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Abstract

We examine how changing the allocation of hiring decision rights in a multiunit organization affects employee-firm match quality, contingent on a unit's local circumstances. Our research site switched from its traditional decentralized hiring model (hiring by business unit managers—in our case, store managers of a US retail chain) to centralized hiring (in this study, by the head office). While centralized hiring can ensure that enough resources are invested in hiring people aligned with company values, it can also neglect the unit managers' local knowledge. Using difference-in-differences analyses, we find that the switch is associated with relatively *higher* employee departure rates (poorer matches) if the business unit manager has a local advantage; that is, if the store serves repeat customers, serves a demographically atypical market, or poses higher information-gathering costs for headquarters. In these cases, the unit manager may be more informed than headquarters about which candidates would best match local conditions.

Keywords: Control; selection; organizational design; decentralization; company values; retail chains.

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I. INTRODUCTION

Employee selection (or "hiring"—we use the terms interchangeably) has long been recognized as a key management control mechanism to align employees with a company's values and goals. For instance, Merchant (1985, pp. 48-49) suggests that once an organization has established the need to directly employ personnel, it should implement systems to hire, train, and socialize reliable employees before choosing other types of control. Selection is particularly relevant when alignment cannot be achieved through traditional management controls, such as monitoring or rewards (Ouchi 1979; Chatman 1989; Merchant and Van der Stede 2017). Manufacturers committed to organizational learning and innovation and firms emphasizing customer service often require behaviors and attitudes that are difficult to prescribe and monitor and difficult to infer from results. Yet, despite its importance as a control system, selection has received little attention in the empirical management accounting and control literature. Recent studies, however, examine its efficacy in achieving desirable employee behaviors and performance and whether and when firms use selection and incentive contracting as substitutive or complementary control mechanisms (Campbell 2012; Abernethy, Dekker, and Schulz 2015; Swaney 2017; Liu, Liu, and Chu 2019).

We examine how the *allocation of decision rights* in hiring impacts hiring efficacy in a context in which monitoring and rewards alone cannot fully achieve goal alignment.[1] We contribute to the literatures on employee selection and on the delegation of decision rights by identifying circumstances under which centralized (vis-à-vis decentralized) hiring is likely to be more or less beneficial. While centralized selection could provide a means to consistently align employees with company values, consistent with the importance of selection as a control mechanism, the literature on the delegation of decision rights also highlights the importance of decentralization if unit

[1] Allocation of hiring rights relates directly to the design of personnel control systems, which are intended to increase the likelihood of employees pursuing the organization's strategic goals (Merchant and Van der Stede 2017).

managers have superior information relative to headquarters (Brickley and Dark 1987; Campbell, Datar, and Sandino 2009). Unit managers may know better which types of hire would best deliver the company's goals and values locally. We examine circumstances affecting the relation between centralized (versus decentralized) hiring and the quality of new employee-firm matches (i.e., the extent to which new employees' preferences, attitudes, and abilities are naturally aligned with the company's goals and values).[2]

We use data from a US retail chain facing the control problem of motivating employees not only to perform their day-to-day tasks, but also to embrace company values such as teamwork, kindness, and friendly service. Bonuses to motivate store staff have proven insufficient and management strongly believes in the need to hire individuals with the "right personality."

We exploit variation in this company's allocation of hiring decision authority that arose as it switched, in a staggered manner, from a decentralized to a centralized model of hiring. This natural experimental setting provides a unique opportunity to study decision rights in the context of employee selection, since (a) it enables us to more convincingly draw causal inferences than would be the case in a cross-sectional regression study; (b) we need not rely on survey instruments to capture the extent of delegation (cf. Baiman, Larcker, and Rajan 1995; Nagar 2002; Abernethy, Bouwens, and van Lent 2004); (c) we can isolate the effects of centralization of hiring rights while holding other management control systems constant; and (d) it allows us to measure effects on the quality of new employee-firm matches (using employee departure rates).

Research on delegation of authority recognizes inherent tradeoffs in the decision to centralize or decentralize: centralized decisions tend to be better aligned with corporate goals but less well-

[2] Jovanovic (1979) describes a high-quality employee-firm match as one in which the employee is most productive. Chatman (1989, p. 335) argues that higher-quality matches (or person-organization fit) exist "when there is congruence between the norms and values of organizations and the values of persons."

informed, while the converse is true of decentralized decisions (Jensen and Meckling 1995, Dessein and Prat 2016). In the context of the allocation of hiring rights, we consider contingencies at the business-unit level that are likely to give headquarters a hiring advantage over the unit ("Headquarters Has a Hiring Advantage") as well as those likely to give the unit a local advantage ("Business Unit Manager Has a Local Advantage").

For "Headquarters Has a Hiring Advantage," we identify unit-level factors—lower ex-ante alignment with company values, busyness, and operational complexity—that theoretically give headquarters a hiring advantage over the unit manager, due to its greater ability and motivation to select employees matching corporate goals and values. We conjecture that managers of units displaying these factors will benefit the most from centralized hiring, since under a decentralized model they are less likely to base selection on company values or have time to fill vacant positions.

For "Business Unit Manager Has a Local Advantage," we consider three factors that should put the unit manager at an informational advantage (relative to headquarters) in identifying employees matching company needs locally; they are (a) higher information-gathering costs for headquarters, (b) serving a more demographically atypical market, and (c) serving repeat customers. Figure 1 summarizes the relations that we test.

[Insert Figure 1 here]

We conduct difference-in-differences analyses using a proportional hazards regression model to examine effects on the quality of new employee-firm matches (measured as rate of employee departures). Our analyses find centralization of hiring rights associated with relatively worse outcomes (higher relative rate of employee departures) when the store manager had an information advantage relative to headquarters. To understand the implications of this finding in terms of the economic magnitude, we examined the effects of the switch for stores with an information

advantage relative to headquarters, and for stores without this advantage. We considered a store to have an information advantage if it met at least two of the following conditions—high information-gathering costs, more divergent markets, and repeat customers. For stores with such an advantage, our estimates indicate that the likelihood of an employee remaining with the company past 180 days was essentially unchanged in the period following the switch from decentralized to centralized hiring relative to the pre-period, while over the same period, that likelihood increased by 133 percent (from 10 percent to 23 percent) for those stores that did not switch, indicating a negative effect of centralized hiring on employee-firm match quality. Conversely, for stores *without* an information advantage relative to the headquarters, the likelihood of an employee remaining with the company past 180 days was essentially unchanged in the period following the switch in hiring regime, while over the same period, that likelihood decreased by 23 percent (from 35 percent to 27 percent) for stores with the decentralized regime throughout, indicating a positive effect of centralized hiring on match quality.

Our results enrich the emerging empirical literature in management accounting and control on the relevance of employee selection as a control mechanism to improve alignment in circumstances where other control mechanisms may fail. We extend this literature by shedding light on the circumstances under which headquarters or local unit managers may be better placed to select new employees to improve alignment and commitment. These insights are likely to be relevant to practitioners; 52 percent of respondents to a 2010 survey of managers at leading retail, hospitality, and quick-service restaurant organizations included "brand fit" as one of the top three characteristics they seek in new hires (Aon Hewitt 2010). Fifty-seven percent managed store hiring at the store level, as did the retailer in our study before it began to change its hiring system.

We also contribute to the research—particularly on geographically dispersed organizations (Bradach 1997; Campbell et al. 2009)—examining tradeoffs between centralizing to ensure uniformity and decentralizing to promote entrepreneurial behaviors and apply local information to market adaptation. We extend this empirical literature by identifying circumstances in which switching from decentralized to centralized hiring may be less beneficial—circumstances where local information is important.

The paper proceeds as follows: Section II reviews the literature and develops hypotheses, Section III describes our research setting, Section IV presents our empirical analyses and results, and Section V concludes.

II. HYPOTHESIS DEVELOPMENT

Theoretical studies in accounting, management, and economics have long recognized that employee selection can be used as a management control to mitigate agency problems. Merchant and Van der Stede (2017, p. 8) describe management control systems as those that "managers use to ensure the behaviors and decisions of their employees are consistent with the organization's objectives and strategies." Building on Ouchi (1979), Merchant and Van der Stede explain that, to align employees with organizational goals, a firm should not only consider action controls—to guide and monitor employees to ensure they perform (do not perform) actions that benefit (harm) the organization—and results controls—to reward good results—but also personnel and cultural controls (such as selection systems) to attract and develop employees who are naturally aligned.

Selection systems can identify employees whose preferences, attitudes, and abilities are naturally aligned with the company's goals and values. Such employees are generally more satisfied and motivated, less prone to agency problems (less likely to deviate from company goals), and more productive than their peers (Jovanovic 1979; Chatman 1989; Dessein 2002; Van den

Steen 2010), so there is less need to monitor and reward them (Akerlof and Kranton 2005; Van den Steen 2010; Campbell 2012). Selection is especially relevant when standards, monitoring, and/or rewards alone cannot be used to achieve alignment successfully because management (a) cannot specify in advance the specific actions that will attain organizational goals in all circumstances, (b) has trouble monitoring or verifying standardized actions, (c) cannot measure or verify employee results, and/or (d) cannot reliably attribute organizational results to employee actions (Campbell 2012; Merchant and Van der Stede 2017, pp. 46, 86). Even if management could rely on monitoring or rewards to align individuals' interests with company goals, these systems could be prohibitively costly. Finding people who can be trusted to do the right thing could reduce the costs of prescribing actions and enforcing them, or of setting targets and measuring results.

Empirical research has examined the employee-firm matching process using economy-level datasets (e.g. Berman 1997; Nagypál 2007). However, there are few firm-level empirical studies of the effects of selection mechanisms as management controls to drive alignment (Van Iddekinge et al. 2009; Oyer and Schaefer 2011). Chatman (1991) finds a positive relation between a new hire's fit with organizational values and subsequent tenure; Liu, Liu, and Chu (2019) show that a fit-focused selection program yields longer tenures. Campbell (2012) finds that the use and outcomes of decision-making authority by employees selected through referrals are more reflective of organizational goals. Hoffman, Kahn, and Li (2018) find that managers who deviate from the recommendations of job-testing technologies often make poorer hiring decisions, as reflected in shorter tenure. Abernethy et al. (2015) find instances in which selection mechanisms are either more or less likely to be used as substitutes for incentive contracts.

We extend prior research by empirically examining the effects of a key design choice in a selection process—centralize or decentralize—on match quality. While the allocation of hiring

decision rights can fall along a continuum, we define it as a choice (easily generalized to most geographically dispersed organizations) between centralizing to headquarters and decentralizing to unit managers. We expect this choice to affect match quality: if the quality is poor, we expect new hires to be fired or leave sooner than if it is high (Oyer and Schaefer 2011).

Theoretical studies suggest that the choice to centralize or decentralize decision rights involves trade-offs, such as informed decisions based on local knowledge (more likely with decentralization) versus decisions fully aligned with long-term goals (more likely with centralization) (Jensen and Meckling 1995; Dessein and Prat 2015). Consistent with these tradeoffs, research using cross-sectional data shows different conditions affecting that choice. Abernethy et al. (2004) find firms are more likely to decentralize authority to business units when (a) the cost of transferring knowledge from the units to the head office is higher and (b) the need for alignment with shared goals is less relevant (due to lower unit interdependencies). Baiman, Larcker, and Rajan (1995) and Nagar (2002) find companies are more likely to allocate decision rights to business units when the units have better information than the head office. Specifically, Baiman et al. (1995) find CEOs are more likely to delegate decision rights (that is, to grant direct control over core functions) to units that operate in a different two-digit SIC code than the corporation. Nagar (2002) shows greater decentralization of decision rights in higher-growth, more volatile, and more innovative banks.

In the context of selection of business unit employees, we expect that in some cases headquarters will be more able than the unit manager to identify well-matched job candidates, and in some cases not. The impact of correctly allocating decision authority to hire unit employees should matter more when other forms of control, such as monitoring and incentives, are harder to implement, since the relevance of identifying well-matched employees is more critical in resolving

agency problems. Our study focuses on two sets of factors affecting the tradeoffs between centralizing and decentralizing hiring: factors that give headquarters an advantage in hiring and factors that give the unit managers an advantage due to their superior information.

