The Reproduction of Managerial Styles in an Internal Labor Market*

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Abstract

Why do managerial styles persist even when managers turn over? Using personnel records from a large Japanese manufacturer, we estimate manager fixed effects on subordinates' overtime hours as a measure of managerial style. Before promotion, some managers worked under supervisors who demanded more overtime from their subordinates. These managers later tend to demand more overtime from their own subordinates. Additional evidence supports the interpretation that managers learn styles from former supervisors. Moreover, the firm repeatedly assigns managers with similar styles to the same sections over time, sustaining section-level persistence. Together, these processes reproduce managerial styles within an internal labor market.

Keywords: manager effects, managerial style, internal labor markets, organizational persistence, working hours.

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1 Introduction

Managers are an important source of heterogeneity inside organizations. Who manages a firm or organizational unit is associated with substantial differences in how organizations operate and perform, and some manager-specific differences remain visible as managers move across assignments (Bertrand and Schoar, 2003; Fenizia, 2022; Lazear et al., 2015; Minni, 2026; Muñoz and Otero, 2025). Yet managers themselves turn over. Evidence on what remains after managerial separation or turnover is mixed: some effects dissipate, while others persist in workers, practices, or organizational arrangements (Adhvaryu et al., 2026b; Bianchi and Giorcelli, 2022; Bloom et al., 2020; Lazear et al., 2015; Minni, 2026). What makes managerial styles persist in organizational positions when individual managers turn over?

Existing research identifies one route to such persistence: departing managers can leave behind changes in workers, routines, organizational structures, or other organizational arrangements that survive them (Adhvaryu et al., 2026b; Bianchi and Giorcelli, 2022; Bloom et al., 2020; Giorcelli, 2019; Minni et al., 2026). But persistence need not arise only through changes embedded in the unit. Today's managers were yesterday's subordinates. In organizations that promote from within, workers may learn how to manage from the supervisors they work under. Once promoted, the firm decides where to assign them. Managerial style may remain manager-specific and portable across assignments, yet similar styles can recur in the same organizational positions if the personnel system develops and assigns managers in ways that reproduce them. This paper shows empirically how pre-promotion supervisory experience and post-promotion manager assignment combine to generate persistence across manager turnover. Persistence can therefore arise not only through changes embedded in the unit, but also through people who later become managers and are subsequently assigned across positions.

We study this process using personnel records from a large Japanese manufacturer covering the universe of its regular employees from 2013 through 2025. Section managers are promoted predominantly from within, managers rotate across sections, and a central personnel department assigns both workers and managers. We can therefore observe future managers while they are still subordinates and identify their supervisors before promotion, follow them into management and across managerial assignments, and observe the same sections under successive managers. We focus on subordinates' overtime hours, an economically important margin over which section managers organize work and exercise supervisory authority. Our object is managerial style rather than output-based manager quality: we study persistent differences in how managers organize subordinate work, not which managers maximize firm performance.

We first establish an empirical measure of managerial style. We estimate manager fixed effects using an AKM-style decomposition (Abowd et al., 1999) that absorbs persistent section differences and common time shocks. The manager FE has a standard deviation of 9.4 hours of monthly overtime per subordinate, about forty percent of mean monthly overtime in a section–quarter. More important for its interpretation, the manager-specific component is portable across sections: managers associated with more overtime in one set of sections tend also to be associated with more overtime elsewhere. A concern is that this pattern could arise because the firm assigns managers to sections whose overtime was already high or rising. The incoming manager's FE does not predict the section's overtime level or trend before arrival. We therefore use the manager FE as the empirical counterpart of a portable managerial style.

We next ask whether managers resemble the supervisors they worked under before promotion. A potential source of bias is that both FEs may partly use outcomes from the section in which they worked together, which can mechanically induce a positive association. We therefore measure both FEs using outcomes from other sections. A one-hour higher FE for the pre-promotion supervisor is associated with a 0.539-hour higher FE for the future manager. Future managers therefore resemble their pre-promotion supervisors even when both FEs are measured outside the section in which they worked together.

