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Three Essays on Management and Organization

by

Bryan Abraham Hong

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

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in the

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of the

University of California, Berkeley

Committee in charge:

Professor Rui de Figueiredo, Chair

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Bryan Abraham Hong

Abstract

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Doctor of Philosophy in Business Administration

University of California, Berkeley

Professor Rui de Figueiredo, Chair

This dissertation examines how managers influence firm behavior and performance. Managers play an important role in the performance and activities of firms, given their decision-making role within organizations. I conduct three separate empirical analyses examining specific factors that influence the impact that managers have on firm behavior and performance.

The first chapter investigates the following question: How does the performance impact of supervisor changes differ across levels in a hierarchy? In my results, I find that supervisor changes at higher levels result in more severe performance declines relative to lower levels in the hierarchy, even when accounting for differences in span of control. The findings suggest that reassignment and turnover of managers at higher levels may be more costly for firms, independent of their ability and other individual characteristics.

The second chapter examines the following: What is the effect of replacing experienced managers with rookie managers on firm performance? And, how does this change if they are instead replaced with experienced managers? At the individual store level, I observe the behavior and performance effects of management changes when successors are newly promoted store managers, and compare this to changes where successors are experienced store managers that are reassigned. In my results, newly promoted store managers systematically cut costs that briefly lead to profit increases, but ultimately result in profit declines in subsequent months. By contrast, successors that have prior experience as a manager do not make any changes observable in my data, and I find no evidence of performance changes. These findings suggest that inexperienced managers within firms may engage in well-intentioned behavior that may be costly for firms, at least in the short run. However, managerial experience may reduce the likelihood that the same costly behavior is repeated. The results shed additional insight into how managerial experience may matter for performance, and provide a tangible estimate of the performance costs of being a rookie manager.

In the final essay, I investigate the influence of top managers on corporate social responsibility (CSR). A growing body of literature suggests that individual managers may play a critical role in determining corporate social responsibility (CSR) activities. However, attempts to quantitatively measure the individual influence managers have on CSR face significant empirical challenges.

Estimation methods unable to adequately control for firm-specific factors influencing CSR are likely to overstate the importance of individual managers in their findings. To address these concerns, I use an identification approach allowing for the simultaneous estimation of manager and firm fixed effects, and provide quantitative estimates of the degree to which individual managers might influence CSR. The results suggest that managers do exert some degree of individual influence on CSR outside of firm-specific factors, but that the magnitude of their effect is relatively small. Also, when managers switch firms, I find no evidence of a relationship between their influence on CSR in their first and second firm, suggesting that managers do not exert a persistent influence on CSR independent of the firm where they are employed.

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Chapter 1

The Cost of a New Boss: Supervisor changes, hierarchy, and firm performance

1 Introduction

Within firms, workers may experience supervisor changes as a result of personnel reassignments within the organization, or the replacement of managers over time. Although prior literature has recognized the importance of managerial turnover in affecting firm performance¹, few studies have empirically examined the degree to which the cumulative experience of managers and their subordinates working together may contribute to these effects. Also, little is known about how variation within the internal structure of organizations influences the effect of supervisor changes. Given that a critical function of the structure of organizations is to govern the productive activities performed by its workers, variation in structure may play a significant role in determining the performance impact of supervisor changes. Examining how an organization's internal structure influences these effects is important for understanding the conditions under which maintaining stability in the formal working relationships between managers and subordinates is vital for performance.

This paper empirically estimates the effect of supervisor changes on firm performance, and considers how one important dimension of variation within an organization's structure—a manager's level in an organizational hierarchy— influences the magnitude of these effects. The theoretical basis for this study draws primarily from the economics, organizational learning and organization theory literatures. Within the economics literature, components of human capital specific to the teams or networks individuals belong to are discussed (Mailath and Postlewaite 1990, Chillemi and Gui 1997), although little attention has been given in this literature to the actual performance implications of these types of human capital. In the literature on organizational learning, recent evidence suggests increasing cumulative experience of individuals working together leads to improved group performance (Reagans et al. 2005, Huckman et al. 2009). When considering this effect in the context of supervisor changes, the component of performance specific to this experience may be lost when supervisor changes occur, especially if the benefits of cumulative experience cannot be easily transferred (Argote 1999).

In the organization theory literature, the use of codified rules has been proposed as a restraining force on the degree to which individual workers are able to influence activities in idiosyncratic ways (Blau 1955, Selznick 1957). Within organizational hierarchies, the use of codified rules varies across different levels. At the lowest level of a hierarchy, workers are characterized as being governed by well-defined, codified rules, while managers at progressively higher levels are directed by fewer rules (Selznick 1963, Daft and Lengel 1984). Considering the interaction between the component of performance specific to cumulative experience and the use of codified rules within organizational hierarchies offers a testable proposition—given that

¹ See Fazel and D'Itri (1997) for a literature review.

managers at upper levels within a hierarchy have fewer codified rules to dictate their activities and are allowed greater discretion in their activities, the experience of managers and their subordinates working together may play a more important role in affecting performance. As a consequence, supervisor changes at upper levels should lead to more adverse performance consequences relative to supervisor changes at lower levels.

In most empirical settings, comparing performance effects at different levels of management in an organizational hierarchy is confounded by structural differences that give managers at higher levels greater influence over firm performance. Unless these differences are addressed, supervisor changes at upper levels in a hierarchy are likely to have more impact on performance simply due to greater assigned responsibility over firm outcomes. To control for these differences, this study considers an approach that exploits the unique characteristics of the empirical setting used here. To compare the performance impact of supervisor changes at different layers in the hierarchy, all performance effects are estimated at the level of individual business units, where only one manager from each layer of the hierarchy is assigned to each business unit. By considering the effect of supervisor changes only at the level of individual business units, differences in assigned responsibility can be ruled out as a possible explanation for the empirical findings shown here.

The empirical setting for this study is the US company-owned operation of a quick-service restaurant firm, hereafter referred to as RestaurantCo. The quick-service restaurant industry is an important sector of the economy, with the U.S. sales of the 50 largest firms totaling over \$139 billion in 2009 (QSR Magazine 2010). The use of codified rules to dictate production activities is extensive within quick-service restaurant organizations, although significant variation exists in their use even across the lowest levels of the hierarchy. RestaurantCo is one of the largest firms in the industry, with several billion dollars in annual sales operating in over 30 countries with a globally recognized brand. With a large number of similar business units using identical performance metrics, performance changes can be relatively easily compared within the organization. The contribution of individual managers to firm performance is also more easily identified due to the standardization of job functions within each level of the hierarchy, alleviating concerns of unobserved differences in assigned tasks, objectives, or resource support provided by the firm confounding estimation. The standardization of operating procedures is especially pronounced in the context of RestaurantCo, which rigorously trains its workforce to enforce high levels of consistency in production and management activities. Taken together with the high frequency of supervisor changes that normally occur within organizations in this industry, RestaurantCo provides a compelling setting to estimate the performance impact of supervisor changes at different levels of the organizational hierarchy.

Within RestaurantCo, supervisor changes at two levels of management are considered: store managers, who supervise workers directly engaged in production and actively manage the production process in real time; and district managers, who supervise store managers, and are primarily occupied with monitoring performance and remotely issuing commands to subordinates. Workers below the store manager level have highly standardized training and well-defined tasks governed by codified rules. Store managers themselves also have standardized training, but engage in tasks that are less defined by codified rules, and are given a wider degree of discretion in performing their role. Similarly, district managers have fewer codified rules to govern their activities, and greater levels of discretion. Given this variation in the use of codified rules across levels in the hierarchy, changes in supervising district manager are predicted to be more adverse for store performance relative to store manager changes.

I find that changes in supervising district manager result in a 3% average decline in gross profits immediately following the supervisor change, while store manager changes exhibit no statistically significant effect on performance. Performance declines resulting from changes in supervising district managers are attenuated when subordinate store managers have relatively little experience working with their previous supervisor, and the larger the previous supervisor's span of control. No evidence of a relationship was found between the degree of performance declines and replacing district manager's span of control.

This paper makes several contributions. First, it provides a direct empirical estimate of the effect of supervisor changes on firm profits. This goes beyond answering the general question of whether the experience of managers and subordinates working together matters for firm performance by also addressing *how much* it matters. However, the results found in this paper demonstrate only the short run performance cost, and not the full consequence of supervisor changes. Firms may reassign managers optimally for a net gain in performance over a longer time horizon, or consider other factors above the business unit level. Second, it suggests that the internal structure of organizations may be an important factor when considering the importance of managerial stability for firm performance. The results also have important implications for managerial practice. Firms that reassign managers within the organization should consider preserving working relationships between managers and subordinates at upper levels in the hierarchy, but can be less concerned with individual movements at lower levels. Organizations may also be forced to make performance tradeoffs between the ability of managers and the value of their existing working relationships, and in some cases may actually be better off not terminating managers of lower ability at upper levels in the hierarchy.

In the next section, I examine the theories and literature relevant to linking supervisor changes, a manager's level within an organizational hierarchy, and firm performance. Section 3 discusses the empirical setting and data. Section 4 details the empirical identification strategy. Section 5 presents and discusses the results, with concluding remarks offered in the final section.

2 Supervisor changes, Codified Rules, and Performance

Predicting the relationship between the impact of supervisor changes on firm performance and a manager's level in the hierarchy integrates the distinct theoretical literatures addressing each phenomenon. The relevant theories describing each phenomenon are presented separately here first, then considered together to develop the hypotheses.

2.1 Supervisor changes and firm performance

Understanding the effect of supervisor changes on firm performance begins with the notion that some degree of performance is dependent upon the cumulative experience of managers and subordinates working together. Within the economics literature, human capital theory distinguishes between general and specific types of human capital, and provides the foundation for predicting differences in individual worker performance (Becker 1962). Several variations of specific human capital have been proposed, including firm-specific (Becker 1962), task-specific (Gibbons and Waldman 2004), network-specific (Mailath and Postlewaite 1990), and team human capital (Chillemi and Gui 1997). Of these, network-specific and team human capital are the most closely related to the manager-subordinate context being explored in this paper. Mailath and Postlewaite (1990) argue that firms can be viewed as a network of workers, where

workers “know whom to contact about particular problems that may arise and know the strengths and weaknesses of their co-workers. A worker has substantial network specific human capital, which is of no value outside this network.” (pp. 369-370) Chillemi and Gui (1997) offer a similar argument for teams of workers, defining team human capital as a “nonmaterial asset” that is a collection of “effective customs developed by the team members themselves.” (p. 567) A central theme of these two types of specific human capital is the importance of familiarity between workers in determining their economic productivity.

The importance of experience working together among individuals is also highlighted in prior work relating team familiarity and performance (Edmonson et al. 2001, Moreland et al. 1998). If some portion of the knowledge required for effective team coordination is tacit or difficult to transfer, then increasing familiarity among team members may improve performance (Polanyi 1967, von Hippel 1994, Monteverde 1995). Workers may also become increasingly willing to engage with other members of the team as shared experience increases, improving communication among team members (Granovetter 1985, Uzzi 1997, Levin and Cross 2004). Consistent with the human capital literature, these theories generally predict that increasing cumulative experience of individuals working together improves team performance.

Similar to the empirical literature examining the effect of cumulative experience on performance at the organizational level (Wright 1936, Argote et al. 1990), empirical studies addressing the effect of cumulative experience within teams on performance generally find a positive relationship. Using a teaching hospital as their setting, Reagans et al. (2005) find that the performance of workers engaged in surgery improves with increasing cumulative experience working together. Weick and Roberts (1993) reach a similar conclusion when explaining the high reliability of operations on aircraft carrier flight decks, attributing their high degree of performance to the development of a “collective mind.” Huckman et al. (2009) examine the context of fluid teams within an Indian software services firm, and also find a positive relationship between increasing team familiarity and improved performance.

If the relationship found in these studies is symmetric, supervisor changes may result in negative performance consequences if the benefits gained from the experience of managers and subordinates working together cannot be completely transferred from one manager-subordinate combination to another. Transferring knowledge of idiosyncratic human interactions is argued to be more difficult relative to transferring knowledge of tools or tasks (Argote and Ingram 2000), suggesting that supervisor changes may result in the loss of at least some portion of this knowledge. Supervisor changes may also lead to individuals being less willing to communicate at initially lower levels of cumulative experience working together, resulting in adverse performance effects.²³

From this theoretical framework the first set of formal hypotheses are derived:

Hypothesis 1A: *Lower-level supervisor changes result in operating performance declines.*

Hypothesis 1B: *Higher-level supervisor changes result in operating performance declines.*

² Groysberg et al. (2008) finds results that are closely related—star knowledge workers who switch firms together with colleagues experience mitigated performance declines.

³ Several studies have also proposed and found evidence for potential costs of increasing experience. (Katz 1982, Berman et al. 2002) However, incremental negative returns to team tenure found by Katz (1982) did not occur until after five years of working together. Based on these results, the tenure of working relationships in the empirical setting used for this study is unlikely to be sufficiently long enough that the entire benefit of cumulative experience is completely negated.

2.2 Codified rules, organizational hierarchies, and firm performance

Within organizations, the use of codified rules defines to at least some degree how individual members of an organization interact with each other, and allows each member to better anticipate the behavior of others (Merton 1940). The use of these rules is also argued to limit the discretion available to workers in performing their duties, ultimately reducing the scope of their ability to idiosyncratically influence performance (Blau 1955, Selznick 1957). From a knowledge-based perspective, codified rules have been widely recognized as a mechanism of storing knowledge within organizations (Cyert and March 1963, Nelson and Winter 1982, Levitt and March 1988, Gersick and Hackman 1990, Cohen and Bacdayan 1994, Feldman and Pentland, 2003). If a greater degree of knowledge is stored within these rules, then individual-specific knowledge is argued to become less relevant for performance (Mowday et al. 1982, Rao and Argote 2006). A common premise across these studies is the existence of a tradeoff between the use of codified rules to dictate worker activities and the degree to which idiosyncratic characteristics of individual workers matter for organizational performance. As codified rules are implemented to a greater degree to govern and standardize worker activities, performance becomes less dependent upon any specific individual within the organization.

In related empirical literature on worker turnover, several studies have proposed similar explanations to account for their findings of mitigated effects of worker turnover (Argote et al. 1990, Shaw et al. 2005a, Shaw et al. 2005b). Ton and Huckman (2008) examine the performance consequences of turnover when store managers vary in their adherence to standardized operating procedures in a retail store chain, and find that greater adherence to prescribed procedures mitigates the negative effects of turnover. Rao and Argote (2006) also find supporting evidence in a laboratory setting, arguing that explicit definition of tasks and routines directly embeds production knowledge into the organizational structure, and mitigates performance losses resulting from turnover.

Within organizational hierarchies, the use of codified rules varies depending on the level of hierarchy being considered. Formal models of hierarchy in the economics literature typically describe a layer of production workers at the bottom of the hierarchy, who are influenced by workers at higher levels in the organization (Williamson 1967, Rosen 1982). The activities of workers at lower levels in the hierarchy are primarily defined by codified rules, which describe in detail the steps necessary to complete productive tasks (Selznick 1963, Daft and Lengel 1984). At successively higher levels, worker roles are less defined by codified rules, and workers have greater degrees of discretion in performing their responsibilities (Selznick 1963, Daft and Lengel 1984).

2.3 Influence of codified rules on the performance impact of supervisor changes

This paper builds upon the theoretical literatures addressing the function of codified rules within organizations and the performance effects of supervisor changes by proposing that the use of these rules diminishes the negative impact of supervisor changes on firm performance. If supervisor changes adversely affect performance due to the loss of knowledge relevant for coordination between workers, then codified rules may be an alternative repository for much of the knowledge that would otherwise be specific to the experience of managers and subordinates working together. Standardization of tasks through the use of codified rules may also make knowledge gained through experience less critical for performance if managers and subordinates

are homogeneous in their activities and unable to exercise significant discretion (Argote 1999). If true, then the use of codified rules may diminish performance losses resulting from supervisor changes.

In the alternative view that increasing experience working together improves performance by increasing the willingness to engage in relationships with other workers, the use of codified rules may reduce the degree of trust necessary to achieve a given level of performance. In the context of organizations, trust has been proposed as an important organizing principle when formal procedures are costly or difficult to use (McEvily et al. 2003). Codified rules determined at the organizational level may reduce uncertainty regarding the possible behavior of other workers, and enable workers to more openly communicate with each other at lower levels of trust (Merton 1940). If coordination requires lower levels of trust, then the accumulation of experience working together may be less valuable for performance, and the adverse performance consequences of supervisor changes may also be mitigated.

If the use of codified rules is less prevalent at upper levels in the hierarchy relative to lower levels, then supervisor changes should have more adverse performance effects at higher levels. This prediction is presented formally here:

Hypothesis 2: *Supervisor changes at upper levels in the hierarchy result in more severe operating performance declines relative to supervisor changes at lower levels.*

3 Empirical Setting and Data

RestaurantCo is a global chain of quick-service restaurants, selling a variety of food and beverage products to consumers. The firm's US-based company-owned stores provide the empirical setting for this study. Within the organization, store and district managers are typically the only two levels of management within the hierarchy that directly influence operating performance at the individual store level.⁴ The structure of the relevant hierarchy being considered is displayed in Figure 1.1.