Factors Giving Headquarters a Hiring Advantage

Centralized hiring could enable an organization to develop the expertise and invest the resources necessary to consistently select workers aligned with its goals and values. In contrast, unit managers are likely to show considerable variation in their ability to select well-matched employees (Sah and Stiglitz 1991): a process which requires not only a clear understanding of—and motivation to pursue—corporate goals and values, but also expertise and a significant amount of time for screening, assessment, interviews, and more. Head office personnel generally have an informational advantage in their understanding of and alignment with corporate values, due to their exposure to the top management setting those values (Van den Steen 2010). Head office employees may not only have more resources and more time to build hiring skills, but also be in a better position to learn which job applicants fit the company well and which not, based on their experience filling many more positions than unit managers do (Baird and Meshoulam 1988).

We expect headquarters to have a hiring advantage, and centralized hiring to be relatively more beneficial when the unit is (a) ex-ante less aligned with corporate goals and values, (b) busier, and/or (c) more operationally complex. These conditions may hinder the managers' desire and/or ability to hire new staff matching company values.[3] Headquarters should then be in an ideal position to (a) create and foster standards for new hires, serving as "standards bearers" for the organization, (b) alleviate the strain placed on unit managers, and (c) clarify the broader

[3] Although managers of units with more time-constrained personnel or added complexity may know better what skills and traits they need in new hires, their ability to act on this knowledge may be constrained by lack of time and the need to perform multiple tasks at once. Research highlights the increased knowledge and skill requirements required to perform multiple tasks and process various information cues (Wood 1986, Bonner et al. 2000).

organizational purpose to employees, especially to those in misaligned units (who may be less aware of corporate goals), who may then be more likely to find their work meaningful and envision a career with the company. Research suggests that employees who are more aligned with their company's purpose and values are more productive, more satisfied, and less likely to quit (Chatman 1991; Gartenberg, Prat, and Serafeim 2019). Especially when unit managers do not have enough time for hiring, centralizing hiring may improve decision-making by shifting responsibility to qualified human resources employees at headquarters.[4] We therefore hypothesize:

Hypothesis 1: When headquarters has a hiring advantage over the business unit, switching from decentralized to centralized hiring will lead to *relatively* better employee-firm matches than when headquarters does not have such an advantage.

Hypothesis 1a: In business units that are ex-ante less aligned with company goals and values, switching from decentralized to centralized hiring will lead to *relatively* better employee-firm matches than in units that are ex-ante more aligned.

Hypothesis 1b: In busier business units, switching from decentralized to centralized hiring will lead to *relatively* better employee-firm matches than in units that are less busy.

Hypothesis 1c: In business units with more complex operations, switching from decentralized to centralized hiring will lead to *relatively* better employee-firm matches than in units with less complex operations.

Factors Giving the Unit Manager a Local Information Advantage in Hiring

Relative to decentralized hiring, centralized hiring could incur significant costs by neglecting the informational advantages of unit managers (Brickley and Dark 1987; Campbell et al. 2009). The unit manager may better recognize who could achieve corporate goals and uphold company values in the local circumstances (Fladmoe-Lindquist and Jacque 1995; Campbell et al. 2009). A retailer differentiating itself through friendly service needs employees to be friendly in different

[4] A counterargument adding tension to our prediction is that busyness could also proxy for the unit manager's ability to operate efficiently and effectively. If so, managers of busy units could be more (rather than less) effective at hiring well-matched employees. In the context of our study, however, busyness was largely determined by local demand.

ways in different locations, in ways that store managers may understand better than headquarters; for example, a store employee in a busy city may need to be alert and help customers find what they need quickly, while a store employee in a small rural town may need to be a good conversationalist. These local informational advantages cannot necessarily be removed by having the unit manager communicate what he or she knows to headquarters, since such knowledge may be costly, hard, or even impossible to transfer (Jensen and Meckling 1992; Baiman, Larker, and Rajan 1995; Christie, Joye, and Watts 2003). For instance, a unit manager may not be able to describe the characteristics of a person that would fit the local team, or to know ahead of time what information may be critical in a recruiting decision. Furthermore, one may infer from the literature that managers' information gathering (e.g., being on the lookout for job candidates) may depend on whether or not they need to do the hiring themselves (Aghion and Tirole 1997).

A unit's information advantage over headquarters could make delegation of hiring more desirable for the reasons described above. A separate effect of this information advantage is that it makes it harder for headquarters to implement control systems designed to ensure that employees are doing the right thing—where the unit is at an informational advantage, it is more difficult for headquarters to know the specific actions and behaviors that employees should exhibit in the local environment or to know how to assess their performance (Baiman et al. 1995). This is turn, makes it all the more relevant to hire well-matched employees. Overall, information asymmetries suggest that centralizing decision rights should be least beneficial or most harmful for more-informed units, whose managers could be best able to select and motivate employees to exert effort.

We refer to situations in which the unit manager has such an informational advantage as "Business Unit Manager Has a Local Advantage." These situations involve three kinds of information asymmetry shown in the literature to lead geographically dispersed organizations to

decentralize decision rights (Brickley and Dark 1987; Martin 1988; Fladmoe-Lindquist and Jacque 1995; Nagar 2002; Campbell et al. 2009; Dessein and Prat 2016): (a) the cost to headquarters of gathering information from the unit is relatively high; (b) the unit's market diverges from the company's typical market; and (c) the unit has strong relationships with repeat customers.

The literature suggests that headquarters should delegate decision rights to unit managers with superior local information (Dessein 2002). However, agency conflicts may be more pervasive in these cases, since more autonomous managers may (a) have greater opportunity for nepotism or (b) feel less connection to the organization. Thus, centralized hiring could reduce opportunistic hiring, expose new hires to the broader organization, and increase their sense of belonging.[5]

As it is unclear whether circumstances in which unit managers have an informational advantage would be made relatively better or worse by centralized hiring, we rely on the most direct findings from prior empirical research to support the following hypotheses:

Hypothesis 2: When the business unit has a local information advantage over headquarters, switching from decentralized to centralized hiring will lead to *relatively* worse employee-firm matches than when the unit does not have such an advantage.

Hypothesis 2a: When the cost to headquarters of gathering information from a given unit is relatively high, switching from decentralized to centralized hiring will lead to *relatively* worse matches than in units where this cost is relatively low.

Hypothesis 2b: In business units serving markets that differ more from the organization's typical markets, switching from decentralized to centralized hiring will lead to *relatively* worse employee-firm matches than in units serving typical markets.

[5] The tensions between decentralization and centralization have been examined since the emergence in the 1950s of multidivisional organizations such as General Motors and Sears Roebuck. Following the growth and expansion of many of these organizations, senior managers recognized the need to decentralize operations as they themselves lacked the specific local knowledge necessary to run the operations of their geographically dispersed divisions. However, many confronted crises as they lost control of their operations, which forced them to develop control structures and pursue economies through integration (Chandler 1990). It remains unclear whether centralized hiring should or should not be used by organizations to maintain control.

Hypothesis 2c: In business units more likely to serve repeat customers, switching from decentralized to centralized hiring will lead to *relatively* worse employee-firm matches than in units less likely to serve such customers.

III. RESEARCH SETTING, DATA, AND SAMPLE

We test our hypotheses using data from a US retail chain.[6] Each store has a store manager (responsible for day-to-day operations) and a small team of full-time and part-time employees (roughly 11, though team sizes exhibit significant variation). Since the sector's product offerings are relatively homogeneous, the chain seeks to differentiate itself through superior service.

As usual for retailers, turnover is high: 70–80 percent for part-timers and 15–20 percent for full-timers.[7] As shown in Figure 2, median and mean tenure were just 97 and 156 days, respectively, for employees in our main sample (described later) who had joined and left the company during our sample period (these employees make up 71 percent of the 2,297 employees detailed in Table 1; the remaining 29 percent were still employed at the end of our sample period).

[Insert Figure 2 and Table 1 here]

Prior studies show that companies perform better when their employees stay longer. A recent meta-analysis published in the psychology literature suggests that a one-standard-deviation increase in turnover rate is associated with a 0.15-standard-deviation reduction in organizational performance (Park and Shaw 2013) and that the relation between turnover and performance is strongest for more proximal performance dimensions, such as customer satisfaction, and weakest for more distal measures, such as financial performance. In retailing, specifically, Ton and Huckman (2008) find turnover is negatively associated with customer service and profit margins in a bookstore chain, while Kacmar et al. (2006) find that turnover is associated with longer

[6] A nondisclosure agreement with the company prevents us from disclosing information that would identify it, including store locations.
[7] Per a conversation with the CEO and the Chief Human Resources Officer in 2015.

customer wait times and higher food waste in a fast-food chain. Kacmar et al. (2006) also find that the relation between turnover and financial performance (sales and profit) is mediated by customer wait times. Drawing on several turnover cost studies, Ton and Huckman (2008) highlight that the estimated costs of one employee (earning $8 an hour) turning over were reported to be $3,500 or higher. Turnover in retail is costly.

At our research site, store teams' performance is primarily assessed based on mystery shopper scores (measuring the overall customer experience: a clean store, friendly staff, quick service, and so on) rather than financial results. This is because store teams have little or no control over various decisions affecting profitability (pricing, labor budget, etc.) or over influential external factors, such as foot traffic and local regulations. Appendix 1 provides evidence that, consistent with research findings, turnover in our setting is negatively associated with mystery shopper scores.[8]

In addition to the customer experience in their stores, monitoring and fostering employee engagement and retention are issues that are top of mind for executives at the company. For the CEO, one benefit of low turnover (with the right supporting conditions) is that longer-tenured employees can be empowered to use their discretion to satisfy customers. The company aims to hire and retain employees (responsible for relatively low-skill tasks) who not only can drive results, but also embrace company goals and values by providing friendly service, delivering customer value, and showing respect, kindness, ownership, teamwork, initiative, and candor. To align team interests with company interests, the company grants bonuses based on mystery shopper ratings. The CEO believes, however, that it takes more:

> I walk in, you're behind the counter. You look at me and you don't say anything. You're getting paid to say hello and yet you choose not to. I don't believe it's because they don't believe there is an incentive program, because we tell every teammate every month, here's

[8] For the sample examined and the empirical specification used in column (1), a one-standard-deviation increase in turnover (0.383, where the mean is 0.582 [58.2%]) is associated with a 0.14-standard-deviation reduction in the average mystery shopper score for the year.

what you earned, here's what you left on the table in terms of that incentive. And yet, they still don't do it! If that isn't about recruiting the wrong person, I don't know what it is.

The Retail Chain's Hiring Process

As a major part of broader efforts to create a culture-focused organization and a more engaged workforce, the CEO decided to switch from decentralized to centralized hiring.[9] The switch was staggered, providing us with a natural experimental setting in which to study the allocation of hiring rights.

In the chain's decentralized hiring model, store managers assume full responsibility for screening, interviewing, and hiring, with the exception that the area sales managers conduct second-round interviews.[10] According to the CEO and the chief human resources officer (CHRO), a store manager doing a poor job of hiring would offer a vacancy to the first available candidate fitting the job's schedule, rather than gathering and evaluating a broader talent pool—possibly hundreds of candidates. A store manager doing a great job would always be on the lookout for great talent and great fit with company values, often identifying candidates among the store's customers and engaging other team members in the interview process to ensure good rapport. In the centralized model, headquarters assumed all administrative responsibility:[11] screening, interviewing (paying particular attention, according to the CHRO, to whether the company's core values resonated with the candidate), and making a final recommendation (often providing two

[9] Other goals stated by executives included making the hiring process more efficient, and ensuring compliance with legal and ethical practices, especially those related to preventing discrimination. Other notable initiatives included increasing the percentage of full-time (versus part-time) employees, increasing pay and benefits, and adopting a new set of company values that were more memorable and actionable than the previous values. In contrast to the staggered nature of the employee selection initiative, these initiatives came into effect for the entire chain at once.

