American Sociological Review
76(2) 291–319

DOI: 10.1177/0003122411401250
http://asr.sagepub.com

The Initial Assignment Effect: Local Employer Practices and Positive Career Outcomes for Work-Family Program Users

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Abstract

One of the great paradoxes of inequality in organizations is that even when organizations introduce new programs designed to help employees in traditionally disadvantaged groups succeed, employees who use these programs often suffer negative career consequences. This study helps to fill a significant gap in the literature by investigating how local employer practices can enable employees to successfully use the programs designed to benefit them. Using a research approach that controls for regulatory environment and program design, we analyze unique longitudinal personnel data from a large law firm to demonstrate that assignment to powerful supervisors upon organization entry improves career outcomes for individuals who later use a reduced-hours program. Additionally, we find that initial assignment to powerful supervisors is more important to positive career outcomes—that is, employee retention and performance-based pay—than are factors such as supervisor assignment at the time of program use. Initial assignment affects career outcomes for later program users through the mechanism of improved access to reputation-building work opportunities. These findings have implications for research on work-family programs and other employee-rights programs and for the role of social capital in careers.

Keywords

organizations, equal opportunity, social capital, law, work-family, work hours

Across many professions, employers are modifying traditional career and promotion systems by implementing work-family programs. Reduced hours with prorated pay are now widely available to employees who have family responsibilities. Virtually all (98 percent) large and medium-sized U.S. law firms have adopted such programs (National Association of Law Placement 2007). Reduced-hours programs are catching on in academia, too. State systems (notably California) are moving toward longer tenure clocks and part-time status for faculty (Mason et al. 2005). Introduction of these programs is driven by organizations' need for legitimacy (Kelly and Dobbin 1999) and by their attempts to address the formidable challenges of attracting and retaining women

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(Gorman 1999). However, employees often choose not to use work-family programs because they suspect such programs actually hinder rather than advance their careers (Bailyn [1993] 2006; Blair-Loy and Wharton 2002, 2004). At leading law firms, for instance, employees state that reduced-hours programs are perceived as ''mommy track'' options, and they are concerned their superiors will think them uncommitted or even incompetent if they take advantage of the programs. These employees are right to be concerned. Although programs designed to assist traditionally disadvantaged groups do sometimes help vulnerable employees succeed (Kalev and Dobbin 2006; Kalev, Dobbin, and Kelly 2006), across sectors, employees who use these programs are at risk of fewer promotions (Kalleberg and Reskin 1995), lower wages (Kalleberg 1996), and lower wage growth (Glass 2004) than other employees.

To our knowledge, no prior studies have identified employer practices associated with positive career outcomes for work-family program users. This is due, in part, to the difficulty of obtaining longitudinal data tracking how employees fare over time when they use the programs (Kelly et al. 2009). Our unique, time-varying career data allow us to follow a sample of employees over a number of years to determine the impact of exposure to particular employer practices on the career outcomes of program users and non-users.

While the current literature on work-family programs gives us little sense of the levers that might be used to mitigate negative career outcomes for program users, two related bodies of research contribute to our understanding of this issue. The first explains the conditions under which employees are likely to use work-family programs (but not the conditions under which they can successfully use them). This research suggests that the support of proximate supervisors is critical to employees' use of work-family programs (Blair-Loy and Wharton 2002). The second body of research explains conditions that lead to effective implementation of such programs for the organization overall (but not the conditions that mitigate negative career outcomes for individual program users). This research suggests that a supportive regulatory environment and particular program designs are critical to these programs' organization-level effectiveness (Kalev and Dobbin 2006; Kalev et al. 2006).

Both bodies of research point to the importance of protecting employees from negative evaluation at the time of program use. In contrast, we propose that such protection can begin much earlier. Without denying that conditions at the time of program use can protect career outcomes, we suggest that conditions at the time of employees' entry into an organization can be no less decisive and can exert an enduring influence on how their careers evolve (Sørensen 2004). In particular, we expect employees in traditionally disadvantaged groups who are assigned to powerful supervisors when they enter the organization—and who later become program users—will have better career outcomes than similar later users who are not initially assigned to powerful supervisors. There are three possible reasons for this. First, early exposure to powerful supervisors can provide later program users with better skill development and relevant knowledge (Cross and Cummings 2004). Second, such exposure can allow for dissemination of positive opinions about employees for use in performance evaluation (Lin 2001). Third, such exposure can provide employees with access to subsequent reputation-building project assignments, because powerful supervisors may have connections to other powerful supervisors and influence others involved in the assignment process (Epstein 1981). In these three ways, initial assignment to powerful supervisors can protect future users of work-family programs from supervisors' potential negative evaluations at the time of program use.

In this article, we analyze unique longitudinal data on associates from a large U.S. law firm to test whether assignment to powerful

supervisors upon entry has a positive effect on the career outcomes of later users of a work-family program that allows employees to work reduced hours for reduced pay while remaining on the track to partnership. We also assess which of the three postulated mechanisms can best explain this effect. We control for regulatory environment, program design, selection into program use, power of proximate supervisors at the time of program use, and co-worker relationships at time of program use. We demonstrate that initial assignment to powerful supervisors predicts positive career outcomes among later program users, and we find that access to reputation-building projects underlies this effect.

Our findings contribute to the work-family and social capital literatures and build on key ideas from the literature on inequality remediation in organizations. Many organizations have adopted work-family programs and other equal opportunity initiatives such as disability, sexual harassment, diversity, and dispute resolution programs designed to promote equal treatment for women, minorities, and employees with disabilities (Dobbin 2009; Dobbin et al. 1988; Dobbin and Sutton 1998; Edelman 1990; Kalev et al. 2006). While such programs are designed to help traditionally disadvantaged employees, the employees whom these programs are intended to benefit often choose not to use them because they are concerned about potential negative career consequences. We end the article by discussing the implications of the practice of initial assignment for enabling successful career outcomes for users of work-family and other employee-rights programs.

WORK-FAMILY PROGRAM USERS

Overall Negative Career Outcomes for Work-Family Program Users

In response to changing labor force and family demographics, many organizations have adopted work-family programs to improve employee recruitment, commitment, and retention and to comply with coercive or normative institutional pressure (Davis and Kalleberg 2006; Glass and Fujimoto 1995; Kelly and Dobbin 1999; Osterman 1995). Employees, however, often choose not to use these programs because they fear retaliation (Bailyn [1993] 2006; Blair-Loy and Wharton 2002, 2004; Eaton 2003; Hochschild 1997; Perlow 1997; Williams 2000). In fact, research has found that using such programs is associated with negative career outcomes. Users of reduced-hours programs, for example, suffer from fewer promotions (Dau-Schmidt et al. 2009; Kalleberg and Reskin 1995), lower wages (Kalleberg 1996), and lower wage growth (Glass 2004). Similarly, users of Family and Medical Leave Act (FMLA) programs suffer from future lower wages (Jacobsen and Levin 1995), fewer promotions (Hagan and Kay 1995; Judiesch and Lyness 1999), and less retention (Lyness and Judiesch 2001).

The literature suggests that employees who use these programs are evaluated negatively. Indeed, these employees are doubly vulnerable. Even before they become program users, women, mothers, and male primary caregivers are often rated as less competent (Biernat and Kobrynowicz 1997), seen as less capable of assuming positions of authority (Ridgeway 2001), and awarded lower wages (Correll, Benard, and Paik 2007) than other employees. This is true even after controlling for factors related to skill and productivity (Anderson, Binder, and Krause 2003; Budig and England 2001). Once these employees begin to use work-family programs, they may suffer even further; by simply using the programs, they highlight their membership in traditionally disadvantaged groups. This may invite supervisors to question the employees' commitment, abilities, and marketability (Albiston 2007; Blair-Loy 2003; Epstein et al. 1999; Ridgeway and Correll 2004).

Despite documented negative outcomes, these kinds of programs do sometimes help

individuals in traditionally disadvantaged groups succeed (Kalev and Dobbin 2006; Kalev et al. 2006). This variation in career outcomes raises the question: under what conditions can employees in traditionally disadvantaged groups successfully use the work-family programs that have been established for their benefit?

