THE CAUSAL STATUS OF SOCIAL CAPITAL IN LABOR MARKETS

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ABSTRACT

Recent labor market research has called into question whether social capital effects are causal, or are spuriously due to the influence of social homophily. This essay adopts the demand-side perspective of organizations to examine the causal status of social capital. In contrast with supply-side approaches, we argue that homophily is a key mechanism by which organizations derive social capital. We develop an approach to bolster inferences about the causal status of social capital, and illustrate these ideas using data from a retail bank.

Keywords: Labor markets; social networks; hiring; employee referrals; homophily

There is a rich tradition of research on social capital and the labor market. Evidence suggests that the phenomenon of obtaining jobs via social contacts is widespread (e.g., Granovetter, 1995; Loury, 2006). More than being simply descriptive, much of this research emphasizes the ways in which personal contacts improve labor market success for job seekers (e.g., Lin, 2001). Other research, however, has found the effect of informal
networks on labor market outcomes to be negative (e.g., Bridges & Villemez, 1986; Falcon, 1995). Most of the focus of this research has been on the supply side of the labor market, and the data typically used to study the impact of personal networks on job search have been surveys of individuals and their job seeking behavior (e.g., the Panel Study of Income Dynamics, Corcoran, Datcher, & Duncan, 1980), National Longitudinal Survey of Youth (Mouw, 2003), Multi-City Study of Urban Inequality (Elliott, 2001; Stainback, 2008).

While the idea that social networks influence labor market outcomes has intuitive appeal, the causal status of network effects in extant studies has been called into question. Recent research by Mouw (2003, 2006) has questioned whether the observed effects of social capital in extant studies are causal in nature, or are spuriously due to the influence of social homophily between job seekers and individuals in their networks. Mouw (2006) points out that these studies typically have no way to control for “the central fact that individuals choose who they want to be friends with and what groups they want to join” (p. 80). Given this fact, he worries that “much of the estimated effect of social capital simply reflects selection effects based on the myriad non-random ways in which people become friends” (p. 80). The paper then goes on to review the various approaches to addressing the problematic causal status of the evidence, and discusses ways in which progress has been made in dealing with nonrandom selection due to homophily (Mouw, 2006, pp. 84–99; especially Table 2 on pp. 86–87).

Mouw’s (2006) critique and review addresses the causal status of social capital from the perspective of the supply side of the labor market, that is, it adopts the job seekers’ perspective. What has been missing in this discussion is the demand-side perspective of organizations that hire job-seekers. While Mouw (2003) acknowledges that other studies that adopt the hiring firm’s perspective have offered evidence of social contacts mattering in the hiring process (e.g., he cites Fernandez & Weinberg, 1997; Fernandez, Castilla, & Moore, 2000; Petersen, Saporta, & Seidel, 2000), he sets these studies aside when discussing the problem of causal inference in Mouw (2006).

In this paper, we tackle the problem of the causal status of social capital from the demand side of the labor market. In this respect, we follow the suggestion of Marsden and Campbell (1990) by developing the implications of studying the organizational side of the job search process. In contrast to the supply-side approaches reviewed in Mouw (2006), in the demand-side perspective we discuss here, we ask what value social networks provide to the hiring organization, and whether observed effects in studies showing that firms derive such value are causal. Although identifying whether
the network effects on the firm side are causal is not straightforward, the challenges are not the same as those for supply-side studies. Specifically, from the demand-side perspective discussed here, homophily is not a feature which confounds causal inference, but instead is likely a key mechanism by which organizations derives value from social capital.

A little thought experiment illustrates why homophily between an applicant and her referrer—a person already working for the employer and providing the referral—may be specifically desired by the employer. The referrer’s current employment suggests that the referrer fits the employer’s criteria on dimensions important for productivity. Homophily means the applicant is similar to the referrer on some dimensions and thus increases the likelihood that the applicant will also fit the employer’s criteria. If we imagine a systematic absence of homophily—that is, that the applicant and the referrer became friends randomly, as Mouw (2006) implies would be ideal for making causal claims about referral effects—the employer would not be able to use the referral as a proxy for homophily and as a signal of the applicant’s good fit on the relevant criteria. The practice of encouraging and paying for referrals would then be of dubious benefit to the employers and we should not observe any preferential treatment of referred applicants—an outcome that goes counter to the existing evidence on the demand side of the labor market (see below).

