

# Doing Exemplary Research

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# War and Peace: The Evolution of Modern Personnel Administration in U.S. Industry<sup>1</sup>

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This paper charts the transformation of the employment relationship in different industries during the second quarter of this century and is based on a representative sampling of U.S. business organizations. The first section documents changes in the control systems that prevailed in U.S. industries between the Depression and the end of World War II. The descriptive analyses generally corroborate portraits that have recently been provided by neo-Marxists of how and where technical and bureaucratic controls evolved. The second section sketches an explanation for the rapid diffusion of bureaucratic controls that apparently occurred between 1939 and 1946. It examines the role of three key constituencies in shaping modern systems of work force control: labor unions, personnel professionals, and the state. In particular, the analyses underscore the large role of government intervention in manpower activities during World War II in bureaucratizing employment. This effect of the state blurs the distinction between "efficiency" and "control" explanations of bureaucratic controls and internal labor markets, calling attention to institutional sources of change in organizations' employment structures. The concluding section highlights the implications of the findings for efforts to understand the employment relationship.

Recent research has examined how organizational and institutional arrangements shape labor market outcomes, rekindling interest in how and why employment practices vary across sectors of the economy. Differences among firms and industries in work arrangements, control systems, and the presence of internal labor markets are claimed to account for

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## Introductory Remarks: Journey 6

"War and Peace: The Evolution of Modern Personnel Administration in U.S. Industry," by James N. Baron, Frank R. Dobbin, and P. Devereaux Jennings. Published in the *American Journal of Sociology*, 92 (1986), 350-383. Copyright 1986 by The University of Chicago. Excerpts from this article are reprinted in this volume by permission.

"Jim Baron was rummaging through old personnel documents in the library . . ." Thus Jennings, Dobbin, and Baron describe the serendipitous beginning of the project that led to publication of "War and Peace." Baron's rediscovery of long-forgotten Conference Board surveys of personnel practices in U.S. industries unleashed a research process that constantly threatened the authors' sense of intellectual equilibrium. They wanted to recover the understandings of the original respondents and organizational actors and researchers. Against this they balanced their desire to address a contemporary organizational debate regarding the emergence of bureaucratic personnel practices. The debate pits the functionalist explanation rooted in efficiency against the radical emphasis on managerial control of the workplace. The data ultimately included surveys in 1927, 1935, 1939, and 1946, as well as a diverse set of historical documents.

The authors utilized these data to make two key contributions. The first relates to the evidence for the radical account. The radical perspective has largely emerged out of case studies of particular firms and industries. The research paper establishes the external generalizability of the pattern of technical and bureaucratic control practices that feature in radical accounts. Baron, Dobbin, and Jennings document the

emergence of this pattern in most U.S. industries. The second contribution is their questioning of the parameters of the debate. Both perspectives emphasize internal organizational imperatives as the source of change in personnel practice. The data simply do not allow Baron, Dobbin, and Jennings to limit their search for explanations to internal mechanisms. As Pamela Tolbert succinctly puts it in her commentary, a fuller explanation "locates the source of the institutionalization of personnel structures in the interwoven interests of the state, industrial managers, unions, and an emergent occupational group."

This contribution depended on the authors' attention to the complex interplay of theory, data, and historical context. Theoretical controversy motivated the search for data and initial attempts to analyze it. Insights from institutional theory helped to direct the search for historical documentation of important aspects of context and subsequent data analysis. The data themselves constrained the domain of theoretical issues that could be addressed. For example, the data could not be disaggregated to the organizational level. Initially confusing patterns in the data analysis drove the search for both additional understanding of the historical context and utilization of diverse theoretical resources.

In their commentary, the authors describe the difficult road to mastery of the meaning that these archival data have for us when interpreted within the historical context. They weave a fascinating and convincing story relying on careful reasoning, the relatively simple data analyses that the data quality will support, and relevant historical documentation. Pam Tolbert and Woody Powell contribute to our appreciation of this research by placing the authors' contribution within the larger theoretical context of organizational studies. Tolbert emphasizes the relevance of the research to current concerns and praises the authors' sensitivity to the theoretical complexity of the phenomenon they study. Powell focuses on the authors' break with traditional explanations for the growth of personnel systems. Both commentators point to another important characteristic of exemplary research: the fact that it is never finished. Exemplary research opens up inquiry. It makes possible and motivates the asking and answering of new questions. In this respect, Baron, Dobbin, and Jennings's research is clearly exemplary.