Traditional Approach: Conditions at the Time of Program Use

Two bodies of research shed light on this issue. The first explains the conditions under which employees are likely to use such programs. ''Good'' employees—no matter how that ''goodness'' is achieved—report having greater latitude when it comes to doing things outside the norm, like using reduced-hours programs (Kelly and Kalev 2006; Kelly and Moen 2007). Furthermore, proximate supervisors at the time of decisions about program use play an important role in whether employees use work-family programs (Briscoe 2006; Hochschild 1997; Kelly and Kalev 2006; Perlow 1998). Employees are more likely to use these programs if they work with powerful proximate supervisors who may buffer them from possible negative career outcomes (Blair-Loy and Wharton 2002).

The second body of research explains conditions that lead to effective organizational implementation of employee-rights programs that are designed to assist traditionally disadvantaged groups. A supportive regulatory environment is critical to the effectiveness of these programs because it creates disincentives for organizations to discriminate (Kalev and Dobbin 2006; Skaggs 2008). In addition, particular program designs facilitate programs' effectiveness in law firms by requiring relatively low billable hours (Gorman 2006) or allowing longer partnership tracks (Chambliss 1997). In other kinds of organizations, program designs facilitate effectiveness by assigning accountability for diversity outcomes (Kalev et al. 2006), increasing employees' schedule control (Kelly and Moen 2007), or combining the evaluation step of performance review with the payment step (Castilla 2008).

Program User Vulnerability to Negative Evaluation

Both bodies of research suggest that career outcomes may be affected by the degree to which employees are protected from potential negative evaluation at the time they use work-family programs. Indeed, research shows that employees who use these programs are vulnerable to negative evaluation of their commitment, abilities, and marketability to clients (Kellogg 2009). Supervisors may question the commitment of employees using reduced-hours programs, for example, by penalizing them for not acting like ''ideal workers'' who are willing to work long hours because they have no responsibilities outside of work (Acker 1990; Kanter 1977). Research also shows that supervisors question the abilities of employees who choose to use similar kinds of employee-rights programs; some supervisors believe that women and minorities who use affirmative action and diversity programs achieved their positions on the basis of reverse discrimination (Heilman, Block, and Stathatos 1997) or that employees who use disability programs are incompetent (Harlan and Robert 1998). Finally, supervisors may question whether such employees, especially those who use reduced-hours or FMLA programs, are marketable to clients or customers; supervisors may worry that employees will not be available when a client wants them (Epstein 1992; Epstein et al. 1999) or that they will not develop productive, in-depth relationships with key clients (Thornton and Bagust 2007).

Supervisors who question employees' commitment, abilities, and marketability can damage careers because they have the power to award wages and promotions,

provide access to workplace opportunities, fire at will, and invoke formal organizational programs to discriminate against particular groups (Bisom-Rapp 1999; Mong and Roscigno 2009; Roscigno 2007). Finding factors that protect program users from negative evaluation based on program use is therefore critical.

The Initial Assignment Effect: Conditions at the Time of Entry

These traditional approaches highlight how conditions at the time of employee program use shape career outcomes, but we propose that the seeds of success may be planted much earlier. Evidence connects success later in one's career with conditions in the early years of one's organizational tenure (DiPrete and Eirich 2006; Sørensen 2004). Furthermore, research shows that individuals' advancement and success in organizations depend on their access to powerful supervisors or mentors (Thomas and Kram 1988) who can provide positive social capital (Burt 2000). Powerful initial supervisors could protect program users from negative evaluations in three ways: by training them in superior skills, by disseminating positive opinions about them for use in performance evaluations, and by giving others in the organization reasons to extend reputation-building work opportunities to the employees (i.e., because the supervisors' own powerful resources are visibly associated with those employees).

Regarding provision of superior skills, a primary route for employees to accumulate human capital is through on-the-job training (Doeringer and Piore 1971). In our research setting, most employees arrive from law school without any relevant work experience; hence, their first exposure to the practical aspects of work in their profession may be especially formative. Galanter and Palay (1991) argue that associates advance in law firms based on the lawyering skills they gain as they work on projects assigned to them by supervisors. In general, lawyers are more receptive to learning from supervisors early in their careers (Katz 1980), and initial supervisors strongly influence career socialization to work routines and practices (Burton and Beckman 2007; Van Maanen and Schein 1979). Because employees with greater exposure to powerful supervisors may gain access to better learning opportunities, these employees may develop superior skills that can protect them from negative evaluation once they become program users.

Regarding dissemination of positive opinions, supervisors who are positioned more centrally in the network structure of relationships at a workplace likely control more resources in the organization (Brass and Burkhardt 1993) and therefore have greater influence and control over information (Burt 1992). In the social capital literature, the most commonly cited benefit of a relationship is the transfer of more and better information (Lin 2001). Powerful supervisors can effectively disseminate opinions and contribute to performance evaluations and promotion decisions for employees in whom they develop an interest. This can counteract negative evaluations of employees' commitment, abilities, or marketability once they become program users.

Regarding the provision of access to reputation-building work opportunities, powerful supervisors who have ties to others who control valuable projects can generate opportunities for their subordinates. In social capital terms, these supervisors can lend their social capital to subordinates to facilitate their access to reputation-building projects (Burt 2000). For associate lawyers, the portfolio of projects they build up over time becomes a visible track record by which they are evaluated as potential partners (Beckman and Phillips 2005; Epstein et al. 1999). The quality of projects is thus a crucial factor for employees as they accrue the reputation and relationships that make them attractive candidates for senior positions in

an organization. For members of traditionally disadvantaged groups in particular, research shows that projects that provide opportunities for exposure can reduce career disadvantage (Kalev 2009).

While such career advantages may benefit all employees who are initially exposed to powerful supervisors, the process we theorize is not simply a case of the generalized rule that ''the rich get richer''—that is, early experiences set the stage for later career advances for all employees. Instead, we argue, program use leads employees in traditionally disadvantaged groups to signal their membership in these groups and to become vulnerable to negative evaluation. Thus, we expect that although later program users and non-users will both benefit from early assignment to powerful supervisors (the rich will get richer in all cases), employees who become program users will benefit more from this early assignment (because they are at risk of negative evaluation) than will those who do not become users. Provision of superior skills and dissemination of positive opinions can help these employees counteract negative evaluations, and access to reputation-building projects can ensure that a wide range of powerful supervisors and clients have had direct experience with their commitment, abilities, and marketability by the time they become program users.

Finally, it is important to rule out a competing explanation for the relationship between initial assignment and career outcomes. If initial assignment is not random, then powerful supervisors could select superior protégés from the start. This could explain positive career outcomes for program users who were initially assigned to powerful supervisors. However, our data suggest that such sorting is not the mechanism by which initial assignment affects career outcomes at this firm. We find that initial assignment is not correlated with observable characteristics in any way that could be consistent with sorting. We will address this competing explanation below.

METHOD

Research Site and Reduced-Hours Program

Our unique longitudinal data come from a full-service law firm with offices in several U.S. cities. The firm has a long history and an established reputation with clients in a range of industries. Near the end of our study period, the firm employed approximately 1,000 attorneys and reported several hundred million dollars in annual revenues.[1] The firm generally recruits entry-level associates from top law schools nationally. The associate career consists of an up-or-out path to partnership. Fewer than one in four associates achieve partnership. On entry, associates are assigned to a range of projects, partners, and clients based largely on the ebb and flow of work demands. As they gain experience in their first few years, assignments become more substantive and the process of matching employees to projects becomes more meaningful.

Throughout the study period, the firm had a policy authorizing associates to participate in a reduced-hours program. Under normal circumstances, any associate could be eligible for the program after at least two years of tenure. Associates in our dataset who participated in the reduced-hours program enrolled, on average, during the beginning of their fourth year of tenure in the firm (mean = 4.12, SD = 1.62).

Once associates enrolled in the program, they were assigned specific Full-Time Equivalent (FTE) statuses—for example, 80 percent FTE status or 60 percent FTE status. According to the firm's policy guidelines, work conducted by reduced-time associates was evaluated in accordance with the firm's review process for full-time lawyers. Compensation was prorated, and the year-end bonus for reduced-hours associates was subject to the same considerations as were applicable to associates working on a full-time basis. Once enrolled in the program,

associates could remain in it for a short or long period of time, with the expectation that if they exited the program to return to full-time associate status, they would remain on the track to partnership.