NETWORKS FROM THE DEMAND SIDE OF THE LABOR MARKET

Early labor market studies of hiring organizations documented that many employers engaged in hiring workers through informal contacts (Myers & Shultz, 1951; Rees, 1966; Rees & Shultz, 1970; Sheppard & Belitsky, 1966). More recent evidence that the practice is widespread is available as well. For example, in Bewley’s (1999, pp. 295–298) convenience sample of employers, 60 percent of the businesses mentioned that hiring through personal connections was important for them. More systematic surveys of organizations also show that employers commonly use referrals from employees as a recruitment method. For example, using the National Organizations Study, Kalleberg, Knoke, Marsden, and Spaeth (1996, p. 138) report 36.7 percent of employers in their study use referrals in recruitment. Furthermore, many firms pay their current employees for employee referrals (Halerow, 1988; LoPresto, 1986). More recently, new forms of intermediary firms have
appeared seeking to help hiring firms improve the efficiency of their network hiring.¹

In addition to documenting the prevalence of the phenomenon, some of these studies also offer insight into why employers use informal networks to recruit new hires. Both Rees and Shultz (1970) and Bewley (1999) discuss employers’ self-reports about using networks in hiring. In this respect, the older evidence from Rees and Shultz’s (1970) Chicago area labor market study and the newer evidence from Bewley’s (1999) largely Connecticut area sample coincide. In both studies, many employers reported numerous benefits to network-based recruitment.² For example, both argue that worker referrals have the advantage that they are relatively cheap and pre-screened on characteristics that are difficult to discern from application materials. As one of Bewley’s (1999, p. 297) employers put it: “We generally hire through personal connections. We prefer this, for we take fewer chances that way. This eliminates the possibility of hiring a crook or a drug addict.” In addition, referrals probably bring in relatively high quality candidates since the referring employee is likely to feel that their reputation is on the line. Another of Bewley’s employers said: “Those who bring people in know that their own reputation is at stake. The success or failure of the referral will be remembered” (Bewley, 1999, p. 297; see also, Saloner, 1985; Sheppard & Belitsky, 1966, pp. 187–188; Smith, 2005, 2007).³

In addition to the labor market studies, there have been a series of case studies investigating whether employee referrals are more likely to be hired than individuals applying via other recruitment channels (Fernandez & Abraham, 2010, 2011; Fernandez & Campero, 2012; Fernandez et al., 2000; Fernandez & Fernandez-Mateo, 2006; Fernandez & Greenberg, 2013; Fernandez & Mors, 2008; Fernandez & Sosa, 2005; Fernandez & Weinberg, 1997; Kirnan, Farley, & Geisinger, 1989; Petersen et al., 2000). With the exception of the Fernandez and Fernandez-Mateo (2006) paper, employers in all of these case studies showed a preference for referrals during screening, even after controls were applied.⁴

CAUSAL OR SPURIOUS?

While all of these studies suggest that firms invest substantial time, energy and resources in recruiting candidates via social networks, this evidence can only be considered circumstantial for the purposes of identifying causal effects. And while some costs of referral recruitment have been mentioned,
the vast majority of studies report that firms reap a number of benefits from relying on hiring through employee referral networks. The question remains whether these efforts are causal in the sense that firms actually behave differently toward employee referral applicants, or whether their behavior toward referrals is the result of spuriously associated other factors. The key to answering this question comes down to the counterfactual for firms recruiting workers via networks. Simply put, for the causal effect to be identified, the analysis needs to address what the hiring outcome would have been if not for the firm’s use of the network in recruiting. As noted above, since employers are apparently aware of both the potential benefits and costs (see note 3) of recruiting through networks, this question is not simple to answer. Adding to the complexity here is the fact that networks may affect firms’ hiring outcomes through a number of mechanisms. Fernandez et al. (2000) identify five such mechanisms, labeled M1–M5. While some of these advantages are likely to manifest themselves only after the person has been hired (e.g., M5), others may produce value for the firm by affecting the efficiency with which people are screened. The first (M1) refers to the expansion of the pool of applicants. In this case, the firm potentially benefits from gaining access to pools of candidates that otherwise might not be available to the firm. The second mechanism — M2 — suggests that firms can benefit from homophily between the referrer and the candidate since referrers have already made it past the company’s screening criteria. Third mechanism (M3) traces benefits to the firm of the tendency of the referrer to protect their reputation. M4 focuses on the potential benefit to the firm of gaining extra information about the candidate from the referrer. M5 identifies a social enrichment mechanism whereby referrers serve to mentor and introduce the referral to the new setting, thereby potentially reducing turnover. There are probably others as well. However, as a first step toward developing a framework for determining whether firms derive benefits from network hiring, we simplify the question by focusing on the net effects of using networks to hire, that is, summing the costs and benefits, across the various mechanisms. While we adopt this reduced form approach here, in future research, we will explore ways to unpack the mechanisms, and develop counterfactuals for the isolated mechanisms.