We see several aspects of their research process that deserve attention. First is the advantage of multiple authorship. This is a case in which the project benefited from a specialized division of labor. Dobbin developed a familiarity with the historical materials while Jennings buried himself in piles of statistical analyses. The interaction of these two types of data collection and analysis was essential to the

insights the project produced. The second aspect is tenacity. The authors stuck with the project despite numerous dead ends, disappointments, and frustrations. Third is the willingness to let go. The authors dropped favored lines of inquiry, unconfirmed hypotheses, and the factor analysis results. This aspect is potentially in conflict with tenacity, thus a tension between the two will inevitably develop. It is not easy to drop lines of inquiry, pet hypotheses, and the products of hard work simply because they do not contribute to the final research outcome. We suspect that the ability to accept and live with this tension is a useful skill to have on the journey to exemplary research.

## JOURNEY SIX

### Making War and Peace

P. Devereaux Jennings

Frank R. Dobbin

James N. Baron

There is a temptation to depict the origins and evolution of our project as deliberate and well planned, but the making of "War and Peace" was neither. The beginning of the project was particularly serendipitous. Jim Baron was rummaging through old personnel documents in the library and happened across the data we eventually analyzed in the paper. Once under way, the project progressed through a series of seeming missteps and backward moves. Thinking we might perform elaborate econometric analyses, we coded dozens of industry-level variables, over time, and estimated scads of preliminary regression models. Thinking that we would focus on the professionalization of personnel administration, we scoured every article we could locate on the nature of personnel from the turn of the century through the 1950s. Ultimately, we abandoned these and various other approaches in favor of one that focused primarily on the impact of the state during World War II, documented by both qualitative and quantitative historical data.

In retrospect, this somewhat disorganized process of doing organizational research was actually critical for the end result. Perhaps the most important lesson we learned is that doing quantitative analysis with historical data must be a highly iterative process. Moving back and forth frequently between the Conference Board data and primary historical sources was indispensable. Doing so prevented us from developing ad hoc arguments about relationships we found in the quantitative data that were inconsistent with the historical record and, at the same time, prevented us from making arguments based on historical documents that were contradicted by the data.

“ This paper charts the transformation of the employment relationship in different industries during the second quarter of this century and is based on a representative sampling of U.S. business organizations. The first section documents changes in the control systems that prevailed in the U.S. industries between the Depression and the end of World War II. The descriptive analyses generally corroborate portraits that have recently been provided by neo-Marxists of how and where technical and bureaucratic controls evolved. The second section sketches an explanation for the rapid diffusion of bureaucratic controls that apparently occurred between 1939 and 1946. It examines the role of three key constituencies in shaping modern systems of work force control: labor unions, personnel professionals, and the state. In particular, the analyses underscore the large role of government intervention in manpower activities during World War II in bureaucratizing employment. This effect of the state blurs the distinction between “efficiency” and “control” explanations of bureaucratic controls and internal labor markets, calling attention to institutional sources of change in organizations’ employment structures. The concluding section highlights the implications of the findings for efforts to understand the employment relationship. ”

(Baron, Dobbin, & Jennings, 1986, p. 350)

Yet this research strategy was frustrating because it meant that we were continually disproving many of our pet hypotheses. Just when we had developed an elegant theory about something we found in the data, we discovered contradictory evidence in the extant historical materials. Just when we had crafted an interesting argument based on the historical materials, our Conference Board data failed to back it up. Had we stuck to one method or the other, our conclusions would doubtless have been very different, and we would probably have reached them considerably sooner than we did. On the whole, our experience showed us why “many quantitative analyses of historical series fail to realize their potential [by neglecting history]” (Isaac & Griffin, 1989, p. 873): It is much easier to neglect history. In the following paragraphs we offer some additional details and illustrative examples of the research process involved in the making of “War and Peace.”

### The Serendipitous Start

What led Baron to the particular section of the library where he found our data was a combination of foresight and luck. Baron was interested in assessing theories regarding the determinants of internal labor markets and other aspects of organizational personnel systems.