Each store is assigned a single store manager with authority over all routine operating decisions, including scheduling labor, managing inventory, training employees, and hiring and firing of all subordinates. Store managers typically spend roughly 30 hours per week directly managing the production process during store operating hours, and 10 hours per week on administrative responsibilities, including budgeting and reporting. When store managers are not physically present to manage operations, staffing of labor is decided in advance. All non operations-related decisions, including advertising, pricing, and capital expenditures, are either made centrally at corporate headquarters or by senior management at the regional level. Store managers have two levels of subordinates, assistant managers and crew employees, both of which report directly to the store manager. Stores have on average one assistant manager and between 10 to 40 crew employees, depending on average store sales volume.

Workers within a store can experience changes in supervising store manager as a result of the previous store manager exiting the firm, getting promoted, or being reassigned. Exits from

⁴ District managers report to Regional Managers, who oversee 80 stores on average. Managers at this level are primarily engaged in reporting to senior management and managing operating personnel issues. Typical responsibilities include redefining geographic districts; hiring and terminating district managers; approving store manager hires, terminations and reassignments; and controlling labor costs, which are already directly accounted for explicitly in the regression specification.

the firm are typically voluntary and not related to store performance trends.⁵ The reassignment of store managers to other stores is primarily based upon the experience level and past performance of store managers, and is conducted at the discretion of supervising district managers. As a general rule, assignments are designed to match store sales levels with the experience level of the store manager. More experienced store managers are assigned to stores with higher sales volumes, and less experienced managers to lower volume stores. Store managers may be reassigned to progressively higher volume stores over the course of their career, and are typically not given any choice in the assignment process. Also, the firm has no policy of reassigning managers based upon store performance trends. For example, experienced managers are not systematically reassigned to underperforming stores to improve them, or given outperforming stores as a reward. The general assignment process of store managers follows a straightforward path--as store managers gain experience and maintain an adequate level of performance, they are given more responsibility by being reassigned to stores with larger sales volumes. Figure 1.2 provides a more detailed decomposition of store manager changes.

District managers are responsible for the operations of 9 stores on average within a given geographic area, which are referred to as districts. RestaurantCo has a stated firmwide policy of assigning less than ten stores to each district, with supervising regional managers determining the assignment of stores to districts based upon geographic proximity.⁶ The primary work activities of a district manager include analyzing daily performance data from each store, issuing specific commands to store managers to improve performance or execute corporate initiatives, and reporting aggregated operating results to superiors within the hierarchy. Individual stores may be visited as often as once per week, depending upon business requirements. Direct interactions with subordinates are generally limited only to store managers. District managers have the authority to hire and terminate all subordinates, although this is typically only relevant for the store managers under their span of control.

Changes in supervising district managers can occur in one of three ways: 1) departure of the previous district manager, either through firm exit or promotion to a higher level, resulting in the arrival of a replacement district manager; 2) reassignment of a store manager to another store supervised by a different district manager; and 3) reassignment of district managers to other stores. The reassignment of district managers is motivated by new store openings, coupled with the firm's policy of limiting the number of stores assigned to each district. As new stores are built, some stores are redistricted to comply with the firm's policy, while at the same time attempting to minimize the geographic distance between stores within each district. This third case is the most common explanation for changes in supervising district managers in the empirical setting used here, as shown in Figure 1.3.

The use of codified rules differs significantly by each level of RestaurantCo's hierarchy considered in this study. At both the crew and assistant manager levels, virtually all activities ranging from food production to cleaning are governed by standardized, well-defined procedures. Each new hire at the crew level is trained for approximately 40 hours in the procedures necessary to fulfill their role, and is tested through a series of both written and practical exams. New hires at the crew level are not permitted to engage in the production of

⁵ This was according to interviews with RestaurantCo employees. As an additional test of the validity of this claim, I compare RestaurantCo's published voluntary turnover rate in the same fiscal year to an estimate of total turnover implied in my data, and find that they are similar.

⁶ In practice, there is some variation in the span of control for district managers, as shown in the descriptive statistics.

food and beverage items until all exams are passed according to firmwide objective criteria. Assistant store managers are typically former crew workers and also engage in similar tasks, but may reallocate crew task assignments to optimize production when store managers are not physically present.

Store managers have considerably fewer codified rules governing their activities relative to subordinates. For labor scheduling, a standard minimum staffing requirement is enforced across all stores, but managers are otherwise given discretion to determine the actual labor levels needed to operate the store optimally. Procedures for the hiring and termination of subordinate workers are also not strictly defined, although desirable worker characteristics such as “willingness to meet customer needs” are identified as important criteria. In general, most store manager responsibilities are characterized more by general guidelines than precise, detailed procedures. District managers have even fewer rules to guide their daily activities, and exercise relatively greater discretion in the way they perform their job function. The method, frequency, and timing of communication to each subordinate store manager are not well-defined by codified rules, and considerable variation exists in how different district managers interact personally with subordinates.⁷

The data used in this study contains RestaurantCo’s internal performance and manager staffing data for approximately 7,100 stores from October 2007 through June 2008 in a monthly panel. Financial data at the store level includes monthly sales, transaction count, cost of goods sold, and labor expenditures.⁸ Manager assignments to stores are recorded for each month at all levels in the hierarchy. For store managers, tenure at the firm, at the store manager level, and as manager at the currently assigned store are all recorded. Additional relevant data include monthly sales and operating profit bonus targets for each store, capital expenditures⁹, store employee turnover rates, and store characteristics such as format type (e.g. drive-thru).

4 Empirical Strategy

4.1 Sample selection

Given the short panel period and structure of the data, several adjustments to the sample were made to provide the clearest comparisons for estimation. First, store months with less than three weeks of operating activity were dropped to reduce measurement error. Also, months in which store manager changes occur are dropped, since the performance in these months reflects both contributions of the previous and replacing store managers, creating measurement error in the estimation of manager fixed effects.¹⁰

In a small fraction of cases, both store managers and stores experience months with no assigned supervisor. For these instances, subsequent performance after a replacing supervisor arrives may reflect unusual circumstances as a result of prolonged activity with no supervisor,

⁷ This is a consistent observation from interviews with multiple store managers.

⁸ Monthly sales, transaction count, and operating profits are available from October 2004 through June 2008.

⁹ Capital expenditure amounts, types and dates are provided at the store level from October 2004 through June 2008.

¹⁰ This procedure was not done for district manager supervisor changes, as each district manager typically has a significant number of observations to identify their fixed effect (on average 9 observations for each month), so the single store-month containing the activity of the two district managers is unlikely to provide significant measurement error. In the case of store managers, the primary concern is that very few observations exist in the panel to identify store manager fixed effects.

confounded with the ability and effort of the newly arriving manager. To address this concern, both the months during which no supervisor is assigned and all subsequent months are dropped from the sample.

To capture the average effect of a single supervisor change with the maximum possible time window of observation and avoid possible interactions of multiple instances of treatment, all subsequent supervisor changes after an initial supervisor change during the panel period were dropped from the sample for both store managers and stores.¹¹ By the same rationale, the few remaining observations reflecting more than one district manager change for a store were also dropped.¹² The final sample used in this study consists of 6,934 stores, 7,864 store managers, and 888 district managers.¹³

4.2 Identification and Estimation

In principle, the ideal experiment to identify the effect of supervisor changes on firm performance would be to randomly reassign district managers and store managers to different stores, and observe the subsequent performance of both groups relative to store managers and stores that experience no supervisor changes. However, supervisor changes occur in a variety of ways within RestaurantCo, and are unlikely to occur randomly. For example, district managers with greater ability might be systematically assigned to store managers of higher (or possibly lower) ability. Store managers of higher ability may also be reassigned to stores with higher sales volumes, potentially confounding the comparison between reassigned managers to those who are not reassigned. Many of these (often unobservable) factors that may confound identification are likely to vary by store, store manager, or district manager, but remain fixed over time. Fortunately, the structure of panel data allows these types of factors to be controlled for through the use of fixed effects in a difference-in-differences regression approach.

The differences-in-differences approach used here is a four-way fixed effects model, controlling for both observable and unobservable time-invariant store, store manager, and district manager characteristics that may affect gross profits, as well as any common time-varying factors that may impact all stores and managers within each month. Variables that are likely to affect gross profits such as store location, manager ability, and common seasonal changes in demand are all controlled for through the use of these fixed effects. In principle, the gross profit trends of the control groups—which include store managers that never change supervisors and stores that never experience a store manager change—represent what would have happened to both groups if no supervisor change had occurred.

I formally present the baseline differences in differences regression model used here:

¹¹ Supervisor changes in the beginning months of the panel period may also be confounded by the interaction of multiple supervisor changes occurring just prior to the beginning of the panel. To test whether this may be influencing the results, supervisor changes occurring in the second, third, and fourth months of the panel period are dropped, and the baseline regression is estimated on the adjusted sample, producing similar results at the same levels of significance.

¹² This accounts for a small number of instances when district managers may change more than once during the panel period, but store managers assigned to the store may not experience a supervisor change. This occurs because store managers also change during the panel period.

¹³ This represents 98% of the stores, 94% of the store managers, and 97% of the district managers in the total sample.

$$PROFIT_{ijkt} = \alpha_1 STORE\ MGR\ CHG_{ijkt} + \alpha_2 DM\ SUPERVISOR\ CHG_{ijkt} + \beta CONTROLS_{ijkt} + STORE_i + STORE\ MGR_j + DISTRICT\ MGR_k + MONTH_t + \varepsilon_{ijkt},$$

where PROFIT is the log average weekly gross profit for store i , store manager j , and district manager k in month t .¹⁴ STORE MGR CHG and DM SUPERVISOR CHG are indicator variables for the time period following a change in supervising district manager and store manager, respectively. CONTROLS is a vector of control variables that includes changes in production inputs as well as controls for changes in bonus target incentives. STORE, STORE MGR, DISTRICT MGR, and MONTH are fixed effects for store, store manager, district manager, and month. The coefficients of interest are α_1 and α_2 . To account for possible serial correlation of performance across time and between stores within local areas, standard errors are clustered at the district level.¹⁵

4.2.1 Control variables

Aside from the use of fixed effects, several additional control variables that impact gross profits were added. Since two levels of workers provide labor below the district and store manager levels, each level's contribution is considered separately. At the assistant store manager level, a logged assistant store manager count variable is added.¹⁶ For crew employees, labor input is measured in terms of total labor cost incurred for all crew hours worked, measured as logged average weekly labor expenditure. Capital expenditures are accounted for by creating a logged capital expenditure stock variable. A capital stock measure was created by taking the cumulative sum of all capital expenditure projects over \$50,000 from October 2004 up to the specific month recorded in the panel period for a given store, less accumulated depreciation.^{17,18}

Managers and workers may face variation in incentives over time, especially if the marginal returns to effort change as actual performance achievement gets closer or further away from quarterly bonus targets. To construct a control variable to account for changes in these incentives, several features of the bonus contract and bonus target setting process are utilized. For store and assistant store managers, sales and operating profit bonus targets are identical, since they are assigned at the store level for each month. Bonus targets are set ex ante to require an equivalent level of effort to obtain 100% achievement in each month, implying that variation in incentives within each quarter should reflect the level of cumulative sales and operating profit achievement relative to the bonus target set for that point. For example, if a manager is at the end of the first two months of a quarter, and actual sales for the two months is below the total target sales for the two months, sales achievement is below target. If the bonus scheme is designed to require an equivalent level of effort each month, then the manager will be required to exert more effort in the third month to achieve 100% of the quarterly sales target. To control for

¹⁴ It should be noted that most of the treatment observations that can be separately estimated from the controls are the result of store and district manager reassignments in the sample. As an additional robustness check, separately dropping stores where turnover and promotions occur from the sample at both levels of management yield similar results.

¹⁵ To account for possible serial correlation of performance for a given region-month, the baseline specification was also run with standard errors clustered at the region-month level, which also gave statistically significant results.

¹⁶ Calculated as $\log(\text{Assistant store manager count} + 1)$

¹⁷ Based on conversations with the firm, capital expenditures below \$50,000 are typically for purposes that are unlikely to have any meaningful effect on gross profits.

¹⁸ Depreciation for each project was calculated using a linear method, with a useful life assumption of 10 years. Ten years was suggested by the firm as a reasonable estimate of useful economic life.

these possible changes in incentives over time, separate dummy variables are included for 0-90% and 90-100% bands of achievement of both sales and operating profit targets in each month of the quarter for both store managers and assistant store managers.^{19 20} For the case of district managers the same type of control variables are implemented, but sales and operating profit achievement are aggregated at the district level instead of an individual store level.

4.2.2 Tenure working together

If the effect of supervisor changes on performance is a result of cumulative experience working together, then performance declines resulting from supervisor changes should be mitigated when relatively little experience has been accumulated. While this tenure variable was not recorded by RestaurantCo, the data provided allow for an indirect test of the predicted effect at the district manager level. Using the job tenure of store managers (at the store manager level) recorded in the data, managers with less than 6 months of job tenure who experience a supervisor change are compared to managers with greater than 6 months tenure who also experience a supervisor change. These “young” store managers by definition have a maximum of 6 months of experience working for their previous supervisor at the time a supervisor change occurs, a considerably shorter tenure of shared experience than the average implied by the available data. In order to understand how the effect of supervisor changes may vary by previous shared experience, a dummy for “young” store managers with less than 6 months job tenure when a supervisor change occurs is interacted with DM SUPERVISOR CHG.²¹

4.2.3 Managerial attention constraints

A fundamental constraint faced by every manager is the finite amount of time allotted to them to execute their role. For managers who supervise multiple subordinate workers, this also constrains their ability to provide individual attention to each subordinate. (Calvo and Wellisz 1979, Rosen 1982) As a consequence, if managers have a greater number of subordinates, interactions between managers and subordinates may be less frequent or more limited, leading to a lesser degree of cumulative experience working together between them. If previous supervisors with a greater number of subordinates are less familiar with subordinates on average, the performance loss resulting from a supervisor change should be attenuated. Also, replacing supervisors who are more constrained in their ability to provide attention may be unable to build the same degree of shared experience and familiarity. As a result, replacement supervisors who are more constrained may be forced to accept greater performance losses. To test for the existence of these effects, separate variables are constructed to measure managerial attention constraints for both the previous and replacing supervising district managers. For previous supervisors, the average number of stores under their control *prior* to the supervisor change is used. In the case of replacement supervisors, the average number of stores under their control *after* the supervisor change is used. Both measures of managerial attention constraints are interacted with DM SUPERVISOR CHG.

¹⁹ Achievement of targets is calculated only for the time period that has already passed. For example, at the end of the second month of a quarter, if actual sales for the two months is 90 dollars and total target sales for the two months is 100 dollars, sales achievement is calculated to be 90%. The 90% value is used in the regression for the third month of that quarter.

²⁰ The choice of 0-90% and 90-100% as achievement bands was based on observing the histogram of achievement across all managers during the panel period.

²¹ Job tenure is at the store manager level, as opposed to firm tenure or tenure at the current store.

5 Results and Discussion

5.1 Baseline and interaction regressions

Table 1.1 shows the descriptive statistics for the variables used in the analysis, excluding the dummy controls for manager incentives. Regression results are displayed in Table 1.2 for both the baseline and interaction specifications. As shown in Column 4 in Table 1.2, the coefficient for STORE MGR CHG is not statistically significant, providing no evidence for Hypothesis 1A. However, the coefficient for DM SUPERVISOR CHG is negative and significant at the 1% level, providing support for Hypothesis 1B. Store managers who receive a new supervisor experience a 3% average decline in gross profits during the panel period. Also, changes in supervising district manager result in greater performance declines relative to store manager changes, consistent with Hypothesis 2. To confirm formally that the coefficient for DM SUPERVISOR CHG is strictly less than STORE MGR CHG, the hypothesis that both coefficients are equal was tested and rejected at the 5% level.²²

Column 5 in Table 1.2 displays the results for the specification including interaction terms for tenure working with the previous supervisor and district manager span of control. When interacted with DM SUPERVISOR CHG, the coefficient on “young” managers is positive and significant at the 1% level, consistent with the prediction that performance losses are driven by lost shared experience. Store managers with short job tenure and relatively little shared experience with their previous supervisor experience on average a 4.3 percentage point greater performance outcome relative to store managers with longer job tenure that also experience a supervisor change.

Results for the interactions between both previous and replacing supervisor spans of control and DM SUPERVISOR CHG are also reported in Column 5 of Table 1.2. Providing evidence for increasing managerial attention constraints mitigating performance losses, the coefficient on the interaction term between the previous supervisor’s span of control and DM SUPERVISOR CHG is positive and significant at the 5% level. Performance declines following changes in supervising district manager are attenuated the larger the previous supervisor’s span of control, with gross profits increasing 1.1% for every additional store under their span of control. No evidence is found for a relationship between performance declines and the replacing supervisor’s span of control at standard levels of significance.

