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**Network Intervention:
Assessing the Effects of Formal Mentoring on Workplace Networks¹**

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**Network Intervention:
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Abstract: This article assesses the effects of formal mentoring on workplace networks. It also provides conceptual clarity and empirical evidence on expected gender differences in the effects of such programs. Qualitative interviews with 40 past participants in a formal mentoring program at a software laboratory in Beijing, China provided insight into the core mechanisms by which such programs produce network change: access to organizational elites, participation in semiformal foci, enhanced social skills, and legitimacy-enhancing signals. These mechanisms are theorized to lead to an expansion in protégés' networks, relative to those of non-participants in formal mentoring. Legitimacy-enhancing signals are theorized to enable female protégés to derive greater network benefit from formal mentoring than their male counterparts. Empirical support for these propositions came from a longitudinal quasi-experiment involving 75 employees who experienced the treatment of formal mentoring and 64 employees in a matched control group. A second empirical strategy, which exploited exogenous variation in the timing of treatment and enabled a comparison of the post-program networks of one treated group to the pre-program networks of another treated group, provided corroborating support. These findings contribute to research on the efficacy of formal mentoring, gender and workplace networks, and the cumulative advantage or disadvantage that can arise from network change.

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INTRODUCTION

There is by now a wealth of evidence linking the nature and quality of interpersonal networks within organizations to various indicators of individual attainment—for example, performance evaluations and rewards (Burt 1992), promotions (Podolny and Baron 1997), relative power and influence (Brass 1984), and career satisfaction (Seibert, Kramer, and Liden 2001). Yet, despite the importance of networks for individual success, remarkably little is known about what organizational practices, if any, help employees build interpersonal connections and how these effects might vary by type of employee. The evidence that does exist on how networks change in response to new practices such as training (Burt and Ronchi 2007), job rotation (Campion, Cheraskin, and Stevens 1994), and mentorship (Dreher and Ash 1990) is based on research designs that do not support causal identification—for example, studies that lack a credible control group against which to compare the outcomes of the treatment group, draw inferences based on cross-sectional or correlational data, or inadequately separate selection from treatment effects (for a review, see Van de Valk and Constas [2011]).

This article helps to fill the void in our understanding of the efficacy of organizational practices designed to change workplace networks. It does this by examining a pervasive practice—formal mentoring—that is widely thought to alter protégés’ networks in a manner that supports individual attainment (Hezlett and Gibson 2007). Formal mentoring is also believed to help women overcome deficiencies in network access—for example, to powerful actors or dominant coalitions in the organization—and is therefore proposed as an important means to addressing gender inequality in the workplace (Noe 1988). Yet the conceptual arguments and empirical evidence on the effectiveness of formal mentoring, in general, and as a practice to ameliorate gender inequality, in particular, are still inconclusive (Allen et al. 2008; O’Brien et al. 2010).

In this article, I seek to make three main contributions. First, I draw on qualitative interviews with 40 past protégés in a formal mentoring program to surface the core mechanisms linking formal mentoring to network change. These mechanisms serve as the building blocks for theory development about the conditions under which formal mentoring can be expected to produce network expansion and gender differences in who stands to experience the greatest network benefit. The qualitative evidence also helped inform the design of a subsequent quasi-experiment conducted with a formal mentoring program targeted to high-potential employees in a software development laboratory in Beijing, China. Second, I report the results of this experiment, which involved 75 employees who experienced the treatment of formal mentoring and 64 employees in a matched control group. The research design overcame many of the limitations prior studies faced in identifying causal effects. It provided two pathways for causal identification: (1) a differences-in-differences analysis (Angrist and Pischke 2009) of changes between the pre- and post-program networks of treatment and control group members; and (2) a comparison of the pre- and post-program networks of two treatment group cohorts whose timing of program participation varied exogenously.

Finally, I elucidate and help to resolve a conceptual puzzle about the differential effects of formal mentoring on the workplace networks of women and men. One set of arguments suggests that men will experience greater network expansion as protégés in formal mentoring than women will experience, while another predicts the opposite effect. I hypothesize that, through the mechanism of legitimacy-enhancing signals, such programs will provide greater network benefit to female participants than to their male counterparts. Results from the quasi-experiment were consistent with this expectation. These findings contribute to research on

formal mentoring, gender and workplace networks, and the cumulative advantage or disadvantage that can arise from network change.

The theoretical arguments and empirical analyses that follow are informed—and to some extent constrained—by the distinctive cultural and institutional features of work organizations in China. For example, work organizations in China often have power structures that are more starkly gendered than those found in organizations based in western societies. Indeed, in the software development lab that served as the research site, there was a dearth of senior women who could potentially serve as formal mentors. It was therefore not possible to investigate the potential moderating role of gender match between mentors and protégés in network expansion. Similarly, the patriarchal nature of many Chinese work organizations requires even greater attention to the potential threat to causal inference arising from selection bias. For example, if women have to demonstrate greater competence than men to be designated as high-potentials, then unobserved gender differences in ability could potentially confound estimates of the treatment effect of formal mentoring on the networks of male versus female protégés. This possibility underscores the importance of employing fixed effect specifications of the kind described below to account for such unobserved heterogeneity.

THEORY

Workplace Networks and Attainment

Research on social networks and individual attainment has considered the roles of network structure—for example, weak ties (Granovetter 1995) or structural holes (Burt 1992)—of social resources that can be accessed through ties (Lin 2001), and of the interplay between the two (Seibert, Kramer, and Liden 2001). The present investigation draws on the social resources

perspective, which suggests that the size and nature of workplace contacts mobilized can influence the quality of resources that flow to employees and thereby influence their attainment. For example, in their longitudinal study of social networks and upward mobility, Podolny and Baron (1997, 687) reported that each additional (recently formed) task advice tie more than doubled an employee's odds of promotion in the following year. Each additional strategic intelligence tie had a comparable effect size.

Indeed, research on interpersonal networks and employee performance has consistently documented a positive association between network size, or degree centrality, and individual outcomes such as job satisfaction (Flap and Völker 2001), sales performance (Moran 2005), job performance ratings (Sparrowe et al. 2001), and income (Carroll and Teo 1996). While studies linking social resources obtained through networks to attainment have proliferated, very little is known about the efficacy of network intervention.