Data

The data encompass 958 associates who entered the firm between the years 1997 and 2005. For most analyses in this study, we focus on 71 program users during those years. Personnel data include associates' pre-hire characteristics used for recruitment and hiring purposes, as well as records of career events, life events, and pay throughout an associate's tenure in the firm. We compiled information on working relationships among associates, partners, and clients using annual billing records through the end of 2007. Data are complete, with the exception of compensation data, which are available only for the years 2001 to 2007. In addition to these quantitative data, we conducted 24 interviews with partners, staffing managers, and associates. We also reviewed interview transcripts conducted by an internal task force assessing the reduced-hours program.

Analytic Strategy

Because program use involves a self-selection process among employees, we begin our analysis by examining which associates are more likely to use the program in the first place. We then consider the possibility that employees are selected upon entry, rather than randomly assigned to particular supervisors. We find that workers assigned to powerful supervisors do not differ from other workers on a wide range of pre-hire individual characteristics.

Next, we turn to our primary interest—modeling the success of program users. Our analytic strategy is to include independent variables reflecting the organizational context for employees at the time of program enrollment, as well as variables reflecting organizational context during their first year of tenure in the firm. To model outcomes, we estimate a series of regressions predicting performance pay and attrition outcomes, adjusting for selection effects. As the final step in our analysis, we report the results of a supplemental investigation of the possible mechanisms that lie behind the initial assignment effect. To do this, we enter three different mechanism variables into the outcomes models and consider whether the results provide evidence that the mechanism variables moderate the effect of initial assignment.

Dependent Variables

We use two variables for successful career outcomes: (1) performance-based pay relative to cohort and (2) associate retention along the path to partnership.

Performance-based pay. Associate pay has two components, a base salary and a year-end bonus. Base salary is constant across associates from the same class year and increases in lock-step by year of tenure. Bonus is assigned according to individual performance and is evaluated by the practice group leader with input from the partners who work most closely with the associate.

Reduced-hours associates are awarded a bonus percentage based on their full-FTE (full-time equivalent) pay, and their entire pay is then prorated to their particular FTE level. This payment practice allows for comparison across reduced-hours and full-time associates in terms of their relative performance and contributions. Because bonuses are assigned to reduced-hours associates prior to their total pay being prorated, in principle, a full-time associate who transitions to reduced hours and continues to perform at the same level should receive the same percentage bonus, and hence the same non-prorated total pay.

The variable we use in our analyses is each associate's annual bonus, net of his class

cohort average that year. This variable directly reflects differences in the bonus assigned to an associate relative to what the firm considers to be his appropriate peers. This approach dampens any variance related to yearly fluctuation in funds allocated for the associate bonus pool; a significant amount was allocated in every year of the study period.[2]

Retention on the path to partnership. Fewer than one in four associates become a partner. During the study period, associates either exited after a certain number of fixed years or were awarded partnership. Toward the end of the study period, the firm developed options for associates who wanted to stay in the firm without becoming partners. Only a few individuals in our analysis took advantage of this option, and we considered the moment of their transition to be equivalent to firm exit in terms of representing attrition from the path toward partnership.

We have precise data on each associate's date of hire and exit (if any), taken from the firm's human resource databases. In analyses conducted on all associates, individuals are allowed to be at risk of attrition during their entire tenure in the organization, from date of hire until they exit the firm (or to the end of 2007, whichever comes first). In analyses of attrition among program users, individuals are at risk from the day marking the onset of program use until they leave (or to the end of 2007, whichever comes first).[3]

Independent Variable: Exposure to Powerful Supervisors

Our primary independent variable reflects an associate's exposure to powerful supervisors (partners) at various points in her tenure at the firm. We took three factors into account in constructing this variable: (1) defining powerful supervisors, (2) operationalizing exposure of associates to those supervisors, and (3) capturing that exposure at different times in an associate's tenure in the firm.

Defining powerful supervisors. A law firm's revenues are almost entirely a function of hours billed to clients. Interviews suggest that the most powerful supervisors (partners) are those who have the highest client billings. Thus, we define a supervisor's power as a function of the client billings for which she could claim responsibility in a given year, based on whether she was designated as the lawyer responsible for that client.

For each partner, we added up the number of annual billable hours billed by any of the firm's lawyers to that partner's clients. This total number of billable hours became the partner's supervisor-power value for that year. Partners vary widely in their power values, and power changed over time. To simplify matters for the next step, we define power supervisors in each year as those whose power-supervisor values exceed a threshold (withheld to preserve anonymity). Different thresholds and a continuous weighting alternative yield only minor differences.

Operationalizing exposure. We assume that associates are exposed to partners primarily by being assigned to client projects for which those partners are responsible. Rather than focusing on specific associate–partner links, our variable captures an associate's total exposure through client projects to partners with particular attributes, such as being power supervisors. To accomplish this, we calculate the portion of all billable hours an associate reported in a given year to projects led by power supervisors. The resulting variable ranges from 0 to 1.0 and varies for each associate in each year.

Timing of exposure. We include exposure variables in our models in several different ways. The first variable captures exposure during the year prior to an associate's transition to program use. The second variable captures exposure at the time of an associate's entry into the firm.

Other Independent Variables and Controls

Factors at the time of program enrollment. First, because prior research suggests that co-worker support may play an important role in the use of work-family programs (Blair-Loy and Wharton 2004), we control for the strength of an associate's working relationships with proximal co-workers at the time of enrollment. To do so, we use a variable that measures the portion of co-workers from an associate's projects in the year prior to enrollment that he had been working with to any degree two years earlier (e.g., 20 percent of co-workers worked with two years earlier). Second, because research suggests that employee tenure may shape program usage, we include a variable for an employee's tenure in the firm at enrollment in the reduced-hours program. Finally, because larger projects may make it easier for program users to share work with others (Briscoe 2007), we control for project size at the time of enrollment with a variable reflecting exposure to large projects—that is, client projects on which at least 20 other lawyers reported at least 100 hours in that year. Other variables capturing exposure to large projects yield only minor differences in the results.

Other control variables. We control for sex, minority status, parental status (time varying), human capital in the form of law school rank and undergraduate grade-point average, department (base case is corporate), city locations, a dummy for hires entering via smaller law firm acquisition, and size of an associate's incoming cohort (for additional details, see Part A in the online supplement [http://asr.sagepub.com/supplemental]). An additional control for varying Full-Time Equivalent (FTE) levels of program users had no discernable effect on outcomes and was omitted from the final models.

Mechanism Variables

Superior skills development. On a four-point scale, we give each partner a point for each of the following: (1) uniformly positive upward feedback responses from subordinates; (2) high marks (at least 4 out of 5) for "training and development" from subordinates; (3) partner chosen by firm to interview prospective associate hires; and (4) all subordinates retained by the firm in the following year.

Superior information provision. A supervisor social capital variable consists of a partner's eigenvector centrality score in a network among the firm's partners with ties defined by billings from one partner to another's clients.

Superior access to future reputation-building quality projects. We created a project-portfolio quality variable based on factors that interviewees perceived to be important for career success. We characterize projects on four dimensions: (1) billings to the firm's major clients; (2) number of different partners with whom an associate was substantially involved; (3) number of different clients with whom an associate was substantially involved; and (4) portion of an associate's billings to projects whose lead partners were located outside of the associate's department or office. We then assigned each associate a time varying project-portfolio quality index for each year of tenure in the firm (for additional details, see Part A in the online supplement).