[10] The nature of the vacant positions was the same regardless of whether the hiring rights were allocated to the store manager or to headquarters. Store managers held firing rights throughout the sample period under both conditions.

[11] Headquarters used recruiters in this process – in some locations, physically present, in others, working virtually.

candidates) to the store manager. While the manager had the final say, the company advised us that they nearly always hired a recommended candidate.[12]

Executives stated that they did not follow any particular strategy in selecting the order in which stores adopted the centralized hiring model, except for occasional opportunism (e.g., starting with areas where they had recruiters) and a desire to prioritize areas with stores that were to be converted to a new format in the near future. Since the new format required more personnel and demanded more of them (due to greater product variety and greater operational complexity to support it), the head office sought to alleviate some of this pressure by centralizing hiring when possible. Our sample exclude hires at stores that changed format during our sample period.[13]

Although the ordering of the centralized hiring rollout was not directly related to our outcome (employee-firm match quality), it did take place area by area, which could have led to significant differences between treatment and control stores. To overcome problems of non-random treatment assignment, we use a propensity score matched sample for our analyses (described later).

Data

We had 33 months of hiring data—January 2013 through September 2015—which included each employee's hire date, exit date, position, and store[14] and whether he or she had been hired under the centralized or decentralized system. The sample selection process is documented in Table 1. The final sample had 7,678 employees. We supplement the hiring data with store-level

[12] Giving store managers two choices could have helped incorporate relevant local knowledge into the selection process. However, the final interview may be too late to incorporate such information. Furthermore, managers may have been reluctant to reject candidate(s) recommended by headquarters, even if they seemed a poor fit on the basis of local information.

[13] Company executives advised us that in some cases, even stores that had not yet switched to centralized hiring received hiring support from headquarters during the format change. Due to this confound, we eliminate all hires and store-months for stores that converted during our study.

[14] Note that we did not have access to systematic data on employee movements between stores (we did exclude from our analyses any employees we could identify as having moved stores because we received the data in two waves; see Table 1). However, we expect any unidentified movements to represent a small percentage of the sample and to simply add noise which would reduce our chances of finding significant results.

data provided by the company and additional data from other sources. Specifically, from each store's address, we determined the longitude and latitude, to match the store with data about local market characteristics from Esri databases. Using the store's ZIP code, we obtained the number of competitors in the same ZIP code, defined as those with a NAICS classification most related to the chain's primary operations.

Dependent variable: Following prior literature (e.g., Oyer and Schaefer 2011; Hoffman et al. 2018), we use employment duration as our empirical proxy for employee-firm match quality; *Time to Employee Departure* is the number of days between hire date and exit date. For employees still active as of December 14, 2015 (the last recorded exit date in our dataset), *Time to Employee Departure* is the number of days between the hire date and December 14, 2015. (These employee observations are marked as "censored" as of this date.)

Headquarters' hiring advantage: We use three variables to proxy for the circumstances in which we expect headquarters to have a hiring advantage, measured before our sample period begins. *Ex-ante Less Aligned* is an indicator variable equal to 1 if the store's average mystery shopper score in December 2012 is less than 90 out of a possible 100 (approximately the bottom 20th percentile). *Busyness* is monthly store sales divided by monthly labor-hours for December 2012. From this, we create an indicator variable, *Busier*, equal to 1 if *Busyness* is above median (we calculate the median for each sample used for our empirical analyses, using one observation per store) and 0 otherwise. *Complex Operations* is an indicator variable equal to 1 if the store is operating under the organization's new format (which entailed greater product variety and operational complexity) throughout our sample period, and 0 otherwise.

For some empirical tests, we use an aggregate indicator variable, *HQ Hiring Advantage*, which equals 1 if two of the above proxies equal 1 and 0 otherwise.

Business unit's local advantage: We use three variables to capture the circumstances in which we expect the business unit manager to have a hiring advantage. *Distance to Headquarters*, the miles between the store and headquarters, proxies for the cost to headquarters of gathering information from the unit (the further the unit, the greater the cost). From this, we create *Further from Headquarters*, equal to 1 if *Distance to Headquarters* is above the median and 0 otherwise. *Market Divergence* captures the extent to which the store location's demographic characteristics (population density, income, age, ethnicity, and household size) differ from the average demographic characteristics of the chain's units.[15] From this, we create *Higher Market Divergence*, equal to 1 if *Market Divergence* is above median and 0 otherwise. For *Serves Repeat Customers*, we follow prior literature in assuming a higher proportion of repeat customers in areas with low population density and away from highway exits (Brickley and Dark 1987; Martin 1988). Both the CEO and the CHRO confirmed that this would be a reliable proxy for relatively more repeat customers. *Serves Repeat Customers* therefore indicates that the store is in a Census Block Group with less than 1,000 people per square mile (according to the 2013 Esri Demographics data) and is not within two miles of an interstate or US highway exit.

For some of our empirical tests, we use an aggregate indicator variable, *BU Local Advantage*, equal to 1 if two of the three proxies for unit manager's hiring advantage equal 1, and 0 otherwise.

[15] We measure market divergence according to Campbell et al. (2009). First, we estimate **"**normalized divergences**"** for each demographic location characteristic between the store and the chain's average store by subtracting the value of each of the demographic variables for the store from its mean across all stores and then dividing this difference by the standard deviation of the variable across all stores. The mean values and standard deviations are calculated as of 2013 using stores in the entire chain (not just those in our empirical analyses) operating at the start of 2013. The values of the demographic variables are obtained from Esri Demographics data (which draws on the US Census and other data sources) at the Census-Block-Group level (the smallest geographical unit for which the US Census publishes sample data, generally containing 600–3,000 people) and include population density (inhabitants per square mile), per capita income (in dollars), inhabitants' median age (in years), ethnicity (percentage of white individuals in the population), and average household size. Second, we aggregate the normalized divergences of the five demographic variables for each store by adding them. We use the 2013 Esri Demographics data and treat market divergence as a time-invariant variable for each store.

Other variables: Our propensity score matching model (described below) includes variables pertaining to characteristics of the store, its team, and its environment that could be correlated both with being selected for centralized hiring and with employee turnover (a store-level variable similar in spirit to our individual-level dependent variable). For the environment, we include (using 2013 values) the yearly unemployment rate for the store's Census Block Group (*Unemployment Rate*); the income per capita in the store's Census Block Group (*Per Capita Income*); whether the store was the only one in the chain in that ZIP code (*One Store*); and the number of direct competitors in the same ZIP code (*Competition*). For the store and its team, we include the store's size in square feet (*Size*), its age in months (*Store Age*), its *Team Size* (number of employees at month end), and the percentage of full-time employees (*% Full-time Employees*).[16] We measure *Store Size* as of October 2013, the earliest date for which we could get information, and the others as of December 2012, the month before our sample period begins.

Our empirical analyses include *Team Size* and *% Full-time Employees* as control variables and a variable capturing the percentage of employees hired within the same calendar year in the store's ZIP code and subsequently promoted during our sample period (*Promotion Opportunities*). We include month-year fixed effects, store fixed effects, and an indicator, *Full-tim*e, for whether or not the employee was full-time at the end of the sample period or upon exit. Because we include store fixed effects, we exclude the other variables from above to avoid multicollinearity issues (the variables are a mix of time-invariant variables and variables that capture relatively stable store or environmental characteristics, resulting in high variance inflation factors).

[16] Our propensity score matching model does not include an indicator for whether the store switched to the new format during our sample period, since we excluded those from the sample, or an indicator for new format as of the beginning of the sample period (we retained these), since we do not match on any of the examined variables as moderators; we use new store format to proxy for *Complex Operations*. When we ran the matching with an indicator for new store format (to see if it was an important determinant of selection), this variable was not significant.

Propensity Score Matched Sample

While the organization stated that the order of centralizing stores was not driven by store characteristics (with the exception of those soon converting to the new format), the fact that it centralized stores area by area could have led to important differences at any given time between centralized and decentralized stores with respect to operations and/or employee turnover.

To ensure that the stores which transitioned during our sample period (our treatment stores) are comparable to those which did not (our control stores) and to assign a "post" period to the controls, we use a propensity score matching model based on store conditions at the start of our sample period. Specifically, we obtain a propensity score for each store, reflecting its probability of being selected for centralized hiring, using a logit model that includes identifiable variables that could explain both a store's turnover and the company's decision to centralize that store's hiring:[17]

$$\begin{aligned} Pr(\text{Centralized Hiring}) = {} & \beta_0 + \beta_1\, \text{December 2012 Quarterly Sales} + \\ & \beta_2\, \text{2012 Employee Turnover} + \beta_3\, \text{Unemployment Rate} + \beta_4\, \text{One Store} + \\ & \beta_5\, \text{Competition} + \beta_6\, \text{Per Capita Income} + \beta_7\, \text{Store Size} + \beta_8\, \text{Store Age} + \\ & \beta_9\, \text{Team Size} + \beta_{10}\, \text{\% Full-time Employees} + \beta_n\, (\text{State Fixed Effects}) + \varepsilon. \end{aligned} \quad (1)$$

The variables in Equation (1) primarily identify contextual and unit-level variables affecting turnover (see Griffeth, Hom, and Gaertner 2000). They are described in *Other variables* (see Data section above), with the addition of *December 2012 Quarterly Sales*, a measure of the store's pre-period sales (total quarterly sales for October 2012–December 2012[18]); *2012 Employee Turnover*, a measure of the store's pre-period turnover (annual turnover for 2012); and state fixed effects.

The logit results reported in Panel A of Table 2 show that the main drivers explaining whether or not a store was centralized during our sample period are related to its location—stores in certain

[17] We do not match on the key explanatory variables that we aim to study as moderators to avoid losing variation in them (similar approaches to retain variation in key explanatory variables have been used in the literature, see for example, Armstrong, Jagolinzer, and Larcker 2010).

[18] We could not use yearly sales for 2012; the earliest month for which we had sales data was October 2012.

states were more likely to be chosen—and percentage of full-time employees—the higher the percentage, the lower the likelihood. We matched each treatment store with the nearest control store that (a) had a propensity score within a distance (or caliper) of 0.10 and (b) hired employees both before and after the matched treatment store switched to centralized hiring.[19, 20]

Our final matched sample includes 2,305 hires from 64 treatment and 64 control stores.[21] Panel B of Table 2 presents a covariate balance analysis using t-tests and chi-squared tests to compare differences in means and proportions, respectively, between the variables used to match treatment and control stores.[22] The results suggest no significant differences, except for *Per Capita Income* (p-value = 0.08), providing validation of our matching procedure.

[Insert Table 2 here]

IV. EMPIRICAL ANALYSES AND RESULTS

Research Design

We test our hypotheses using two sets of analyses, one combining the moderators into *HQ Hiring Advantage* and *BU Local Advantage* (see Section 3) and one testing their effects separately.

[19] Our caliper is equal to 0.10 of the standard deviation of the logit of the propensity scores. We use a smaller, more conservative caliper than that suggested by Austin (2011) (0.20 of the standard deviation of the logit of the propensity scores) to ensure comparability across matched stores.

[20] Of the 296 treatment and control stores available for our analyses, three were excluded from the matching because 2012 employee turnover could not be calculated. 56 were dropped in the logit regression, because the state in which the store was located perfectly predicted "success" or "failure" – due to all stores switching to centralized hiring during the sample period or all stores remaining decentralized. Thus, 237 stores are represented in Panel A of Table 2.

[21] Our sample includes a few treatment stores that switched to centralized hiring, reverted to decentralized hiring, then switched back again to centralized hiring. We exclude from our sample any hires or store-months corresponding to the initial period of centralization. As a robustness test, we also exclude all observations pertaining to these stores and their corresponding control stores. Our results are similar to those reported in the paper, with the moderating effects of *Further from Headquarters* and *Serves Repeat Customers* just losing statistical significance (p-values of 0.104 and 0.111 in one-sided tests, respectively).

[22] To protect the company's confidentiality, we do not tabulate store age averages; the difference in means between the treatment and control group is insignificant (t-test=0.67).