Network Intervention

Network intervention refers to “purposeful efforts to use social networks or social network data to generate influence, accelerate behavior change, improve performance, and/ or achieve desirable outcomes among individuals, communities, organizations, or populations” (for a review, see Valente [2012], 49). There are four broad kinds of network intervention: (1) identifying individuals based on a network property to exert influence on others (e.g., Valente and Pumpuang 2007); (2) targeting a change initiative to a subgroup within a network (e.g., Meltzer et al. 2010); (3) stimulating peer-to-peer interaction to create information cascades (e.g., Aral and Walker 2011); and (4) deliberately altering the network to change outcomes of interest (e.g., Thomas et al. 1998). My focus is on the fourth kind of network intervention—specifically,

organizational practices such as formal mentoring that are designed to help employees build valuable social connections in the workplace.

Formal Mentoring and Workplace Networks

Formal mentoring programs have diffused broadly across organizations, including work organizations in China (Bozionelos and Wang 2006). Survey estimates indicate that one-third to two-thirds of workers have participated in a mentoring relationship (Seibert 1999). Mentors are defined as experienced and knowledgeable individuals who are committed to providing career and psychosocial support to one or more protégés (Kram 1985). Although many relationships in the workplace can include a developmental component, I focus on traditional forms of mentoring—hierarchical relationships focused on protégé development (Higgins and Kram 2001). Informal mentoring refers to relationships that develop spontaneously and often last a long time, whereas formal mentoring involves relationships that arise from organizational intervention—typically in the form of voluntary assignment or matching of mentors and protégés—and often exist for a shorter duration (Ragins and Cotton 1991).

Formal mentoring programs are often targeted to specific employee populations such as new employees, senior managers, or high-potential employees. Indeed, a survey of 246 U.S. corporations found that only 10% of firms with formal mentoring programs made them generally available to all employees; the remainder targeted them to specific populations (Douglas and McCauley 1999). The conceptual arguments below pertain to formal mentoring targeted to high-potential employees. Surveys indicate that nearly a third of the formal mentoring programs in place in US corporations are targeted to this population (Douglas and McCauley 1999).

Formal mentoring, whether targeted or not, is widely believed to have a positive influence on protégés' career outcomes and subjective well-being—for example, promotions,

income, organizational commitment, turnover intentions, job satisfaction, self-esteem, work stress, and work-family conflict (for a review, see Underhill [2006]). These distal outcomes are thought to arise in part through formal mentoring's proximal effects on workplace networks (Hezlett and Gibson 2007). For example, introductions made by mentors on behalf of protégés in the course of formal mentoring lead to “an expanded social network that provides the protégé with other sources of contacts, advice, social support or strategic information” (Wanberg, Welsh, and Hezlett 2003, 94). In other words, network expansion is often a core objective of formal mentoring programs.

Yet the empirical evidence on formal mentoring's overall effectiveness remains inconclusive—in part because the extant literature has not employed research designs that support causal identification—and the mechanisms by which mentoring changes networks remain unclear. Indeed, a recent meta-analysis of research methods used in more than 200 mentorship studies found that over 90% were based on cross-sectional data. Approximately 70% did not specify or failed to distinguish the form of mentoring studied—for example, formal versus informal—even though these distinctions are conceptually relevant. Only 5% used qualitative methods to uncover the mechanisms by which mentoring works or does not work (Allen et al. 2008).

To my knowledge, only three prior studies have employed designs that enable researchers to make causal claims—one employed a longitudinal quasi-experiment (Seibert 1999) and the other two random assignment of participants to control and treatment conditions (Eesley and Wang 2014; Egan and Song 2008). These studies examined outcomes such as employee attitudes, supervisor performance ratings, and rates of entrepreneurship but did not seek to measure changes in workplace networks. Thus, we have heretofore lacked credible causal

evidence on whether formal mentoring affects protégés' workplace networks and, if so, whether it has differential consequences for women relative to men.

Gender Differences in Expected Effects of Formal Mentoring

A robust literature has examined gender differences in workplace networks. For example, Brass (1985) reported that, although men and women in a newspaper publishing company appeared to build networks equally well, they tended to form sex-segregated networks. As a result, women were less central in men's networks, especially those of the dominant coalition in the organization. Campbell (1988) also found differences in the networks of employed men and women, with the former having networks with greater occupational range and socioeconomic diversity than the latter. In a similar vein, Ibarra (1992) found in a study of an advertising firm that men tended to form homophilous networks for both instrumental and expressive purposes, whereas women exhibited a differentiated pattern of homophily in forming expressive ties and heterophily in building instrumental ties.

In a large financial services organization, women were less likely than men to have the resources and positions that would bring them into contact with and build relationships with high-status employees (McGuire 2000), and women received less informal help than men even when they had jobs in which they controlled resources and had network contacts who also controlled resources (McGuire 2002). More recently, Kleinbaum, Stuart, and Tushman (2013) analyzed email data in a large technology firm and found that women had a greater number of contacts than men and communicated at an elevated rate with other women inside and outside their business units and offices. Men only exhibited homophily in within-office communication.

To the extent that formal mentoring changes workplace networks, existing theory leads to competing expectations about potential gender differences in these effects. On one hand, men

can be expected to derive greater benefits from formal mentoring than women because they are better able to translate a given structural position into network advantage (McGuire 2002; Roth 2006). As protégés, men also receive more career development support such as career counseling or introductions by mentors to powerful others (Ragins and Cotton 1991). Moreover, for men, taking action on this support—for example, following up on introductions made—is more consistent with their gender role than it is for women (Ragins 1999). These advantages for men over women are likely to be amplified in settings with sharply gendered power structures of the kind found in many Chinese work organizations.

On the other hand, women can be expected to accrue a disproportionate share of benefits from formal mentoring because they often enter these programs from structural positions that, relative those held by men, provide inferior access to powerful organizational actors (Moore 1992). Prior to participation in formal mentoring, women are also less likely to be visible in the organization (Kalev 2009) and more likely to be marginalized as result of exclusionary pressures (Mehra, Kilduff, and Brass 1998). For example, McGuire's (2000, 519) study of employees in a large financial services company concluded: "Structural exclusion from high-ranking and resourceful positions, not a lack of networking knowledge or skills, prevented...women...from forming ties to powerful network members." Insofar as formal mentoring serves to rectify these past inequities, it should provide greater benefit to women than to comparably skilled men. In short, the question of whether formal mentoring will differentially expand men's or women's networks has heretofore remained a conceptual puzzle.