RESULTS

Our findings support the importance of the initial assignment effect in the career outcomes of reduced-hours program users. Initial assignment to powerful supervisors is associated with positive career outcomes for program users in the form of higher performance pay and lower attrition. Initial assignment appears to trump factors at the time of program use, including exposure to powerful proximate supervisors. Furthermore, initial assignment effect is magnified for program users relative to non-users; as a result, upon program use, employees with high initial

Table 1. Descriptive Statistics

Variable	Program Users $n = 71$[a]		All Associates $n = 958$[a]	
	Mean	SD	Mean	SD
Program Use (0/1)			.074	.251
Performance-Based Pay (person-year)[b]			.589	11.955
Performance-Based Pay (post-enrollment)[b]	–5.810	23.020		
Exit (0/1)	.472	.499	.429	.498
Litigation Department	.214	.413	.275	.446
Other Department	.100	.302	.049	.216
Location 2	.114	.320	.221	.415
Location 3	.129	.337	.102	.303
Entering Cohort Size	.589	.498	.546	.430
Female	.871	.337	.476	.499
Parent	.657	.478	.119	.323
Minority	.186	.391	.176	.381
Acquisition Unit	.086	.448	.096	.294
Law School Ranking	17.560	32.000	2.790	32.410
Undergraduate Grades	1.271	.700	1.191	.670
Megaproject Exposure at Enrollment	.129	.337		
Co-worker Ties at Enrollment	.081	.176		
Prior Tenure at Enrollment (days)	1,503	591		
Power Supervisor Exposure at Enrollment	.105	.173		
Power Supervisor Exposure at Organization Entry	.111	.167	.136	.223
Supervisor Provides Skills Development	.887	.597	.958	.622
Supervisor is Central in Task Network	6.044	8.213	5.778	7.725
Cumulative Project Quality at Transition	1.079	.587		
Cumulative Project Quality (person-year)			1.0784	.674

[a] n for person-year data is 3,350 for all associates' person-years and 142 for program user person-years (post-enrollment).
[b] Employee's annual non-prorated performance-based bonus, in thousands, net of the class cohort average for that year. This variable reflects how much more or less of a bonus the employee received that year than others at the same level of tenure.

exposure do not suffer the decline in career outcomes experienced by other program users. This finding is robust to a range of modeling choices. We also find support for one particular mechanism underlying the initial assignment effect: exposure to powerful initial supervisors helps employees gain access to reputation-building project opportunities, which in turn allows them to build a significant track record with a wide range of supervisors and clients by the time they use the reduced-hours program.

Demographics of Program Users

We begin by examining program users and the initial assignment process before turning to our main focus, success among program users. Table 1 provides a summary of descriptive statistics for the variables used in our analyses. Table 2 presents results from a model predicting program use; corresponding hazard ratios for each covariate are provided in the right-hand column. Female employees are three times more likely to become program users ($p < .001$), and employees who become parents are more than four times as likely to become users ($p < .001$). The likelihood of program use also rises with organizational tenure ($p < .001$). Associates from better-ranked law schools are more likely to participate ($p < .001$). Our other key human capital variable, undergraduate grades, is not significant. An alternative model specification

Table 2. Coefficients from Discrete-Time Event History Model Predicting Program Use among All Law Firm Associates

	Model 1	
	Coeff.	Hazard Ratio
Constant	−3.034***	.050
	(.372)	
Litigation Department	−.278	.753
	(.186)	
Other Department	.464	1.577
	(.320)	
Location 2	−.310	.731
	(.389)	
Location 3	.146	1.161
	(.400)	
Entering Cohort Size	−.616	.544
	(.723)	
Tenure	.052**	1.048
	(.020)	
Female	1.119***	3.065
	(.223)	
Parent	1.504***	4.482
	(.174)	
Minority	.316	1.369
	(.224)	
Acquisition Unit	.660	1.927
	(.517)	
Law School Ranking	−.011*	.990
	(.004)	
Undergraduate Grades	.171	1.191
	(.135)	
−2LL	−496.6	
Observations	3,350	

Note: Robust standard errors in parentheses.
*$p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

including all the covariates from our main analyses (i.e., from Tables 4 and 5) does not produce any other significant coefficients. Because we find several significant factors in these models predicting program use, we infer that it is important to address selection into use in our main analyses of outcomes among program users.

A Random Initial Assignment Process

Our earlier arguments regarding initial assignment to powerful supervisors lead naturally to the question of what factors influence this initial assignment (Rivera 2008). Qualitative and quantitative evidence indicate that the assignment process at this firm is random with respect to individual characteristics. The firm's staffing system matches partner requests for associates, based on general project requirements, to associate availability. For associates in their first two years, work experience and preference are not part of the calculation (for additional details, see Part B in the online supplement).

To investigate this issue further, we use pre-hire characteristics to compare employees who varied in their assignment to powerful supervisors on entry. Instead of regressing initial assignment on a panel of covariates (which could obscure associations through collinearity), we present basic mean and frequency comparisons for a series of these pre-hire human capital and demographic variables. We compare pre-hire characteristics of employees who have no exposure to powerful supervisors in their first year versus employees who have between 50 and 100 percent exposure in the first year (based on their total billable hours).[4] We include key variables from our main analyses, as well as some additional variables that are available only for subsets of employees and therefore are not included in the main analyses.

Table 3 shows the results. Overall, there are few significant differences in pre-hire characteristics between employees with no exposure to powerful supervisors and employees with high exposure. On two variables—ranking of an associate's law school and whether an associate completed a court clerkship—employees who were assigned to powerful supervisors actually scored significantly *lower* than those not assigned to powerful supervisors. Associates with more exposure to powerful supervisors were also less likely to have any recorded work experience prior to starting at the firm. Other human capital variables, including undergraduate and law school grades,

Table 3. Pre-hire Characteristics of Associates Who Were Exposed versus Not Exposed to Powerful Supervisors

Pre-hire Characteristic (variable)	0 Percent of Billable Hours to Matters Led by Powerful Supervisors	50 Percent or More of Billable Hours to Matters Led by Powerful Supervisors	Significant Difference?
1. Female	.522	.474	n.s.
2. Minority	.172	.145	n.s.
3. Law School Ranking	17.081	24.730	*
4. Clerkship	.232	.159	*
5. Undergraduate Grades	1.249	1.282	n.s.
6. Work Experience (0/1)	.545	.449	*
7. Years of Client Service Business Experience	.686	.726	n.s.
8. Summer Associate	.812	.797	n.s.
9. Law School Grades	3.566	3.542	n.s.
10. Married at Hire	.662	.531	+
11. Law Review	.765	.776	n.s.
12. Overall Evaluation	2.312	2.290	n.s.
13. Intellect	3.469	3.511	n.s.
14. Articulateness	3.393	3.388	n.s.
15. Presence	3.284	3.362	n.s.
16. Judgment	3.500	3.396	n.s.
17. Motivation	3.519	3.250	n.s.
18. Client Potential	3.357	3.415	n.s.
19. Lawyer Potential	3.429	3.485	n.s.

Note: n is reduced to 275 (183/92) for items 9 to 11, and 52 (31/21) for items 12 to 19. Sample reductions reflect the fact that some variables were collected by the firm only during a few years (or just one year in the case of items 12 to 19).
*$p < .05$; ** $p < .01$; *** $p < .001$; significance tests based on chi-squared for dichotomous variables (1, 2, 4, 6, 8, 10, and 11) and two-sample t-tests for continuous variables (3, 5, 7, 9, and 12 to 19).

whether an associate had been on the editorial board of a law review during law school (a competitive process), summer associate experience at the firm, and demographic characteristics such as being female, being of a minority race or ethnicity, or being married at the time of hire are not significantly different between the two groups.

These results are consistent with the notion that associates' initial assignment to supervisors is not correlated with human capital or other observable factors that could conceivably influence associate success. Of course, it is possible that associates' unobservable characteristics influence the initial assignment process. However, the quantitative evidence we have on observable characteristics, and the qualitative evidence we have from interviews, do not point to a sorting process until after the first two years of tenure, once associates gain skills, reputations, and relationships that distinguish them in ways meaningful for the workplace.