We test how these variables moderate the effect of centralization of hiring rights using the following hazard rate model specification with robust standard errors and clustering by store: [23]

$$\begin{aligned}\textit{Time to Employee Departure} = {} & \beta_0 + \beta_1\, \textit{Post} + \beta_2\, \textit{Treated} \times \textit{Post} \\ & + \beta_3\, \textit{Post} \times \textit{HQ Hiring Advantage} + \beta_4\, \textit{Post} \times \textit{BU Local Advantage} \\ & + \beta_5\, \textit{Treated} \times \textit{Post} \times \textit{HQ Hiring Advantage} + \beta_6\, \textit{Treated} \times \textit{Post} \times \textit{BU Local Advantage} \\ & + \beta_m\, (\textit{Control Variables}) + \beta_n\, (\textit{Month-Year Fixed Effects}) + \beta_o\, (\textit{Store Fixed Effects}) + \varepsilon \qquad (2)\end{aligned}$$

This analysis models the probability that an employee will depart at a point in time, given that he or she hasn't already. In our model, *Treated* is an indicator that the store switched from decentralized to centralized hiring during our sample period and *Post* is an indicator specifying that the hire occurred after the relevant treatment store switched to centralized hiring. Since each control store is matched to a treatment store, *Post* for the control store is defined as *Post* for the corresponding matching treatment store. Our coefficients of interest are β_5 and β_6; that is, the triple interaction terms between *Treated*, *Post*, and the two aggregate moderating variables. We include *Team Size*, *Promotion Opportunities*, and % *Full-time Employees* (described previously) as controls, add a *Full-time* indicator, and include time (month-year) and store fixed effects.[24]

The second set of analyses tests Hypotheses 1a–1c and 2a–2c with the specification:

$$\begin{aligned}\textit{Time to Employee Departure} = {} & \beta_0 + \beta_1\, \textit{Post} + \beta_2\, \textit{Treated} \times \textit{Post} \\ & + \beta_3\, \textit{Post} \times \textit{Ex-ante Less Aligned} + \beta_4\, \textit{Post} \times \textit{Busier} + \beta_5\, \textit{Post} \times \textit{Complex Operations} \\ & + \beta_6\, \textit{Post} \times \textit{Further from Headquarters} + \beta_7\, \textit{Post} \times \textit{Higher Market Divergence} \\ & + \beta_8\, \textit{Post} \times \textit{Serves Repeat Customers} + \beta_9\, \textit{Treated} \times \textit{Post} \times \textit{Ex-ante Less Aligned} \\ & + \beta_{10}\, \textit{Treated} \times \textit{Post} \times \textit{Busier} + \beta_{11}\, \textit{Treated} \times \textit{Post} \times \textit{Complex Operations} \\ & + \beta_{12}\, \textit{Treated} \times \textit{Post} \times \textit{Higher Information Gathering} \\ & + \beta_{13}\, \textit{Treated} \times \textit{Post} \times \textit{Higher Market Divergence}\end{aligned}$$

[23] *HQ Hiring Advantage*, *BU Local Advantage*, *Treated*, *Treated x HQ Hiring Advantage*, and *Treated x BU Local Advantage* do not appear in Equation (2) since they are time-invariant and thus absorbed by the store fixed effects.

[24] We do not control for individual characteristics in our analyses of employee-firm match quality because we want to test the overall effect of a centralized vs. decentralized process and do not want to control for hiring choices (in terms of characteristics of those hired) that may result from the recruiting process itself. Future studies could potentially incorporate the hires' individual characteristics as a potential outcome of the recruiting process or a mediator explaining the effect of centralization on employment duration.

$$+ \beta_{14}\, Treated \times Post \times Serves\ Repeat\ Customers + \beta_m\, (Control\ Variables)$$
$$+ \beta_n\, (Month\text{-}Year\ Fixed\ Effects) + \beta_o\, (Store\ Fixed\ Effects) + \varepsilon \quad (3)$$

We use the same variables and estimation method described for Equation (2). Our coefficients of interest are β_9 through β_{14}.

Results

Descriptive Statistics

Panel A of Table 3 presents summary statistics for the main variables used in our analyses. With respect to our main dependent variable, newly hired employees worked an average of 230 days (ranging from 1 to 1,062 days) during our sample period (recall that these days were censored for any employee still there as of our last exit date in December 2015).

Our main moderating variables (*Ex-ante Less Aligned, Busyness, Complex Operations, Distance to Headquarters, Market Divergence,* and *Serves Repeat Customers*) varied widely. Average busyness (in December 2012) was about $100 in sales per labor-hour, but ranged from about $50 to about $150. Half the hires were at new-format stores, 37% at stores serving repeat customers, and 22% at stores that received a mystery shopper score below 90 in December 2012 (our proxy for lower ex-ante alignment). Hiring stores were between 5.5 and 128.3 miles from headquarters and the mean value of market divergence was 3.67: the average store operated in a location diverging roughly 0.73 standard deviations (3.67/5) from the average surrounding demographic characteristics across all the chain's stores. This is greater than the divergence reported for similar retail chains, though within a reasonable range (Campbell et al. 2009).

[Insert Table 3 here]

Our control measures report reasonable variation, even though we matched our stores on some of these variables. Team size varied from 3 to 23 and the percent of full-time employees ranged from 0 to 100. Twenty-two percent of new hires worked full-time. Promotion opportunities

(the percent of employees hired in the store's ZIP code area and year and promoted during our sample period, excluding the employee analyzed) averaged 7%.

Panel B of Table 3 shows correlations amongst the main variables of interest. Time to employee departure (or time to censoring for newly hired employees still active as of our last recorded exit date) was longer for employees of busy stores, employees with greater promotion opportunities and full-timers, and shorter for employees at stores with lower ex-ante alignment with company goals and values, greater market divergence, serving repeat customers and with a higher percentage of full-time employees. Correlations among our explanatory variables were generally low, except for that between team size and complex operations. Since *Complex Operations* is time-invariant for our sample, it is absorbed by store fixed effects. Our calculated VIFs (estimated using ordinary least squares regressions) for team size and the individual store coefficients are all <10 for our main tables, suggesting multicollinearity due to the team-size–complex-operations correlation is not a concern.

Moderators of the Effects of the Switch from Decentralized to Centralized Hiring

Tables 4 and 5 explore circumstances in which centralization could have had a more favorable or less favorable effect.[25] They present the results of the regressions specified in Equations (2) and (3). Our findings provide strong support for Hypothesis 2, but not Hypothesis 1 (we find support for Hypothesis 1 when we use our "randomly-matched, full sample" (described later), but not our propensity score matched sample).[26]

[25] Untabulated analyses examining whether there was a main effect of centralized hiring on new employee duration (that is, a test of β_2, when regressing: *Time to Employee Departure*$= \beta_0 + \beta_1$ *Post* $+ \beta_2$ *Treated* $\times$ *Post* $+ \beta_m$ *(Control Variables)* $+ \beta_n$ *(Month-Year Fixed Effects)* $+ \beta_o$ *(Store Fixed Effects)* $+ \varepsilon$), suggest that, on average, the switch from decentralized to centralized hiring was not associated with the rate of new-hire departures. It is worth noting that there may have been positive effects of centralized hiring not captured in our empirical tests focused on match quality, such as shorter lead times to fill positions or greater comfort that the organization was complying with legal requirements.

[26] Since we use a difference-in-differences approach, we conduct a test to assess whether our data satisfies the parallel trends assumption. We calculate the monthly percentage change in employee turnover at the store level and then

Moderating Effects of Factors Giving Headquarters a Hiring Advantage

Hypothesis 1 predicts that when headquarters has a hiring advantage over the store, switching from decentralized to centralized hiring should lead to relatively better employee-firm matches (i.e., a lower rate of new employee departures) than when headquarters does not have such an advantage. Inconsistent with H1, Table 4, columns (1) and (2), provides no evidence that the switch from decentralized to centralized hiring resulted in more favorable outcomes (in terms of employee departure rates[27]) for stores in which headquarters has a hiring advantage, relative to stores where no such advantage exists.

Table 5, columns (1) and (2), examines the proposed sources of the headquarters hiring advantage; that is, the moderating effects of a store's ex-ante alignment with company goals and values, its busyness, and its operational complexity. This analysis does not support H1a–H1c.

[Insert Tables 4 and 5 here]

Moderating Effects of Factors Giving the Unit Manager an Information Advantage

Hypothesis 2 predicts that when the store has a local information advantage over headquarters, switching form decentralized to centralized hiring should lead to a relatively shorter time to departure for new employees than when the store does not have such an advantage. The results in Table 4 support H2. Columns (1) and (2) show that the introduction of centralized hiring was associated with rates of employee departure that were higher for stores with a local

perform a t-test of whether the difference in means of this variable between the treatment and control groups in the pre-period is statistically significant. It is not (p=0.710), suggesting our use of difference-in-differences analyses is appropriate. This test was performed after first setting the percentage change for employee turnover to zero if both this month's and last month's turnover were zero; if turnover was zero last month but not this month, the percentage change variable is set to missing and excluded from the test.

[27] A hazard ratio greater than 1 (or a positive coefficient) indicates that the variable is associated with shorter time to employee departure, whereas a hazard ratio less than 1 (or a negative coefficient) suggests that the variable is associated with longer time to employee departure.

information advantage. (These were stores with at least two of the following three characteristics: far from headquarters, a divergent market, and repeat customers.)

Table 5, columns (1) and (2), presents a more detailed analysis. Consistent with H2a–H2c, the rate of new-hire departure after the shift to centralization was higher if the store had higher information-gathering costs (above-median distance to headquarters), higher market divergence (above median), or served repeat customers. (We speak to estimates of the economic magnitude of our results in the later subsection titled "Subsample analyses and estimates of economic significance"). These results resonated with company executives, who recognized that managers of such stores could know better than headquarters whom to hire. The CEO offered an example:

> *When I ran a store… I was new to that store and I didn't live in that town, but I hired somebody… A customer came in the next day, someone who was a repeat customer, and he said, "You didn't hire so-and-so, did you?" I said "Yes." And he said* [whispering]*, "Oh that's a big mistake." He was known around town as being a thief or something. It is a perfect example. If I had lived and worked in that town, I would have known.*

In sum, Tables 4 and 5, columns (1) and (2), suggest that the wisdom of centralizing versus decentralizing hiring rights depends on each store's circumstances. We find evidence that centralizing hiring rights can be less beneficial or detrimental (with respect to the rate of employee departures) when a store manager is likely to have an information advantage due to the store's distance from headquarters (indicative of higher information gathering costs for headquarters), market divergence (bearing in mind that some of our results are sensitive to how that is measured, as discussed in Appendix 2) or due to it being more likely to serve repeat customers.

Additional Analyses

Randomly matched sample analyses

As an alternative to the propensity score matched sample, we re-run our analyses in Table 4 and 5 using hires corresponding to all the stores in the states where a switch from decentralized

to centralized hiring occurred during our sample period for some stores, while others remained decentralized (essentially the same sample from which we drew the propensity score matched stores since any states without these two types of stores were dropped in the logit regression used for propensity score matching). We randomly matched (with replacement) each of the 154 treatment stores to one of the 86 control stores available in that sample, requiring the control store to have hires in the treatment store's "pre" and "post" period (60 control stores in total were retained in the matching procedure; for every instance that a control store was "matched" with a treatment store, all of the control store's observations were included in the sample, e.g., a control store that matched twice had each unique hire appear twice in the sample). We assign a "Post" period to each control store, consistent with the period for the corresponding treatment. Results are reported in Tables 4 and 5, columns (3) and (4). Columns (3) and (4) of Table 4 show a significant interaction between *Treated, Post,* and *BU Local Advantage,* consistent with our result for the propensity score matched sample. We also find a significant interaction between *Treated, Post,* and *HQ Hiring Advantage,* which is consistent with Hypothesis 1, and which may suggest that the lack of results in columns (1) and (2) may have been due to lower power. However the smaller size of the coefficients corresponding to the interaction between *Treated, Post, and HQ Hiring Advantage* reported in columns (1) and (2) suggest low power may not fully explain the lack of significance of those results. Columns (3) and (4) of Table 5 show significant interactions between *Treated, Post* and four moderating variables: *Higher Market Divergence* and *Serves Repeat Customers,* consistent with columns (1) and (2), and *Ex-ante Less Aligned* and *Busier* (consistent with Hypotheses 1a and 1b). The significant moderating effect of *Further from Headquarters* that appeared in Table 5 columns (1) and (2) is not present in columns (3) and (4). We interpret these alternative sample results as being largely consistent with our propensity score

matched sample results regarding the informational advantage of the local manager, and refrain from drawing conclusions with respect to headquarters hiring advantage since we do not find significant effects in the propensity score matched sample.