Unpacking the Mechanisms by which Formal Mentoring Produces Network Change

To gain conceptual clarity on this puzzle, I begin by unpacking the mechanisms by which formal mentoring produces network change and consider how each might operate differently for men

versus women. The qualitative interviews I conducted with 40 past participants in a formal mentoring program (details provided below) revealed four core mechanisms that link formal mentoring to network change: (1) access to influential organizational actors; (2) involvement in project teams that serve as foci for new tie formation; (3) social skills acquired by the protégé; and (4) legitimacy that derives from the signal of a protégé's formal association with a respected senior person.

Formal mentoring can be expected provide improved access to organizational elites, as mentors introduce protégés to their network contacts. These introductions will, in turn, enable protégés to expand their networks. For formal mentoring targeted to high-potential employees, however, there is no reason to expect that these introductions will provide differential benefits to one gender over the other. This is because, although women in general are often excluded from an organization's elite circles (Kanter 1977), high-potential women are likely to begin with comparable levels of network access as men. Indeed, in one empirical setting, high-potential women had closer ties and even broader network range than high-potential men (Ibarra 1997). Thus, high-potential men and women will experience a comparable change in network access from participating in formal mentoring.

Insofar as formal mentoring involves the assignment of a protégé to a work group or project team, these allocation choices provide another conduit to network expansion. Work groups and project teams can be thought of as elements of the semiformal organizational structure (Biancani, McFarland, and Dahlander 2014). They serve as foci for interaction and facilitate the formation of new ties (Feld 1981). So long as mentors are not biased in their propensity to assign male or female protégés to work groups or project teams, there is again no reason to expect that gender differences in network expansion will arise from this mechanism.

Social learning represents a third pathway from formal mentoring to network expansion. For example, in qualitative studies of formal mentoring's benefits and challenges, protégés routinely report that the experience sharpened their ability to understand the perspectives of colleagues in other organizational subunits, boosted their self-confidence in navigating complex interpersonal dynamics, and improved their ability to integrate different problem-solving techniques (Eby and Lockwood 2005). This learning arises from exposure to a broader range of organizational subunits and organizational actors. Assuming that male and female protégés receive comparable exposure during formal mentoring to new subunits and actors, they should also experience similar rates of network expansion stemming from enhanced social skills.

Whereas the first three mechanisms are not likely to have differing consequences for men's and women's networks, I theorize that the fourth mechanism will produce greater network expansion for women than for men. Consistent with Spence's (1973) theory of signaling, the assignment of a protégé to a respected senior mentor can convey the protégé's worth to others in the organization (Ramaswami et al. 2010). As Burt (1998, 24) explains, "Company leaders don't have time to check into the credibility of everyone making a bid for broader responsibilities. They are looking for fast, reliable cues about managers on whom they do not already have information." He (1998, 27) goes on to argue that direct supervisors make for poor sponsors because they are expected to endorse their subordinates, while more organizationally distant advocates such as formal mentors add a "corroborating external voice" that constitutes a more credible signal of worth.

In many organizational settings, women—even high-potential women—are likely to be viewed as less legitimate than equally competent men (Ridgeway 1997). Thus, the positive signal that comes with the assignment to a well-regarded mentor will provide a greater

legitimacy benefit to women than to men. Enhanced legitimacy makes a person more attractive as a potential network partner for two reasons. First, people prefer affiliating with others whom they believe to be connected to organizational elites (Kilduff and Krackhardt 1994). Second, more legitimate actors are also more likely to accrue valuable social resources and tend to be sought after as exchange partners (Thye 2000). Assuming that protégés generally seek to forge new network ties, female protégés are therefore likely to experience greater network expansion than will male protégés. In the context of many Chinese work organizations, where women are especially likely to be marginalized, the benefits of legitimacy-enhancing signals are even more likely to accrue to women than to men.

Taken together, these arguments lead to a baseline expectation that formal mentoring will lead to network expansion for protégés, relative to comparable non-participants in formal mentoring. At the same time, the legitimacy-enhancing signals that arise when protégés are affiliated with well-regarded mentors suggest that gender will moderate this effect. In other words, whereas formal mentoring can be expected to provide network benefits to both men and women through increased access to organizational elites, potential participation in semiformal foci, and enhanced social skills, women will receive a “double benefit” (cf. Briscoe and Kellogg 2011) in the form of enhanced legitimacy. I therefore expect:

Hypothesis 1: People who participate in targeted formal mentoring will experience greater network expansion than comparable individuals who do not participate in targeted formal mentoring.

Hypothesis 2: Gender will moderate the effects of formal mentoring on network expansion, such that women will experience greater network expansion from targeted formal mentoring than will men.

METHOD

Empirical Setting and Program Description

I tested these hypotheses in a software development laboratory, which was located in Beijing, China but was part of a US-based global technology products and services firm. The laboratory employed several thousand people and was organized into departments, corresponding to the firm's global software brands and to various cross-brand programs. Although most employees were born and educated in China, they were generally proficient in English.

Over the years, the firm had shifted an increasing share of its software development activity from the United States to less expensive locations such as India and China. As a result, the software development laboratory in China was experiencing rapid growth. The scarcity of competent managerial talent represented an important constraint on the lab's ability to grow. The head of the lab therefore decided to implement a targeted formal mentorship program, referred to internally as the "shadowing program," which was targeted to well-performing employees who were thought to have management potential. Individuals were nominated for the program by their managers. A program manager in human resources made final selection decisions and then matched protégés to mentors based on expressed learning needs.

Matches were made across departmental lines—that is, selected individuals worked in a different department than the mentors to whom they were assigned—so program participants could gain breadth of exposure. Mentors were of comparable rank and thus had similar status in

the organization. As noted above, it was not possible in this setting to test for differences in the treatment effect based on the gender match between mentor and protégé—a key variable that can influence mentorship outcomes (Ragins 1999).

The mechanics of the program worked as follows. Those selected for the program were assigned to “shadow” a more senior leader for a finite period, typically the equivalent of twelve business days spread out over two to three months. The protégé and his or her mentor had an initial meeting to discuss objectives. The mentor would then grant the protégé access to his or her electronic calendar. Protégés could attend any meeting on the calendar, except for sensitive career discussions between the mentor and a direct report—for example, a performance review. In some cases, mentors would also assign protégés a discrete project to complete during the assignment. Although the list of protégés was not formally announced, people generally knew who was in the program and whom they were shadowing at any given point in time. Upon conclusion of the program, protégés returned to their original job roles.