5.2 Contrast in district and store manager results

Consistent with the hypotheses, the results provide evidence of a clear contrast in the performance impact of supervisor changes between higher and lower level managers. Greater use of codified rules at lower levels may be an important buffer against the negative consequences of supervisor changes within the firm, and may explain why no measurable performance impact was found for store manager changes. At higher levels, accumulated experience working together may be more important for performance if effective coordination is more dependent on specific knowledge or trust acquired through shared experience.

Interviews conducted with managers at RestaurantCo also suggest that differences in the use of codified rules play an important role in explaining the results. When a new district manager supervisor arrives, a significant amount of a store manager’s time is typically shifted away from

²² The resulting p-value of the Wald test was 3.2%.

managing production to answering inquiries about store operating norms, as well as adjusting to changes in the specific style of the new manager. This adjustment process by subordinates is recognized explicitly in RestaurantCo's training, where workers are told to expect that new supervisors bring "the same standard, but different style." By contrast, store manager changes require less adjustment between the replacing supervisor and subordinates due to the standardized nature of tasks that subordinates execute. Assistant store managers and crew employees are uniformly trained across the organization to perform tasks using the same methods, allowing store managers and subordinates to efficiently communicate adjustments without detailed explanation.

5.3 Alternative explanations for contrasting results

Several other possibilities may also explain, or at least contribute to, the contrast in results. One explanation may be the relatively high turnover rate of assistant store managers and crew employees compared to store managers. If turnover is normally high, there may be little opportunity to build the experience necessary to observe performance losses when store managers change. To test for this possibility, an alternative specification is estimated where STORE MGR CHG is interacted separately with the average assistant manager and crew turnover rate over the trailing 12 months, ending in the first month of the panel period. If stores have lower levels of assistant manager and crew turnover, performance losses may be more severe when store managers change. However, as shown in Column 1 of Table 1.3, the interaction terms are insignificant, suggesting that this explanation is unlikely to account for the contrasting results between district and store managers. As an additional test, STORE MGR CHG is also interacted with the firm tenure, tenure at the store manager level, and store tenure of the previous store manager, as well as the firm and level tenure of the replacing store manager. As shown in Column 2 of Table 1.3, all of these interactions are also statistically insignificant.

A large fraction of changes in supervising district manager are the direct result of reassigning stores to different districts. Given that these reassignments are ultimately driven by new store openings, performance declines may be the result of cannibalization from new stores, and not the replacement of supervisors. To test whether this might be the case, the baseline regression model is estimated after dropping all stores within an average driving distance of 3 miles of a new store opening during the panel period.²³ The results are shown in Table 1.4, with estimations shown after dropping stores for every incremental half mile up until the 3 mile radius. In every adjusted sample, the results for the coefficients of interest remain similar to the baseline results and do not change monotonically, suggesting that cannibalization from new store openings does not explain the results.

A primary threat to identification in differences in differences regression models is that preexisting time trends between the treatment and control groups being considered were already different prior to the treatment occurring. In the specific case of district manager supervisor changes, gross profits may have already been declining prior to the supervisor change, and the estimation of performance declines found in the results reflect factors other than the supervisor change itself. To test formally whether this is the case, the baseline regression is modified with an adjusted sample to test for differences in pretreatment time trends between treatment and control groups. First, I test whether store managers that experienced supervisor changes

²³ Distances were calculated by obtaining the shortest driving distance in Microsoft MapPoint for both directions between every pair of stores, and taking the average of both distances (Store A to Store B, Store B to Store A).

(treatment group) had different time trends relative to the control group prior to their supervisor change. The sample is adjusted by dropping all observations after treatment occurs, effectively removing all nonzero values of DM SUPERVISOR CHG. Separate month dummies are then included for both the (eventual) treatment and control groups. Then, I test the joint hypothesis that the coefficients for the month dummies are the same for both groups. This procedure is repeated for stores that experience a store manager change. As shown in Table 1.5, the null hypothesis that the pretreatment time trends are the same for treatment and control groups cannot be rejected for either treatment estimated in the baseline regression.

5.3.1 Robustness checks

Although the estimation of DM SUPERVISOR CHG compares store managers that change supervising district managers to a control group that includes store managers that experience no supervisor change, unobservable factors outside of supervisor changes may be influencing performance. As an additional test for this possibility, the set of store managers under the *same* district manager where a change in supervising district manager occurs (where DM SUPERVISOR CHG equals one) is considered separately. If the replacement of supervisors is the underlying mechanism behind the performance losses found in Table 1.2, then other store managers under the same district manager should exhibit no change in performance. A separate indicator variable is added for this group of store managers to the baseline regression, with results shown in Column 3 of Table 1.3. No evidence of performance declines is found for this group of store managers, suggesting that performance losses are unlikely to be due to unobservable factors at the district level.

Differences in regional profit time trends may also be influencing the results, and may not be adequately controlled for with month fixed effects. For example, economic declines may have impacted some areas of the US disproportionately, and may be responsible for some of the performance declines estimated in the baseline regression. To test whether regional trends such as these influence the results, the baseline regression is estimated with a full set of region-time fixed effects replacing the original MONTH variable, where regions are defined by the firm and are similar in size to Designated Market Areas (DMAs). As shown in Column 4 of Table 1.3, including these additional controls does not significantly change the original baseline results.

In order to test for mitigated performance losses for “young” store managers that experienced a change in supervising district manager, store managers with less than 6 months job tenure at the store manager level were selected. To ensure that the results are not sensitive to this choice of tenure, the estimation was repeated for a range of different time horizons, with results shown in Table 1.6. The results remain robust to the precise definition of “young” store managers, showing results consistent with increasing previous shared experience leading to greater performance declines.

In a small number of cases in the data, replacement supervisors arrive from outside the organization. For these supervisor changes, performance losses may reflect not only the effect of supervisor changes, but also the loss of firm specific human capital. To check whether these instances play a meaningful role in explaining the results, district manager and store manager outside hires were identified and dropped from the sample.²⁴ The baseline regression

²⁴ New store managers hired from outside the organization are not directly identified in the data since all new managers must first attend a 2-3 month training program prior to beginning work inside stores, which occurs at regular intervals throughout the year. Using estimates taken from interviews with RestaurantCo managers, store

specification is then estimated for the adjusted sample, with results shown in Column 5 of Table 1.3. The results are virtually identical to the original baseline specification, suggesting that the possible loss of firm specific human capital in these cases is an unlikely explanation for the results.

6 Conclusion

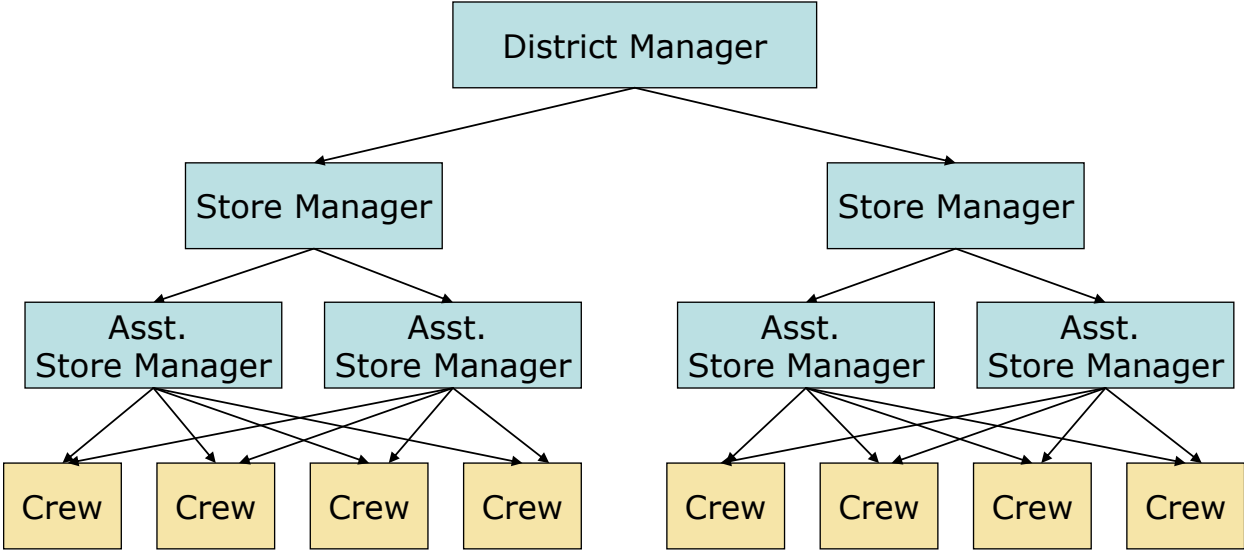
The motivation for this study was to examine how variation in the use of codified rules to govern worker activities influences the impact of supervisor changes on firm performance. The empirical setting used here provides an opportunity to exploit variation in the use of codified rules across different layers of an organizational hierarchy, and compare the performance effects of supervisor changes across this variation. Also, the highly standardized nature of RestaurantCo's organization mitigates many concerns that that would typically confound identification in studies linking the effect of supervisor changes with characteristics of internal organization.

The results suggest that the use of codified rules plays an important role in determining the performance impact of supervisor changes. In the specific context of the analysis done here, a manager's level in the hierarchy influences the degree to which supervisor changes affect performance. The experience of managers and their subordinates working together is more valuable at upper levels in a hierarchy, but less important at lower levels where worker activities are more explicitly governed by codified rules. This contrast is particularly important for firms considering the redeployment or turnover of managers within the organization. The decision to reassign or terminate higher level managers should consider factors beyond a manager's raw ability or assigned responsibility, given the greater degree of performance losses that can result when supervisor changes at these levels occur.

While the results in this study present an intriguing set of findings, further study is required to establish whether they are robust across a variety of empirical settings. Organizations within the fast food industry may be particularly unique in the degree to which codified rules can be implemented to dictate the activities of workers relative to firms in other industries. If true, the performance costs of supervisor changes documented here may represent a relatively low estimate of the average cost of these changes across most firms, which are often impossible or very difficult to measure. Also, understanding how the interaction between the use of codified rules and the effect of supervisor changes might differ in organizations that primarily engage in innovative activity (e.g. R&D) would help shed further light on the specific contextual factors that dictate the performance effects of management changes within organizations.

managers who have a firm hire date within 6 months prior to their actual working start date as store manager are defined as outside hires.

Figure 1.1 Management hierarchy



Note: Crew employees are at a lower level in the hierarchy relative to assistant store managers, but both assistant store managers and crew employees report directly to store managers.

Figure 1.2 Changes in supervising store manager

1,235 store manager changes total, tabulated separately by new vs. experienced replacement managers

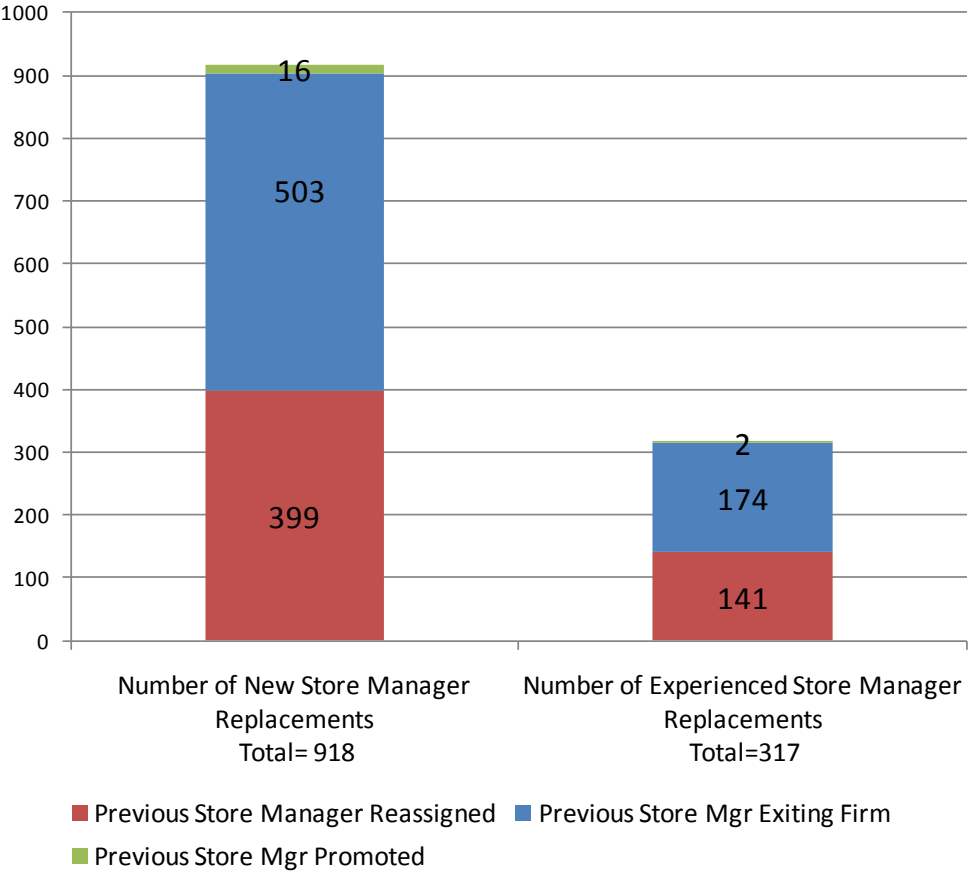
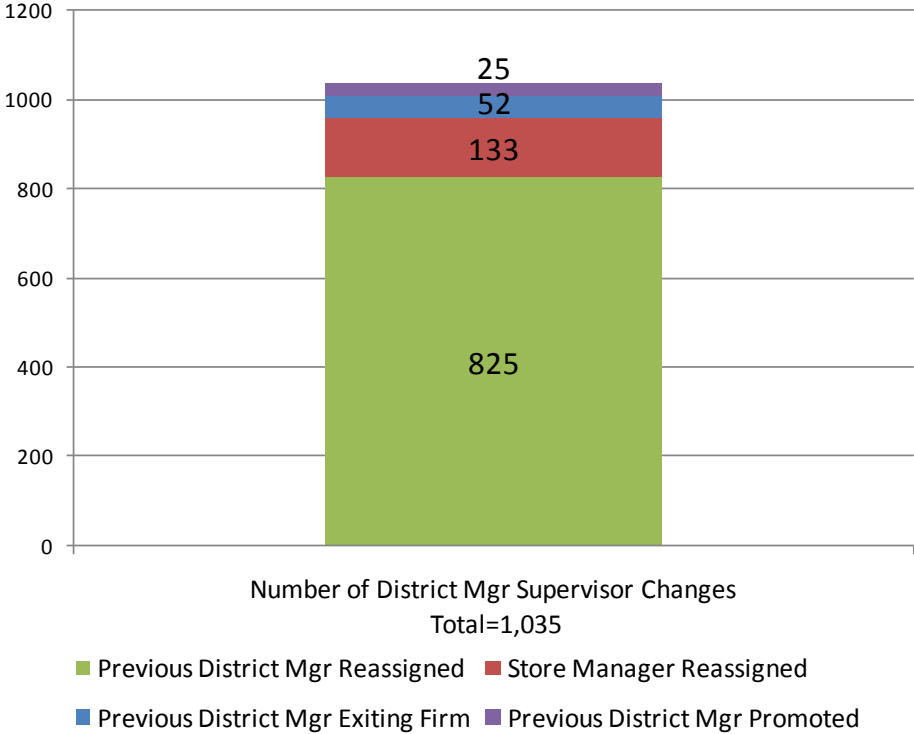


Figure 1.3 Changes in supervising district manager



Note: In the majority cases, the replacement supervisor is an experienced district manager. New district managers replace 42 of the 52 supervisor changes resulting from the previous district manager exiting, 9 of the 25 changes due to promotion to regional manager, and 126 of the 865 instances where previous district managers are reassigned.

Table 1.1 Descriptive statistics

Variable	Mean	σ	Min	Max
1. Average Weekly Gross Profit (in USD)	12,704	4,959	478	33,650
2. Average Weekly Labor Expenditure (in USD)	4,527	1,176	61	9,551
3. Assistant Manger Staffing Count	0.8	0.7	0	5
4. Capital Expenditure Stock (in USD)	28,267	45,745	0	951,827
5. log(Average Weekly Gross Profit)	9.4	0.4	6.2	10.4
6. log(Average Weekly Labor Expenditure)	8.4	0.3	4.1	9.2
7. log(Assistant Manger Staffing +1)	0.5	0.4	0.0	1.8
8. log(Capital Expenditure Stock+1)	7.1	4.6	0.0	13.8
9. District Manager Span of Control (store count)	9.4	1.9	1	24
10. Store manager job tenure (at store mgr level) in months, full sample	28.3	23.5	1.0	97.6
11. Store manager job tenure (at store mgr level) in months, for changes in supervising district manager only	28.5	23.3	1.0	97.6
12. Average Previous District Manager Span of Control, for changes in supervising district manager only	10.3	3.4	5.0	24.0
13. Average Replacing District Manager Span of Control, for changes in supervising district manager only	9.7	2.1	4.0	21.4

N = 54,768 (full sample)

Table 1.2 Baseline and interaction specification regression results

Dependent Variable: Log Average Weekly Gross Profits

VARIABLES	(1)	(2)	(3)	(4)	(5)
STORE MGR CHG	-0.002 (0.004)	-0.002 (0.004)	0.012 (0.019)	0.012 (0.019)	0.010 (0.019)
DM SUPERVISOR CHG	0.005 (0.003)	-0.004 (0.007)	-0.031*** (0.009)	-0.030*** (0.009)	-0.175*** (0.049)
DM SUPERVISOR CHG X "Young" Store Manager					0.043*** (0.012)
DM SUPERVISOR CHG X Previous Supervisor Span of Control					0.011** (0.005)
DM SUPERVISOR CHG X Replacing Supervisor Span of Control					0.004 (0.004)
Labor and Capital Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Store FE	Y	Y	Y	Y	Y
District Manager FE	N	Y	Y	Y	Y
Store Manager FE	N	N	Y	Y	Y
Incentive Controls	N	N	N	Y	Y
Observations	54,768	54,768	54,768	54,768	54,768
Number of stores	6,934	6,934	6,934	6,934	6,934
R-squared (within)	0.41	0.42	0.54	0.54	0.54

Standard errors in parentheses, clustered at the district level.