Baron thought that if he could locate historical data across a range of firms or industries, he could not only shed light on long-standing debates about the origins of internal labor markets, but perhaps also offer insights into recent trends *away* from reliance on internal labor markets and long-term employment relations (Pfeffer & Baron, 1988). So Baron began scavenging through the Stanford libraries, where, in late 1982, he happened upon a dusty volume containing the most recent (1946) of the Conference Board personnel surveys that we eventually analyzed in our paper. That report referred cryptically to three prior surveys that had been done on related topics, but it gave no references or titles; another six months of detective work had to be invested before we tracked them down. (No one at the Conference Board could recall such ancient history. Eventually, they produced a chronological listing for us of their publications since 1916, which helped us find the three other surveys. Perhaps unduly influenced by the Marxist literature on control systems, it never occurred to us, for instance, that the results of the 1936 personnel survey would have been published under a title like "What Employers Are Doing for Employees.") After repeated inquiries and efforts to track down Conference Board old-timers, we learned, unfortunately, that they had seen no reason to retain the original questionnaires, which prevented us from undertaking the organization-level analyses we had been shooting for.

“ The NICB members have traditionally had higher response rates than nonmembers, and “in any survey that is conducted solely through correspondence it seems justifiable to assume that it is the more progressive companies that will cooperate” (NICB 1947, p. 3). Yet despite these real and potential biases, the prevalence of the personnel practices that we examine is actually *lower* than in other surveys conducted during this period, which probably suffered from even more severe response and sample biases. For example, although the mean firm size in the 1935 NICB sample (1,836) was higher than the population mean for establishments, two other surveys done at that time reported mean firm sizes of 2,557 and 4,753 (Pierce School 1935; Parks 1936). In addition, three surveys from the early 1930s of personnel practices, all more limited in coverage, reported even greater prevalence of personnel departments, job analyses, rating systems, and employment tests than did the 1935 NICB survey, both in the aggregate and for specific industries (Pierce School 1935; Parks 1936; Timmons 1931). For instance, Pierce School (1935) reported that 43% of the 254 firms surveyed used job analysis, whereas 18% of firms in the NICB sample reported using job analysis (or 27% when NICB industry groups are weighted to reflect the industry mix in the Pierce School study). For those few industries represented by a relatively large number of cases in these smaller surveys, estimates of the prevalence of various personnel practices correspond more closely



to the NICB data. This, in turn, gives us some confidence in the NICB data.

In short, the NICB surveys apparently provide the most reliable and comprehensive data available on personnel practices in the early decades of this century. One historian of the period refers to the NICB studies as "excellent in every respect, . . . [they] offer the best statistical evidence available on most aspects of welfare capitalism" (Brandes 1976, p. 193), and scholars have recently used these data in charting the spread of personnel departments and internal labor markets (e.g., Jacoby 1983, 1984, 1985; Kochan and Cappelli 1984). Although the samples are perhaps biased toward organizations that were likely to adopt bureaucratic personnel practices, and thus may misrepresent overall levels of usage, they portray employment practices across industries and over time more accurately and in greater detail than other possible data sources. 99

(Baron, Dobbin, & Jennings, 1986, p. 359)

Baron was first intrigued by these data because they promised to help adjudicate economic efficiency and neo-Marxist control arguments about why firms adopt personnel practices (e.g., Edwards, 1979). However, the data he uncovered helped frame the research question at least as much as the research questions determined the kinds of data he sought. This feature of the research process seems worth emphasizing: Success in archival research depends not only on the ability to find data that allow important questions to be addressed, but also on divining the questions that can (and cannot) be illuminated by data that happen to come your way. Baron had a similar experience earlier in his career, when he came across a vast, rich archive of information gathered by the U.S. Employment Service describing the job and promotion structures among a large and diverse sample of California firms (Baron & Bielby, 1980). His interest in structural bases of inequality sent him looking for these kinds of data, but finding them also steered him toward a whole new set of organizational concerns that he might not otherwise have pursued.

After locating the data, Baron began searching for research assistants to help him realize the full potential of the project. He presented his new ideas to Dobbin in a prearranged meeting to discuss Dobbin's research on the origins of public policy and its effects on personnel practices. Baron asked Dobbin if he knew any graduate students who were interested: Dobbin was. Baron was introduced to Jennings by Jennings' adviser, who worked next door to Baron and had heard he was looking for research assistants. Jennings had been researching various issues relating to organizational control and was interested in

supplementing his student stipend. Baron wanted a second research assistant, so he hired Jennings, at least for the summer of 1983.