Qualitative Analysis: Mechanisms Linking Formal Mentoring to Network Change

Insight into the mechanisms by which formal mentoring produced network change for protégés came from an analysis of 40 semi-structured interviews conducted with past program participants. The interview protocol is provided in the Appendix. Of the 31 program alumni who were invited to participate in the interviews, 22 agreed to do so. In addition, all 11 mentors who had taken on a protégé in the past and all 7 program administrators from human resources agreed to participate in the interviews. Thus, the overall response rate was 82%. Interviews with mentors lasted 30 minutes, while those with protégés and program administrators lasted 45 to 60 minutes. Interviews were tape recorded and transcribed. Because most interviewees were not native English speakers, I edited some of the quotations reported below for grammar and syntax.

I analyzed the qualitative data using a software tool—Atlas.ti. I coded all mentions of factors that respondents felt contributed to network change following program participation. I started by developing detailed codes for each of these mentions, such as “Status Enhancement,” “Sponsorship,” “Project Teams,” and “Increased Confidence.” Some mentions were assigned to multiple codes. Later I grouped these specific codes into four code families: “Access to Influential Organizational Actors,” “Participation in Semiformal Foci (e.g., work groups and project teams),” “Enhanced Social Skills,” and “Legitimacy-Enhancing Signals.” Table 1 provides representative quotations associated with each of these code families and shows the number of times they were mentioned by interviewees.

- Table 1 about here -

A Network Intervention Quasi-Experiment

After completing the qualitative interviews, I worked with the company to design a quasi-experiment of the program’s effects on workplace networks. The company identified 102 people to participate in an upcoming iteration of the program. Consistent with the practice from prior years, these individuals were selected for the program through a two-stage process: (1) supervisors nominated subordinates whom they believed to have significant potential for advancement within the organization; (2) the human resource professionals who managed the program chose a subset of nominated employees based on the program’s available capacity in that year, employees’ past performance ratings, and the strength of support they received from supervisors and other senior leaders who had knowledge of their work.

Once program participants were notified of their selection to the program, the head of the software lab sent them an email informing them of the study and inviting them to participate. They were told that the study’s objective was to help assess the effectiveness of the program and

to identify ways to improve its design. I then sent them a follow up email explaining that participating in the study entailed completing surveys before, during, and after the program. Of the 102 people invited to participate, 91 agreed to do so by completing the pre-program survey (89% participation rate). Based on their responses to the pre-program survey, which included a section on work history, I identified 16 individuals who had previously participated in a shadowing or other comparable formal mentoring program. For example, the company sometimes assigned high-potential employees to serve as an executive assistant to a senior leader and ran other smaller-scale mentorship programs. I excluded those individuals from the estimation of the treatment effect because their pre-program networks likely included the past effects of participation in such programs. The results reported below were substantively unchanged—though somewhat attenuated—when these 16 individuals were included in the analysis. The resulting treatment group consisted of 75 people.

The company did not agree to random assignment of eligible participants into treatment and control groups because they worried about unintended signals such a procedure might send to their most valued employees. Instead, I worked with the company's human resources (HR) department to construct a matched control group. Because the firm's internal policies prohibited the sharing of employee records with external researchers, it was not possible to employ standard techniques such as propensity score matching (Rubin 2006). Instead, the firm agreed to implement the following matching procedure: for each program participant, a human resources representative identified two people who: (1) were at the same salary band; (2) had the same performance rating in the prior year; (3) had the same tenure within the organization; (4) worked in the same office; and (5) had not previously participated in the formal mentoring program.

When, as in most cases, more than two people met these criteria, the HR representative randomly selected two from the eligible list. In some cases, there was only one person who matched these criteria. In total, 189 people were identified through this procedure and invited to participate in a research study on the topic of workplace social networks. No mention was made of the formal mentoring program in the communication to these individuals. They were simply told that they would need to complete three network surveys to participate in the study. Of the matched control group, 85 agreed to participate by completing the first survey (45% participation rate). Based on their responses to the first survey, which included questions about their work history, I identified 21 who had previously participated in some form of targeted formal mentorship or shadowing program. For comparability with the treatment group, these individuals were excluded from the analysis, resulting in 64 matched control group employees.

Table 2 provides evidence that the matching procedure was effective: there were no statistically significant differences between the treatment and matched control groups on observable characteristics, including the number of contacts they reported mobilizing in the two months prior to the start of the formal mentoring program. At the same time, it is important to note that the treatment and matched control groups may have varied on unobserved characteristics. The former were, for example, selected partly on the basis of their (unobserved) advancement potential, while the latter were not selected on this basis. Because the treatment and matched control groups may have differed in unobserved ways, it was necessary to implement an alternative identification strategy (described below) based on a comparison of two subsets of treatment group individuals.

- Table 2 about here -

Both groups completed on-line network surveys before and two months after their formal mentoring assignments. The pre-program survey included questions about respondents' work histories and sociodemographic characteristics. Each survey included four network name generators, which were adapted from previous studies of workplace social networks (Podolny and Baron 1997): (1) task advice; (2) mentorship; (3) strategic intelligence; and (4) friendship. Because employees could potentially have obtained social resources from several thousand potential colleagues, it was not possible to use the roster method to identify contacts. Instead, participants listed initials of network contacts through free recall.

Sample Attrition

Sample attrition is a well-recognized problem in longitudinal network studies (Huisman and Steglich 2008). Of the 139 people who completed the pre-program survey, 73 did not complete the post-program survey (attrition rate of 52%). Those who did not complete the post-program survey were statistically indistinguishable from those who did on all observable characteristics, including reported network size prior to treatment. Nevertheless, to account for potential bias stemming from sample attrition, I conducted a robustness check (described below) using inverse probability treatment weights (Robins, Rotnitzky, and Zhao 1995).

Two Empirical Strategies for Causal Identification

Because the quasi-experiment did not entail random assignment of participants to treatment and control groups, I employed two complementary empirical strategies to recover causal effects. The first identification strategy included both the treatment and matched control groups. I used differences-in-differences estimation. The differences-in-differences estimator represents the difference between the pre-post, within-subjects differences of the treatment and control groups (Angrist and Pischke 2009). In some specifications, I included individual fixed effects to account

for all time-invariant unobserved heterogeneity among participants and non-participants. Fixed effects were especially important to include given potential unobserved differences in the abilities of women and men selected for the program.

The second identification strategy helped address threats to the validity of the first strategy. The matched control group was not involved in this analysis, thereby obviating the concern that this group might have differed from the treatment group on unobserved characteristics (e.g., advancement potential or susceptibility to survey fatigue). Instead, this strategy involved comparing two subsets of treatment group employees. In particular, it took advantage of a unique feature of the way the program was implemented.