Subsample analyses and estimates of economic significance

To shed further light on our results we run sub-sample analyses (using our propensity score matched sample) for the contingencies where we found significant interaction effects, i.e., *BU Local Advantage*, *Further from Headquarters*, *Higher Market Divergence*, and *Serves Repeat Customers*. Specifically, we examine the main effect of centralized hiring where *BU Local Advantage* = 0, *BU Local Advantage* = 1, *Further from Headquarters* = 0, and so on. The results are shown in Table 6. These subsample analyses provide some evidence that centralized hiring was beneficial for stores where the local manager did not possess an informational advantage relative to headquarters, while centralized hiring was detrimental or had no discernible effect where the local manager did possess such an advantage.[28]

[Insert Table 6 here]

Since the hazard rate analyses use a non-linear model, we estimate the economic significance of our *BU Local Advantage* subsample results as follows. First, we repeat the *BU Local Advantage* split but (in order to include the main effect of "Treated") rather than include store fixed effects we instead include *Treated*, *HQ Hiring Advantage* (for the *BU Local Advantage* splits) and variables capturing the store and local conditions (e.g. *Competition*, *Per Capita Income*, *Team Size*, etc., plus state fixed effects). Second, for each subsample split (e.g. *BU Local Advantage* =

[28] In untabulated tests, we repeated these subsample analyses using censored normal regressions (following Hoffman et al. 2018) in order to test whether the difference in coefficients on *Treated x Post* differed between each subsample (e.g. between *BU Local Advantage* = 0 and *BU Local Advantage* =1). The difference was statistically significant in all cases, except for the comparison between *Serves Repeat Customers* = 0 and *Serves Repeat Customers* = 1.

0), we estimate the "survival rate" at 180 days for the following four combinations: *Treated* = 0, *Post* = 0; *Treated* = 0, *Post* = 1; *Treated* = 1, *Post* = 0; *Treated* = 1, *Post* =1, using the regression coefficients and the median value for all other regressors.[29] This survival rate tells us the likelihood of an employee remaining with the company beyond 180 days.[30] Third, we examine how the survival rate changes between the pre and post period for control stores and for treated stores. Where *BU Local Advantage* = 0, we find that the estimated survival rate at a control store decreased by 23.07 percent in the post-period relative to the pre-period (from a survival rate of 34.59 percent to 26.61 percent), while the estimated survival rate at a treatment store was essentially unchanged (from a survival rate of 30.66 percent to 30.63 percent). Thus, in the subsample of stores where the local manager did not possess an information advantage, those stores that switched to centralized hiring fared better in terms of survival rates of new hires than those stores that remained under the decentralized regime. Where *BU Local Advantage* = 1, we find that the estimated survival rate at a control store increased by 133.26 percent in the post-period relative to the pre-period (from a survival rate of 9.68 percent to 22.58 percent), while the estimated survival rate at a treatment store decreased by 7.27 percent (from a survival rate of 12.38 percent to 11.48 percent). Thus, in the subsample of stores where the local manager had an information advantage, those stores that switched to centralized hiring fared much worse in terms of survival rates of new hires than those stores that remained under the decentralized regime.

[29] With this alternative regression specification (which is used purely for the purposes of these calculations, we believe using store fixed effects is a superior specification), the p-value on *Treated x Post* is 0.15 where *BU Local Advantage* = 0, and 0.03 where *BU Local Advantage* = 1.

[30] We repeated our estimates using 90 days rather than 180 days. Using 90 days, the results are qualitatively similar: The estimated survival rate at a control store decreased by 13.25 percent in the post-period relative to the pre-period where *BU Local Advantage* = 0, while the rate at a treatment store was essentially unchanged. Where *BU Local Advantage* = 1, the estimated survival rate at a control store increased by 61.63 percent in the post-period relative to the pre-period, while it declined by 3.73 percent at a treatment store.

Robustness tests for Tables 4 and 5

We use a few alternative measures and specifications to validate our results in columns (1) and (2) of Tables 4 and 5. Appendix 2 provides a summary of our various robustness checks, indicating in each case whether or not the moderating effects of *BU Local Advantage*, *Further from Headquarters*, *Higher Market Divergence*, and *Serves Repeat Customers* are ('Yes') or are not ('No') robust to the specific robustness check. The moderating effect of the aggregate variable, *BU Local Advantage*, is robust to all of our alternative measures and specifications, while the moderating effects of the underlying variables are robust to some checks but not others.

We also ran placebo tests to examine whether our results could be artifacts of the data structure (Bertrand, Duflo, and Mullainathan 2004). Following a methodology used by Pierce, Snow, and McAfee (2015) and Song, Tucker, Murrell, and Vinson (2018), among others, we randomly assigned treatment dates to each treatment-control pair, randomly assigned "treatment" within each pair (ensuring that, in half the cases, "treatment" was assigned to an actual treatment store and in the other half to a control store), and then re-ran our hazard rate analyses. We repeated this process 100 times. Using a one-tailed test (consistent with our tables) and a 5% level, we found that the coefficient on *Treated* × *Post* × *BU Local Advantage* was significant 4 times, *Treated* × *Post* × *Further From Headquarters* was significant twice, *Treated* × *Post* × *Higher Market Divergence* was significant once, and *Treated* × *Post* × *Serves Repeat Customers* was never significant. These results suggest that our findings are unlikely to be driven by chance.

V. CONCLUSIONS

For the many retailers that rely on their employees to deliver superior customer service, hiring is a critical management control mechanism. Recruiting employees naturally aligned with

company goals and values—and those of their local team—might foster productivity and customer loyalty as well as employee attitudes that are otherwise difficult to induce.

We examine how a switch from decentralized to centralized hiring affects the quality of employee-firm matches (measured as rate of new employee departures), holding other control systems constant. First, we expected the switch to centralization to lead to relatively lower rates of departures in busy units and units facing operational complexity or low ex-ante alignment with company values, as headquarters would have a hiring advantage: more time and ability than the unit manager to choose new hires aligned with company goals and values. Second, we expected the switch to centralized hiring to lead to relatively higher rates of departures in any unit where the manager would have an advantage because there was an information asymmetry between headquarters and the unit—greater distance to headquarters (a proxy for information-gathering costs), higher market divergence, and/or repeat customers. We find support for our second but not for our first prediction: the switch to centralization was associated with *relatively* higher departure rates if the unit manager had an informational advantage relative to headquarters.

We provide some of the first evidence on the importance of delegation of decision rights in employee selection. Practically, these findings suggest that chains should carefully consider information asymmetries between headquarters and their units when allocating hiring rights.

Our study has some limitations. First, although the rate of employee departures has been used in labor economics studies to capture the quality of employee-firm matches (e.g., Hoffman et al. 2018), this measure may be affected by employee-manager biases. For example, Schoorman (1988) shows that managers positively bias performance assessments of subordinates hired by them. Judge, Higgins, and Cable (2000) show that managers are more likely to hire individuals demographically similar to them, while Vecchio and Bullis (2001) show that individuals stay

longer in their jobs when their manager is demographically similar to them. Collectively, these biases might suggest that centralized hiring could lead to higher employee departure rates for reasons other than the quality of employee-firm matches. However, we do not believe that these biases explain our main results, since they are unlikely to be systematically correlated with the factors giving the unit managers an informational advantage.

Second, we were unable to directly examine the effects of the company's switch from decentralized to centralized hiring on performance since we did not have access to the newly hired employees' individual performance. We establish, however, that employee turnover negatively affected a store's mystery shopper score performance, suggesting that the effects on newly hired employee duration would, in aggregate, affect mystery shopper scores.

Third, our findings may not be generalizable to all settings since they are based on a single company and since we examine only the switch from decentralized to centralized hiring and not the other way. The organization we studied used performance-based rewards based on mystery shopper audits, which may have encouraged store managers with an information advantage and decentralized hiring rights to hire more carefully than they might have otherwise. Future research could explore whether the effects of centralizing versus decentralizing hiring depend not only on location contingencies, but also on the use of other management control systems (Grabner and Moers 2013). However, working with one large, multiunit organization allowed us to isolate the effects of centralized and decentralized hiring because (a) the transition was staggered, naturally generating a treatment sample and a control sample, (b) we could hold constant the use of other management control systems, and (c) we could control for unobservable firm characteristics that might explain centralization or decentralization decisions across firms.

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Figure 1: Summary of Relations Tested

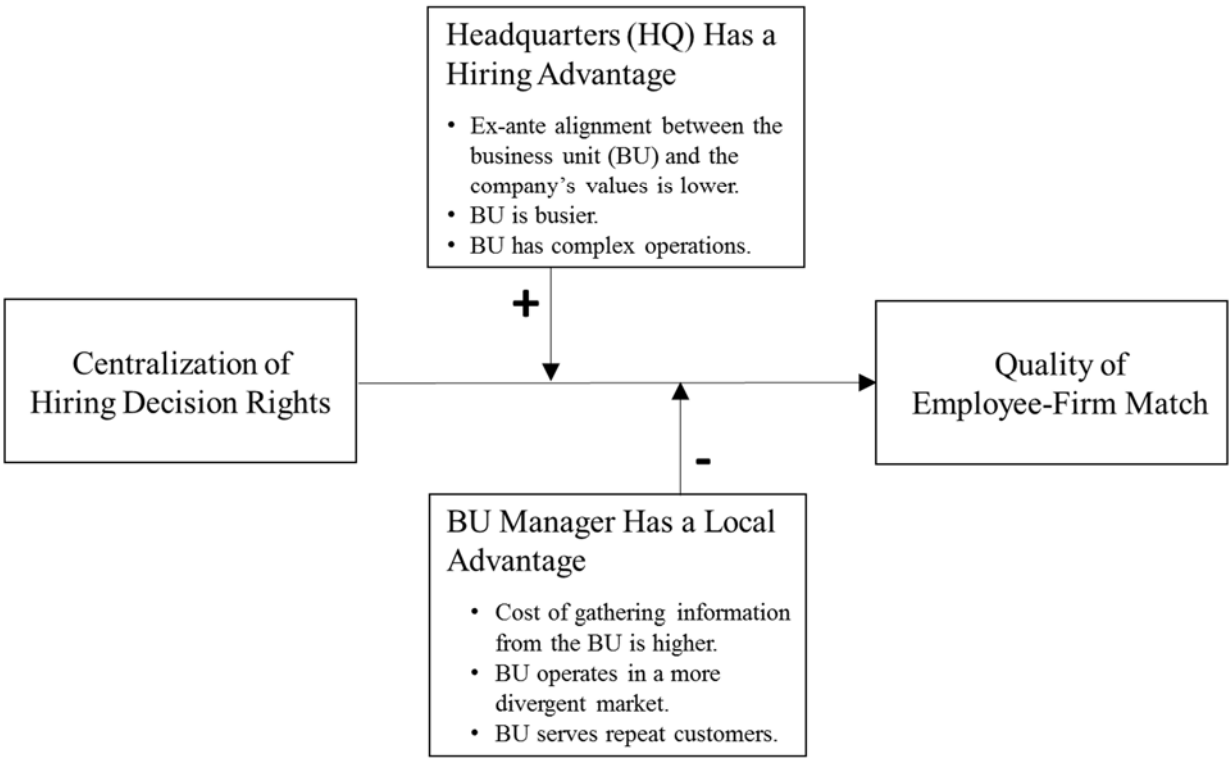
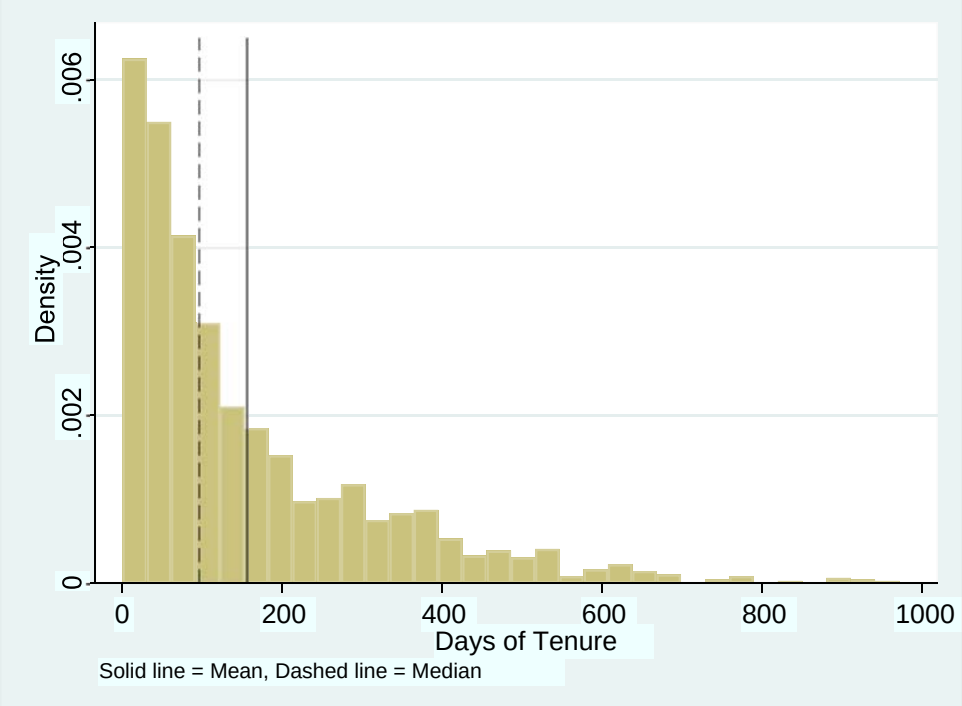