### A Series of Missteps

The threesome began with what seemed to be a very straightforward set of hypotheses about the determinants of organizational employment practices. These hypotheses were generated by a major debate among theorists of the employment relationship. Efficiency theorists argued that organizational size, turnover, and work force demographics were primarily responsible for the development of bureaucratic employment practices, while other theorists, especially neo-Marxists, argued that unionization, efforts to reduce worker autonomy, and increasing firm size led to the development of new methods of technical and bureaucratic control over labor. We sought to relate variations in control practices across industries to differences in these efficiency- and control-related imperatives.

After a year of collecting new variables, running exploratory regressions every which way, and playing with varieties of statistical methods to collapse the sets of personnel practices into coherent employment regimes, we came to a dead end: Our analyses did not explain much of the variance in practices across industries. The main reason was that the variation over time appeared greater than the variation across industries. There was an enormous growth across all industries in the use of "bureaucratic control" between 1939 and 1946, and less variation within each time period in the use of different control regimes by industries than we had anticipated. Moreover, some of the interindustry differences made little sense to us in terms of either efficiency or control imperatives.

At first we did not know exactly what to make of this. We were somewhat disappointed that interindustry variation in the cross section paled in comparison to variation over time by industry. Most historical studies and organizational theories highlighted the role of interindustry differences in firm-specific skills, firm size, work force race and gender, unionization, and the like. The data seemed somewhat inconsistent with all these accounts. Yet we soon realized that if the data could not help adjudicate among competing theories very well, perhaps it was because those theories were deficient, not because the data were flawed. We became excited by the prospect of sorting

out why many features of bureaucratic control proliferated during World War II.

“ An employment-stabilization plan introduced in shipbuilding in 1941 illustrates how the state attempted to limit competition among firms for employees, which had been producing high turnover and wage inflation, by developing a bureaucratic model of personnel relations for an entire industry. In southern California, the creation of large shipbuilding and aerospace industries within the same labor market wreaked havoc on both industries (Gray 1943, p. 7). In April 1941, the National Defense Advisory Committee, predecessor of the WLB, organized a conference of management and labor representatives from the Pacific shipbuilding industry. Guidelines concerning wages, hours, shift work, strike avoidance, and apprentice training were adopted (U.S. Bureau of Labor Statistics 1941, p. 1162). These guidelines effectively standardized industry work arrangements and working conditions to prevent “pirating” of workers. Similarly, in June 1942, the building trades unions agreed to a WLB proposal to stabilize wage rates on all federal projects. Though less complex, this agreement had the same effect as the shipbuilding stabilization plan: drastically reducing turnover and wage competition by creating a standard model of employment relations within the industry. Jacoby has suggested that governmental intervention during World War I had a similar effect (1985, pp. 140-47). ”

(Baron, Dobbin, & Jennings, 1986, p. 369)

Dobbin began to research wartime federal policy. He examined a variety of sources, including the orders, directives, and reports of such agencies as the War Production Board and the War Labor Board. As it turned out, these and other agencies took direct and indirect steps that encouraged firms to use bureaucratic personnel practices and to develop other internal labor market mechanisms. Government boards and agencies assumed control of coal mines and railways during the war and intervened in labor negotiations in steel. The government also instructed the auto industry to retool to make tanks, and it pumped money into the airplane, explosives, and rubber industries. A review of wartime management publications found that managers responded to such changes by promoting the new employment practices in order to either cope with or circumvent federal control. We thought we had finally figured out what was going on: Our survey data should show exceptional rises in the use of bureaucratic practices in strategic, war-related industries, where federal intervention was greatest.

Yet when we returned to these data we found that the war-related industries showed only slightly higher than average increases in the

use of bureaucratic personnel practices, compared with other manufacturing and nonmanufacturing industries. Still convinced that the war-related industry hypothesis was right, we tried analyzing the impact of the war differently, looking at industries that had the greatest problems with turnover, those that had the lion's share of war contracts, and those that saw the greatest production increases during the war. Again, war industries did not seem to differ much from other industries in the Conference Board data. We thought we had come to another dead end.

Our response—almost a reflex after a year and a half of work—was to return to the primary historical sources. According to federal documents and corporate histories, government policies affected not only industries that were directly involved in the war effort, but those that were not as well. For instance, federal hiring controls were often used to prevent the loss of munitions workers to other industries, which meant nonmunitions industries had at least as much need for personnel practices that might quell turnover as had munitions industries. The state appeared to be an important source of and stimulus for the development of the modern bureaucratic regime, which diffused through both obvious and subtle means, including coercive and mimetic pressures as well as personnel functionaries who were active in interpreting the institutional environment.

