved through the institutions and configurations of education in their own ways" (p. 42). With regard to the 19th century in particular, Cremin (1980) noted "the striking range of human character that always issues, to greater or lesser extent, from any particular set of educational arrangements, whatever the time or the place in human history" (p. 451).

4. I have detailed this research process in the introduction to Transforming Juvenile Justice: Reform Ideals and Institutional Realities (Schlossman, 2005, pp. xiii-xxxi).

5. Ellen Lagemann, now dean of Harvard Graduate School of Education, served as Cremin's research assistant for his case study of New York City and for other topics as well. American Education: The National Experience (Cremin, 1980) was awarded the Pulitzer Prize in history.

6. Professional collaboration is one of the joys of scholarly research. Throughout my career, I have been blessed with co-authors who have enriched my life immeasurably as a scholar. To avoid confusion and awkwardness, I have not mentioned any of the co-authors' names in the text, but each co-author is clearly identified in the references and citations. Let me extend special recognition to those colleagues with whom I have been fortunate to co-author multiple articles: Brian Gill (RAND), Robert Gleeson (Northern Illinois), Sherie Mershon (Carnegie Mellon), Michael Sedlak (Michigan State), Susan Turner (RAND), Harold Wechsler (Rochester), David Wolcott (Miami of Ohio), and Gail Zellman (RAND).

7. Data for analyzing trends during the 1970s to 1990s came from the National Assessment of Educational Progress, the National Longitudinal Survey, High School and Beyond, Monitoring the Future (University of Michigan), the Higher Education Research Institute (UCLA), and the Institute for Social Research (University of Michigan). In "A Nation at Rest" (Gill & Schlossman, 2003a), we explained in detail the adjustments we made to compare these data sets with one another and with the data from the Purdue Opinion Panel from the 1940s to 1960s.

8. It should also be noted that the RAND study argued for a very different policy solution from the one ultimately endorsed by President Clinton as, informally, "Don't ask, don't tell, don't pursue."

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