*** p<0.01, ** p<0.05, * p<0.1

Table 1.3 Robustness check specifications

Dependent Variable: Log Average Weekly Gross Profits

VARIABLES	Store Mgr Changes x Trailing 12 month turnover rates (1)	SM Change x Previous and Replacing SM Tenures (2)	Separate indicator for other Store Mgrs under District Mgr (3)	Region- Time Fixed Effects Substitution (4)	Outside Hire District Mgrs and Store Mgrs Dropped (5)
STORE MGR CHG	-0.035 (0.035)	0.032 (0.055)	0.012 (0.019)	0.009 (0.016)	0.001 (0.019)
STORE MGR CHG X Asst. Store Mgr Turnover	-0.096 (0.102)				
STORE MGR CHG X Crew Turnover	0.004 (0.040)				
STORE MGR CHG X Previous Store Manager's Firm Tenure		0.002 (0.001)			
STORE MGR CHG X Previous Store Manager's Tenure as Store Manager		-0.002 (0.003)			
STORE MGR CHG X Previous Store Manager's Tenure Managing Current Store		-0.000 (0.004)			
STORE MGR CHG X Replacing Store Manager's Firm Tenure		-0.001 (0.001)			
STORE MGR CHG X Replacing Store Manager's Tenure as Store Mgr		0.001 (0.002)			
DM SUPERVISOR CHG	-0.031*** (0.008)	-0.029*** (0.009)	-0.031*** (0.009)	-0.025*** (0.008)	-0.030*** (0.009)
Other Store Mgrs under same District Mgr where DM SUPERVISOR CHG does not occur			-0.004 (0.004)		
Labor and Capital Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	N	Y
Store FE	Y	Y	Y	Y	Y
District Manager FE	Y	Y	Y	Y	Y
Store Manager FE	Y	Y	Y	Y	Y
Incentive Controls	Y	Y	Y	Y	Y
Observations	54,101	53,702	54,768	54,768	53,557
Number of stores	6,934	6,934	6,934	6,934	6,928
R-squared (within)	0.53	0.51	0.54	0.61	0.53

Standard errors in parentheses, clustered at the district level. The sample in Column 1 is adjusted to remove the influence of extreme outliers first, then outliers at 6 standard deviations above the mean. Both Asst. Store Mgr Turnover and Crew Turnover are the trailing 12 month averages ending the first month of the panel period. The sample in Column 2 changes due to missing values in the data. The results in Column 2 remain insignificant when estimating interactions for the previous and replacing store manager tenures in separate specifications.

*** p<0.01, ** p<0.05, * p<0.1

Table 1.4 Robustness check for new store openings

Dependent Variable: Log Average Weekly Gross Profits						
VARIABLES	Dropped stores within 0.5 miles of new store openings (1)	Dropped stores within 1 mile of new store openings (2)	Dropped stores within 1.5 miles of new store openings (3)	Dropped stores within 2 miles of new store openings (4)	Dropped stores within 2.5 miles of new store openings (5)	Dropped stores within 3 miles of new store openings (6)
DM SUPERVISOR CHG	-0.030*** (0.009)	-0.028*** (0.009)	-0.022*** (0.009)	-0.030*** (0.009)	-0.029*** (0.010)	-0.021* (0.012)
STORE MGR CHG	0.013 (0.019)	0.018 (0.021)	0.018 (0.021)	0.006 (0.021)	0.005 (0.021)	0.005 (0.021)
Labor and Capital Controls	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Store FE	Y	Y	Y	Y	Y	Y
District Manager FE	Y	Y	Y	Y	Y	Y
Store Manager FE	Y	Y	Y	Y	Y	Y
Incentive Controls	Y	Y	Y	Y	Y	Y
Observations	53,899	52,204	50,666	48,925	47,359	45,883
Number of stores	6,823	6,606	6,407	6,187	5,994	5,802
R-squared (within)	0.54	0.55	0.55	0.54	0.54	0.54

Standard errors in parentheses, clustered at the district level.

*** p<0.01, ** p<0.05, * p<0.1

Table 1.5 Pretreatment time trend test results

	F-statistic	p-value
Supervising district mgr change	1.06	0.39
Store manager change	1.04	0.40

Table 1.6 Robustness check for definition of “young” store manager

Dependent Variable: Log Average Weekly Gross Profits

VARIABLES	"Young" store managers							
	3 months job tenure (1)	4 months job tenure (2)	5 months job tenure (3)	6 months job tenure (4)	7 months job tenure (5)	8 months job tenure (6)	9 months job tenure (7)	10 months job tenure (8)
DM SUPERVISOR CHG	-0.184*** (0.049)	-0.181*** (0.048)	-0.180*** (0.048)	-0.175*** (0.049)	-0.185*** (0.047)	-0.187*** (0.049)	-0.186*** (0.049)	-0.185*** (0.049)
DM SUPERVISOR CHG X "Young" Store Manager	0.053 (0.033)	0.048** (0.019)	0.034** (0.016)	0.043*** (0.012)	0.031*** (0.011)	0.014 (0.011)	0.014* (0.009)	0.008 (0.008)
DM SUPERVISOR CHG X Previous Supervisor Span of Control	0.012*** (0.005)	0.012*** (0.005)	0.012*** (0.005)	0.011** (0.005)	0.012*** (0.004)	0.012*** (0.005)	0.012*** (0.005)	0.012*** (0.005)
DM SUPERVISOR CHG X Replacing Supervisor Span of Control	0.005 (0.004)	0.004 (0.004)	0.004 (0.004)	0.004 (0.004)	0.005 (0.004)	0.005 (0.004)	0.005 (0.004)	0.005 (0.004)
STORE MGR CHG	0.010 (0.019)	0.010 (0.019)	0.010 (0.019)	0.010 (0.019)	0.011 (0.019)	0.011 (0.019)	0.011 (0.019)	0.011 (0.019)
Labor and Capital Controls	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Store FE	Y	Y	Y	Y	Y	Y	Y	Y
District Manager FE	Y	Y	Y	Y	Y	Y	Y	Y
Store Manager FE	Y	Y	Y	Y	Y	Y	Y	Y
Incentive Controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	54,768	54,768	54,768	54,768	54,768	54,768	54,768	54,768
Number of stores	6,934	6,934	6,934	6,934	6,934	6,934	6,934	6,934
R-squared (within)	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54

Standard errors in parentheses, clustered at the district level.

*** p<0.01, ** p<0.05, * p<0.1

Chapter 2

Avoiding Rookie Mistakes: The Value of Managerial Experience for Firm Performance

1 Introduction

It is conventional wisdom that a manager's previous experience in management roles plays an important role in determining firm performance. When firms consider candidates for a management position, prior managerial experience is often a key factor for selection. Compensation packages for managers are also often directly tied to the level of managerial experience they have, suggesting that such experiences provide significant value for firms. Consistent with these practices, previous studies have found a positive relationship between the level of prior managerial experience managers possess and a variety of performance measures (Borman et al. 1993, Kor 2003, Huckman et al. 2009). However, little empirical evidence exists demonstrating *how* different levels of managerial experience lead to different performance outcomes. Examining the underlying mechanism linking managerial experience and performance is important for understanding why this experience is valuable for firms, and how differences in managerial characteristics contribute to the heterogeneity in performance across firms.

In a wide range of contexts, cumulative experience has been found to be an important driver of performance at the organizational, team, and individual levels (Wright 1936, Newell and Rosenbloom 1981, Reagans et al. 2005, Huckman et al. 2009). Knowledge gained from experience may be tacit and difficult to transfer, making the accumulation of experience itself a valuable process which cannot be easily substituted (Holcomb et al. 2008). In the case of managers, a necessary step in their decision-making process is estimating the costs and benefits of their decisions prior to implementing them. Knowledge acquired through previous managerial experience may increase a manager's precision in performing this function, and ultimately lead to improved performance. If managerial experience influences performance by increasing their competence in assessing the effect of their decisions, managers with different levels of experience may ultimately make different decisions, resulting in different performance outcomes. In this study, a manager's experience level, decision-making, and performance outcomes are examined within the same empirical setting to investigate how managerial experience affects performance.

In most settings, comparing the decisions of managers and their performance outcomes is typically confounded by a wide range of (often unobservable) factors. For example, managers at two different firms may have differences in their level of discretion, available resources under their control, or tasks associated with their role. Within the same firm, differences such as these can also exist between managers at different levels in the hierarchy, or even between managers at the same level in different functional areas of the firm. Because this variation is often unobservable and difficult to control for, comparing the performance of managers poses a significant empirical challenge for estimation. In principle, choosing an empirical setting where

many of these differences are naturally controlled for by the setting itself would help mitigate many of these estimation concerns.

In this paper, I examine how different degrees of managerial experience affect performance by considering a set of managers at the same level within a firm who are each responsible for the operation of a single business unit. In the empirical setting used here, all business units within the firm have similar operations that are highly standardized, with the job function of managers clearly defined and identical across all business units. This standardization of managerial roles and operating procedures alleviates many concerns of unobserved differences across managers confounding estimation. To compare differences in decision-making and performance between inexperienced and experienced managers, I observe how both types of managers affect the operations of the store once they are placed in charge of a business unit, and estimate the impact of these changes on business unit performance.

The empirical setting for this study is the US company-owned operation of a quick-service restaurant firm, hereafter referred to as RestaurantCo. The use of standard operating procedures to dictate worker activities is extensive within quick-service restaurant organizations, and business units (individual stores) within each organization are designed with a high degree of similarity to provide a consistent customer experience across all locations. RestaurantCo is one of the largest firms in the industry, with several billion dollars in annual sales operating in over 30 countries with a globally recognized brand. With a large number of similar business units using identical performance metrics and standardized management roles, performance changes can be relatively easily compared within the organization. Taken together with the large number of managers within the organization that can be used for empirical comparison, RestaurantCo provides a compelling setting to investigate how different levels of managerial experience affect firm performance.

In my results, I find that managers with no previous managerial experience, hereafter referred to as *rookie managers*, cut labor costs immediately after assuming control of a business unit. This leads to an immediate increase in operating profits in the rookie manager's first full month on the job, but then results in an average decline in profits of 2.7% below the previous manager's levels in subsequent months. By contrast, managers with prior experience in the same position do not make any significant changes in labor costs after arriving in a business unit, and operating profits do not change from the previous manager's levels in the following months. In the case of rookie managers, both labor cost cuts and profit declines persist in the subsequent months available in the data. The results suggest that rookie managers systematically miscalculate the positive performance effects of their decisions.

In the next section, I examine the theories and literature relevant to linking managerial experience, decision-making, and firm performance. Section 3 discusses the empirical setting and data. Section 4 details the empirical identification strategy. Section 5 presents and discusses the results, with concluding remarks offered in the final section.

2 Managerial experience, decision-making and performance

The relationship between cumulative experience and improved performance outcomes has been established in a variety of empirical settings (Wright 1936, Yelle 1979, Argote et al. 1990, Reagans et al. 2005, Huckman et al. 2009). These "learning curves" are a fundamental phenomenon of study in the organizational learning literature, and more recent work has focused on explaining the significant variation in learning rates across organizations (Argote 1999,

Pisano et al. 2001). One important factor determining heterogeneity in learning rates identified in this literature is the proficiency of individual workers within the organization, including their level of experience (Shafer et al. 2001, Reagans et al. 2005).

Prior empirical field studies examining the relationship between managerial experience and performance have primarily focused on demonstrating a direct correlation between managerial experience and specific performance variables (Kor 2003, Huckman et al. 2009). However, surprisingly few studies have empirically examined the mechanisms through which managerial experience may influence firm outcomes. Using data on the performance of head coaches in the National Football League, Holcomb et al. (2009) found that managers with higher ability, which included measures of experience level, had greater levels of resource productivity, although the study did not directly examine the decisions of the managers. In an experiment comparing how prior professional experiences influence managerial decision-making, Melone (1994) examined whether Chief Financial Officers (CFOs) or Vice Presidents of Corporate Development (VPs) made systematically different managerial decisions when faced with the same set of acquisition candidates. In her results, CFOs tended to place greater emphasis on financial characteristics, while VPs took a more balanced view of both strategic and financial characteristics in their decision-making process. When considered together, these studies suggest important relationships may exist linking managerial experience, decision-making and performance, but also highlight the need for further research to explain how managerial experience influences performance.

In this paper, I propose that managerial experience improves a manager's precision in estimating the costs and benefits of their decisions prior to implementing them, and that this increase in accuracy leads to different decisions being made by the manager. In principle, managers with different levels of experience should make different decisions if decision-making is one important mechanism through which managerial experience matters. In the empirical setting used here, I consider variation in the level of cumulative experience a manager has in the same management role. Specifically, I compare the production input decisions (e.g. labor allocation) of rookie managers, who have no prior experience in their role, to managers who possess prior experience in the same role.

From this theoretical framework, I derive the first formal hypothesis:

Hypothesis 1: Rookie managers will make different production input decisions relative to experienced managers.

If managerial experience is relevant for firm performance due to differences in the decisions that are made, these decisions should have meaningful performance consequences. In principle, individual managers add value by performing any task, service, or job that provides greater utility for the firm or lowers unit costs (Lepak et al. 2007). If experience matters for performance because of differences in the decisions that are made to perform this function, then these differences should necessarily result in different performance outcomes. Given the positive relationship found in prior studies linking managerial experience and organizational performance (Holcomb et al. 2009, Kor 2003), decisions made by inexperienced managers should result in worse performance outcomes relative to the decisions of their more experienced peers.

This prediction is presented formally here:

Hypothesis 2: The decisions made by rookie managers will result in lower levels of performance relative to the decisions made by experienced managers.

3 Empirical Setting and Data

RestaurantCo is a global chain of quick-service restaurants, selling a variety of food and beverage products to consumers. The firm's US-based company-owned stores provide the empirical setting for this study. Within the organization, each store is assigned a single store manager with authority over all routine operating decisions, including scheduling labor, managing inventory, training employees, and hiring and firing of all subordinates. Store managers typically spend roughly 30 hours per week directly managing the production process during store operating hours, and 10 hours per week on administrative responsibilities, including budgeting and reporting. When store managers are not physically present to manage operations, staffing of labor is decided in advance. All non operations-related decisions, including advertising, pricing, and capital expenditures, are either made centrally at corporate headquarters or by senior management at the regional level.²⁵ Store managers have two levels of subordinates, assistant managers and crew employees, both of which report directly to the store manager. Stores have on average one assistant manager and between 10 to 40 crew employees, depending on average store sales volume.

The most significant impact store managers have in affecting profits is through determining the store's level of expenditure on crew employee labor. Since crew members are compensated for each hour they work at a standardized fixed wage, store managers are able to increase or cut labor costs by making corresponding changes to the total number of hours they assign to crew employees each month.²⁶ If labor is not allocated at an optimal level that maximizes profits, then profits can decline in one of two ways. If labor expenditure exceeds the optimal level, then profits decline because marginal sales increases do not exceed the marginal cost of adding more crew employee hours. Below the optimal labor level, the number of workers within the store is insufficient to process customer orders in a timely manner. Slower processing times lead to longer wait times for customers from the moment they enter the store to the receipt of their order, which ultimately leads to customers being lost that would otherwise have entered the store, or the loss of repeat customers who become dissatisfied with longer wait times. In quick-service restaurant stores, this effect is particularly evident during peak demand times of the day, such as breakfast, lunch, or dinner hours.

Within RestaurantCo, store managers are faced with asymmetric institutional consequences with regard to changes in labor expenditures. According to interviews with store managers, observable sales gains stemming from labor cost increases may take up to several months to materialize, if they occur at all. As a result, labor cost increases typically face greater scrutiny by a store manager's supervising district manager, and entail greater risk for a store manager's career if expected future sales increases fail to occur, since profits are likely to decline initially due to increased labor expenditure. By contrast, labor cuts typically do not face the same degree of scrutiny since the potential efficiency gains should occur relatively quickly. Given this institutional bias favoring labor cost cuts over increases, store managers may be more likely to make systematic errors by making labor cuts that are too severe instead of increasing labor beyond optimal levels.