Exposure to Powerful Supervisors upon Entry and Program Users' Career Outcomes

Tables 4 and 5 present the results of analyses predicting our two main outcomes, performance pay and organizational attrition, respectively. These analyses use person-year observations with robust errors clustered on individual employees. The performance pay analyses use least-squares models, and the

Table 4. Least-Squares Coefficients for Models Predicting Performance Pay among Program Users and All Associates

	Model 1: All Person-Years	Model 2: Program User Person-Years	Model 3: Program User Person-Years, Selection Adjustment	Model 4: All Person-Years, Treatment Effects	Model 5: All Person-Years, Fixed Effects
Intercept	−1.319	−1.364	12.339	1.791	
	(.920)	(1.078)	(37.802)	(.981)	
Litigation Department	−.933	−1.672	−8.260	−2.588***	
	(.634)	(6.407)	(5.572)	(.772)	
Other Department	−6.401**	−9.213	−3.303	−7.245***	
	(2.308)	(9.009)	(6.157)	(1.450)	
Location 2	3.344***	14.101	8.614	2.804**	
	(1.002)	(12.607)	(11.083)	(1.005)	
Location 3	−2.955*	6.288	21.331**	−4.796***	
	(1.185)	(17.339)	(7.259)	(1.074)	
Entering Cohort Size	1.001	−11.880	23.258	−1.148	
	(1.090)	(26.102)	(22.851)	(1.420)	
Tenure at Enrollment[a]	1.233***	2.676	1.890	1.861***	1.021***
	(.277)	(1.872)	(1.543)	(.187)	(.282)
Female	−.551	11.403	−3.104	−1.167	
	(.495)	(6.902)	(13.684)	(.671)	
Parent at Enrollment[a]	−1.442	.737	−7.962	−1.429	−2.069
	(1.365)	(7.004)	(16.357)	(1.187)	(1.608)
Minority	−.775	3.358	−1.725	−1.589	
	(.798)	(6.721)	(5.605)	(.860)	
Acquisition Unit	−1.319	−10.961	−34.386*	−4.149**	
	(1.477)	(15.970)	(16.671)	(1.458)	
Law School Ranking	−.003	.053	.060	−.010	
	(.010)	(.124)	(.234)	(.195)	
Undergraduate Grades	.127	−.241	−3.113	.195	
	(.460)	(4.004)	(3.226)	(.440)	
Megaproject Exposure at Enrollment[a]		7.554	5.647	2.700**	
		(6.802)	(5.348)	(.808)	

(continued)

Table 4. *(continued)*

	Model 1: All Person-Years	Model 2: Program User Person-Years	Model 3: Program User Person-Years, Selection Adjustment	Model 4: All Person-Years, Treatment Effects	Model 5: All Person-Years, Fixed Effects
Co-worker Ties at Enrollment[a]		7.850*	2.681	2.392**	
		(3.730)	(3.930)	(.889)	
Power Supervisor Exposure at Enrollment[a]		9.442	6.660	1.318	
		(4.777)	(6.779)	(1.578)	
Power Supervisor Exposure at Organization Entry		25.042*	30.350**	6.164*	
		(7.025)	(7.724)	(2.479)	
Program Use	−20.575*			−20.460	−8.130**
	(10.226)			(10.299)	(2.943)
Program Use X Power Supervisor Exposure at Organizational Entry				20.425*	10.335*
				(9.243)	(3.005)
Type of Model	OLS	OLS	Heckman Selection (OLS with probit)	Heckman Treatment (OLS with probit)	Individual Fixed Effects
R^2	.080	.259			.611
Change in Wald χ^2 (*df*)			6.9 (1)*	8.4 (2)*	
ρ			−.71**	−.04	
n Person-Years for Selection/Treatment Model	n/a	n/a	3,350	3,350	n/a
n Person-Years for Outcomes Model	3,350	142	142	3,350	2,948

Note: Dependent variable is an associate's annual bonus for the given year, net of her class-cohort average bonus for that year. Robust standard errors in parentheses.

[a]For Model 4 (treatment-effects model), these are simply time varying variables corresponding to each person-year, because there is no time-of-enrollment for non-users in this analysis.

*$p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

attrition analyses use probit models that include dummies for years of tenure, making them the functional equivalent of discrete-time event history analyses. To aid in interpretation, the probit model results in Table 5 include a column showing the change in probability for a one-unit change in each independent variable.

The sequence of models is parallel in both tables. We start each series with a basic multivariate model showing how program use affects outcomes (Model 1). Then, we examine how our focal variable—the initial assignment effect—predicts outcomes among program users. We do this without adjusting (Model 2) and then after adjusting (Model 3) for selection into the program. The next model asks how the initial assignment effect differs for program users (during years of program use) relative to all other employees (including all non-use person-years), also conducted while adjusting for selection (Model 4). Finally, for the performance pay outcome, we estimate an individual fixed-effects model to see how the initial assignment effect differs during program use versus non-use (Model 5, shown in Table 4 only).

Model 1 in both tables shows how program use itself affects outcomes. Changes in work assignments and relationships experienced by employees after program use (summarized in Table 2) suggest that program use itself may influence outcomes. Model 1 indicates that after controls are included, program use has a negative effect on performance pay ($p < .05$) and that program use increases the probability of attrition, although this effect is of marginal significance ($p < .10$). Hence, program use appears to have a generally negative direct effect on outcomes, at least before taking selection into account.[5]

Model 2 of both tables includes the initial assignment effect, and Model 3 adds the selection adjustment. Selection into program use (the treatment group in Rubin's [1974] causal framework) is salient in our context if individuals who choose to enroll in the program are also likely to have different career outcomes. Specifically, the selection adjustment in Model 3 of Table 4 consists of an OLS model predicting performance pay with a simultaneous probit model for the probability of program use (not shown in the table). We include an exclusion restriction in this and all other selection and treatment models discussed below.[6] Model 3 in Table 5 provides parallel results from a bivariate probit model predicting the probability of exit for each person-year of use, while simultaneously predicting the probability of program use across all person-years. Dummies for each year of tenure in the exit model absorb any variance related to changes in probability over time. As a result, the attrition model is equivalent to a discrete-time event history analysis (using probability rather than odds; see Allison [2004] for a discussion, including the appropriateness of the probit functional form for this type of discrete-time model). These models include additional controls for time-varying factors that could influence success among program users, derived from literature on work-family programs discussed earlier.

Results indicate that exposure variables captured immediately at entry into the organization are key predictors of success among program users, even though program use does not begin until several years later. As anticipated, initial assignment is significantly associated with increased performance pay and lower probability of attrition ($p < .05$). In the case of performance pay (see Table 4), the impact of the selection adjustment on the initial assignment effect is to strengthen its statistical significance. In the case of attrition (see Table 5), accounting for selection leads to an increase in the magnitude of the initial assignment effect while maintaining the same general level of significance.[7]

To assess the overall magnitude of these effects from initial entry, we can use the coefficients from Model 3 in Tables 4 and 5 to compare the difference between an

Table 5. Probit Coefficients for Models Predicting Attrition among Program Users and All Associates

	Model 1: All Person-Years		Model 2: Program User Person-Years		Model 3: Program User Person-Years, Selection Adjustment		Model 4: All Person-Years, Treatment Effects	
	Coeff.	Change in P[a]	Coeff.	Change in P	Coeff.	Change in P	Coeff.	Change in P
Intercept	−1.212***		−2.844***		−2.386		−1.691***	
	(.042)		(.760)		(2.119)		(.091)	
Litigation Department	.210**	.057	.344	.073	.201	.006	.198***	.062
	(.067)		(.361)		(.333)		(.065)	
Other Department	−.030	−.006	−.104	−.021	−.266	−.008	.004	.001
	(.022)		(.473)		(.489)		(.121)	
Location 2	.493***	.143	2.727***	.539	1.907***	.060	.409***	.116
	(.110)		(.609)		(.534)		(.094)	
Location 3	.381**	.104	.454	.090	.158	.005	.243**	.073
	(.108)		(.366)		(.415)		(.094)	
Entering Cohort Size	3.094***	.840	8.075***	1.599	7.031***	.223	2.959***	.890
	(.125)		(1.789)		(1.465)		(.105)	
Female	−.020	−.010	−.139	−.029	.067	.002	−.043	−.013
	(.012)		(.530)		(.789)		(.063)	
Parent	−.081	−.024	.298	.057	.168	.005	−.338***	−.104
	(.079)		(.336)		(.853)		(.131)	
Minority	−.050	−.009	−.149	−.032	−.401	−.012	−.016	−.004
	(.064)		(.328)		(.353)		(.068)	
Acquisition Unit	−1.042***	−.281	−1.538**	−.305	−.992	−.031	−.995***	−.288
	(.117)		(.716)		(.635)		(.098)	
Law School Ranking	−.003**	−.001	−.008	−.002	−.004	−.0001	.002***	.0007
	(.001)		(.006)		(.006)		(.0008)	
Undergraduate Grades	−.090*	−.030	−.099	−.020	−.174	−.005	.038	.015
	(.039)		(.247)		(.253)		(.027)	