The major challenge to extant demand-side evidence on firms’ hiring of networked candidates is the problem of controlling for the influence of unobserved variables. Although a number of the studies cited above include a rich set of controls when assessing whether those applying via referrals are advantaged in the hiring process, even the most detailed of these studies
cannot fully eliminate the influence of unobserved factors that might account for the observed association between networks and hiring success. Of particular concern are characteristics of the candidate that are observable to the firm’s screeners, but not observable to the analyst. Since these factors may well be correlated with whether or not the person is a network candidate, analyses such as these run the risk of attributing candidates’ relative success to their being network candidates rather than to those unobserved factors. Of course, some of employers’ behavior—for example, the employers’ belief that homophily delivers good candidates—can operate through unobservables. However, the rub is that if we find good outcomes for network candidates, we cannot be sure whether screeners are reacting to the person being a networked candidate per se, or whether the screeners’ behavior is due to the influence of other uncontrolled, unobserved factors.

This is precisely the problem that demand-side studies that try to infer whether discrimination is occurring face (National Research Council, 2004, especially Chapter 7). Although not perfect (Pager, 2007), the best solution in that case is to adopt an audit approach, where fictitious identical candidates are presented to employers, while experimentally manipulating key features of those candidates that are hypothesized to produce discriminatory behavior on the part of employers (e.g., Bertrand & Mullainathan, 2004; Pager, 2003). In the case of network contacts, this solution is not practical, however. In live hiring situations, employers will reasonably want to know who is providing the network tie, and experimental manipulation of such information is likely beyond acceptable ethical limits on research. Unless they were voluntarily adopted, “blind” hiring procedures too would likely be unacceptable (Goldin & Rouse, 2000).

In this paper, we report some results based on an approach to identifying causality on the demand side of the labor market using data on live hiring. The key idea is to develop the counterfactual for how networked candidates would be treated if they were not networked by focusing on naturally occurring “experiments.” Specifically, the goal is to compare the employer’s reaction when a person is being considered as a candidate without a network tie, to the employer’s reaction when the same person is applying via a network tie (also see Fernandez, 2010; Yakubovich, 2005). While this approach does not eliminate the influence of all possible confounds—it cannot control for the influence of any factors that change between each application—it does control for the influence of all unobserved factors that do not change across applications from the same person. While not a substitute for a true random assignment study, in light
of the infeasibility of conducting such research on live hiring, this approach provides a path to make progress on addressing the thorny issue of causal effects of networks seen from the demand side of the labor market.

AN ILLUSTRATION

The data we analyze here are taken from the western U.S. region of a large retail bank. The bank is part of a globally diversified financial services institution. As of December, 1995, the western region bank employed a total of 3,641 workers. Using the company’s internal records, we analyzed data on the hiring process for the western region bank’s job openings during the 27-month period from January, 1993 through March, 1995. Most important for the purposes of this research, the firm’s standard operating procedure is to log applications for job openings and to record the candidate’s recruitment source, specifically recording whether the person was referred by a company employee. For more details about the hiring process at this firm, see Fernandez and Abraham (2010).