Store manager changes can occur as a result of the previous store manager exiting the firm, being promoted, or being reassigned. Exits from the firm are typically voluntary and not related

²⁵ This refers to management above the Regional Manager level.

²⁶ This can either occur through giving crew employees fewer hours, or hiring and firing crew employees.

to store performance trends.²⁷ As shown in Figure 2.1, most replacing store managers have no prior experience in their role, with the vast majority being promoted from the assistant manager level within RestaurantCo.²⁸ A significant number of replacements are also experienced store managers reassigned from other stores. The reassignment of store managers to other stores is primarily based upon the experience level and past performance of store managers. As a general rule, assignments are designed to match store sales levels with the experience level of the store manager. More experienced store managers are assigned to stores with higher sales volumes, and less experienced managers to lower volume stores. Store managers are commonly reassigned to progressively higher volume stores over the course of their career, and are typically not given any choice in the assignment process. Also, the firm has no policy of reassigning managers based upon store performance trends. For example, experienced managers are not systematically reassigned to underperforming stores to improve them, or given outperforming stores as a reward. The general assignment process of store managers follows a straightforward path--as store managers gain experience and maintain an adequate level of performance, they are given more responsibility by being reassigned to stores with larger sales volumes. A more detailed decomposition of store manager changes is shown in Figure 2.1.

The data used in this study contains RestaurantCo's internal performance and manager staffing data for approximately 7,100 stores from October 2007 through June 2008 in a monthly panel. Financial data at the store level includes monthly sales, transaction count, cost of goods sold, and labor expenditures.²⁹ Manager assignments to stores are recorded for each month at all levels in the hierarchy. For store managers, tenure at the firm, at the store manager level, and as manager at the currently assigned store are all recorded. Additional relevant data include monthly sales and operating profit bonus targets for each store, capital expenditures³⁰, store employee turnover rates, and store characteristics such as format type (e.g. drive-thru).

4 Empirical Strategy

4.1 Sample selection

Due to the short panel period and structure of the data, several adjustments to the sample were made to provide the clearest comparisons between groups. First, store months with less than three weeks of operating activity were dropped to reduce measurement error. Also, store managers hired from outside the firm (and not promoted from within) during the panel period are dropped from the sample, both due to their small numbers (only 34 managers total) and lack of data regarding their work experience prior to joining RestaurantCo. New store openings during the panel period are also dropped from the sample, as the sales and profit trends of newly opened

²⁷ This was according to interviews with RestaurantCo employees. As an additional test of the validity of this claim, I compare RestaurantCo's published voluntary turnover rate in the same fiscal year to an estimate of total turnover implied in my data, and find that they are similar.

²⁸ Newly promoted store managers are typically never promoted from within the same store.

²⁹ Monthly sales, transaction count, and operating profits are available from October 2004 through June 2008.

³⁰ Capital expenditure amounts, types and dates are provided at the store level from October 2004 through June 2008.

stores are unlikely to provide a reasonable counterfactual to the older stores where store manager changes occur.³¹

In a small fraction of cases in the sample, stores experience months with no assigned store manager. For these instances, store performance during these months may reflect unusual circumstances. To address this concern, the months during which no store manager is assigned are dropped from the sample. Also, to capture the average effect of a single supervisor change with the maximum possible time window of observation and avoid possible interactions of multiple instances of treatment, stores that experience more than one store manager change during the panel period are dropped from the sample. The final sample used in this study consists of 6,437 stores and 7,527 store managers.

4.2 Identification and Estimation

The impact of a store manager change in the case of both rookie and experienced replacements is considered using three separate dependent variables—labor cost, gross profit, and operating profit. Gross profit is defined as store sales minus cost of goods sold, and operating profit is simply gross profit minus labor costs. Other costs (e.g. real estate rental costs) are excluded from this profit measure since store managers have no control over and are not responsible for these expenses. Changes in gross profit closely match changes in sales levels, since the majority of raw ingredients have a relatively long shelf life, product mixes are generally stable during the panel period, and each store utilizes the same inventory management software that regularly provides detailed recommended order amounts of items needed.^{32,33} Store labor cost is the most economically significant variable store managers control in affecting store profits, and is used here to measure differences in decision-making between rookie and experienced managers. To measure the effect of labor cost decisions on performance, both gross profit and operating profit are used.

To compare rookie and experienced managers, the effects of store manager changes for each type of replacement are calculated first using a differences-in-differences approach. The regression model used here is a two-way fixed effects model, controlling for both observable and unobservable time-invariant store factors that may influence the dependent variables of interest, as well as any common time-varying factors that may impact all stores within each month. Variables that are likely to affect store performance such as store location, physical store size, and seasonal changes in demand are controlled for through the use of these fixed effects. In principle, the dependent variable trends of the control groups--in this case, stores that never experience a store manager change combined with store-months prior to a store manager change--represent what would have happened to both stores that receive rookie and experienced replacements if no change in store manager had occurred.

In many differences-in-differences regression tests using panel data, effects are estimated using the average of the entire time period after the “treatment” event occurs. For instance, in order to estimate the effect of a store manager change, the entire period after a replacing store

³¹ This difference in performance between newer and more mature stores is recognized explicitly in the industry, with financial statements of many of these firms reporting sales growth separately for stores open longer and less than 12 months.

³² If necessary, inventory can also be ordered and delivered to a store typically within 24 hours.

³³ To validate the interchangeable relationship between gross profits and sales for the analysis done here, the baseline gross profit regression is also run on sales, with very similar results.

manager arrives might be grouped together to calculate a single coefficient. However, prior estimation methods in the organizational learning literature emphasize the importance of recognizing dynamic changes in performance over time (Darr et al. 1995). To explicitly incorporate this in the estimation used here, a separate dummy variable is created for each time-indexed month prior to and after a store manager change occurs in cases of both rookie and experienced replacements. For example, a store that replaces its manager with a rookie manager in the fourth month during the 9 month panel period would contain a value of “1” for dummy variables at time index months -3, -2, -1, 0, +1, +2, +3, +4, and +5. Since store manager changes occur in every month of the 9 month period in the data, a full set of time indexed variables is created to account for all months prior to and after rookie replacements arrive. This set of variables is defined as ROOKIE TIME 1- ROOKIE TIME N. For the case of experienced replacement managers, another set of variables EXPERIENCED TIME 1- EXPERIENCED TIME N is defined.

I formally present the baseline differences in differences regression model used here:

$$y_{it} = \alpha_1 \text{ROOKIE TIME } 1_{it} + \dots + \alpha_n \text{ROOKIE TIME } N_{it} + \theta_1 \text{EXPERIENCED TIME } 1_{it} + \dots + \theta_n \text{EXPERIENCED TIME } N_{it} + \beta \text{CONTROLS}_{it} + \text{STORE}_i + \text{MONTH}_t + \epsilon_{it}$$

where y is the outcome variable of interest (labor cost, gross profit, operating profit) for store i in month t . CONTROLS is a vector of control variables that includes capital expenditure stock. A capital stock measure was created by taking the logged cumulative sum of all capital expenditure projects over \$50,000 from October 2004 up to the specific month recorded in the panel period for a given store, less accumulated depreciation.³⁴ Depreciation for each project was calculated using a linear method, with a useful life assumption of 10 years.³⁵ STORE and MONTH are fixed effects for store and month. The coefficients of interest are α_1 through α_n and θ_1 through θ_n . To account for possible serial correlation of performance across time within stores, standard errors are clustered at the district level.

5 Results and Discussion

5.1 Labor cost

Regression results for labor cost are shown graphically in Figures 2.2.1 and 2.2.2, which show the point estimate for each time-indexed dummy variable with its respective 95% standard error band shown in shaded gray. In both Figures, the control group is represented by the zero horizontal line, since the point estimates and standard error bands are for dummy variables relative to the baseline control group. Point estimates statistically different from the baseline have standard error bands strictly above or below the horizontal zero line. Labor costs decline immediately after rookie managers arrive in a store, and lower labor cost levels continue to persist in the following months. By contrast, no evidence of changes in labor costs is found in the case of experienced replacements. The average effect across all subsequent months after

³⁴ Based on conversations with the firm, capital expenditures below \$50,000 are typically for purposes that are unlikely to have any meaningful effect on gross profits.

³⁵ Ten years was suggested by the firm as a reasonable estimate of useful economic life.

replacement occurs is shown in Column 1 of Table 2.2, with the change in labor costs for both rookie and experienced managers estimated with the variables ROOKIE MANAGER and EXPERIENCED MANAGER. Labor costs decline by an average of 1.5% in the months following the arrival of a rookie manager, while experienced managers exhibit no change. Taken together, the results provide evidence supporting Hypothesis 1. To confirm formally that the coefficient for ROOKIE MANAGER is statistically different than EXPERIENCED MANAGER, the hypothesis that both coefficients are equal was tested and rejected at the 5% level.³⁶

5.2 Gross and Operating Profit

Figures 2.3.1 and 2.3.2 show the gross profit trends for both rookie and experienced replacements. In the case of rookie replacements, gross profits appear to exhibit no change until the second full month after the rookie manager's arrival (time index +2), after which gross profit declines and remains at lower levels in subsequent months. In the case of experienced replacements, no evidence of changes in gross profits is found. The average effect across all months for both cases is estimated in Column 2 of Table 2.2, using the same variable names ROOKIE MANAGER and EXPERIENCED MANAGER. Gross profits decline by an average of 1.1% after a rookie manager arrives, while experienced managers exhibit no average change in the subsequent months after arrival.³⁷

In order to understand the net impact of both labor cost and gross profit changes, both effects must be considered together. If labor costs and gross profits both decline, then operating profits should measure the net effect, and show whether the positive benefit of labor cost cuts or the negative impact of gross profit declines dominates. The results of this net effect are shown graphically in Figures 2.4.1 and 2.4.2. Rookie managers initially outperform their predecessors with higher operating profit levels, but the performance increase quickly reverses and declines below the previous manager's levels beginning in the rookie's second full month on the job (time index +2). Experienced replacements exhibit no statistically significant change in performance from the previous manager. Given the dynamic changes in performance shown in Figure 2.4.1, separate indicator variables are created for rookie replacements to separate out the first full month of the rookie's tenure as store manager (defined as ROOKIE MANAGER, FIRST MONTH) from all subsequent months (defined as ROOKIE MANAGER, AFTER FIRST MONTH). Results using these indicator variables are shown in Column 3 of Table 2.2. Rookie replacements initially increase operating profits by 1.4% in their first full month on the job, but then lose these gains quickly, with operating profits declining by an average of 2.7% in subsequent months. Consistent with Figure 2.4.2, experienced managers also exhibit no change in operating profit also when all subsequent months are grouped together for estimation. The results provide partial evidence supporting Hypothesis 2, but suggest a more complex mechanism than a simple difference in average effects. Both operating profit gains and losses are observed following the arrival of a rookie manager, but profit gains are short-lived, while losses persist for a longer period in the available data. To confirm formally that the coefficient for ROOKIE MANAGER, AFTER FIRST MONTH is statistically different from

³⁶ The resulting p-value of the Wald test was 2.2%.

³⁷ Although gross profits don't decline until the second full month, ROOKIE MANAGER includes the first full month to allow for direct comparison with EXPERIENCED MANAGER. When just considering the second full month onwards, the average decline in gross profits is 2.9%.

EXPERIENCED MANAGER, the hypothesis that both coefficients are equal was tested and rejected at the 1% level.³⁸

5.3 Rookie vs. experienced managers

The results provide evidence of a clear contrast in the decision-making and performance of rookie and experienced managers. Rookie managers appear to systematically miscalculate the net benefit of their decisions when cutting labor costs, while experienced managers avoid making the same changes. The inference of a miscalculation is based upon the assumption that changes in labor costs are designed for a net positive gain in operating profits, and that a net zero or negative effect is unintended. Also, as shown in Figure 2.4.1, both labor cost and operating profit declines persist in subsequent months, suggesting that a dramatic increase in profits during the months immediately following the available data is unlikely. Managerial experience, at least for the types of production input decisions made here, may increase a manager's ability to accurately estimate the benefits and costs of the choices available to them. Greater competence in this assessment can result in improved performance, or at least mitigate performance declines from costly decisions.

The persistence of labor cost cuts even after operating profits decline, however, suggests that managerial experience alone is unlikely to explain the entire phenomenon shown in the results. In principle, if labor cost cuts have a negative effect on performance, this "mistake" can simply be reversed by increasing labor costs back to prior levels. However, as Figure 2.2.1 shows, rookie managers systematically avoid making this choice and continue to maintain lower labor cost levels. According to interviews with store managers at RestaurantCo, the asymmetric institutional consequences of changes in labor costs provide a significant deterrent to any upward adjustments in labor expenditure, even after a decline in performance. As mentioned earlier, labor cost cuts can adversely impact sales levels primarily by increasing the average wait time for customers, which in turn results in the loss of some customers. To win customers back, store managers may be able to increase labor staffing at the store, but a significant time lag exists between labor cost increases and potential sales gains. Consequently, labor cost increases cause an immediate decline in store profits by definition, which must be compensated for in the future by higher sales. After rookie managers experience profit declines resulting from their labor cuts, immediately decreasing profits further in the short run by increasing labor expenditure may put their careers at additional risk, especially if expected future increases in sales fail to occur. Given these incentives, rookie managers on average appear to prefer staying with the changes they make, at least in the short run.

5.4 Alternative explanations for results

A primary concern when estimating the effect of store manager changes is that these changes are unlikely to occur exogenously within the data, which may bias coefficient estimates of the effects. For example, selection bias may exist regarding the types of stores that change store managers. Selection can occur on time-invariant store characteristics, such as store format or location, or time-varying store characteristics such as store sales and profit trends over time. To address concerns of possible selection on time-invariant store characteristics, store fixed effects are included as a control variable in the regression estimation. When considering

³⁸ The resulting p-value of the Wald test was 0.02%. To match the timeframes of each variable, the first month of EXPERIENCE MANAGER for each case was also excluded.

possible selection on time-varying characteristics, Figures 2.2.1-2.4.2 already show graphical evidence that pre-existing time trends for labor, gross profit, and operating profit are similar between stores that change managers for both rookie and experienced replacements and their control groups.

To test formally whether pre-existing trends in the dependent variable differed between stores that changed managers and their control groups, the baseline regression is modified with an adjusted sample to test for differences in pretreatment time trends between “treatment” and control groups. First, I test whether stores that change store managers (treatment group) had different time trends relative to their control group prior to the store manager change. The sample is adjusted by dropping all observations after a change in store manager occurs, effectively removing all nonzero values of ROOKIE MANAGER and EXPERIENCED MANAGER. Separate month dummies are then included for both the (eventual) treatment and control groups. Then, I test the joint hypothesis that the coefficients for the month dummies are the same for both groups. As shown in Table 2.3, the null hypothesis that the pretreatment time trends are the same for treatment and control groups cannot be rejected for any of the empirical results.³⁹

Although preexisting time trends may not explain the results, rookie store manager replacements may occur in expectation of future changes in performance, and labor cuts are simply the response to these expected changes. In order to explain the results shown here, rookie managers would have to be more likely to be assigned to stores with expected sales declines, and the labor cuts are simply in anticipation of these predicted declines in sales. In order to test for this, store sales and operating profit bonus targets for each store-month observation are used as measures of expected future store performance. Targets for both performance measures are set centrally at RestaurantCo’s corporate headquarters up to one year in advance, and are designed to be achievable for any competent store manager given a reasonable level of effort. Both measures are used as dependent variables in the baseline regression specification originally used for gross profits. As shown in Table 2.4, coefficients ROOKIE MANAGER and EXPERIENCED MANAGER show no significance for either sales or operating profit bonus targets, suggesting that the gross profit declines that occur in the case of rookie replacements were not anticipated by the firm.⁴⁰

Another possible explanation for the results is that operating profit declines occur as a consequence of rookie managers having less experience than their predecessors, but that other factors aside from labor cost cuts explain the decline. For example, previous store managers may simply have had greater idiosyncratic knowledge of the store and its workers, and as a result allocated labor more efficiently while actively managing the production process. Rookie managers may lack this idiosyncratic knowledge, and be less efficient at executing this task. To test for the possibility of alternative mechanisms, the variables ROOKIE MANAGER, FIRST MONTH and ROOKIE MANAGER, AFTER FIRST MONTH in the operating profit regression specification are interacted with the previous manager’s tenure at the firm, as a store manager at RestaurantCo, and as a store manager at the store just prior to departure. The rookie manager’s tenure at the firm is also included as an additional interaction term. As shown in Column 4 of Table 2.2, only the previous manager’s tenure at the store is significant, with longer store tenures

³⁹ Gross profit pretreatment time trend tests show similar results with similar levels of statistical insignificance, but were omitted for brevity purposes.