(continued)

Table 5. *(continued)*

	Model 1: All Person-Years		Model 2: Program User Person-Years		Model 3: Program User Person-Years, Selection Adjustment		Model 4: All Person-Years, Treatment Effects	
	Coeff.	Change in P[a]	Coeff.	Change in P	Coeff.	Change in P	Coeff.	Change in P
Megaproject Exposure at Enrollment[b]			.371	.073	.283	.008	.411***	.123
			(.393)		(.358)		(.068)	
Co-worker Ties at Enrollment[b]			.901	.178	.408	.013	.303	.092
			(.964)		(.889)		(.218)	
Power Supervisor Exposure at Enrollment[b]			−.953	−.122	.639	−.093	−.145	−.044
			(.523)		(.550)		(.118)	
Power Supervisor Exposure at Organization Entry			−1.301**	−.257	−1.303**	−.181	−.273	−.047
			(.653)		(.629)		(.152)	
Program Use	.222***	.051					1.204**	.361
	(.061)						(.499)	
Program Use X Power Supervisor Exposure at Organization Entry							−.787**	−.229
							(.404)	
Tenure Year Dummies Included in Outcome Models	Yes		Yes		Yes		Yes	
Type of Model	Binomial Probit		Binomial Probit		Bivariate Probit with Sample Selection		Seemingly Unrelated Bivariate Probit	
−2LL	4964.7		87.6		1001.2		2565.6	
Improvement LR $\chi 2$ (*df*)					8.4*(1)		12.4(2)	
ρ					.063		–.550*	
n Person-Years for Selection/Treatment Model	n/a		n/a		3,350		3,350	
n Person-Years for Outcomes Model	3,350		142		142		3,350	

Note: These probit models are the direct equivalent of discrete-time event history models, with the advantage that they can accommodate selection and treatment effects using bivariate probit. LR Chi-sq tests relative to baseline models. Model 3 baseline omits Power Supervisor Exposure at Organization Entry, Model 4 baseline omits Program Use and interactions. Robust standard errors in parentheses.

[a]Changes in the conditional probability of attrition associated with a one-unit change in the independent variable, for an employee with baseline characteristics on all dummy variables and average levels of all continuous variables.

[b]For Model 4, these are time varying variables corresponding to each person-year, because there is no time-of-enrollment for non-users.

*$p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

associate who spent all her time with powerful supervisors at organizational entry (1.0) and an associate who spent no time with powerful supervisors at organizational entry (.0). This difference translates into a $30,350 increase in performance-based pay relative to cohort (30.350 x 1,000 x 1.0) and an 18.1 percent reduction in the probability of attrition (we calculate the latter figure for the hypothetical employee described in the table notes).[8] Fit improves when we add the exposure variables to each model.

In Model 4, after finding evidence of the initial assignment effect for program users, we turn to the question of how this effect differs for program users relative to non-users—or, more precisely, whether there is a significant difference in the initial assignment effect for participant person-years relative to non-participant person-years. Because selection into treatment could bias our estimates of interest, we investigate this question using a treatment-effects model with an interaction of the initial exposure effect with the treatment of program use. Results in Model 4 (Tables 4 and 5) indicate a significant increase in the initial assignment effect for program users. The interaction term is significant, and its inclusion improves model fit for both outcomes.

For a given difference in initial exposure, the coefficients from Model 4 indicate a significantly magnified effect on outcomes for program users. Whereas a 1.0 versus .0 difference in exposure leads to a $6,164 increase in performance pay for non-users (6.16 – .0), it leads to a $26,624 increase for program users ([6.164 + 20.425 – 20.460] – [–20.460]). This more than offsets the reduction in performance pay associated with program use. Because only a few employees have either 100 or 0 percent exposure to powerful partners, we also calculate the effects resulting from a difference of two standard deviations (2 SD) in initial exposure. A 2 SD increase in initial exposure corresponds to gains in performance pay of $8,771 for participants compared with $2,030 for non-participants. Turning to attrition, we find that the beneficial effect of initial assignment on the probability of attrition is again stronger for program users. A 1.0 versus .0 difference in exposure leads to a 4.7 percent decline in probability of attrition for non-users (again for the hypothetical employee described earlier), compared with a 27.2 percent decline probability for program users. A 2 SD gain in initial exposure corresponds to a 9.0 percent decline in the probability of attrition for participants versus 1.6 percent for non-participants.[9]Overall, these magnitudes for program users are broadly consistent with results from the selection adjusted models described earlier.

For performance pay, we are able to implement a fixed-effects model by interacting program use with initial exposure to powerful supervisors. The advantage of the fixed-effects model is that it implicitly controls for any unobservable (or observable) variation across employees. Only variables that vary within persons are included in the model, except for initial exposure to powerful supervisors, which is entered in the interaction term. The results, presented in Model 5 of Table 4, are consistent with our other models predicting performance pay. Specifically, the effect of powerful supervisor exposure at organization entry is significantly greater during program use, relative to non-program use.[10] We find similar and generally stronger results for the initial assignment effect if we omit the selection and treatment adjustments shown in Tables 4 and 5. As a further sensitivity analysis, we implemented a modified Propensity Score Match based on program use and re-ran the career outcomes models using fixed effects for each matched set of program users and non-users. The results are consistent with those presented in Tables 4 and 5.

In the above analyses, some control variables for the social context at time of

enrollment are significant. In Table 4, having more project co-workers at the time of enrollment who were also project co-workers two years earlier increases performance-based pay (for program users and all employees, but this effect loses significance for program users after adjusting for selection). In Table 5, spending more time on projects with 20 or more co-workers at the time of enrollment increases pay and decreases attrition for all associates, but it is not significant for program users. Exposure to power supervisors at the time of enrollment is marginally significant in unadjusted models of both outcomes for program users, but this effect loses significance after adjusting for selection and is not significant for all employees. In additional analyses (not shown here), we looked for effects from other time-of-enrollment variables suggested in the literature, including average tenure of project workgroup members and gender composition of workgroups; we found no significant effects.[11]

Mechanism Investigation: Why Is There an Initial Assignment Effect?

Table 6 summarizes results from additional analyses in which we added separate variables designed to assess the relative importance in the initial assignment effect of three different possible mechanisms. The models predict performance pay and attrition; the modeling strategy and control variables are the same as those presented in Model 3 of Tables 4 and 5. In this table, coefficients (and standard errors and significance levels) are provided for each of the three variables when they are entered separately into a baseline model. We also show the coefficients for power supervisor exposure at organization entry, and whether model fit improves after adding the mechanism variable. We are looking for evidence consistent with mediation. Evidence would include a significant coefficient on the mechanism variable while the power supervisor exposure at organization entry coefficient is diminished in size.

The mechanisms we considered are supervisors' provision of skills, provision of positive opinions, and provision of access to future reputation-building projects. Neither supervisor developmental quality (our indicator for provision of skills) nor supervisor centrality (our indicator for provision of positive opinions) explains the effect of initial exposure to powerful supervisors. We find no significant effects of supervisor developmental quality or supervisor social capital for program users on either outcome. Exposure to power supervisors continues to have a statistically significant and substantial influence on post-enrollment success even in the presence of these variables. And there is not a statistically significant improvement in model fit.