We identified a total of 31,656 applications to the firm over this period. Virtually complete information is available to distinguish internal and external applicants (31,627 or 99.9 percent of the 31,656 applications), and whether the applicant was referred to the firm by an employee (99.5 percent). After eliminating the applications which were from internal candidates, and applications for which we could not identify the recruitment source or the hiring outcome, we are left with a working N of 29,225 external applications. These applications were produced by 25,139 people, however. Three-quarters (22,069 or 75.5 percent) of these applications were from people who applied only once. The balance of the applications were from people who applied between 2 (15.9 percent) and 15 times (.01 percent) over the course of the study. These repeat applications provide opportunities to observe differences in screening outcomes for the same person when they switch between applying as an employee referral, and as a non-referral.

Table 1 shows applicants’ rates of success in progressing through the firm’s hiring pipeline by recruitment source and whether or not the candidate was a repeat applicant. Considering first the non-repeat applications, employee referrals are much more likely to progress through the three stages of the hiring pipeline than are non-referrals. Specifically, referrals are 5 times more likely to be interviewed by the human resources managers
(interview rates of 54.1 vs. 10.1 percent), 5.7 times more likely to be inter-
viewed by hiring managers (42.9 vs. 7.5 percent), and over 10 times more
likely to be offered a job (24.2 vs. 2.1 percent) than non-referrals. In light of
the studies referenced above, this pattern is not surprising. In this case, the
referral effects are being estimated across different individuals, and thus
reflect between-person variation in whether or not the application is from a
referral.

Consequently, this analysis cannot address the question of whether or
not the observed preference for referrals by the firm’s hiring agents is mask-
ing the effect of other spuriously associated factors.

We next turn to repeat applications. Similar to the top panel of Table 1,
we find a strong preference for referrals in the hiring process. Among
applications from people who applied multiple times, referral applications
are 4.9 times more likely to result in interviews by the human resources
managers (interview rates of 58.4 vs. 11.8), 5.8 times more likely to be
interviewed by hiring managers (49.4 vs. 8.5 percent), and over 10 times
more likely to be offered a job (20.7 vs. 2.0 percent) than non-referral appli-
cations. Here, too, these referral advantages might reflect the spurious
influence of unobserved factors. However, the fact that the pattern of
results for repeat applications is similar to that of non-repeats suggests that
selection bias due to repeat applications being those who have not accepted
a job on their first application is not a serious concern.
Continuing the analyses, we focus on a subset of these repeat applications, specifically, time-adjacent pairings of repeat applications from the same person. Table 2 shows the results of these pairings. The vast majority of these pairings reflect candidates where their initial and subsequent applications are both non-referral applications. The first row of Table 2 shows the hiring outcomes for the second application of the pair, while the second row shows the hiring outcomes for the initial application. For all three outcomes, rates of success are higher for the second application than for the initial application. Although exhibiting much higher success rates than the non-referrals, the results for the modest number of referral × referral pairings of applications also show the same time pattern (cf. rows of the second

<table>
<thead>
<tr>
<th>Repeat Applications</th>
<th>Human Resources Manager Interview Rate</th>
<th>Hiring Manager Interview Rate</th>
<th>Job Offer Rate</th>
<th>Total Number of Applications</th>
</tr>
</thead>
<tbody>
<tr>
<td><em>Non-referral → Non-referral</em></td>
<td>12.3</td>
<td>8.6</td>
<td>2.5</td>
<td>3,781</td>
</tr>
<tr>
<td>After = Non-referral</td>
<td>9.5</td>
<td>6.9</td>
<td>1.0</td>
<td>3,781</td>
</tr>
<tr>
<td>Paired t tests of significance of contrast (with 3,780 df)</td>
<td>(p &lt; .0001)</td>
<td>(p &lt; .0008)</td>
<td>(p &lt; .0001)</td>
<td></td>
</tr>
<tr>
<td><em>Referral → Referral</em></td>
<td>70.4</td>
<td>64.2</td>
<td>25.9</td>
<td>81</td>
</tr>
<tr>
<td>After = Referral</td>
<td>55.5</td>
<td>45.7</td>
<td>14.8</td>
<td>81</td>
</tr>
<tr>
<td>Paired t tests of significance of contrast (with 80 df)</td>
<td>(p &lt; .0067)</td>
<td>(p &lt; .003)</td>
<td>(p &lt; .019)</td>
<td></td>
</tr>
<tr>
<td><em>Non-referral → Referral</em></td>
<td>56.8</td>
<td>46.2</td>
<td>19.7</td>
<td>132</td>
</tr>
<tr>
<td>After = Referral</td>
<td>23.4</td>
<td>18.2</td>
<td>3.8</td>
<td>132</td>
</tr>
<tr>
<td>Paired t tests of significance of contrast (with 131 df)</td>
<td>(p &lt; .0001)</td>
<td>(p &lt; .0001)</td>
<td>(p &lt; .0001)</td>
<td></td>
</tr>
<tr>
<td><em>Referral → Non-referral</em></td>
<td>31.9</td>
<td>22.0</td>
<td>7.7</td>
<td>91</td>
</tr>
<tr>
<td>After = Non-referral</td>
<td>47.3</td>
<td>39.6</td>
<td>9.9</td>
<td>91</td>
</tr>
<tr>
<td>Paired t tests of significance of contrast (with 90 df)</td>
<td>(p &lt; .013)</td>
<td>(p &lt; .005)</td>
<td>(p = .ns)</td>
<td></td>
</tr>
</tbody>
</table>
panel of Table 2). This suggests that candidates are learning about the hiring process across their multiple applications. While this learning might indeed be going on for this subset of candidates, it is important to note that with the exception of the success rates for the second application from repeat referrals, the success rates for these groups are not appreciably higher than the success rate for single applications for both single non-referral and single referral applications (Table 1).