“ Firms were therefore compelled to initiate or expand personnel departments to document their needs. Because employees had to classify jobs by skill and wage categories to satisfy the new national stabilization plans, job analysis and evaluation flourished (Walters 1945, pp. 10-11), as documented in table 1. The government also required firms to file “manning tables” detailing skill and manpower needs and encouraged them to enumerate jobs in terms of new *Dictionary of Occupational Titles (DOT)* guidelines (U.S. Bureau of Labor Statistics 1945, pp. 419-20). Thus, the government encouraged the formalization of work roles and the diffusion of standard job definitions across firms by providing employers with a free and easily accessible job-analysis system. Other reporting requirements necessitated employment and turnover records, rating and salary classification systems, and promotion paths reflecting skill gradients among jobs—all designed to aid the war effort by ensuring maximum utilization of human resources within and among firms. Companies without competent personnel departments were hard-pressed to justify their staffing requirements, and firms that had not previously done so moved quickly to implement or augment personnel departments, job analysis and evaluation systems, wage surveys, and manpower analyses to substantiate their labor needs. ”

(Baron, Dobbin, & Jennings, 1986, pp. 370-371)

## Assembling the Article

Persistence, sometimes verging on desperation, was beginning to pay off. We felt that we had pinned down what was going on during the war; now we had to write up different sections of the project and try to assemble them into a coherent paper. Unwilling to give up our concern with control versus efficiency arguments, we relied on the original introduction that had focused on that debate. We incorporated sections on turnover and size, on unionization, and on federal intervention—each the length of a journal article. In addition, we had run hundreds of descriptive analyses of the data, using very imaginable data-reduction technique—from k-means clustering to multidimensional scaling to factor analysis—in order to group practices and industries. We decided to report only the results of our principal components analyses because the results were fairly similar using each technique. We also had dozens of quotes to back up every point we wanted to make. Here we ran up against one of the disadvantages of combining quantitative and historical methods: It is not easy to give up all of the evidentiary quotes and historical asides you amass, especially when each author feels somewhat committed to the specific prose he has penned. We felt we had enough material to write a book, and we even toyed with the idea of following up the paper with one. But we eventually managed to pare the paper down to a piece merely twice the length of an unusually verbose journal article. We presented the paper at a session of the American Sociological Association meetings, and after a few more rounds of revisions, submitted it to the *American Journal of Sociology*.

We received a "revise and resubmit." The editor said that reviewers found "substantial merit" in the manuscript, but that it was too long. Questions were raised about the factor analyses. One reviewer liked the analyses, but suggested that we had not fully mined the results we presented. Another reader of the paper had already proposed that we eliminate the factor analyses, since they were tangential to the central argument but might serve as the focus for a second paper. The somewhat contradictory nature of the reviews was frustrating, and we were all exasperated by the prospect of having to go through another round of revisions and rewrites. We let the paper sit—a case of "out of sight, out of mind." After calming down, we went back to work.

We had resisted removing the factor analyses before because of the time and effort we had spent generating them, but eliminating the six tables of factor loadings and factor scores now made sense: we could save space, avoid additional explanation, and put them in the pile of

already omitted items to craft a sequel to "War and Peace" (Baron, Jennings, & Dobbin, 1988). In the fall of 1985, we submitted the final draft with only summary data on differences in practices across industries and over time and with fewer quotes and historical passages. The paper also had more on the representativeness of the data and a refocused introduction and conclusion. *AJS* accepted it. We were happy.

“<sup>8</sup>These conclusions (and those that follow in this section) are buttressed by supplemental analyses of the 1935-46 data, including factor and cluster analyses of industries and personnel practices. Detailed results are available on request.”

(Baron, Dobbin, & Jennings, 1986, p. 361)

### Learning from "War and Peace"

If "War and Peace" has any strengths, they are based on the way it tries to make sense of a complex set of theories about the employment relationship, using unique and varied data. The match of data, methods, and theory occurred only after a great deal of effort and debate. As it turned out, our aim was not simply to determine which theory explained more variance, but to develop a coherent explanation using a wide range of theories, methods, and data. We did not discard data along the way when they seemed to make no theoretical sense; nor did we ever completely falsify the different sets of hypotheses. We held them in reserve until we could use them to help assemble a more refined, complete picture of the evolution of modern employment relationships.