The third mechanism we proposed is provision of access to future reputation-building projects. We noted that powerful early supervisors may provide employees with access to future reputation-building projects if these supervisors have more ties, or more ties to other supervisors who control more valuable projects, than do other supervisors. One way to examine this mechanism is simply to compare the networks of powerful supervisors versus other supervisors in the organization. Powerful supervisors do, in fact, have links to a greater number of supervisors (13.20 versus 7.48 supervisors, t-test $p < .001$), and to supervisors who control more valuable clients on the dimensions that contribute to the project-quality index ($p < .001$ on all three other variables in addition to the supervisor count, both in absolute comparisons and when the other variables are normalized by number of alter contacts per focal supervisor). Initial assignment to powerful supervisors might also lead employees to better subsequent projects if individuals staffing the projects simply prefer employees exposed to powerful

Table 6. Summary of Results from Inclusion of Mechanism Variables in Performance Pay and Attrition Models

		Mechanism Variables Added to Models		
	Baseline Model	Supervisor Provides Skills Development	Supervisor is Central in Task Network	Supervisor Provides Access to Future Quality Projects
Model Predicting Performance Pay				
Coeff. (SE) for Added Variable		−.805	−.018	−.311*
		(.498)	(.032)	(.134)
Coeff. (SE) for Power Supervisor Exposure at Organization Entry	30.350**	29.241*	25.100*	13.703
	(7.724)	(12.198)	(12.202)	(15.299)
Sig. improvement in model fit from added variable?		No	No	Yes
				(.05 level)
Model Predicting Attrition				
Coeff. (SE) for Added Variable		−.030	−.039	4.94*
		(.072)	(.052)	(2.90)
Coeff. (SE) for Power Supervisor Exposure at Organization Entry	−1.303*	−1.296*	−1.300*	.838
	(.629)	(.594)	(.613)	(.587)
Sig. improvement in model fit from added variable?		No	No	Yes
				(.05 level)

Note: Results when each mechanism variable is added separately to Model 3 of Table 4 (Model Predicting Performance Pay) and Table 5 (Model Predicting Attrition). See Methods section and Part A in the online supplement for descriptions of each specific mechanism variable added to the models. Sample size and other modeling specifications are the same. Robust standard errors in parentheses.
*$p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

supervisors, independent of any network ties between supervisors.

Results suggest that the project quality index does mediate the initial assignment effect. When added to the models, project quality has a significant impact on performance-based pay ($p < .05$) and hazard of attrition ($p < .05$). Inclusion of project quality curtails the magnitude and eliminates the statistical significance of the powerful supervisor variable, consistent with its playing a mediation role in the impact of powerful supervisors on success among program users. In addition, project-portfolio quality is itself predicted by the initial assignment effect. Results from analyses predicting project quality among program users (see Table 7) indicate that exposure to powerful supervisors at entry increases project-portfolio quality measured prior to enrollment, as well as project quality in the year after enrollment. Taken together, these findings are consistent with the mechanism of access to reputation-building projects mediating the effect of initial exposure to powerful supervisors on program user career outcomes.

DISCUSSION

The Initial Assignment Effect

We found that initial assignment to powerful supervisors facilitated positive career outcomes for later work-family program users, that initial assignment affected users more than non-users, and that it operated through the mechanism of improved access to reputation-building projects. Initial assignment to powerful supervisors upon entry, while random in this organization, was a key predictor of success among program users, even though program use did not begin until several years later. Conditions at the time of program use, such as assignment to a proximate powerful supervisor, were less important. Full initial exposure to powerful supervisors led to a $26,624 boost in annual performance pay and a 27.2 percent lower probability of exit. We observed these effects in the presence of a range of controls as well as an adjustment for the simultaneous effects of selection into program use.

The initial assignment effect operated through the mechanism of providing employees with access to a range of reputation-building projects over time. By the time they enrolled in the reduced-hours program, employees who had been initially assigned to powerful supervisors had gained access to a greater range of reputation-building work opportunities than had other employees. We posit that this exposure to reputation-building project opportunities was important because the very use of a work-family program signals employees' membership in traditionally disadvantaged groups (i.e., mothers and male primary caregivers) and leads supervisors to negatively evaluate employees' abilities, commitment, and marketability to clients. Project opportunities that provide employees in traditionally disadvantaged groups with exposure to a wide range of supervisors and clients allow these employees to solidify their standing as able, committed, and marketable professionals in the eyes of this large invisible college before becoming program users. This large invisible college directly experiences vulnerable employees' abilities, commitment, and marketability prior to program use, and this experience helps protect vulnerable employees from negative evaluation at the point of later program use.

Although all employees who were initially exposed to powerful supervisors benefited from reputation-building projects, the initial assignment effect was greater for those who eventually used the reduced-hours program. Whereas program users with full initial exposure to supervisors saw a $26,624 boost in annual performance pay, non-users saw a $6,164 boost. Similarly, whereas program users saw a 27.2 percent lower probability of exit, non-users saw a 4.7 percent lower probability of exit. Program users and non-users both benefited from the exposure to reputation-building

Table 7. Least-Squares Coefficients for Models Predicting Project-Portfolio Quality

	Model 1: Cumulative Project Quality at Time of Enrollment	Model 2: Project Quality during Year after Enrollment (non-cumulative)
Intercept	1.205***	1.501**
	(.268)	(.431)
Litigation Department	.014	.656
	(.200)	(.367)
Other Department	−.155	−.268
	(.180)	(.301)
Location 2	.522*	.455*
	(.256)	(.218)
Location 3	−.012	−.510
	(.229)	(.337)
Entering Cohort Size	−.204	−.082
	(.165)	(.263)
Female	−.326	−.186
	(.214)	(.304)
Parent	−.015	−.023
	(.130)	(.183)
Minority	−.369*	−.222
	(.180)	(.247)
Acquisition Unit	−.319	−.077
	(.213)	(.326)
Law School Ranking	.003	.002
	(.003)	(.005)
Undergraduate Grades	−.264*	−.136
	(.110)	(.175)
Power Supervisor Exposure at Transition	.386	.487
	(.449)	(.560)
Power Supervisor Exposure at Organization Entry	.925*	1.667*
	(.445)	(.788)
Supervisor Provides Skills Development	−.102	−.190
	(.154)	(.224)
Supervisor is Central in Task Network	.020	.008
	(.014)	(.015)
Adjusted R^2	.28	.32

Note: $n = 71$. Model 1 predicts cumulative project quality at time of enrollment. Model 2 predicts (non-cumulative) project quality during the first post-enrollment year; it therefore does not incorporate any memory of pre-enrollment projects. Because their dependent variables differ, coefficients cannot be compared across the two models. Standard errors in parentheses.
*$p < .05$; ** $p < .01$; *** $p < .001$. (two-tailed tests).

opportunities that early assignment affords; why did program users benefit *more* than non-users from these opportunities? We posit that, because program users were vulnerable to negative evaluation due to their very use of the programs, they were more positively affected than non-users by having a large invisible college of supervisors and clients directly experience their abilities, commitment, and marketability. High exposure to reputation-building projects buffered against the negative effects of program use on evaluation, so that highly exposed users suffered minor or no declines in career outcomes.

Low exposure did not buffer against negative effects, so poorly exposed users suffered average or worse declines in outcomes.[12]

Contributions to Our Understanding of Work-Family Programs and Social Capital

Although many organizations adopt work-family programs to attract and retain employees or to comply with institutional pressure (Davis and Kalleberg 2006; Glass and Fujimoto 1995; Kelly and Dobbin 1999; Osterman 1995), employees often choose not to use these programs because they are concerned about potential retaliation (Bailyn [1993] 2006; Blair-Loy and Wharton 2002, 2004; Eaton 2003; Hochschild 1997; Perlow 1997; Williams 2000). Indeed, such programs often have a negative effect on program users' career outcomes (Glass 2004; Judiesch and Lyness 1999).

Our findings contribute to this understanding of work-family programs in several ways. First, prior studies highlight either the conditions under which employees are likely to use work-family programs (e.g., Blair-Loy and Wharton 2002, 2004) or the conditions that lead to effective implementation of employee-rights programs at the *organizational level* (e.g., Kalev et al. 2006). By contrast, we identify conditions that allow *individual* employees to *successfully use* work-family programs.

Second, while prior studies have not specifically investigated the conditions associated with positive career outcomes for program users, they do suggest that positive outcomes are likely facilitated by organizational conditions at the time of program use, such as the power of employees' proximate supervisors and the design of the program. In contrast, we demonstrate that the seeds of success can actually be planted much earlier: initial assignment to powerful supervisors upon organization entry improves career outcomes of later program users, even when organizational conditions at the time of program use are held constant. Our findings provide only limited support for the notion that powerful proximate supervisors can protect employees from negative career outcomes; instead, we find that once we include powerful initial supervisors in the analysis, the effect of powerful proximate supervisors loses significance.