The next two panels of Table 2 report the key results aimed at identifying causal effects. We formed pairs of applications into natural experiments where we can compare hiring agents’ reactions when a person is being considered as a candidate without an employee referral, to the employer’s reaction when the same person is applying via an employee referral. If referral effects are causal, then for non-referral → referral transitions, we would expect the success rates for the second application to be higher than the success rates for the first application. Moreover, for referral → non-referral transitions, we would expect that success rates for the second application to be lower than the success rates for the first application.10

There were a total of 132 pairs of applications where the person applied as a non-referral in the first application, but as a referral on the subsequent application. The third panel of Table 2 compares the rates of HR and HM interviews and job offer for the second — that is, referral application — of these pairs (first row in the panel), with the rates of HR and HM interviews and job offer for the first — that is, non-referral application — of the pair (second row of the panel). As such, the outcomes for the first application form the counterfactual baseline of what the outcome that individual would achieve in the absence of the referral tie. Comparing these two rows, we find that applications are considerably more successful when the person applied as an employee referral, compared to the prior application where the candidate was a non-referral. For example, the job offer rate is 5.2 times (19.7 vs. 3.8 percent) higher for the second (referral) application of the pair than the offer rate for the first (non-referral) application. Also, both interview outcomes are better for the referral application than for the non-referral application (for referrals and non-referrals, respectively, HR interview rate: 56.8 vs. 23.4; Hiring manager interview rate: 46.2 vs. 18.2). All of these contrasts are statistically reliable. For all three outcomes, compared to the counterfactual of not having a network tie, the rates of success significantly improve when the person applies with the benefit of a network tie.

The last panel of Table 2 further explores the causal relationship between hiring success and having a network tie. If the relationship between success and network tie is causal, then removing the network tie
should depress rates of success. The data are consistent with this interpretation: for the same person, applying first as a referral has a higher success rate than the applying second as a non-referral. In this case, the contrasts are statistically reliable for the two interview outcomes, but not for the job offer outcome. With only this latter exception, these analyses offer strong support that being referred improves your chances of being hired. The fact that the referral effect is evident across applications from the same person serves to bolster confidence that the organizational screeners are indeed showing a preference for referrals. Taken together, these results support the idea that network effects are causal: the firm’s hiring agents change their behavior when candidates apply via an employee referral tie compared to when the same person does not use a network tie.

**CONCLUSION**

This paper makes a simple point: when thinking about the causal status of social capital in labor market contexts, it is important to remember that labor markets have two sides. As we have argued before, employee referral networks can be used to create value for the firm (Fernandez et al., 2000), but also for job seekers (Fernandez & Castilla, 2001). While we have offered a “reduced form” example of how causal inferences can be bolstered when looking at the demand side of the market, this is not to minimize the complexity of identifying the other links in the causal chain. Network accounts consist of multiple moving parts (cf. Fernandez & Fernandez-Mateo, 2006), involving actions on the part of employees (to refer or not), referrals (to apply or not), and screeners (to prefer them or not). Seen from the perspective of the hiring firm, for networks to pay off requires that these processes align.