⁴⁰ As mentioned earlier, gross profits and sales behave very similarly in this study. The insignificant results on operating profit targets provide additional evidence of no expected change in performance by RestaurantCo.

leading to greater declines in operating profits. The results provide evidence suggesting that the store-specific managerial experience of the previous manager may have a negative influence on operating profits when changes in store manager occur. However, the baseline coefficient for ROOKIE MANAGER, AFTER FIRST MONTH remains negative and significant after accounting for variation in these different types of experience with a larger magnitude of decline than in the original baseline regression (3.4% vs. 2.7% decline), suggesting that alternative mechanisms aside from labor cost changes arising from differences in experience do not explain the significance of the baseline results.

6 Conclusion

The empirical setting for this study provides a unique opportunity to observe the relationship between managerial experience, managerial decision-making, and its effect on firm profits. The results show that rookie managers differ in the decisions they make relative to their more experienced counterparts when assuming responsibility of a business unit, and that the benefits of decisions made by rookie managers are short-lived, ultimately leading to profit declines. Assuming rookie managers changed labor costs with the intention of increasing operating profits, the subsequent decline in operating profits suggests that rookie managers systematically miscalculate the benefits and costs of their decisions prior to cutting labor costs. In contrast with experienced managers, who do not cut labor costs on average, the relative underperformance of rookies relative to experienced managers suggests that managerial experience may be beneficial for performance by decreasing the likelihood of making rookie “mistakes” when making decisions affecting performance.

This study has several limitations. First, the short panel period of the data prevents further observation of rookie managers after gaining a significant degree of experience on the job. Ideally, rookie managers could be observed later in the course of their tenure, and their behavior when reassigned would confirm the comparison shown here. Replicating the results shown here with data spanning longer timeframes would help to complement this study, as well as shed light on the magnitude of the marginal benefit of increasing managerial experience on decision-making. Second, the results shown here apply to situations where managers actively manage a highly structured production process in an industry where the use of detailed codified rules to dictate worker activities is particularly extensive. In settings where production is much more unstructured and decisions are significantly more complex, the results here may be less (or more) significant for performance. Studies in other types of empirical settings would help to clarify how context may matter in determining the mechanism through which managerial experience influences performance.

The results also have important implications for firms. In principle, codifying the knowledge gained by experienced managers and incorporating this knowledge into training for rookie managers may be able to substitute for at least some of the benefits gained from experience, and mitigate the likelihood that rookie managers miscalculate the benefits and costs of their decisions. The benefits of collecting, codifying, and distributing such knowledge for firm performance may be significant, especially for the type of rookie “mistake” shown in the results of this study, where the decision and average outcome can be easily described. Organizations that invest in improved capabilities in retaining and transferring these types of knowledge may gain a competitive advantage over rivals (Argote and Ingram 2000).

Figure 2.1 Changes in supervising store manager

1,174 store manager changes total, tabulated separately by rookie vs. experienced replacement managers

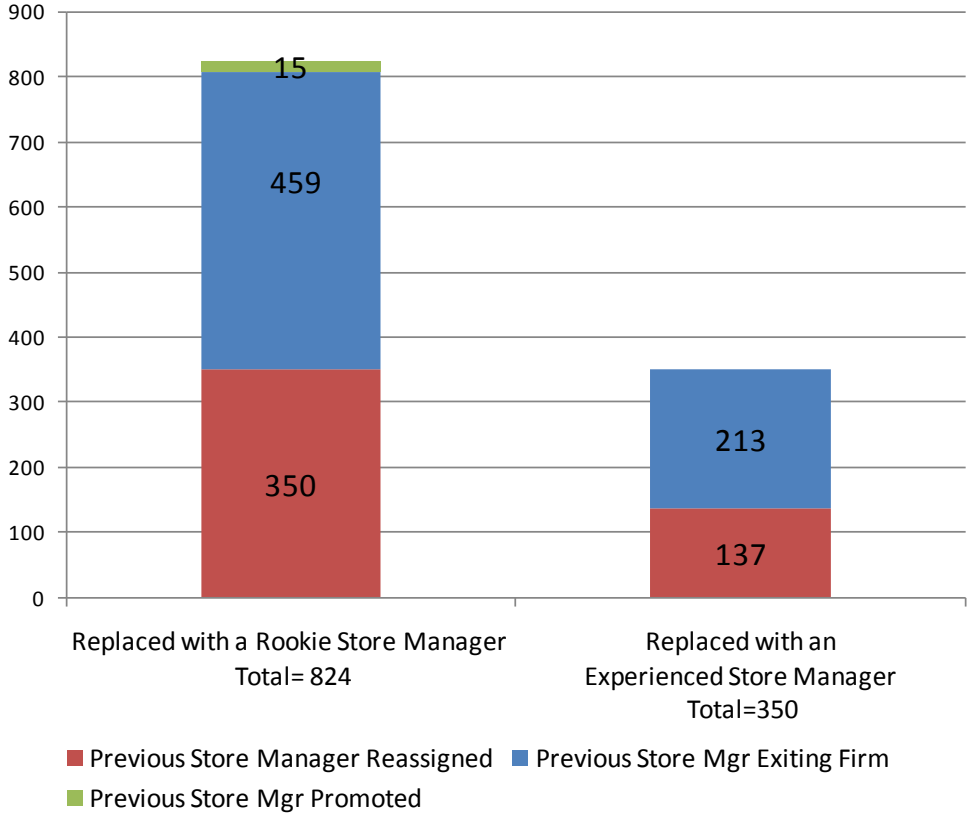


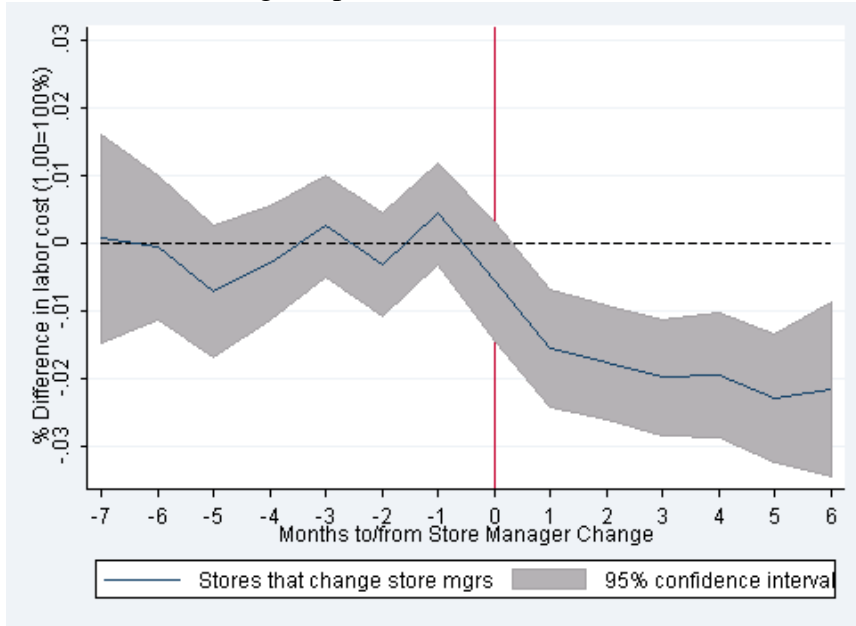
Table 2.1 Descriptive statistics

Variable	Mean	σ	Min	Max
1. Average Weekly Labor Expenditure (in USD)	4,533	1,181	60	9,551
1. Average Weekly Gross Profit (in USD)	12,823	4,920	591	33,650
2. Average Weekly Operating Profit (in USD)	8,289	3,959	1,874	58,302
3. Capital Expenditure Stock (in USD)	29,542	46,082	0	951,827
4. log(Average Weekly Labor Expenditure)	8.4	0.3	4.1	9.2
5. log(Average Weekly Gross Profit)	9.4	0.4	6.4	10.4
6. log(Average Weekly Operating Profit)	8.9	0.6	1.8	10.2
7. log(Capital Expenditure Stock+1)	7.4	4.5	0.0	13.8
8. Store manager job tenure (at store mgr level) in months, full sample	28.6	23.6	1.0	97.6
9. Store manager job tenure (at store mgr level) in months, when reassigned to another store	32.7	24.5	1.0	97.6
10. Store manager tenure at firm, when promoted to store manager	41.9	30.0	3.0	167.9

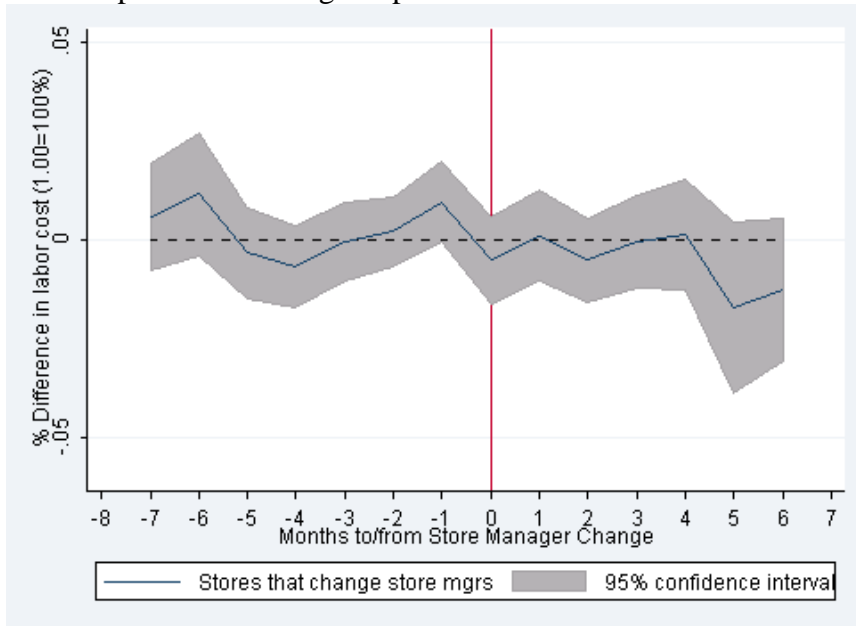
N = 54,024 (full sample)

Figures 2.2.1, 2.2.2 Time-indexed plots of labor costs before and after store manager changes

2.2.1. Rookie manager replacements

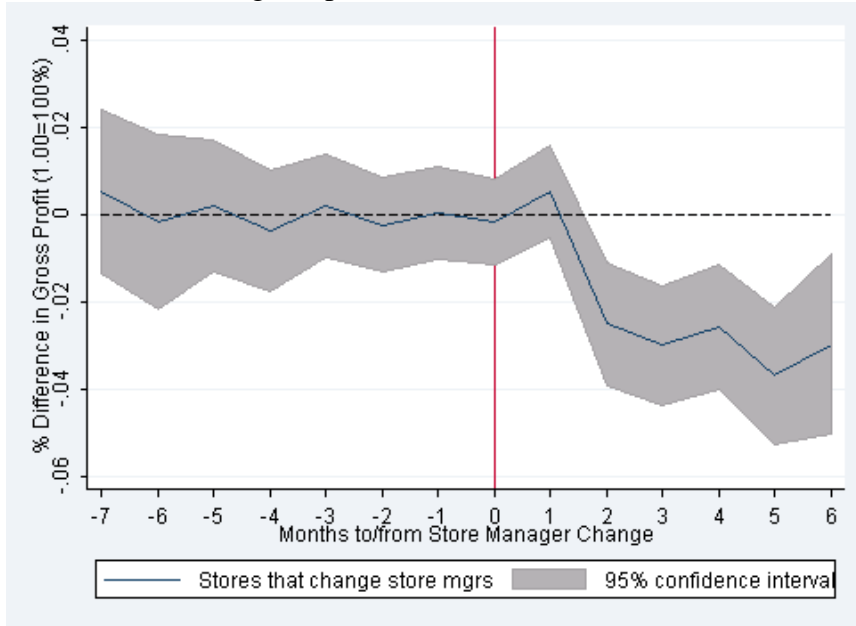


2.2.2 Experienced manager replacements

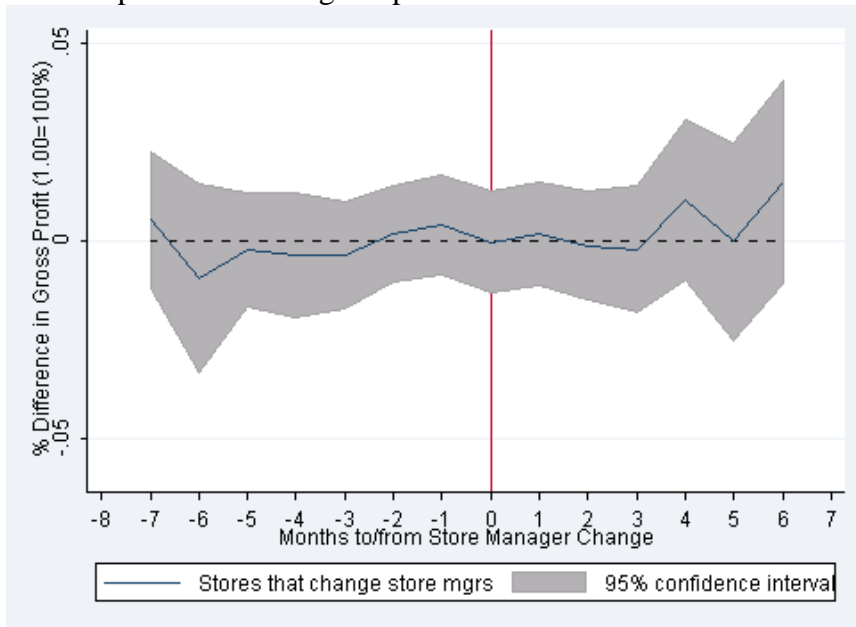


Figures 2.3.1, 2.3.2 Time-indexed plots of gross profit before and after store manager changes

2.3.1 Rookie manager replacements

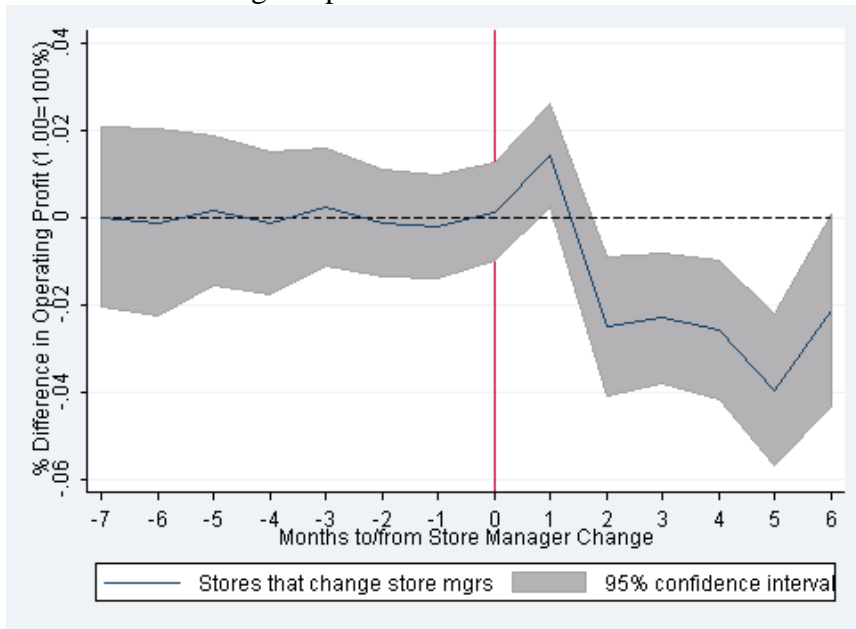


2.3.2 Experienced manager replacements



Figures 2.4.1, 2.4.2 Time-indexed plots of operating profit (gross profit – labor cost) before and after store manager changes

2.4.1 Rookie manager replacements



2.4.2 Experienced manager replacements

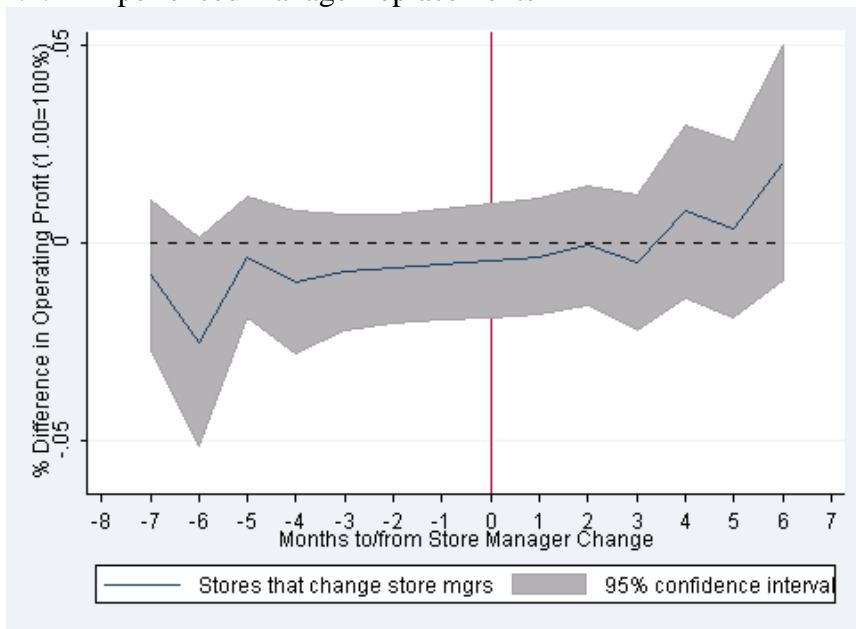


Table 2.2 Baseline and interaction specification regression results

Dependent Variable:	Labor Cost	Gross Profit	Operating Profit	Operating Profit
VARIABLES	(1)	(2)	(3)	(4)
ROOKIE MANAGER	-0.015*** (0.003)	-0.011*** (0.004)		
EXPERIENCED MANAGER	-0.004 (0.004)	0.002 (0.005)	0.003 (0.006)	0.003 (0.006)
ROOKIE MANAGER, FIRST MONTH			0.014*** (0.005)	0.004 (0.012)
ROOKIE MANAGER, AFTER FIRST MONTH			-0.027*** (0.007)	-0.034** (0.015)
ROOKIE MANAGER, FIRST MONTH X Rookie Mgr Tenure at Firm				0.007 (0.005)
ROOKIE MANAGER, FIRST MONTH X Previous Mgr Tenure at Firm				0.004 (0.008)
ROOKIE MANAGER, FIRST MONTH X Previous Mgr Tenure in Store Mgr Role				0.008 (0.009)
ROOKIE MANAGER, FIRST MONTH X Previous Mgr Tenure as Store Mgr at Store				-0.019** (0.009)
ROOKIE MANAGER, AFTER FIRST MONTH X Rookie Mgr Tenure at Firm				0.010 (0.008)
ROOKIE MANAGER, AFTER FIRST MONTH X Previous Mgr Tenure at Firm				0.007 (0.010)
ROOKIE MANAGER, AFTER FIRST MONTH X Previous Mgr Tenure in Store Mgr Role				0.003 (0.012)
ROOKIE MANAGER, AFTER FIRST MONTH X Previous Mgr Tenure as Store Mgr at Store				-0.024* (0.013)
Capital Stock Control	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Observations	54,024	54,024	54,024	54,024
Number of stores	6,436	6,436	6,436	6,436
R-squared (within)	0.05	0.34	0.44	0.44

Standard errors in parentheses, clustered at the district level.