Third, we identify the key mechanism through which the initial assignment effect operates—initial assignment to powerful supervisors matters because it helps employees gain access to reputation-building project opportunities over time. This finding is consistent with Kalev's (2009) finding that collaborative work relations can weaken stereotypes and lead to promotion opportunities. But our finding differs from Kalev's in a substantive way: her study points to the levers of self-directed teams and cross-training, while ours points to the lever of initial assignment to powerful supervisors.

These findings also add to our understanding of how relationships matter for career success. In many ways, our findings are consistent with the broad prediction of social capital theory—relationships are important to careers because they serve as valuable sources of information, influence, social credentials, and identity reinforcements (Blair-Loy 2001; Burt 1992; Fernandez and Fernandez-Mateo 2006; Ibarra 1993; Lin 2001; Podolny and Baron 1997). For these reasons, mentors in organizations are important because they provide protégés with access to new opportunities (Kay and Wallace 2009; Thomas and Kram 1988). We make two contributions to this research. First, we help to unpack the mechanism through which initial access to social capital shapes subsequent outcomes. We find that weak ties to powerful supervisors—ties that are formal, random, and relatively short-lived—can set off a virtuous spiral of reputation-building project opportunities.

Second, past studies find that women in male-dominated organizations suffer social capital deficits (Kay and Hagan 1998), yet little research directly connects the effect of

social capital with actors' vulnerability. We demonstrate that the value from prior relationships is heightened when employees in traditionally disadvantaged groups become doubly vulnerable through participation in a controversial workplace program.

Implications for Our Understanding of Inequality Remediation in Organizations

To what extent are these findings generalizable to other kinds of employee-rights programs designed to remediate inequality in organizations? To help employees in traditionally disadvantaged groups succeed, organizations adopt not only work-family programs but also diversity, disability, dispute resolution, and sexual harassment programs (Dobbin 2009; Dobbin et al. 1988; Dobbin and Sutton 1998; Edelman 1990; Kalev et al. 2006). Individuals eligible for employee-rights programs often suffer the same vulnerabilities as those who are eligible for work-family programs—women, minorities, and disabled employees are often perceived as less competent than other employees. Using these programs highlights employees' membership in traditionally disadvantaged groups and thus may invite supervisors to question their commitment, abilities, and marketability (Albiston 2007; Edelman, Erlanger, and Lande 1993; Harlan and Robert 1998; Heimer and Staffen 1998; Morrill 1995; Silbey, Huising, and Coslovsky 2009). Our findings suggest that initial assignment to a powerful supervisor would likely promote positive career outcomes for users of these other kinds of employee-rights programs as well. However, the timing of program use may be important. Vulnerable employees need to solidify their standing as professionals in the eyes of supervisors and clients before becoming program users; if they use disability or sexual harassment programs, for example, shortly after entering an organization, they may not benefit greatly from initial assignment to powerful supervisors for two reasons. First, the initial powerful supervisors may negatively evaluate them if they are already program users and choose not to provide them with a stream of reputation-building projects. Second, even if initial supervisors do provide these employees with such projects, the employees will have exposure to supervisors and clients under circumstances in which their ability, commitment, and marketability is already in doubt because of their choice to use the programs. Thus, the invisible college may negatively rather than positively evaluate them.

To what extent is the employer practice of initial assignment generalizable to other organizations? We expect this practice to be most important to program users in organizations where powerful initial supervisors can provide employees with access to a stream of future project opportunities that would allow them to solidify their standing as able, committed, and marketable professionals and where a large invisible college of prior supervisors or clients participates in the evaluation of employees at the time of later program use. In short, we expect to find similar results in investment banks, consulting firms, and accounting firms, but not necessarily in traditional manufacturing firms, where employees work with fewer supervisors and supervisors are typically not involved in later evaluation of their prior employees. Future research can help determine whether and how the initial assignment effect applies to other employee-rights programs and other kinds of organizations.

Practical Implications

For individuals, the practical implications of the findings presented here are clear, if not encouraging: individuals who did not happen to be assigned to a powerful supervisor at the outset probably should not choose to use a reduced-hours program. For organizations, the practical implications are more complicated. On the one hand, initial assignment

of female employees to powerful supervisors may be a way for employers to help these vulnerable employees succeed. On the other hand, if initial assignment became a program in its own right, it might develop the same stigmatizing effect associated with other employee-rights programs. Perhaps initial assignment was so powerful in this case precisely because it was randomly assigned. This suggests that organizations should proceed with caution, piloting initial assignment in particular offices or departments to see if it is possible to implement the practice in a way that does not invite retaliation.

In summary, one of the great challenges associated with remediating inequality in organizations is that even when organizations introduce new programs designed to help employees in traditionally disadvantaged groups succeed, employees who use the programs often suffer negative career consequences. This study demonstrates that assignment to powerful supervisors upon organization entry can improve career outcomes of later work-family program users by giving them improved access to reputation-building work opportunities. This allows these employees to solidify their standing as able, committed, and marketable professionals in the eyes of a large invisible college of evaluators before becoming program users. In a world where particular groups are discriminated against in the workplace and where employee-rights programs designed to remedy inequality are often avoided by the intended beneficiaries, identification of employer practices that improve career outcomes for vulnerable employees is critical. Initial assignment to powerful supervisors upon organization entry could be a way for employers to help level the playing field for traditionally disadvantaged groups in the workplace.

Acknowledgments

We thank Lotte Bailyn, Matthew Bidwell, Mary Blair-Loy, Emilio Castilla, Frank Dobbin, Roberto Fernandez, Isabel Fernandez-Mateo, Elizabeth Gorman, Alexandra Kalev, Erin Kelly, Jesper Sørensen, Wenpin Tsai, Ezra Zuckerman, members of the Slump Management Research Group, the Harvard Program on the Legal Profession, our research site, *ASR*'s anonymous reviewers, and the *ASR* editors.

Funding

This research was supported in part by an Alfred P. Sloan Foundation Industry Studies Fellowship to the first author.

Notes

1. We provide these numbers to give a sense of the category of firms to which our research site belongs. Here and elsewhere, we withhold precise details about our research site to maintain anonymity.
2. An additional advantage of this approach is that non-prorated pay nets out the effects of any irregular (partial) pay years that may arise if employees take leaves of absence.
3. We also have data on partnering. However, analyzing partnering reduces the usable sample size greatly because many associates started at the firm too recently to have become partners.
4. Results are similar if we choose other cut-points in the distribution of initial exposure.
5. We find similar results using other model specifications, including a simple hazard rate model for attrition. Results are also similar for female employees only, although the negative effect of program use on performance pay rises from –20.575 ($20,575, $p < .05$) to –30.405 ($30,405, $p < .01$).
6. The selection and treatment models include as coefficients all variables that are significant from Table 2 (full models available in Part C of the online supplement). Although these models can be identified without it, we include a weak exclusion restriction in the form of a dummy variable for the first two years of tenure. This variable is correlated with the treatment but is not correlated with career outcomes across person-years. Few other options are available because many factors that select people into the program (e.g., sex or family status) are also correlated with exiting the firm. We patterned our approach after Fernandez and Sosa (2005) who modeled a two-stage hiring process in which many factors influencing first stage outcomes also influence second stage outcomes.
7. Attrition analyses are also robust to an alternative specification omitting the small number of exits that were internally designated by the firm as "unregretted attrition."
8. As an additional analysis, we re-ran this analysis on female employees only. This produces similar

results: a $28,980 increase in pay and a 21 percent reduction in the probability of attrition.

9. We generated these interaction magnitudes using the *margins, predict() dydx()* postestimation command (available in Stata v.11) following the *biprobit* regression command. We computed the marginal estimates of initial exposure separately for program users and non-users.
10. A parallel fixed-effects approach to the retention outcome, using a conditional logit model estimated on discrete-time event history data as suggested by Allison (2005), did not converge.
11. Future research could examine the durability over time of the beneficial initial assignment effect we identified, including after program un-enrollment (i.e., following a return to regular employment status).
12. Highly exposed users had performance pay outcomes around the same level as highly exposed non-users.

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