Because the available capacity of mentors was limited, the program was implemented in two separate cycles. Interviews with program administrators and participants, as well as my own observations, confirmed that assignment of participants to cycles was based on factors that were exogenous to individual ability or perceived managerial potential—for example, departments facing an impending deadline might prefer to send people nominated from the department to the later program cycle while departments that had just completed an important project milestone or recently hired new staff might prefer to send their nominated people to the first cycle. In other cases, individual-level factors that were unrelated to ability or perceived potential—for example, previously scheduled business trips or training programs—determined the choice of cycle.

Table 3 compares the observed characteristics of these two groups. Consistent with the notion that exogenous factors led to the assignment of people to program cycles, none of the differences—including the number of contacts mobilized in the two months before the start of the mentoring program—was statistically significant.

- Table 3 about here -

To estimate the causal effect, I compared the post-program reported networks of participants from one cycle to the pre-program reported networks of participants from the other cycle. One cycle of the treatment group therefore served as the “control” against which the outcomes of the other cycle were compared. The key advantage of this approach over having a matched (but not randomly assigned) control group is that it better accounts for potential selection bias and unobserved heterogeneity. Because both groups of participants were selected for (and ultimately received) the treatment of program participation, they were likely to be comparable on observed *and* unobserved factors.

In principle, one could consider either cycle the treatment group and the other the control group. However, if the first cycle were considered the treatment group and the second the control, there would be risk of cross-contamination of the control group. For example, participants from the first cycle might talk about their experiences in the program with those in the second cycle. This communication could then influence the network actions of second cycle participants and thereby distort their reported networks prior to treatment. To address this possibility, I compared the post-program reported networks of participants from the second cycle to the pre-program reported networks of participants from the first cycle.¹

Measures and Estimation

In constructing the dependent variable, I drew on prior research linking workplace networks to various indicators of individual attainment. For example, Podolny and Baron (1997) found that task advice and strategic intelligence ties were associated with increased likelihood of upward job mobility. They reported mixed results for the relationship between mentorship ties and promotion chances: such ties were only beneficial when mentors had “fate control.” By contrast, Seibert, Kramer, and Liden (2001) found strong support for the notion that larger mentorship

networks are associated with greater career success. In the present empirical setting, mentors had “fate control” over protégés in that they often provided feedback to protégés’ line managers and offered input on later promotion decisions. Thus, all three kinds of network ties—task advice, strategic intelligence, and mentorship—could be expected to yield instrumental career benefits for employees in this setting.

Prior work indicates that Chinese workers often do not draw sharp distinctions between instrumental and expressive ties (Bian and Ang 1997; Bozionelos and Wang 2006), suggesting that friendship ties should also be considered alongside the other three kinds of ties. Thus, the dependent variable was a sum of the number of task advice, strategic intelligence, mentorship, and friendship ties a respondent mobilized in a given period. Comparable results to those reported below were obtained when the dependent variable was based on just instrumental ties—that is, excluding friendship ties—and when it was weighted by tie strength.²

For the differences-in-differences estimation, I used an indicator variable, *Treatment*, which was set to 1 for program participants, and *Post-Program* was set to 1 for the period following the program. The interaction term, *Post-Program x Treatment*, thus represents the overall treatment effect. To identify gender differences in the treatment effect, I used an indicator, *Female*, and the interaction terms: *Post-Program x Female* and *Post-Program x Treatment x Female*. (Note that the other variables that would typically be included to test for interaction effects—*Female*, *Treatment*, and *Treatment x Female*—are time-invariant and therefore subsumed by the fixed effects.)

For differences-in-differences estimation, I report results of conditional fixed effect Poisson quasi-maximum likelihood regression models (Wooldridge 1997). This estimator is

consistent under relatively weak assumptions: only the conditional mean needs to be correctly specified, and the standard errors account for potential over- or under-dispersion.

RESULTS

Table 4 reports the results of the differences-in-differences estimation. Model 1, which represented the baseline, did not include employee fixed effects but instead included a number of control variables. None of the controls was statistically significant. In Model 2, *Post-Program* is significant and negative, reflecting the decline in reported contacts among matched control group respondents. By contrast, *Post-Program*×*Treatment* is positive and significant, suggesting an overall treatment effect consistent with Hypothesis 1. In Model 3, *Post-Program*×*Treatment* is not significant, while the three-way interaction, *Post-Program*×*Treatment*×*Female*, is positive and significant. That is, consistent with Hypothesis 2, the program led to an expansion of female participants' networks but not those of male participants. In Models 4 and 5, I introduce employee fixed effects to account for time-invariant unobserved heterogeneity. Model 4 replicates the findings from Model 2, also lending support to Hypothesis 1. Similarly, Model 5's results largely mirror those from Model 3 and provide support for Hypothesis 2.

- Table 4 about here -

Although the differences-in-differences estimates support Hypothesis 1 and Hypothesis 2, the descriptive statistics raise some concerns. Table 5 shows how the reported networks of treatment group and matched control group employees changed between the pre-program to the post-program survey. The treatment group reported an increase in the number of contacts mobilized, and this increase was statistically significant for female respondents. By contrast, the

matched control group reported a decrease in the number of contacts mobilized, and this decrease was significant for the group as a whole and for male respondents.

It is unclear why the matched control group would report fewer contacts in the post-program survey than in the pre-program survey. To my knowledge, there were no changes in their job roles or responsibilities during this period. One possibility is that the control group suffered a relative decline in status because they were considered but ultimately not selected for the formal mentoring program and therefore became less attractive as network partners for their colleagues. Alternatively, not being selected for the program might have made them less motivated and caused them to expend less effort in mobilizing social resources or responding to name generator questions in repeated surveys. Unfortunately, it is impossible to know which of these explanations might have accounted for the observed changes in the networks of matched control group employees.

- Table 5 about here -

These lingering questions about the comparability of the treatment group and matched control group underscore the importance of considering the second identification strategy, which took advantage of exogenous variation in *when* treatment group employees participated in the program. Table 6 reports these results. Cycle 2 participants reported mobilizing 11.81 contacts in the post-program survey (two months after treatment), while Cycle 1 participants reported mobilizing 8.07 contacts in the pre-program survey (prior to treatment). This difference of 3.74 contacts was significant ($p < .05$). Female Cycle 2 participants reported mobilizing 17.0 contacts in the post-program survey, while female Cycle 1 participants reported mobilizing 6.44 contacts. This difference of 10.55 contacts was also significant ($p < .01$). Male Cycle 2 participants reported mobilizing 10.19 contacts, while male Cycle 1 participants reported mobilizing 8.49 contacts.