While not the ideal experiment, these results offer strong support for the ideas that employers change their behavior when dealing with people who have been referred to the firm. A specific weakness of the approach taken here is that we cannot eliminate the influence of factors that change between each application. While we have shown that in aggregate, candidates are most successful in being matched to a job opening when they are referred than when they are not, we cannot claim that this effect is observed for the same job opening. Part of the way in which the referral effect might work is that the firm’s hiring agents — human resources screeners and hiring managers — might be willing to reconsider applications from
referrals for other job openings at higher rates than they do non-referrals. We attempted to see whether such reconsiderations are happening within-HR screener or within-hiring manager, but the numbers of such occasions were too small to analyze with the paired counterfactual approach we developed here. The fact that reconsiderations are so rare suggests that a good deal of the observed referral advantage for the repeat applicants is likely to be working through improving the ways in which such candidates navigate through the maze of job openings. To put it in causal terms, if not for the firm’s use of network hiring, fewer of these multiple applications would result in successful matches. Of course, other mechanisms producing a referral advantage may also be at work. Although we have set them aside for these tests of causality, the fact that there is a huge referral advantage observed among non-repeat applicants could reflect other processes favoring referrals.

The “reduced-form” – net effect approach to the problem of identifying causal effects on the demand side of the labor market developed here leaves open several opportunities for isolating at least some of the network mechanisms likely to be at work at the hiring interface. As mentioned above, while audits or blinding procedures are not feasible for conducting fully randomized experiments in live hiring situations, experimental manipulations can be made to work to study parts of the referral hiring process. One path for doing this is to design experimental studies of hypothetical hiring situations. Indeed, the first author is currently pursuing this approach measuring how much deference experienced hiring managers give to different types of employee referrals where the relationship between the referrer and the person referred is experimentally manipulated. These vignettes with experienced hiring managers are also being used to test the “courteous extra look” hypothesis mentioned above. In a parallel set of experiments, the first author is also using a random-assignment to vignettes approach to assess the impact of firms’ recruitment bonus on potential referrers’ tendency to protect their reputation (identified as M3 in Fernandez et al., 2000).

Most important for this research, the specific role of homophily in providing value to the firm is important to address in future research. Some progress has already been made along these lines. Fernandez et al. (2000, pp. 1309–1314) show the results of random permutation experiments testing whether observed levels of homophily between referrers and referrals (M2) is occurring at above chance levels. With sufficient data, random permutation methods can also be used to good effect in other empirical settings. Recent research by Beaman and Magruder (2012) presents the results of experiments where subjects are allowed to refer actual network members
for casual jobs. Since the homophily mechanism depends on the behavior of potential referrers, such an approach can be adapted to isolate the causal effect of homophily as it manifests itself across different experimental conditions.

Although we have not offered direct evidence of the influence of homophily in the hiring analyses presented here, the qualitative evidence reviewed above shows that employer’s view homophily as one of the mechanisms they are trying to exploit to harvest value from their employees’ networks. Thus, we agree with Mouw (2006) that separating the influence of social homophily is important to address if network researchers studying the supply side of the labor market are going to make causal claims about these processes. A lesson of this paper, however, is that when seen from the other side of the labor market, homophily is not an obstacle to causal inference about social capital. While causal effects may still be difficult to isolate, from the demand-side perspective, homophily per se is not a problem confounding causal inferences. While not perfect, using the strategy developed here, researchers can successfully assess the causal status of demand-side social capital in their organizational settings.

NOTES

1. One example is Branchout.com. In addition to targeting Facebook users who might be seeking jobs, this company is marketing a “RecruiterConnect” feature to companies, which uses the firm’s employees’ Facebook connections to attract candidates. Monster.com offers firms a similar product (called “Employee Referral Application”) which distributes information about the company’s job openings using their employees’ Facebook and Twitter social networks in order to recruit potential hires.

2. See Fernandez et al. (2000) for a review of five mechanisms by which employers might enjoy benefits from recruitment via employee referrals.