*** p<0.01, ** p<0.05, * p<0.1

Table 2.3 Pretreatment time trend test results

	F-statistic	p-value
Labor cost time trend before store manager change, Rookie replacement	0.98	0.45
Labor cost time trend before store manager change, Experienced replacement	0.65	0.72
Operating profit time trend before store manager change, Rookie replacement	0.53	0.82
Operating profit time trend before store manager change, Experienced replacement	0.85	0.54

Table 2.4 Sales and Operating Profit Bonus Target Regressions

Dependent Variable:	Sales Bonus Target	Operating Profit Bonus Target
VARIABLES	(1)	(2)
ROOKIE MANAGER	-0.003 (0.003)	-0.007 (0.005)
EXPERIENCED MANAGER	0.005 (0.004)	0.000 (0.006)
Capital Stock Control	Y	Y
Month FE	Y	Y
Store FE	Y	Y
Observations	54,009	53,872
Number of stores	6,435	6,431
R-squared (within)	0.79	0.66

Standard errors in parentheses, clustered at the district level. Outliers four standard deviations outside of a store's mean value in the panel period were dropped.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Chapter 3

How important are individual managers for corporate social responsibility?

1 Introduction

The socially responsible (and irresponsible) behavior of firms has become an increasingly important issue for both managers and academics. In a 2007 survey of global executives conducted by the Economist Intelligence Unit⁴¹, 56% of managers who responded considered corporate social responsibility (CSR) a "high" or "very high" priority, a substantial increase over the 34% reported three years earlier. Most academic attention on CSR has been focused on investigating the relationship between CSR metrics and financial performance, and in general finds that higher levels of CSR lead to improved corporate financial performance, although the effects are small (Margolis, Elfenbein, and Walsh 2009). However, recent studies have also begun to emphasize the importance of understanding the antecedents of CSR (McWilliams et. al 2006, Waldman et. al 2006). The goal of this study is to empirically investigate the importance of one factor that has received increasing attention in the literature on antecedents of CSR—a firm's individual top managers—and their influence on CSR activities. Empirically examining the significance of individual managers in influencing CSR has important implications for understanding the processes that determine the social performance of firms, and ultimately the underlying motivation for firm investments in CSR.

Academic interest in examining managers as a determinant of CSR is not surprising, given that a substantial literature finds that managers influence a variety of other firm outcomes (Finkelstein and Hambrick 1996, Agle et al. 1999). Several scholars have argued that further work should examine how managerial characteristics affect CSR, since managers are largely responsible for implementing CSR practices (Wood 1991, Thomas and Simerly 1995, Agle et al. 1999). If managerial characteristics do indeed causally affect CSR, this implicitly assumes that managers exert at least some *individual* influence on CSR, independent of firm-specific or other external factors. If true, this relationship would imply that firms, policymakers, and institutions interested in improving a firm's social performance may find it effective to ensure that managers have individual characteristics consistent with a higher expected level of CSR. However, if decisions to invest in CSR are primarily a response to firm-specific or external factors, then the identity of managers may not play a significant causal role in determining CSR, even if managers are making the decisions. The implicit assumption of individual-specific influence by managers provides the primary question motivating this study--how much of a firm's socially responsible behavior is attributable to the individual managers themselves?

In principle, one straightforward method of quantitatively estimating the influence of individual managers on CSR is to estimate manager fixed effects or the effect of managerial characteristics in a regression, controlling for all other relevant firm-level characteristics. However, in practice, obtaining a comprehensive set of control variables is difficult, especially if uncertainty exists about what relevant factors should be included that might affect CSR. Also, a

⁴¹ Economist Intelligence Unit, Global Business Barometer, Nov-Dec 2007

wide range of firm-specific factors that are likely to influence CSR typically lack good or even available measures, such as firm culture, organizational structure, or external influences from a firm's local community. Without a comprehensive set of control variables, any quantitative estimation using this type of regression approach is likely to overstate the importance of individual managers for CSR, and lead to incorrect inferences.

To address this challenge, I estimate the influence of individual managers on CSR by implementing an empirical identification strategy based upon Bertrand and Schoar (2003), and consider a set of managers who switch firms over time. This allows for the simultaneous estimation of firm and manager fixed effects, where firm fixed effects are able to control for all observable and unobservable time-invariant firm characteristics that may influence CSR. The inclusion of firm fixed effects in this case is particularly useful, since measures of many of the variables that influence CSR are likely to be unavailable in existing data, but can still be controlled for in this approach. Also, studying managers who switch firms over time allows for an empirical test of whether managers exert a *persistent* individual effect on CSR, regardless of the firm they are employed at. Finding evidence (or no evidence) of persistent influence across firms provides important insight into the mechanism driving the decisions of managers to engage in CSR activities.

In my results, I find that manager fixed effects are statistically significant, but that these fixed effects explain only a small amount of the variation in CSR, suggesting managers exert relatively little individual influence on CSR. The statistically significant but small magnitude of influence is consistently found across a variety of different dimensions of CSR. Also, I find no evidence that managers exert a persistent effect on CSR across the different firms they are employed at, suggesting that when managers do influence CSR, they are responding to firm-specific or other external factors, and are not imposing a constant individual influence on the organizations they manage. The results provide preliminary evidence that the individual identity of managers plays a relatively small role in influencing CSR, and that firm-specific or other external factors may be more important in determining social performance.

In the next section, I examine the theories and literature relevant to the influence of individual managers on corporate social performance. Section 3 discusses the data used and the empirical identification strategy. Section 4 presents and discusses the results, with concluding remarks offered in the final section.

2 The influence of managers on corporate social performance

The influence that individual managers may have on CSR is fundamentally based upon the notion that decisions made by managers have a significant impact upon actions taken at the firm level, and that managers have at least some degree of discretion in the decisions they make (Finkelstein and Hambrick 1996). If heterogeneity exists in the decisions that managers make irrespective of external or institutional factors, then the identity of individual managers becomes a potentially important factor in determining CSR. For example, consider a CEO with a strong personal commitment to environmentally sustainable practices in their private life. If they operate with these same values in their role as CEO, they may exercise the discretion given to them to reduce waste production or toxic emissions at the firm level, or donate more corporate funds to environmental causes. By contrast, a CEO without such values may make different choices for their organization, resulting in different outcomes in social performance.

Recent literature has focused primarily on the importance of a manager's personal values in determining CSR activities. Hemingway and Maclagan (2004) argue that managers exhibit their personal values by exercising the discretion available to them, and that they may engage in projects to address their personal moral concerns. Other scholars make similar arguments, suggesting that the personal values of managers may causally affect the CSR activities of a firm (Thomas and Simerly 1995, Pant and Lachman 1998, Agle et al. 1999). At the same time, Wood (1991) and Thomas and Simerly (1995) note that relatively little empirical work has been done investigating the relationships between managerial characteristics and CSR activities, and highlight the need for further empirical investigation into how managers choose to meet their corporate social responsibilities. By addressing the broader question of how much individual managers influence CSR, this study aims to contribute to a better understanding of the role managers play in the implementation of CSR activities.

In addition to the question of how important individual managers are in determining CSR, an equally critical question is whether the personal values of managers are truly the causal factor driving CSR. If the personal values of managers remain relatively stable over time, managerial influence on CSR should remain stable irrespective of the firm they are working for, or other changes in the external environment. If a manager's influence on CSR changes across different firms or circumstances, however, then the personal values of managers may not play a critical role in determining their decisions regarding CSR activities. Or, it may also be argued that their values change with their environment or firm, suggesting that firm-specific or external factors are more important considerations when determining CSR. In order to investigate the validity of the personal values explanation, I will test for the persistence of individual manager influence on CSR across the different firms in which they are employed.

3 Data and empirical identification strategy

The data used for this study is a matched sample of managers, firms and CSR measures in a panel structure. Data linking individual managers to firms is taken from the ExecuComp database from 1992-2009, which contains data on the top five executives for a wide range of U.S. publicly listed firms. This data typically contains the firm's CEO, CFO, COO, and subdivision CEOs. As a measure of firm CSR, social performance ratings from Kinder, Lydenberg, Domini Research & Analytics (KLD) from 1992-2009 are used. KLD ratings are among the most influential and widely studied measures of CSR (Chatterji et al. 2009), and provide a logical starting point to conduct empirical analysis. A measure of CSR for several major categories of social performance that KLD publishes ratings for (Community, Corporate Governance, Diversity, Employee Relations, Environmental, and Product) is created by subtracting the total number of strengths minus total concerns, following the convention used by many prior empirical studies in the CSR literature (Chatterji et al. 2009).⁴² The ExecuComp and KLD data are initially merged, with all unmatched observations from both data dropped from the sample. Following the approach by Bertrand and Schoar (2003), the sample is then restricted to firms that contain a manager who switches to at least one other firm (which also must be visible in the data). Only managers who switch firms and have at least 3 years of observable tenure at each firm are considered, to ensure that managers have sufficient opportunity to influence CSR.

⁴² The Human Rights category was excluded from analysis due to too little variation in the data.

The final sample used for this study consists of 247 firms and 152 managers who switch organizations, spanning the years 1992-2009.⁴³⁴⁴

Table 3.1 displays descriptive statistics for the full sample of ExecuComp data, matched KLD and ExecuComp data, and final sample used for the study. Restricting the sample to managers working at firms rated by KLD and to firms containing managers who ultimately switch organizations leads to a sampling of larger firms, as measured by annual sales and total assets. Managers in the final sample also tend to have substantially higher salaries than the average manager in the ExecuComp database, which is not surprising given that their firms are significantly larger than the mean. When considering the results later, it should be noted that the sample selection process used in this study selects larger, U.S. publicly listed firms.⁴⁵ Descriptive statistics comparing managers who switch firms to all other managers in the final sample are displayed in Table 3.2. The age, salary, and bonuses of managers of each category of manager are similar, suggesting managers who switch firms are not systematically different from other managers in the sample, at least for these criteria.

As mentioned earlier, given a set of managers who switch firms over time, empirically estimating the individual influence of managers on CSR can be done through the simultaneous estimation of manager and firm fixed effects.⁴⁶ The baseline regression specification utilizing this approach is presented here:

$$CSR_{it} = \beta(CEO) + \gamma(CFO) + \delta(OTHEREXEC) + FIRM_i + YEAR_t + \varepsilon_{it},$$

where each firm CSR measure is regressed on fixed effects for each manager, firm, and year. Manager fixed effects are grouped by position to test whether influence on CSR might vary based upon whether a manager is a CEO, CFO, or Other executive. Managers are classified into each category based upon their last observed position in the data. Assessing the magnitude of the influence that managers have on CSR is done by first running the regression specification above, and comparing the adjusted R-squared to the adjusted R-squared of the same specification with manager fixed effects excluded. If the adjusted R-squared increases significantly with the inclusion of manager fixed effects, the results would provide evidence that individual managers play an important role in determining the social performance of firms. In addition to comparing R-squared results, the null hypotheses that CEO, CFO, and Other executive fixed effects are equal to zero are also tested as a way to evaluate whether the influence of managers is statistically significant.

To test whether managers exert a persistent individual effect across the different firms they may be employed at, I begin with estimating firm-year residuals by running the baseline specification excluding manager fixed effects. Then, I collapse these residuals by manager-firm spell, and regress a manager's average residual in their second term on the average residual in their first term. Put another way, this approach starts with the measure of CSR and controls for any year and firm-specific effects, but allows the residual to contain any individual effects that managers may have on CSR. Regressing the average residual of a manager's second firm spell on the average residual of a manager's first firm spell tests whether a manager's influence in

⁴³ Only a very small number of managers (8 in total) are observed in more than two firms. For the remainder of the paper, only switches from a manager's first and second firm are considered.

⁴⁴ Of the 152 managers, 64 are CEOs, 17 are CFOs, and 71 are Other executives.

⁴⁵ Bertrand and Schoar (2003) make a similar observation in their sample selection process.

⁴⁶ The empirical approach described here, including the test for persistent managerial influence, is based upon methods used by Bertrand and Schoar (2003).

their second firm is correlated with their influence in their first firm. If managers exert a persistent effect on CSR across the different firms they are employed at, then the coefficient of this regression should be positive and statistically significant. If the coefficient is not statistically significant, then there is no evidence managers have a persistent effect on CSR across different firms.

To see how a positive and statistically significant coefficient provides evidence consistent with the personal values explanation, consider the earlier example of the CEO who has a strong personal commitment to environmental sustainability, and suppose now they assume a managerial position at another firm. If this CEO maintains a consistent set of personal values that guides their CSR decisions, then they should engage in environmentally sustainable practices at both their first and second firm, and their positive influence on CSR should be consistent across both firms. However, if firm-specific or other external factors play a much more important role in determining CSR decisions and CSR, then there may be no statistically significant relationship between the CEO's first and second firm residuals.

Finally, it is important to note that the identification approach used here does not rule out the possibility of selection as an explanation for finding statistical significance of manager fixed effects. For example, a manager may switch firms at exactly the same time a firm independently decides to increase its investments in CSR, creating a correlation between a manager's arrival to a new firm and increases in CSR. As a consequence, the baseline empirical estimation in this study represents the variation in CSR that can be attributed to individual managers, either because firms are independently making changes in CSR that coincide with the movements of managers across firms, or because they are directly influencing CSR themselves. In the case that independent firm changes in CSR are positively correlated with the influence that managers have on firm CSR activities, this would increase the significance of the F-statistics reported for manager fixed effects, as well as the magnitude of changes in adjusted R-squared, and potentially overstate the influence of managers on CSR if the fixed effects are interpreted causally. In this case, the results of this study would reflect an upper bound on the degree to which individual managers might matter for CSR. By the same token, if firms independently change their CSR levels in a way that is negatively correlated with the influence of managers who switch firms (e.g. firms pollute more independent of a new manager who invests heavily in environmentally sustainable practices), manager fixed effects may understate the true effect managers have on CSR.⁴⁷

4 Results and Discussion

Adjusted R-squared results are shown in Table 3.3, along with F-statistics testing the null hypotheses that CEO, CFO, and Other executive fixed effects are equal to zero. Across all measures of CSR, the adjusted R-squared strictly increases with the addition of manager fixed effects, although all increases were less than half of one percentage point, suggesting that individual manager influence may explain only a small amount of a firm's social performance.⁴⁸ However, manager fixed effects are statistically significant across all measures of CSR and managerial positions. Taken together, the results provide evidence that while the influence of

⁴⁷ This limitation of the study is discussed in the conclusion.

⁴⁸ By comparison, Bertrand and Schoar (2005) find an average change in R-squared of 4 percentage points.

managers on CSR may be modest, managers do exert some degree of influence on social performance.

Regression results for the test of a persistent individual influence by managers across different firms are in Table 3.4. Across all measures of CSR, the relationship between the residual from a manager's second firm spell and a manager's first firm spell is not statistically significant, with the maximum R-squared reported from any specification being less than 1 percent. Assuming the personal values of managers do not change significantly with the firms they are employed at, the results suggest that a manager's personal values are not likely to be an important determinant of a manager's influence on CSR. Given the lack of a persistent influence across different firm spells, firm-specific or other external factors appear to play a more important role in determining managerial influence on CSR.