This difference of 1.70 contacts was not significant. Overall, these results corroborate the differences-in-differences estimates and provide support for both Hypothesis 1 and Hypothesis 2. They suggest that the program had a positive treatment effect on workplace networks but that its benefits accrued primarily to female participants.

- Table 6 about here -

Although the changes in reported contacts mobilized might seem modest and longer-term career outcomes were not measured in this study, prior research suggests that network changes of this magnitude can have dramatic career consequences. Recall that Podolny and Baron (1997) found that each additional (recently formed) task advice tie and each additional strategic intelligence tie more than doubled an employee's odds of promotion, and network size, or degree centrality, has been linked to a wide range of individual career outcomes. To the extent that these results generalize to other organizational settings, the changes in workplace networks detected in this experiment likely had meaningful implications for participants' careers.

Robustness Check and Extension

Although there were no significant differences in observable characteristics between those who completed both the pre- and post-program surveys and those who did not, I estimated the differences-in-differences models using inverse probability treatment weighting (IPTW) to better account for potential bias from sample attrition (Horvitz and Thompson 1952; Robins, Rotnitzky, and Zhao 1995). Specifically, I first estimated a logit model in which an indicator variable set to 1 for subjects who completed both surveys was regressed on age, tenure within the firm, and whether or not the person held an advanced degree. Next I re-estimated Table 4, Model 4 and Table 4, Model 5, while weighting the observations by the inverse of predicted probabilities. The results (not reported) were materially unchanged.

I also conducted a supplemental analysis to establish the role of legitimacy-enhancing signals as the primary mechanism producing greater network expansion for women than for men. I separately estimated for men and for women a fixed effects regression with the three-way interaction term, *Post-Program*×*Treatment*×*Tenure*, and all relevant (i.e., time-varying) main effects and two-way interaction terms. Results (not reported) indicate that, for women, this three-way interaction term was significant and negative. In other words, although women who participated in the program experienced network expansion, their tendency to do so declined as their tenure in the organization increased. By contrast, the three-way interaction term was not significant for men. Thus, as women gained legitimacy through their own contributions during their time in the organization, they appeared to benefit less from the signal of being affiliated with a respected senior person. By contrast, perhaps because men did not start with a legitimacy deficit, the benefits of the program did not vary with their tenure in the organization.

DISCUSSION AND CONCLUSION

The goal of this study has been to examine how the introduction of a formal mentoring program changes workplace networks. Qualitative interviews with past program participants surfaced four core mechanisms that link formal mentoring to network expansion: access to influential organizational actors, participation in semiformal foci, enhanced social skills of protégés, and legitimacy-enhancing signals that arise from the affiliation with a prominent mentor. I theorized that, whereas the first three mechanisms can be expected to produce network expansion for both high-potential men and high-potential women, legitimacy-enhancing signals will tend to expand women's networks more than men's. These propositions were tested in the context of a quasi-

experiment conducted with a formal mentoring program targeted to high-potential employees in a software development laboratory in Beijing, China.

Differences-in-differences estimation with a treatment group and a matched control supported the propositions, but the lack of random assignment of people to the two groups left lingering questions about their comparability. Exogenous variation in the timing of treatment enabled a second identification strategy that helped address these concerns: a comparison of the post-treatment networks of one treatment subgroup to the pre-treatment networks of another. This approach accounted for unobserved heterogeneity between the treatment and control groups and selection bias, which have plagued virtually all prior empirical investigations of the effects of formal mentoring (Allen et al. 2008; Underhill 2006; Wanberg, Welsh, and Hezlett 2003). This supplemental analysis corroborated the differences-in-differences estimates, suggesting that the program had a positive treatment effect on workplace networks but that its benefits accrued primarily to women.

Limitations and Directions for Future Research

Because these findings are based on data from a formal mentoring program targeted to high-potential employees and set in China, one must consider the extent to which these findings can be generalized to other employee populations and institutional settings. It is theoretically ambiguous how these effects can be expected to vary in formal mentoring programs that are not targeted to high-potential employees. On one hand, women who are not considered high-potential are more likely to occupy disadvantaged structural positions in the organization. So they might benefit more than the women in this study from changes in opportunity structure. On the other hand, mentors might be less motivated to provide career development support to

women who are not high-potential employees, such that they would benefit less than the women in this study. The net effect remains to be identified through future research.

Another question about generalizability arises from possible differences in the unobserved criteria used to select men and women into the formal mentoring program. Although there were no differences in the explicit criteria, it is possible that the women selected to the program had different underlying qualifications than their male counterparts. For example, they may have needed exceptionally strong abilities to overcome the implicit bias and discrimination that would otherwise have kept them from being noticed as high potentials. The fixed effect specification used in the first empirical strategy helped account for this alternative explanation for why women may have benefited more from the program than men did.

The fact that the study was set in a particular location within China (Beijing) also raises questions about generalizability because of the country's changing institutional context (Zhao and Zhou 2004), the distinctive ways in which people think about and mobilize social resources in China (Bian 1997; Morris, Podolny, and Sullivan 2008; Ruan et al. 1997), and heterogeneity of institutional features and the role of social networks across regions within China (Bian 2002). In this particular empirical setting, national cultural differences were mitigated because the software development lab was part of a US-based multinational. For example, whereas Xiao and Tsui (2007) found that structural holes were detrimental to employees' career outcomes in the national cultural context of China, Merluzzi (2012) reported that—as in Western samples—there were positive returns to structural holes among senior managers working for a US-based multinational in China. Given that recent meta-analyses indicate that only one other study has examined formal mentoring in China (Allen et al. 2008), further work is clearly needed to understand the role of national cultural context in formal mentoring outcomes.

Finally, it was not possible in this particular field experiment to collect other network structural measures—such as density (Sparrowe et al. 2001), range (Reagans and McEvily 2003), or constraint (Burt 1992)—or examine the longer-term consequences of formal mentoring. Future research could profitably examine the effects of formal mentoring on not only the size and content of workplace networks but also on their structure. Similarly, studies with longer time horizons could better examine whether formal mentoring’s effects on networks are ephemeral or enduring and may allow for direct measurement of the theorized mechanisms that produce network change (e.g., legitimacy-enhancing signals).

Contributions

These issues notwithstanding, the study makes a number of noteworthy contributions. It is to my knowledge the first to provide quasi-experimental evidence of one of the main theorized benefits of formal mentoring: network expansion (e.g., Wanberg, Welsh, and Hezlett 2003). In particular, the use of two distinct identification strategies helps to address concerns about selection bias that have plagued all prior attempts to estimate the network benefits of formal mentoring.