Figure 2: Distribution of Length of Complete Job Spells



Appendix 1: Relation between Annual Employee Turnover and Average Mystery Shopper Score for the Year Excluding Fiscal Years Post Centralized Hiring

	Mystery Shopper Score			
	Z-score	Raw Score	Z-score	Raw Score
Variable	(1)	(2)	(3)	(4)
Intercept	1.797***	1.032***	1.477***	1.014***
	(4.883)	(49.945)	(3.909)	(47.795)
Annual Employee Turnover	-0.363***	-0.021***	-0.339***	-0.019***
	(-4.602)	(-4.696)	(-4.248)	(-4.343)
Busyness	-0.004**	-0.000**	-0.004*	-0.000*
	(-2.211)	(-2.245)	(-1.865)	(-1.895)
Complex Operations	-0.115	-0.007	-0.097	-0.006
	(-1.193)	(-1.222)	(-1.042)	(-1.066)
Distance to Headquarters	-0.002	-0.000	-0.000	-0.000
	(-1.400)	(-1.441)	(-0.204)	(-0.237)
Market Divergence	-0.045**	-0.003**	-0.053**	-0.003**
	(-2.094)	(-2.104)	(-2.354)	(-2.366)
Serves Repeat Customers	0.000	-0.000	0.013	0.001
	(0.004)	(-0.038)	(0.166)	(0.124)
Unemployment Rate	0.004	0.000	0.003	0.000
	(0.829)	(0.844)	(0.624)	(0.639)
Store Count	0.033	0.002	0.016	0.001
	(1.037)	(1.101)	(0.477)	(0.538)
Competition	-0.015***	-0.001***	-0.012**	-0.001**
	(-2.717)	(-2.800)	(-1.988)	(-2.061)
Per Capita Income	0.000	0.000	0.000	0.000
	(1.365)	(1.339)	(0.874)	(0.851)
Size (Sqft)	-0.000	-0.000	-0.000	-0.000
	(-0.123)	(-0.154)	(-0.247)	(-0.285)
Store Age	0.000	0.000	0.000	0.000
	(0.077)	(0.038)	(0.400)	(0.366)
% Full-time Employees	-0.115	-0.007	-0.169	-0.010
	(-0.660)	(-0.704)	(-0.925)	(-0.981)
Average Team Size	-0.036*	-0.002*	-0.021	-0.001
	(-1.887)	(-1.921)	(-1.087)	(-1.112)
Observations	567	567	514	514
R-squared	0.479	N/A	0.443	N/A
Sample	S1: Stores available for propensity score matching (including stores switching to new format)		S2: Stores available for propensity score matching (excluding stores switching to new format)	

t-statistics in parentheses are based on robust standard errors clustered by store. Year and state fixed effects are included in all specifications. *, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively. In columns (2) and (4) the dependent variable is the store's average mystery shopper score for the year (bounded at zero, the worst possible score, and one, the best possible score) and tobit regressions are presented (scores are censored at one). In columns (1) and (3), the dependent variable is the z-score of the store's average mystery shopper

score for the year and ordinary least squares regressions are presented. We used z-scores to exploit variation in the mystery shopper score, addressing the apparent leniency in this variable (the average score was 0.929 and the median score was 0.943 for the observations in Sample S1; the average score was 0.931 and the median score was 0.943 for the observations in Sample S2). *Annual Employee Turnover* is the number of terminations during the fiscal year divided by the average number of employees working at the store during the year (the sum of the beginning number of employees and the ending number of employees, divided by two). *Busyness* is the store's December 2012 monthly sales divided by monthly labor hours. *Complex Operations* is an indicator equal to 1 if the store is operating under the organization's new format, which entails greater product variety and operational complexity. *Distance to Headquarters* is the distance from the hiring store to company headquarters in miles. *Market Divergence* is measured as the sum of the absolute values of normalized differences on each location characteristic (population, income, age, ethnicity, household size) between the hiring store and the average value of the location characteristic for the chain. *Serves Repeat Customers* is an indicator equal to 1 if the hiring store is in a Census Block Group with a population density of less than 1,000 per square mile and is not within 2 miles of an interstate highway exit or US highway exit. *Busyness*, *Distance to Headquarters*, *Market Divergence* and *Serves Repeat Customers* are all calculated as of the start of our sample period and thus are time-invariant variables. *Complex Operations* is time-invariant in columns (5) through (8) since those regressions exclude from the sample stores that switched format during our sample period. *Unemployment Rate* is the yearly unemployment rate for the Census Block Group in which the store is located. *Store Count* is the number of stores in the ZIP code in the same retail chain (including the store itself). *Competition* is the number of direct competitors in the same ZIP code as the store. *Per Capita Income* is yearly income per capita in the Census Block Group where the store operates. *Size (Sqft)* is store size in square feet. *Store Age* is store age in months. *Average Team Size* is the average number of nonmanagerial employees working at the store during the year (the sum of the beginning number of employees and the ending number of employees, divided by two). *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the store as of the last month of the company's fiscal year (i.e. September).

Sample Description:
For Sample S1, we take as our starting point all stores represented in the 9,560 hires in the sub-total in Table 1 in the period January 2013 through September 2015, then drop any observations before a store's first decentralized hire in our sample period for those stores identified as switching from centralized hiring back to decentralized hiring. For stores without full observations for the fiscal year (which ends on September 30), we drop any store-months in that year (we require 9 months for the fiscal year ending on 30 September 2013 because the sample we use begins in January 2013, but 12 observations for the 2014 and 2015 fiscal years). Then for the final sample for our analyses, we retain the following observations: i) observations for stores that used the decentralized hiring model through our sample period, and (ii) observations in the fiscal years preceding the year of the switch to centralized hiring for stores that switched from the decentralized hiring model to the centralized model during our sample period. Lastly, we retain one observation (September) per fiscal year per store, yielding 567 observations.
For Sample S2, we begin with the observations from Sample S1 and then drop any observations pertaining to stores that switched to the organization's new format during the sample period (i.e. those where *Complex Operations* changed from 0 to 1 in the period), yielding 514 observations.
We limit our analyses to stores using the decentralized hiring model because any relation between turnover and mystery shopper scores may have been impacted by a switch to centralized hiring, and we show results for Sample S1 and Sample S2 because in some cases stores that had not yet switched to centralized hiring received hiring support from headquarters during the format change, which could potentially confound the relation that we study.

Robustness to Multicollinearity and Store Fixed Effects: We rerun all of the regressions in this table including store fixed effects, and excluding any time-invariant variables or variables with high VIF scores (>10, which suggests that multicollinearity would be a concern were we to retain the variables). Our results in columns (1), (2) and (4) remain robust to this alternative specification, while our result in column (3) loses significance despite reporting a slightly higher coefficient for annual employee turnover (coef.=-0.35, t=-1.43), likely due to the loss in power associated with including store fixed effects.

Appendix 2: Robustness checks for Tables 4 and 5 using the Propensity Score Matched Sample

Robustness check	*BU Local Advantage*	*Further from Headquarters*	*Higher Market Divergence*	*Serves Repeat Customers*
Reconstruct the Table 5 aggregate measures requiring the continuous variables to be above mean rather than above median.	Yes	N/A	N/A	N/A
Drop hires corresponding to treatment stores with only one decentralized or one centralized hiring during our sample period (and drop hires from the corresponding control stores).	Yes	Yes	Yes	Yes
Exclude the variable *Full-Time* due to the possibility of reverse causality. (Since this variable is measured as of a particular date, we are unsure whether a "full-time" employee was initially hired as such or converted to a full-time position).	Yes	Yes	Yes	No (p-value of 0.14, one-sided test)
Recalculate *Time to Employee Departure* as the number of days between hire date and the date the store began centralized hiring, for any employee hired under the decentralized regime at a treatment store who was still there when the store switched.[a]	Yes	No (p-value of 0.11, one-sided test)	Yes	No (p-value of 0.14, one-sided test)
Use censored normal regressions (following Hoffman et al. 2018) as an alternative to the hazard rate model.	Yes	Yes	Yes	No
Reconstruct the market divergence measure by multiplying the normalized divergences on the location characteristics rather than summing them. (Note, however, that Campbell et al. 2009 warn that the multiplication measure is more volatile and sensitive to extreme dispersion values).	Yes	Yes	No	No
Replace the above-median variables (*Busier*, *Further from Headquarters*, *Higher Market Divergence*) with their underlying continuous variables.	-	Yes	No	No
Drop hires from one store (and its corresponding match) with a particularly high market divergence value relative to other stores.	Yes	No (p-value of 0.16, one-sided test)	Yes	Yes

Note: We report the p-value if the result is not robust at the conventional 0.10 level, but reasonably close.

[a] We thank an anonymous reviewer for the suggestion to censor these employee observations in this way.