The connections among data, methods, and theory became apparent only through a long series of missteps, dead ends, and rewrites. At times it appeared to Jennings and Dobbin that they were running analyses aimlessly. Baron had the same view, but also felt pressured to preserve a sense of "progress." Ultimately, each set of analyses led to a set of provisional hypotheses, which, in turn, led to more analyses, often with different techniques, and further modification of the hypotheses. In this way, the analyses were strung together, even if their overall direction and pattern were clear to us only in retrospect. At the same time, we collected historical documents on whole topics, such as work force demographics, which we never really explored in the paper, but which helped give us a sense of context for interpreting those documents and analyses that we did use. Connections

were made, in other words, through a halting process of exploration and iteration, guided by a great deal of thoughtful reflection and some luck.

Any strengths of "War and Peace" are also a result of our diverse talents and backgrounds. All three of us were interested in organizations, but we came to that interest in very different ways. Baron came to it from his work on structural causes of stratification and workplace discrimination; Dobbin, from his work on the institutional effects of public policy on personnel practices; Jennings, from his work on corporate control mechanisms. Given such different orientations and interests, we sometimes had difficulty coming to consensus, but the advantage of this diversity was that we gave full airing to a wide range of theories. In addition to our different theoretical orientations, we had somewhat different empirical and methodological orientations. Both Baron and Jennings had experience in amassing, manipulating, and analyzing large panel data sets, whereas Dobbin had more experience collecting survey data and applying historical research methods. Finally, we were also somewhat diverse (at least originally) in our standards of excellence and timetables for this project. Whether because of temperament or career stage, Baron was probably more focused on the costs of errors and adverse reviews, while Dobbin and Jennings were probably more focused on the returns of research and publication, impatient with the time the project was taking. The fact that the three of us liked one another and were quite similar in many respects generally helped ease these tensions and divergent interests, though on some occasions that may simply have made the role conflicts stickier.

The project had some impact on the work each author subsequently did. It sparked an interest in human resources management for Jennings, who is now pursuing a project on the professionalization of that field (Jennings & Moore, 1990). It fueled Dobbin's interest in the effects of public policy on organizational practices, and he is now using Conference Board data to look at the impact of the federal government on the development and diffusion of fringe benefits (Dobbin, 1988). And it strengthened Baron's interest in understanding how historical forces and different institutional environments help shape the employment arrangements that organizations adopt, interests he has since pursued in a number of different domains, including California's state civil service (Baron, Mittman, & Newman, 1991; Baron & Newman, 1990; Strang & Baron, 1990). The experience has convinced us of the usefulness of supplementing data analysis with primary historical research where possible, and vice versa.

The article also generated some debate—at least initially. After it appeared, the *American Journal of Sociology* published a comment by Charles Denk (1988), who was a year ahead of Jennings and Dobbin in the Stanford sociology doctoral program. The comment made some methodological suggestions concerning longitudinal analyses for the data. The suggestions made good sense, but we had not seen Denk's comment prior to its submission for publication and therefore had no chance to discuss with him our own explorations with dynamic analyses. So Jennings and Baron drafted a formal reply to Denk (Baron, Dobbin, & Jennings, 1988) with some help from Dobbin, who was en route to a new job. We acknowledged that Denk made useful suggestions, but pointed out that the limitations of our data had prevented us from carrying out suggestions such as his.

However, our sense is that "War and Peace" has had little impact on any of its intended audiences. This may have something to do with our effort to reach a number of different audiences, so that we succeeded in reaching none of them. Predictably, economists and historians told us we were reinventing wheels they had discovered long ago. Organization theorists do not seem to have found much of organizational interest in our analyses; many researchers interested in stratification and work no doubt see our work as far afield from their concerns; and historical sociologists typically pose different sorts of questions. Even when your work is cited, it is important to recognize that research articles, like all social phenomena, undergo a process of social construction and labeling. What inevitably seems like a rich, subtle, variegated, and complex argument or set of empirical results to the author gets defined in a pithy, stylized shorthand by scholars who subsequently cite—and thereby define—your contribution to a literature. (An example would be the following hypothetical citation of our paper: "Personnel activities flourished during World War II [Baron, Dobbin, & Jennings, 1986].") This sometimes is frustrating, but it can also be liberating: One's inability to control how the discipline will construe a given piece of research suggests the importance of satisfying internal standards, of writing for *yourself* as much as for any clearly defined reviewer or audience.