Next, it informs our understanding of the role of formal mentoring as a means to address gender inequality in the workplace (e.g., Noe 1988). Although firms that have introduced programs designed to increase the social connectedness of women have experienced modest reductions in gender inequality (Kalev 2009; Kalev, Kelly, and Dobbin 2006), the core mechanism of network change was not directly observed or measured in this line of research. The present study provides direct evidence of this missing link. The result—that formal mentoring provided greater network benefit to women than to men—provides empirical support for McGuire’s (2000, 519) contention that such programs are essential if companies seek to “equalize access to informal networks at work.”

Finally, by considering the consequences of targeted formal mentoring for employees who were otherwise comparable to participants but not selected to participate, the study provides suggestive evidence that can inform longstanding debates about networks and cumulative advantage (DiMaggio and Garip 2012). Participants in formal mentoring reported an expansion in valuable workplace networks, while matched non-participants reported a contraction. A number of factors—for example, survey fatigue or a decline in status from being considered but not selected for the program—could have accounted for the decline in control group members’ reported networks. Insofar as this decline was not just an artifact of survey fatigue but instead presented an actual loss of social capital, the program’s network effects may have been a vehicle for promoting inequality. The benefits to the highest-potential employees may have come at the expense of those who were slightly less well regarded. In considering the effects of network intervention on the workplace as a whole, these findings underscore the need to consider not only the employee populations directly affected by the introduction of a new program or policy but also the consequences for those who might be indirectly affected.

Conclusion

This study demonstrates the value of longitudinal field experiments in uncovering the causal effects of workplace practices such as formal mentoring on workplace networks. Such an approach promises to help network research in making the shift from simply characterizing internal network patterns and associated outcomes to producing tangible prescriptions about organizational practices that can reshape workplace networks in ways that support individual attainment and ameliorate inequality.

About the Author

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Tables

Table 1: Qualitative Evidence—Representative Quotations

Mechanism	Representative Examples	Mentions
Access to Influential Organizational Actors	<p>“Before the experience, I knew only a few people in [the mentor’s] group. Afterward, I built very good relationships with his entire team – not only here but also in the US.” – Male, Protégé</p> <p>“I would say that I added half a dozen people to my network, mostly direct reports of [my mentor] or people two levels down. Now I know their mission and what kind of resources they have. That has given me ideas about resource borrowing and rotation between my team and his.” – Male, Protégé</p> <p>“One reason the shadowing program works is that it gives you visibility in another part of the organization.” – Female, Protégé</p> <p>“As a shadow, I was able to attend meetings of the [lab head’s] direct reports. Before the meeting and during breaks, I got to know those people through informal chats.” – Female, Protégé</p>	32
Participation in Semiformal Foci (e.g., work groups and project teams)	<p>“Because of the shadowing experience, [shadows] extend their networks further – not only to me but to all of my subordinates. If shadows are involved in project work during the assignment, they will often call on my organization in the future when the need arises.” – Male, Mentor</p> <p>“During the shadowing program, [my mentor] had me participate in many projects so I could learn about what other people in his group were working on. That helped me expand my social network.” – Male, Protégé</p> <p>“[My mentor] gave me an assignment - to coordinate between our lab and the US organization to put together an event for a delegation coming to China. I identified the key players on both sides and worked with them to pull it together.” – Female, Protégé</p> <p>“[One of my prior shadows] helped me prepare speeches and presentations during her assignment. As part of that work, she had direct working interaction with my direct reports and other technical people in my group.” – Female, Mentor</p>	16

Table 1: Qualitative Evidence—Representative Quotations (continued)

Mechanism	Representative Examples	Mentions
Enhanced Social Skills	<p>“It will be more comfortable for me now to call [my mentor] and people in [my mentor’s department]. If I have a request, I have a better chance of getting help from them now.” – Male, Protégé</p> <p>“In this culture, the hierarchy of the organization is significant. The shadowing program increases their comfort level with senior people It brings upper levels within the reach of shadows. If you put people in a position where it is okay to ask questions, it changes things.” – Male, Mentor</p> <p>“Before I understood the value of networking theoretically, but the shadowing experience gave me a chance to practice it. I became more confident. It proved to me that I can be helpful to others. Now I feel I can reach out to people even if I don’t know them.” – Female, Protégé</p> <p>“The shadowing program helped me get to know people from the other group so I can ask for help when I need it...It makes collaboration easier.” – Female, Protégé</p>	11
Legitimacy-Enhancing Signals	<p>“Being a shadow says that your manager cares about your career. He wants to increase your exposure. It’s a good sign. You’re considered a high potential person – a technical resource for the future. It makes you desirable for others to get to know.” – Male, Protégé</p> <p>“At every meeting, [my mentor] would introduce me and tell people I was his shadow. I think the introduction helped send a signal about me. When I followed up with people, I got responses very quickly because it was known I was working with [the mentor]. Even when he introduced me by email, they’d respond quickly.” – Male, Protégé</p> <p>“I felt I got some extra respect from being a shadow. It meant that the company recognized me and wanted to develop me.” – Female, Protégé</p> <p>“The shadowing experience boosted my reputation with [my mentor’s] direct reports and his broader network.” – Female, Protégé</p>	32

Table 2: Characteristics of Treatment Group and Matched Control Group—Comparison of Means

Variable	Female Employees			Male Employees			All Employees		
	Matched Control Group	Treatment Group	t-statistic / p value	Matched Control Group	Treatment Group	t-statistic / p value	Matched Control Group	Treatment Group	t-statistic / p value
Age (Years)	38.6	37.9	0.311 / 0.758	35.7	35.1	0.786 / 0.434	36.1	35.7	0.512 / 0.610
Tenure within Firm (Years)	12.5	12.5	0.022 / 0.983	9.85	9.83	0.014 / 0.989	10.3	10.6	-0.274 / 0.785
Proportion Holding Advanced Degree	0.727	0.700	0.155 / 0.878	0.793	0.764	0.357 / 0.722	0.781	0.747	0.475 / 0.636
Number of Prior Promotions	1.91	1.50	1.024 / 0.315	1.34	1.27	0.417 / 0.678	1.44	1.33	0.717 / 0.475
Contacts Reported in Pre-Program Survey	8.18	7.10	0.476 / 0.638	10.94	8.78	1.500 / 0.137	10.47	8.33	1.741 / 0.084
Proportion Female	--	--	--	--	--	--	0.172	0.267	-1.337 / 0.183

N = 64 for matched control group, of which 53 were men. N = 75 for the treatment group, of which 55 were men.