Table 1: Sample Selection

	# Employee hires
Total hires in data provided	11,897
Less employees with initial job title provided and not an entry-level position	(76)
Less hires where the type of hire (centralized or decentralized) was unknown	(270)
Less centralized hires preceding decentralized hires at a small number of stores that switched from centralized hiring back to decentralized hiring	(53)
Less hires with evidence that the employee changed stores during our sample period or where (for an unknown reason) the hire date was reset	(444)
Less hires pertaining to stores that opened during our sample period	(995)
Less hires that occurred when the store appeared to be winding down operations before closing	(3)
Less hires that occurred when the store had non-normal operations due to conversion to the new store format	(392)
Less hires pertaining to one outlier store with a considerably higher number of hires than other stores	(92)
Less hires at the store located at company headquarters	(9)
Less hires where there was only one hire at the store during the sample period	(3)
Subtotal	9,560
Less hires (occurring at times of normal operations) at stores that changed to the organization's new format during our sample period or in the three months before	(1,882)
Total observations available before propensity score matching	7,678
Less hires pertaining to stores that remained un-matched after the propensity score matching procedure	(5,373)
Less hires that had an employment duration of zero days	(18)
Total observations in our main analyses	2,287

Table 2: Propensity Score Matching

Panel A: Logit regression used to find propensity scores based on store conditions at the beginning of the sample period (n = 237 stores)

	Pr(Treated)	
	Coefficients	Z-statistics
Variable	(1)	(2)
Intercept	0.834	0.652
December 2012 Quarterly Sales	0.000	0.729
2012 Employee Turnover	-0.002	-0.536
Unemployment Rate	-0.048	-1.557
One Store	-0.075	-0.221
Competition	0.028	1.088
Per Capita Income	0.000	0.565
Size (Sqft)	-0.000	-0.097
Store Age	-0.002	-1.553
Team Size	-0.080	-1.011
% Full-time Employees	-3.839**	-2.522
State 1 (omitted)	n/a	n/a
State 2	2.036***	4.819
State 3	1.694***	3.189
Pseudo R-squared	0.144	

Note: *, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively. *December 2012 Quarterly Sales* is total store sales for the three-month period, October through December 2012, in US$. *2012 Employee Turnover* is annual employee turnover at the store for 2012, measured as the number of departed nonmanagerial employees for the year divided by the number of month-end nonmanagerial employees as of December 2012. *Unemployment Rate* is the 2013 yearly unemployment rate for the Census Block Group in which the store is located. *One Store* is an indicator equal to 1 if the store is the only store from the chain in the store's ZIP code (using stores with normal operations as of January 2013). *Competition* is the number of direct competitors in the same ZIP code as the store in 2013. *Per Capita Income* is 2013 income per capita in the Census Block Group where the store operates. *Size (Sqft)* is store size in square feet, as of October 2013. *Store Age* is store age in months, as of December 2012. *Team Size* is the number of month-end nonmanagerial employees working at the store, as of December 2012. *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the store, as of December 2012. *State 1*, *State 2*, and *State 3* are indicators for state fixed effects.

Table 2: Propensity Score Matching (Continuation)

Panel B: Covariate balance at the beginning of the sample period (n = 128 stores)				
Variable	Mean - control (1)	Mean - treatment (2)	Difference in means (3)	T-test (4)
December 2012 Quarterly Sales	411,507	397,765	13,742	0.60
Time to Employee Departure	366.58	331.70	34.88	0.60
2012 Employee Turnover	73.58	77.28	-3.70	-0.40
Unemployment Rate	8.14	7.24	0.90	1.01
Store Count	1.66	1.41	0.25	1.53
Competition	8.56	7.78	0.78	0.71
Per Capita Income	31,752	34,760	-3,007	-1.74
Size (Sqft)	2,616.31	2,562.92	53.39	0.33
Team Size	9.50	9.53	-0.03	-0.06
% Full-time Employees	0.22	0.22	-0.00	-0.07
	% control stores	% treatment stores	Difference in percentages	Chi-square
State 1	12.5%	12.5%	0%	0.549
State 2	29.69%	32.81%	-3.12%	
State 3	7.81%	4.69%	3.12%	

Note: None of the differences are significant at the 10% level, except for the difference in *Per Capita Income*, which has a p-value of 0.08. *December 2012 Quarterly Sales* is total store sales for the three-month period, October through December 2012, in US$. *Time to Employee Departure* is the number of days between the employee's hire date and exit date (censored at the final sample period exit date [in December 2015] for employees who were still active with the company at that time). *2012 Employee Turnover* is annual employee turnover at the store for 2012, measured as the number of departed nonmanagerial employees for the year divided by the number of month-end nonmanagerial employees as of December 2012. *Unemployment Rate* is the 2013 yearly unemployment rate for the Census Block Group in which the store is located. *Store Count* is the number of stores in the ZIP code in the same retail chain (including the store itself, using stores with normal operations as of January 2013). *Competition* is the number of direct competitors in the same ZIP code as the store in 2013. *Per Capita Income* is 2013 income per capita in the Census Block Group where the store operates. *Size (Sqft)* is store size in square feet, as of October 2013. *Store Age* is store age in months, as of December 2012. *Team Size* is the number of month-end nonmanagerial employees working at the store, as of December 2012. *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the store, as of December 2012. Percentages are expressed in decimals. *State 1*, *State 2*, and *State 3* represent the three states from which the stores in the sample are drawn.

Table 3: Descriptive Statistics

Panel A: Descriptive statistics for the main variables of interest (n = 2,287 employee hires)

Variable	Mean (1)	Standard deviation (2)	Min (3)	Max (4)
Time to Employee Departure	230.45	227.93	1.00	1,062.00
Treated	0.51	0.50	0.00	1.00
Post	0.52	0.50	0.00	1.00
HQ Hiring Advantage	0.32	0.47	0.00	1.00
Ex-ante Less Aligned	0.22	0.42	0.00	1.00
Busyness	98.02	19.45	48.62	152.93
Complex Operations	0.50	0.50	0.00	1.00
BU Local Advantage	0.44	0.50	0.00	1.00
Distance to Headquarters	48.38	30.86	5.49	128.33
Market Divergence	3.67	1.76	1.01	11.24
Serves Repeat Customers	0.37	0.48	0.00	1.00
Team Size	11.05	3.44	3.00	23.00
Promotion Opportunities	0.07	0.10	0.00	1.00
% Full-time Employees	0.37	0.16	0.00	1.00
Full-time	0.22	0.41	0.00	1.00

Note: *Time to Employee Departure* is the number of days between the employee's hire date and exit date (censored at the final sample period exit date [in December 2015] for employees who were still active with the company at that time). *Treated* is an indicator equal to 1 if the store switched from decentralized hiring to centralized hiring during the sample period. *Post* is an indicator equal to 1 if the employee was hired after the "treated store" or corresponding "treated store" (in the case of matched control stores) had made the switch from decentralized to centralized hiring. *HQ Hiring Advantage* is an indicator equal to 1 if headquarters has a hiring advantage relative to the store manager. That is, if 2 of the following 3 conditions are met: the store's mystery shopper score for December 2012 is less than 90 (*Ex-ante Less Aligned*=1), the store's December 2012 monthly sales divided by monthly labor hours (*Busyness*) is above median relative to the sample stores, and/or the store has *Complex Operations* (where *Complex Operations* is an indicator equal to 1 if the store is operating under the organization's new format, which entails greater product variety and operational complexity). *BU Local Advantage* is an indicator equal to 1 if the store manager has an informational advantage over the headquarters. That is, if 2 of the following 3 conditions are met: the store's distance to headquarters is above median relative to the sample stores, the store's market divergence is above median among the sample stores, and/or the store serves repeat customers. *Distance to Headquarters* is the distance from the hiring store to company headquarters in miles. *Market Divergence* is measured as the sum of the absolute values of normalized differences on each location characteristic (population, income, age, ethnicity, household size) between the hiring store and the average value of the location characteristic for the chain. *Serves Repeat Customers* is an indicator equal to 1 if the hiring store is in a Census Block Group with a population density of less than 1,000 per square mile and is not within 2 miles of an interstate highway exit or US highway exit. *Team Size* is the number of month-end nonmanagerial employees working at the hiring store at the time of hire. *Promotion Opportunities* is the percentage of employees hired in the same year in the store's ZIP code area who were promoted during our sample period. This percentage excludes the employee analyzed in our hazard rate regressions. *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the hiring store at the time of hire. *Full-time* is an indicator equal to 1 if the employee has a full-time position. Percentages are expressed in decimals.

Table 3: Descriptive Statistics

Panel B: Correlation table (n = 2,287 employee hires)

Variable	Time to Employee Departure	Treated	Post	Ex-ante Less Aligned	Busyness	Complex Operations	Distance to Headquarters	Market Divergence	Serves Repeat Customers	Team Size	Promotion Opportunities	% Full-time Employees
Time to Employee Departure	1											
Treated	-0.04**	1										
Post	-0.22***	0.10***	1									
Ex-ante Less Aligned	-0.04**	0.05**	-0.01	1								
Busyness	0.06***	-0.03	-0.16***	0.07***	1							
Complex Operations	-0.01	-0.06***	-0.06***	0.06***	-0.09***	1						
Distance to Headquarters	0.01	0.35***	-0.19***	0.01	0.29***	-0.10***	1					
Market Divergence	-0.06***	0.13***	0.00	0.44***	-0.07***	0.06***	0.18***	1				
Serves Repeat Customers	-0.03*	0.30***	0.13***	0.10***	-0.18***	0.11***	0.03	0.17***	1			
Team Size	-0.01	-0.06***	0.04**	0.03	-0.07***	0.63***	-0.05**	0.12***	0.07***	1		
Promotion Opportunities	0.08***	0.12***	-0.09***	-0.03	0.03	0.01	0.02	0.04*	-0.02	-0.08***	1	
% Full-time Employees	-0.08***	-0.04*	0.12***	-0.02	0.01	0.22***	-0.10***	-0.05**	-0.10***	-0.02	0.02	1
Full-time	0.43***	-0.05**	-0.04	0.04**	0.03	0.05***	-0.07***	-0.02	-0.04**	-0.00	0.01	0.08***

Note: *Time to Employee Departure* is the number of days between the employee's hire date and exit date (censored at the final sample period exit date [in December 2015] for employees who were still active with the company at that time). *Treated* is an indicator equal to 1 if the store switched from decentralized hiring to centralized hiring anytime during the sample period. *Post* is indicator equal to 1 if the employee was hired after the "treated store" or corresponding "treated store" (in the case of matched control stores) had made the switch from decentralized to centralized hiring. *Ex-ante Less Aligned* is an indicator equal to 1 if the store's mystery shopper score for December 2012 is less than 90. *Busyness* is December 2012 monthly store sales divided by monthly labor hours. *Complex Operations* is an indicator equal to 1 if the store is operating under the organization's new format, which entails greater product variety and operational complexity. *Distance to Headquarters* is the distance from the hiring store to company headquarters in miles. *Market Divergence* is measured as the sum of the absolute values of normalized differences on each location characteristic (population, income, age, ethnicity, household size) between the hiring store and the average value of the location characteristic for the chain. *Serves Repeat Customers* is an indicator equal to 1 if the hiring store is in a Census Block Group with a population density of less than 1,000 per square mile and is not within 2 miles of an interstate highway exit or US highway exit. *Team Size* is the number of month-end nonmanagerial employees working at the hiring store at the time of hire. *Promotion Opportunities* is the percentage of employees hired in the same year in the store's ZIP code area who were promoted during our sample period. This percentage excludes the employee analyzed in our hazard rate regressions. *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the hiring store at the time of hire. *Full-time* is an indicator equal to 1 if the employee has a full-time position. Percentages are expressed in decimals.