An alternative explanation for the results shown is that the causality between managers switching across firms and CSR may be exactly the opposite of what has been presumed so far in this study. In other words, instead of the arrival of a new manager affecting CSR outcomes, changes in CSR outcomes may be the impetus for the hiring of a new manager, and these changes may simply continue after the arrival of a new manager. For example, firms that have particularly poor recent social performance may choose to hire new managers from outside the firm, and this preexisting trend may continue to affect social performance for some time even after a new manager arrives. To test for this possibility, I expand the sample to include all firms with available KLD data (not just firms that contain a manager who switches firms), and collapse the sample into unique firm-year observations (instead of firm-manager-year observations). Then, I test whether firms that experience the arrival of a new manager switching from another firm have different preexisting time trends from the other firms in the sample. To test for the existence of a different preexisting time trend, I use a panel OLS regression model to estimate the coefficient of the three-year linear time trend prior to the arrival of a switcher for each measure of CSR, including year and firm fixed effects. As shown in Table 3.5, none of the linear time trends are significant at standard levels of statistical significance, suggesting that firms who hire switching managers do not have different CSR trends compared to other firms in the sample prior the arrival of a manager.

In addition to causality concerns, one possible reason for the relatively small magnitude of changes in the adjusted R-squared with the inclusion of manager fixed effects may be that too little variation exists among the CSR activities of managers in the sample. Such low variation may occur either because managers in general have very little difference in their commitment to CSR activities, or because firms systematically hire managers with similar levels of commitment to CSR. In the first scenario, if the population of managers in general does not differ substantially in their commitment to CSR, then the question of whether individual managers matter in influencing firm CSR has very little practical relevance—simply hire any manager, and similar levels of CSR outcomes should occur (controlling for other factors), and the individual identities of managers are not important. For the second case, the possibility of selection based on similar levels of commitment to CSR lends itself to a statistical test in the data.

To test whether firms systematically hire managers with similar levels of commitment to CSR, I begin by assuming that if firms would like to hire managers from other firms with similar levels of CSR commitment, then a good observable proxy measure of an individual manager's commitment to CSR is the CSR level of the firm the manager is employed at. Then, if firms would like to hire managers with levels of commitment to CSR similar to their own managers, then they are likely to hire managers from firms with similar levels of social performance. Based

on this logic, I model the decision to hire a manager from another firm in a given year as a selection process from the full sample of firms available in the data in that year. First, I again expand the sample to include all firms with available KLD data, and collapse the sample into unique firm-year observations. The data is then expanded again to include every possible pair of firms in the sample in that year, to create the set of possible options that a firm can hire a manager from. Only pairs of firms where the hiring firm actually hires in that year are included in the sample.⁴⁹ A measure of similarity (or difference) in CSR measures between each pair of firms and a series of control variables is then included, and the following specification is run using a logit regression model:

$$\begin{aligned}
 & \text{HIRE MANAGER}_{ijt} \\
 & = F \left(\text{CSR DIFFERENCES}_{ijt}, \text{MGMT DIFF}_{ijt}, \text{FIRM DIFF}_{ijt}, \text{FIRM CONTROLS}_{it}, \right. \\
 & \quad \left. \text{YEAR}_t, \text{INDUSTRY}_i, \text{INDUSTRY}_j \right),
 \end{aligned}$$

where $F(\cdot)$ is the logit function. $\text{HIRE MANAGER}_{ijt}$ is a binary variable equal to one for the firm pair ij and year t where firm i decides to hire a manager from firm j who will arrive in the following year, and zero otherwise. $\text{CSR DIFFERENCES}_{ijt}$ are the absolute value of the difference in aggregated KLD ratings between firm i and firm j in year t for each of the six different dimensions of CSR examined in this study.⁵⁰ MGMT DIFF_{ijt} consists of controls for differences in the average characteristics of executives including differences in the average salary, bonus, and age of the management teams at firm i and firm j in year t .⁵¹ FIRM DIFF_{ijt} includes controls for differences in total assets, net income, total sales, and return on assets. $\text{FIRM CONTROLS}_{it}$ includes controls for total assets, total sales, and return on assets for the hiring firm i in year t .⁵² YEAR_t , INDUSTRY_i , and INDUSTRY_j are fixed effects for year, the industry of the hiring firm, and industry of the firm where the manager is hired from, respectively.⁵³

If firms systematically hire managers with similar levels of CSR commitment, then the lower the difference between the hiring firm and “hired from” firm’s CSR levels, the more likely that a manager will be hired from that firm. Given that the minimum possible value of each of the six variables in CSR DIFFERENCES is zero, a negative and statistically significant coefficient on any of the six dimensions of CSR would provide evidence that firms are more likely to hire from organizations with similar levels of social performance. However, as shown in Table 3.6, none of the coefficients are negative and statistically significant. The results suggest that firms do not hire managers from other firms based upon their similarity in commitment to CSR, and alleviate concerns that firms are deliberately selecting managers into their firms in such a way as to produce the results of this study by construction.

One additional potential concern with the findings is that KLD ratings may be too imprecise a measure of CSR to isolate true individual manager effects. If true, then the study simply

⁴⁹ In other words, if IBM does not hire any managers in a given year, all IBM-Other Firm pairs are dropped from the sample. However, IBM will be included as a possible firm to hire from for other firms that did hire that year. The sample is conditioned on a hiring decision having actually occurred by that firm in that year.

⁵⁰ The same aggregation method (# Strengths - # Concerns) used in the baseline specification is used here.

⁵¹ Differences are measured in absolute values and logged for variables in MGMT DIFF_{ijt} and FIRM DIFF_{ijt} , except for differences in return on assets between each pair of firms. Differences in return on assets are measured in absolute values.

⁵² Total assets and total sales are logged.

⁵³ Industry fixed effects are based on S&P Economic sector codes provided in COMPUSTAT.

establishes that managers have only a small effect on KLD ratings, but managers may still have a significant impact on actual CSR. To address this concern, I replicate the analysis done with KLD ratings on a matched sample of ExecuComp-Toxic Release Inventory (TRI) data to test whether the results are consistent. Created and maintained by the U.S. Environmental Protection Agency, TRI is a database containing annual data on emissions of over 650 toxic chemicals from thousands of facilities in the U.S. Although this only addresses one dimension of CSR covered by the KLD ratings (Environmental), toxic emissions have the advantage of being an objective quantitative measure with clear environmental impact. To create a measure of toxic emissions, I first weight each chemical by its toxicity. Toxicity weighted emissions are calculated by multiplying the quantity of each chemical emitted by the inverse of its reportable quantity, following the method used by King and Lennox (2000). Then, toxicity-weighted emissions of each chemical are summed at the facility level, and ultimately the firm level. The final dependent variable used in estimation is logged toxicity-weighted emissions aggregated at the firm level.

Several adjustments are made from the original approach with the KLD data in analyzing the TRI data, due to the smaller sample. Instead of only considering managers who switch firms that spend at least 3 years at each firm, managers who switch with at least 2 full years at each firm are now included in the sample. Also, CEOs, CFOs, and Other executives are all considered together in a single “manager” category due to the small number of managers who fit the switching criteria.⁵⁴ Finally, the persistence of individual manager influence across firms is not tested, since too few managers who switch firms are in the sample to have an adequate number of observations. When merged with the ExecuComp data, the final dataset consists of 14 firms and 8 managers who ultimately switch firms, with 1,067 observations in total. Similar to the sample selection with matched ExecuComp-KLD data, restricting the sample to firms that contain managers who switch organizations also tends to select larger firms.⁵⁵

The results of the analysis on TRI data are presented in Table 3.7. Similar to the findings from the KLD data, manager fixed effects are statistically significant, but the adjusted R-squared increases only very slightly. The results suggest that managers do have a statistically significant individual influence on firm toxic emissions, but that the magnitude of their impact is very small compared to firm-specific or other external factors. When considered together with the original findings with KLD data, no evidence is found for a strong individual effect of managers on CSR.

5 Conclusion

While the findings of this study provide preliminary insight into the magnitude of individual influence managers may have on CSR, further work is needed to understand how well the results presented here generalize across a variety of empirical settings. In analyzing both the KLD and TRI data, the sample used for analysis in this study consisted of very large U.S. publicly traded firms. Managers in these types of firms may have greater difficulty in influencing firm-level CSR activities simply due to the size of their organizations. By contrast, managers of much smaller firms may also be able to exert greater influence on CSR activities. Also, further work should investigate whether other dimensions of CSR outside of those considered in this study

⁵⁴ The final sample contains 5 CEOs, 1 CFO, and 2 Other executive.

⁵⁵ The ExecuComp-TRI matched sample has average sales of \$2,697.8 million and average total assets of \$2,356.1 million, with the final sample having average firm sales of \$4,268.5 million and total assets of \$3,934.3 million.

may be more sensitive to the individual influence of managers. While I find only small effects with the variables used here, managers may exert much greater influence on other dimensions of CSR, and little is known still about which dimensions of CSR are more sensitive to managerial influence.

An additional limitation of this study is that it does not rule out the alternative explanation that firms may be independently adjusting CSR investments in exactly the opposite direction of the influence of an individual manager who switches firms. For example, a manager may choose to adopt projects that increase a firm's environmentally sustainable business practices, but the firm may be simultaneously and independently increasing toxic emissions at the same time, resulting in no significant observable changes attributable to the manager's fixed effect. While this possibility seems unlikely, it cannot be excluded as a plausible explanation for the results shown here. Further work should attempt to replicate the results here using good instruments for manager movements across firms to test the validity of this alternative explanation.

The results of this study also highlight the need for empirical approaches that are able to better separate the many different factors that may influence CSR. Empirical work in CSR is frequently challenged by unavailable or too few good measures, making causal inference from correlations particularly difficult. Approaches like the one used in this study (collecting panel data and implementing fixed effects), provide at least some remedy to many of these issues. Further empirical work will hopefully make greater use of these types of approaches, and consider the actual magnitudes of their effects on CSR whenever possible.

Finally, the findings presented here beg an important question—why don't managers have a larger individual effect on firm CSR? Are they too constrained by their organizations to influence social performance? Or, are managers so strongly incentivized in other ways by their firms that their CSR decisions ultimately depend on the firm at which they work? Clarifying the mechanisms that drive the results shown here would help identify the factors that motivate CSR practices within firms, and may shed light on why heterogeneity exists in the degree to which different firms invest in CSR.

Table 3.1 Descriptive statistics

Variable	Execucomp Full Data		KLD-Execucomp Matched Sample		Final Sample for Baseline Regression	
	Mean	σ	Mean	σ	Mean	σ
1. Firm Sales (in millions of USD)	4,538.1	13,702.1	6,420.7	16,940.9	12,874.7	23,135.7
2. Firm Total Assets (in millions of USD)	11,820.0	65,456.1	16,884.1	83,079.1	30,108.4	107,110.5
3. Salary (in thousands of USD)	360.7	262.7	427.7	285.7	497.8	301.2
4. Age ¹	52.7	8.1	52.9	7.8	53.9	6.9
5. Average length of firm spell (in years)			6.1	3.5	6.6	3.6
6. Community (# Strengths - # Concerns)			0.14	0.64	0.29	0.89
7. Corporate Governance (# Strengths - # Concerns)			-0.33	0.71	-0.48	0.71
8. Diversity (# Strengths - # Concerns)			0.45	1.29	0.95	1.52
9. Employee Relations (# Strengths - # Concerns)			-0.05	0.94	0.09	1.06
10. Environment (# Strengths - # Concerns)			-0.13	0.80	-0.34	1.21
11. Product (# Strengths - # Concerns)			-0.21	0.71	-0.38	0.87
Sample size (N)	195,750		90,784		14,554	

1. Age was missing for a significant percentage of observations. Observations with nonmissing values were 89,139, 48,997, and 6,153 for each respective sample.

Table 3.2 Comparison between managerial characteristics between managers who switch firms and other managers in the sample

Variable	Managers who switch firms		All other managers in the sample	
	Mean	σ	Mean	σ
1. Age ¹	50.4	5.5	50.9	7.2
2. Salary (in thousands of USD)	342.5	247.0	350.4	209.7
3. Bonus (in thousands of USD)	318.1	466.4	273.7	488.8
Sample size (N)	152		2,921	

1. Age was missing for a significant percentage of observations. Observations with nonmissing values were 27 and 774 for each respective sample.

Table 3.3 Adjusted R-squared comparisons, KLD Ratings

Manager Effects on CSR Measures					
	F-tests on fixed effects for			N	Adjusted R2
	CEOs	CFOs	Other executives		
Community				14,554	0.653
Community	(19.54, <.0001, 64)	(7.71, <.0001, 17)	(401.26, <.0001, 69)	14,554	0.656
Corporate Governance				14,554	0.439
Corporate Governance	(30.12, <.0001, 64)	(18.94, <.0001, 17)	(26.95, <.0001, 69)	14,554	0.442
Diversity				14,554	0.773
Diversity	(30.37, <.0001, 64)	(13.05, <.0001, 17)	(150.08, <.0001, 69)	14,554	0.774
Employee Relations				14,554	0.558
Employee Relations	(67.39, <.0001, 64)	(30.29, <.0001, 17)	(63.28, <.0001, 69)	14,554	0.561
Environmental				14,554	0.709
Environmental	(73.30, <.0001, 64)	(3.66, <.0001, 17)	(53.34, <.0001, 69)	14,554	0.712
Product				14,554	0.669
Product	(4.68, <.0001, 64)	(5.70, <.0001, 17)	(22.37, <.0001, 69)	14,554	0.672

Reported in parentheses is the F-statistic for the joint significance of manager fixed effects, the level of significance of the associated p-value, and the number of constraints.

Table 3.4 Manager influence persistence tests, KLD Ratings

Dependent Variable: Manager's residual from their second firm spell

VARIABLES	Community (1)	Corporate Governance (2)	Diversity (3)	Employee Relations (4)	Environmental (5)	Product (6)
Manager's residual from their first firm spell	-0.000 (0.069)	0.100 (0.109)	0.011 (0.077)	0.005 (0.114)	-0.047 (0.058)	0.080 (0.085)
Observations	152	152	152	152	152	152
R-squared	0.000	0.008	0.000	0.000	0.003	0.006

Standard errors shown are bootstrapped.

*** p<0.01, ** p<0.05, * p<0.1

Table 3.5 Results for time trend estimation prior to the arrival of a switching manager

VARIABLES	Dependent variables					
	Community (1)	Corporate Governance (2)	Diversity (3)	Employee Relations (4)	Environmental (5)	Product (6)
3 year trend prior to the arrival of a switching manager	-0.000 (0.069)	0.100 (0.109)	0.011 (0.077)	0.005 (0.114)	-0.047 (0.058)	0.080 (0.085)
Observations	17,161	17,161	17,161	17,161	17,161	17,161
R-squared	0.68	0.59	0.78	0.61	0.72	0.71
Adjusted R-squared	0.63	0.53	0.75	0.55	0.68	0.66

Standard errors shown are clustered at the firm level.

*** p<0.001, ** p<0.01, * p<0.05

Table 3.6 Results for testing for the importance of similarity in CSR measures when hiring managers who switch firms

Dependent Variable: Hiring a manager from another firm (=1)

VARIABLES	(1)	(2)	(3)	(4)
Difference in Community	0.084 (0.101)	0.099 (0.105)	0.109 (0.099)	0.241** (0.119)
Difference in Corporate Governance	0.188 (0.127)	0.189 (0.127)	0.198 (0.125)	0.078 (0.147)
Difference in Diversity	0.009 (0.070)	0.015 (0.073)	0.015 (0.072)	0.030 (0.085)
Difference in Employee Relations	0.088 (0.085)	0.099 (0.091)	0.098 (0.092)	0.054 (0.093)
Difference in Environmental	0.088 (0.085)	0.099 (0.091)	0.098 (0.092)	0.054 (0.093)
Difference in Product	-0.035 (0.120)	-0.027 (0.119)	-0.024 (0.137)	-0.010 (0.160)
Differences in Avg Executive Characteristics Controls	N	N	N	Y
Differences in Firm Characteristics Controls	N	N	Y	Y
Other Firm Characteristics Controls	N	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Hiring firm Industry Fixed Effects	Y	Y	Y	Y
"Hired from" firm Industry Fixed Effects	Y	Y	Y	Y
Observations	107,913	107,913	107,913	72,552
Pseudo R-squared	0.027	0.027	0.028	0.035

Standard errors in parentheses clustered at the SIC Code level.

Number of observations in Column 4 decreases due to missing data in executive characteristics (e.g. age).

*** p<0.01, ** p<0.05, * p<0.1

Table 3.7 Adjusted R-squared comparisons, Toxic Emissions

Manager Effects on CSR Measures

	Manager Fixed Effects	N	Adjusted R2
Toxic Emissions		1,067	0.855
Toxic Emissions	(9.26, <.001, 8)	1,067	0.856

Reported in parentheses is the F-statistic for the joint significance of manager fixed effects, the level of significance of the associated p-value, and the number of constraints.

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