Table 3: Characteristics of Cycle 1 and Cycle 2 Treatment Groups—Comparison of Means

Variable	Cycle 1	Cycle 2	t-statistic / p value
Age (Years)	35.7	35.7	-0.017 / 0.962
Tenure (Years)	10.6	10.5	-0.077 / 0.939
Proportion Holding Advanced Degree	0.727	0.774	0.454 / 0.651
Proportion Female	0.204	0.354	1.450 / 0.151
Number of Prior Promotions	1.21	1.50	1.313 / 0.194
Number of Contacts Reported in Initial (Pre-Program) Survey – All	8.07	8.71	0.429 / 0.669
Number of Contacts Reported in Initial (Pre-Program) Survey – Females	6.44	7.64	0.407 / 0.689
Number of Contacts Reported in Initial (Pre-Program) Survey – Males	8.49	9.30	0.455 / 0.651

N = 44 for Cycle 1; 31 for Cycle 2

**Table 4: Differences-in-Differences Estimates of Contacts Mobilized
Poisson Quasi-Maximum Likelihood (PQML) Regression Coefficients**

	Model 1	Model 2	Model 3	Model 4	Model 5
Age	-0.005 (0.038)	-0.004 (0.039)	-0.005 (0.039)		
Tenure	0.017 (0.035)	0.017 (0.035)	0.017 (0.035)		
Female	0.127 (0.187)	0.118 (0.192)	0.238 (0.398)		
Advanced Degree (1=yes; 0=no)	0.083 (0.181)	0.085 (0.181)	0.082 (0.182)		
Number of Prior Promotions	0.041 (0.079)	0.042 (0.080)	0.047 (0.083)		
Post-Program		-0.317*** (0.082)	-0.269** (0.086)	-0.308** (0.116)	-0.260* (0.124)
Treatment		-0.149 (0.160)	-0.060 (0.174)		
Post-Program × Treatment		0.384*** (0.106)	0.172 (0.119)	0.406** (0.154)	0.199 (.0172)
Post-Program × Female			-0.497 [†] (0.284)		-0.506** (0.183)
Treatment × Female			-0.385 (0.469)		
Post-Program × Treatment × Female			1.027** (0.321)		1.018*** (0.269)
Constant	2.127* (1.011)	2.217* (1.048)	2.228* (1.060)		
Employee Fixed Effects	No	No	No	Yes	Yes
Log Likelihood	-486	-477	-469	-199	-191
Chi2	2.45	18.40	34.08	8.02	45.41
Prob > Chi2	0.785	0.018	0.000	0.018	0.000
N	158	158	158	158	158

[†] p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed tests). Robust standard errors. Models 4 and 5 include employee fixed effects, which subsume the (time-invariant) main effects of *Female*, *Treatment*, and *Female x Treatment*.

Table 5: Descriptive Statistics: Workplace Networks of Treatment and Matched Control Groups—Comparison of Means

	Treatment Group			Matched Control Group		
	All Respondents	Female Respondents	Male Respondents	All Respondents	Female Respondents	Male Respondents
Pre-Program Survey	8.33	7.10	8.78	10.47	8.18	10.94
Post-Program Survey	10.27	13.54	9.16	7.67	6.67	7.73
Difference: Post-Program Minus Pre-Program	1.94 (1.239)	6.44* (2.625)	0.376 (1.375)	-2.80* (1.405)	-1.52 (3.226)	-3.21* (1.547)

* p<0.05, ** p<0.01, *** p<0.001; Standard error in parentheses.

Table 6: Second Identification Strategy—Mean Number of Contacts Mobilized by Cycle 1 and Cycle 2 Treatment Groups

	All Respondents	Female Respondents	Male Respondents
Cycle 1, Pre-Treatment	8.07	6.44	8.49
Cycle 2, Post-Treatment	11.81	17.00	10.19
Difference: Cycle 2, Post-Treatment Minus Cycle 1, Pre-Treatment	3.74* (1.65)	10.55** (3.29)	1.70 (1.83)

* p<0.05, ** p<0.01, *** p<0.001; Standard error in parentheses. N=65, of which 51 are male and 14 are female.

Appendix: Interview Schedule

1. Could you please give a brief summary of your career history?
2. Why did you choose to participate in the shadowing program? What were you hoping to get out of the experience?
3. How was your participation in the program viewed by others?
4. Who were you matched to in the program? How was the match made? How much influence did you and the other person have in the decision?
5. Which unit [within the software lab] were you in at the time? Which unit was [your mentor / protégé] in?
6. Did you or your [mentor / protégé] have any specific objectives for the shadowing experience? If so, what were they?
7. How did you and [your mentor / protégé] first make contact with one another? What did you discuss? How did your interactions change over time?
8. How many hours per week did [your mentor / protégé] and you spend together? How did this vary over the course of the program? What was a typical day like?
9. Did participating in the program affect the size or composition of your network in the organization? If so, how do you think these changes arose? Could you provide some examples / illustrations? If there was no significant change, why do you think that was the case?
10. [Did your mentor introduce to any of his / her contacts? / Did you introduce your protégé to any of your contacts?] If so, to whom? Were they internal or external contacts? If internal, which unit did they work in? What was the context in which this introduction took place? Did any of these introductions lead to the formation of new relationships? If so, how? If not, why do you think that was the case?
11. Did [your protégé / you] form any relationships as an indirect result of the program? If so, could you provide some examples? How did these relationships come about?
12. Do you believe [your protégé / you] changed personally or professionally as a result of the experience? If so, how?
13. How well do you think the shadowing experience met your objectives? Your [mentor's / protégé's] objectives? The organization's objectives?
14. How did the shadowing experience conclude?
15. What level of contact have you maintained with [your mentor / protégé] since the assignment ended? How would you characterize the relationship today?
16. As you reflect on the shadowing experience as a whole, what do you think were the most helpful aspects? The least helpful aspects? What, if anything, would you change about the experience?

Endnotes

¹ In supplemental analyses (not reported), I also compared the post-program networks of participants from the first cycle to the pre-program networks of participants from the second cycle. The former were somewhat larger than the latter, including for female participants; however, these differences were not statistically significant. This comparison was, however, more susceptible to the threat of cross-contamination than the one reported above.

² I conducted a supplemental analysis to assess how the program's effects might have varied across different kinds of ties. Results (not reported) indicate that the program led to an expansion in task advice and mentorship ties, especially for female participants, but did not affect strategic intelligence or friendship ties.