3. Interestingly, in both studies, some employers also expressed worries about relying on network hiring. In the Chicago labor market study, some employers cite “problems with cliques” as a reason for avoiding referrals from current employees (Ullman, 1966). Several of Bewley’s (1999, p. 297) employers cited virtually identical concerns 30 years later. For example: “...We stay away from employee referrals. We would rather go through our procedures. This controls cronyism. Otherwise, the company becomes cliquey, and the men become hard to control.”

4. Although not the focus of this paper, a prominent theme in research on referral hiring is that relying on referrals reinforces workplace race and sex segregation because of a tendency for people to refer people like themselves (Doeringer & Piore, 1971; Marsden & Gorman, 2001; Moss & Tilly, 2001; Mouw, 2002; Reskin, McBrier, & Kmec, 1999; Tilly, 1999). More recent research by Rubineau and
Fernandez (2010) calls into question the idea that insularity is the necessary result of such practices. Contrary to past understandings, they show that unless network segregation of referral ties is perfect, recruitment via employee referrals integrates rather than segregates. However, the rate at which such integration occurs is slower or faster depending on how much the employer depends on referral hiring as a recruitment strategy. They further argue that by encouraging underrepresented groups to refer at higher rates than over-represented groups, recruitment via employee referrals can speed the rate of integration, and thus be a useful tool to diversify rather than segregate firms. We will discuss the implications of the tendency for referrals to be preferred in demand-side hiring for workplace segregation in the conclusion.

5. Note that this potential advantage can be undermined by turnover on the part of the referrer who can take their referrals with them when they leave. Indeed, Fernandez et al. (2000) shows evidence of precisely this pattern.

6. For example, in our field research with hiring managers, we have heard some employers express the sentiment that referrals from their employees are owed an “extra look” out of courtesy to the referrer.

7. Studying within-individual variation in hiring outcomes mirrors one of the approaches suggested by Mouw (2006) for dealing with the challenge of identifying causality from supply-side data (see also Mouw, 2002; Obukhova & Lan, 2013; Obukhova, 2012). These supply-side studies look at how different employers react to the same person when they apply via a network tie compared with other sources. In contrast, the focus here is on the hiring interface for a single employer.

8. The company’s HR department ran an employee referral program paying bonuses of between $250 and $2,000 depending on the level of the job opening. In order to receive the bonus, however, employees needed to contact the HR department and claim an applicant as their referral (see Neckerman & Fernandez, 2003).

9. Of course, applicants who were hired as a result of their first application to the company will not reapply and are excluded from the set of repeat applications. We will return to discuss some of the possible implications of this fact below.

10. In addition, observing a causally consistent pattern when changing the time order of the treatment (referral → non-referral vs. non-referral → referral) helps to rule out the idea that simple learning over time is accounting for the greater success of the second application. As mentioned above, some learning about the hiring process on the part of the candidate (e.g., which particular job opening might be more appropriate) might indeed be going on at the hiring interface. From the firm’s perspective, however, the fact that referrers might be speeding such learning is one of the reasons that employers might prefer referrals.

11. We can only speculate why this is the case. In light of the very high offer rates for the referred applicants (both repeat and non-repeat) in Table 1, referred candidates not getting an offer on their first application (see note 8) might be undesirable in other ways. This factor could then depress their offer rate, thus, introducing an undesirable confound into the causal test for this contrast. The issue flagged in note 9 (selection bias associated with looking only at repeat applicants) would not surface to the same degree for the non-referral → referral contrasts since non-referral applicants have very low offer rates in Table 1, and thus should produce much less selection bias when the first application is a non-referral.

13. Preliminary results from that experiment ($N = 134$) are consistent with our discussion of homophily as a key mechanism by which employers derive value from referrals. In particular, hiring managers were more likely to hire the applicant, if the applicant was described as a friend of the referrer, compared to the scenario where the applicant was a quasi-random acquaintance of the referrer (the difference was significant at $p < .034$, with one-tailed, unequal variance $t$-test $t = -1.843$, df = 132).

14. These subjects were also more likely to contact the referrer about the applicant if the referrer and the applicant were friends, supporting the “courteous extra look hypothesis” (the difference was significant at $p < .007$, with one-tailed, unequal variance $t$-test $t = -2.514$, df = 117.36).

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