Table 4: Moderating Effects of Hiring Advantage of Headquarters and Information Advantage of Business Unit Manager on the Association between Treated (Centralized Hiring) and Time to Employee Departure

		Time to Employee Departure			
		Propensity Score-Matched Sample		Randomly-Matched Sample	
	Pred.	Hazard ratio	Coefficients	Hazard ratio	Coefficients
Variable		(1)	(2)	(3)	(4)
Post		1.158	0.147	1.212***	0.192***
		(1.078)	(1.078)	(2.654)	(2.654)
Treated x Post		0.628**	-0.466**	0.860	-0.151
		(-2.465)	(-2.465)	(-1.066)	(-1.066)
Post x HQ Hiring Advantage		1.241	0.216	1.508***	0.411***
		(1.109)	(1.109)	(3.171)	(3.171)
Post x BU Local Advantage		0.426***	-0.852***	0.502***	-0.689***
		(-3.312)	(-3.312)	(-5.446)	(-5.446)
Treated x Post x HQ Hiring Advantage	-	0.896	-0.109	0.504***	-0.685***
		(-0.445)	(-0.445)	(-3.180)	(-3.180)
Treated x Post x BU Local Advantage	+	3.186***	1.159***	2.510***	0.920***
		(3.707)	(3.707)	(5.057)	(5.507)
Team Size		1.020	0.020	1.036	0.035
		(0.820)	(0.820)	(1.427)	(1.427)
Promotion Opportunities		0.771	-0.260	0.918	-0.085
		(-0.755)	(-0.755)	(-0.267)	(-0.267)
% Full-time Employees		0.793	-0.232	1.080	0.077
		(-0.703)	(-0.703)	(0.301)	(0.301)
Full-time		0.228***	-1.479***	0.232***	-1.461***
		(-17.795)	(-17.795)	(-18.522)	(-18.522)
Month-year FE?		Yes	Yes	Yes	Yes
Store FE?		Yes	Yes	Yes	Yes
N		2,287	2,287	5,665	5,665

Note: Columns (1) and (2) present hazard ratios and coefficients of a Cox's proportional hazards model (we use "stcox" in Stata). A hazard ratio greater than 1 (or a positive coefficient) indicates that the variable is associated with shorter time to employee departure, whereas a hazard ratio less than 1 (or a negative coefficient) suggests the variable is associated with longer time to employee departure. Z-statistics in parentheses are based on robust standard errors clustered by store. Month-year and store fixed effects are included in all regressions by including separate indicator variables for the month-years and stores in our sample. *, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively (one-tailed for directional predictions and two-tailed otherwise). *Treated* is an indicator equal to 1 if the store switched from decentralized to centralized hiring during the sample period. *Post* is an indicator equal to 1 if the employee was hired after the "treated store" or corresponding "treated store" (in the case of matched control stores) had made the switch from decentralized to centralized hiring. *HQ Hiring Advantage* is an indicator equal to 1 if headquarters has a hiring advantage relative to the store manager. That is, if 2 of the following 3 conditions are met: the store's mystery shopper score for December 2012 is less than 90 (*Ex-ante Less Aligned*=1), the store's December 2012 monthly sales divided by monthly labor hours (*Busyness*) is above median relative to the sample stores, and/or the store has *Complex Operations* (where *Complex Operations* is an indicator equal to 1 if the store is operating under the organization's new format, which entails greater product variety and operational complexity). *BU Local Advantage* is an indicator equal to 1 if the store manager has an informational advantage over the headquarters. That is, if 2 of the following 3 conditions are met: the store's distance to headquarters is above median relative to the sample stores, the store's market divergence is above median among the sample stores, and/or the store serves repeat customers. *Team Size* is the number of month-end nonmanagerial employees working at the hiring store at the time of hire or store-month. *Promotion Opportunities* is the percentage of employees hired in the same year in the store's ZIP code area who were promoted during our sample period. This percentage excludes the employee analyzed in our hazard rate regressions. *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the hiring store at the time of hire or store-month. *Full-time* is an indicator equal to 1 if the employee has a full-time position.

Table 5: Moderating Effects of Factors Affecting the Association between Treated (Centralized Hiring) and Time to Employee Departure

		Time to Employee Departure			
		Propensity Score-Matched Sample		Randomly Matched Sample	
	Pred.	Hazard ratio	Coefficients	Hazard ratio	Coefficients
Variable		(1)	(2)	(3)	(4)
Post		1.413	0.346	1.420**	0.351**
		(1.501)	(1.501)	(2.396)	(2.396)
Treated x Post		0.580*	-0.545*	0.831	-0.185
		(-1.668)	(-1.668)	(-0.821)	(-0.821)
Post x …					
… Ex-ante Less Aligned		1.674*	0.515*	1.784***	0.579***
		(1.932)	(1.932)	(2.787)	(2.787)
…Busier		1.090	0.086	1.167	0.155
		(0.355)	(0.355)	(1.168)	(1.168)
…Complex Operations		1.098	0.093	1.049	0.048
		(0.416)	(0.416)	(0.333)	(0.333)
…Further from Headquarters		0.579**	-0.547**	0.767**	-0.265**
		(-2.035)	(-2.035)	(-1.960)	(-1.960)
…Higher Market Divergence		0.591**	-0.526**	0.649***	-0.432***
		(-2.206)	(-2.206)	(-3.018)	(-3.018)
…Serves Repeat Customers		0.664	-0.409	0.616***	-0.484***
		(-1.565)	(-1.565)	(-3.017)	(-3.017)
Treated x Post x …					
…Ex-ante Less Aligned	-	0.659	-0.416	0.483***	-0.728***
		(-1.216)	(-1.216)	(-2.928)	(-2.928)
…Busier	-	0.715	-0.335	0.742*	-0.299*
		(-1.119)	(-1.119)	(-1.596)	(-1.596)
…Complex Operations	-	0.942	-0.060	0.852	-0.160
		(-0.219)	(-0.219)	(-0.798)	(-0.798)
…Further from Headquarters	+	1.645*	0.498*	1.126	0.119
		(1.498)	(1.498)	(0.650)	(0.650)
…Higher Market Divergence	+	2.160***	0.770***	1.671***	0.513***
		(2.410)	(2.410)	(2.612)	(2.612)
…Serves Repeat Customers	+	1.641*	0.495*	2.126***	0.754***
		(1.566)	(1.566)	(3.502)	(3.502)
Team Size		1.020	0.020	1.035	0.035
		(0.801)	(0.801)	(1.372)	(1.372)
Promotion Opportunities		0.743	-0.297	0.896	-0.110
		(-0.886)	(-0.886)	(-0.359)	(-0.359)
% Full-time Employees		0.760	-0.274	1.070	0.068
		(-0.842)	(-0.842)	(0.262)	(0.262)
Full-time		0.227***	-1.484***	0.231***	-1.463***
		(-17.913)	(-17.913)	(-18.714)	(-18.714)
Month-year FE?		Yes	Yes	Yes	Yes
Store FE?		Yes	Yes	Yes	Yes
N		2,287	2,287	5,665	5,665

Note: Columns (1) and (2) present hazard ratios and coefficients of a Cox's proportional hazards model (we use "stcox" in Stata). A hazard ratio greater than 1 (or a positive coefficient) indicates that the variable is associated with shorter time to employee departure, whereas a hazard ratio less than 1 (or a negative coefficient) suggests the variable is associated with longer time to employee departure. Z-statistics in parentheses are based on robust standard errors clustered by store. Month-year and store fixed effects are included in all regressions by including separate indicator variables for the month-years and stores in our sample. *, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively (one-tailed for directional predictions and two-tailed otherwise). Variables are defined in the appendix. *Treated* is an indicator equal to 1 if the store switched from decentralized to centralized hiring during the sample period. *Post* is an indicator equal to 1 if the employee was hired after the "treated store" or corresponding "treated store" (in the case of matched control stores) had made the switch from decentralized to centralized hiring. *Ex-ante Less Aligned* is an indicator equal to 1 if the store's mystery shopper score for December 2012 is less than 90. *Busier* is an indicator equal to 1 if December 2012 monthly store sales divided by monthly labor hours (*Busyness*) is above median relative to the sample stores. *Complex Operations* is an indicator equal to 1 if the store is operating under the organization's new format, which entails greater product variety and operational complexity. *Further from Headquarters* is an indicator equal to 1 if the distance from the hiring store to company headquarters in miles is above median relative to the sample stores. *Higher Market Divergence* is an indicator equal to 1 if *Market Divergence*, measured as the sum of the absolute values of normalized differences on each location characteristic (population, income, age, ethnicity, household size) between the hiring store and the average value of the location characteristic for the chain, is above median among the sample stores. *Serves Repeat Customers* is an indicator equal to 1 if the hiring store is in a Census Block Group with a population density of less than 1,000 per square mile and is not within 2 miles of an interstate highway exit or US highway exit. *Team Size* is the number of month-end nonmanagerial employees working at the hiring store at the time of hire or store-month. *Promotion Opportunities* is the percentage of employees hired in the same year in the store's ZIP code area who were promoted during our sample period. This percentage excludes the employee analyzed in our hazard rate regressions. *% Full-time Employees* is the percent of month-end nonmanagerial employees working full-time at the hiring store at the time of hire or store-month. *Full-time* is an indicator equal to 1 if the employee has a full-time position.

Table 6: Effect of Treated (Centralized Hiring) on Time to Employee Departure – Hazard Ratios Observed in Subsample Splits

Variable	*BU Local Advantage* =0	*BU Local Advantage* =1	*Further from Headquarters* =0	*Further from Headquarters* =1	*Higher Market Divergence* =0	*Higher Market Divergence* =1	*Serves Repeat Customers* =0	*Serves Repeat Customers* =1
Post	1.164	0.638	1.197	0.742	1.220	0.974	1.090	1.088
	(1.127)	(-1.603)	(1.171)	(-1.202)	(1.127)	(-0.135)	(0.592)	(0.291)
Treated x Post	0.624***	1.764**	0.819	1.238	0.667**	1.051	0.708**	1.133
	(-2.753)	(2.160)	(-0.872)	(0.871)	(-1.987)	(0.268)	(-1.973)	(0.450)
Team Size	1.021	1.023	1.005	1.040	1.070*	1.006	1.005	1.034
	(0.601)	(0.658)	(0.146)	(1.259)	(1.775)	(0.194)	(0.148)	(0.965)
Promotion Opportunities	0.497*	1.354	0.315**	1.189	0.860	0.659	0.724	0.789
	(-1.648)	(0.524)	(-2.253)	(0.398)	(-0.323)	(-0.847)	(-0.772)	(-0.470)
% Full-Time Employees	0.745	0.597	0.729	0.673	1.324	0.603	0.681	0.814
	(-0.853)	(-0.860)	(-0.862)	(-0.652)	(0.580)	(-1.105)	(-1.187)	(-0.303)
Full-time	0.225***	0.224***	0.215***	0.227***	0.214***	0.230***	0.233***	0.211***
	(-12.270)	(-13.206)	(-15.102)	(-10.160)	(-9.652)	(-16.123)	(-13.304)	(-12.551)
Month-Year FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Store FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	1,272	1,015	1,116	1,171	1,027	1,260	1,440	847

Note: Columns present hazard ratios of a Cox's proportional hazards model (we use "stcox" in Stata). A hazard ratio greater than 1 indicates that the variable is associated with shorter time to employee departure, whereas a hazard ratio less than 1 suggests the variable is associated with longer time to employee departure. Z-statistics are based on robust standard errors clustered by store. Month-year and store fixed effects are included in all regressions by including separate indicator variables for the month-years and stores in our sample. *, **, *** denote significance at a 0.10, a 0.05 and a 0.01 level respectively. *BU Local Advantage* is an indicator equal to 1 if the store manager has an informational advantage over the headquarters. This is, if 2 of the following 3 conditions are met: the store's distance to headquarters is above median relative to the sample stores, the store's market divergence is above median among the sample stores, and/or the store serves repeat customers. *Further from Headquarters* is an indicator equal to 1 if the distance from the hiring store to company headquarters in miles is above median relative to the sample stores. *Higher Market Divergence* is an indicator equal to 1 if *Market Divergence*, measured as the sum of the absolute values of normalized differences on each location characteristic (population, income, age, ethnicity, household size) between the hiring store and the average value of the location characteristic for the chain, is above median among the sample stores. *Serves Repeat Customers* is an indicator equal to 1 if the hiring store is located in a Census block group with a population of less than 1000 people per square mile and is not located within 2 miles of an Interstate Highway exit or U.S. Highway exit. *Treated* is an indicator equal to 1 if the store switched from decentralized hiring to centralized hiring anytime during the sample period. *Post* is an indicator equal to 1 if the employee was hired after the "treated store" or corresponding "treated store" (in the case of matched control stores) had made the switch from decentralized hiring to centralized hiring. *Team Size* is the number of month-end nonmanagerial employees working at the hiring store at the time of hire. *Promotion Opportunities* is the percentage of employees hired in the same store's ZIP code area and year that were promoted during our sample period. This percentage excludes the employee analyzed in our hazard rate regressions. *% Full-Time Employees* is the percent of month-end nonmanagerial employees that are working full-time at the hiring store at the time of hire. *Full-time* is an indicator equal to 1 if the employee has a full-time position.