Resemblance alone cannot distinguish learning from initial sorting. A worker's initial section and supervisor may be chosen using information about that worker, so initial sorting can generate resemblance to the supervisor already in place. To distinguish learning from sorting, we use supervisor changes that occur while the worker remains in the same section. The identifying assumption is that the change in supervisor FE is uncorrelated with the worker's pre-existing managerial type, while allowing that type to be correlated with the supervisor already in place. This timing assumption is plausible in this setting because a supervisor change replaces the section head for the whole team rather than assigning a supervisor specifically to the focal worker. Consistent with the timing assumption, recorded worker and section characteristics measured before the switch do not jointly predict the subsequent change in supervisor FE. Workers who experience a shift toward a higher-FE supervisor later have higher manager FEs. The evidence therefore favors learning from supervisors over pure initial sorting.

We then turn to persistence across manager turnover. When a manager leaves, the incoming manager tends to have a similar FE even when both FEs are measured elsewhere. More importantly, similar styles remain visible two and three replacements later. On a common set of four-manager succession chains, the relationship three replacements later is much stronger than would be implied by mechanically carrying adjacent predecessor-successor resemblance forward one transition at a time. The recurrence is therefore tied more closely

to the managerial position than merely to the immediate predecessor. Random-reassignment benchmarks show that part of this recurrence operates through department assignment and additional persistence appears in assignment to particular sections within departments.

The firm develops most section managers internally, so the managerial types available for subsequent assignment are themselves partly shaped inside the organization. Section 4 shows that supervisory experience before promotion helps shape the styles managers later display. To quantify the connection between internal development and section-level recurrence, we decompose persistence using the estimated supervisor–future-manager relationship. About half of section-level persistence in the decomposition sample is associated with incoming managers' pre-promotion supervisors. Viewed alongside the supervisor-switch evidence, this decomposition links internal development before promotion to the recurrence of related styles after assignment.

Finally, we examine the consequences of managerial style for worker outcomes. A manager with a one-standard-deviation higher FE is followed by a 1.78-percentage-point higher probability of exit from the firm within 24 months, relative to a base rate of 7.3 percent. In an internal labor market where exit is uncommon and careers are largely internal, this is an economically substantial difference. Workers under managers with higher FEs also report less favorable working conditions and career perceptions across several dimensions. The additional recorded hours are remunerated and evaluations are somewhat higher, but there is no detectable increase in promotion. These outcomes do not identify workers' net welfare or the net return of these styles to the firm, but they show that the managerial differences at the center of the paper correspond to economically meaningful differences in working conditions and worker outcomes.

We contribute to the literature on organizational persistence by identifying a personnel-based route through which managerial style can persist across generations of managers. The closest conceptual framework is Van den Steen (2010), who shows how managerial influence can persist across generations through learning, screening, and internal succession. In his model, shared beliefs can survive even after the original manager and employees have left.[1] Our setting and empirical object are different: we measure a portable managerial style rather than shared beliefs, and we observe internal assignment rather than a hiring and self-sorting process. The common idea is that persistence can operate through people who later become managers. In our setting, because managers rotate across sections, this process generates persistence in a particular position only when subsequent assignment repeatedly places related managerial types there.

[1]The corporate-culture literature treats such shared beliefs and norms as persistent organizational attributes (Graham et al., 2022; Kreps, 1990).

Empirically, the paper connects several strands of the manager literature. Existing work establishes economically important manager-specific differences in firm policies, worker allocation, productivity, and organizational performance, and shows that some of those differences are portable across assignments (Benson and Shaw, 2025; Bertrand and Schoar, 2003; Best et al., 2023; Fenizia, 2022; Gudgeon et al., 2026; Minni, 2026). Recent work also identifies measurable skills associated with managerial performance (Weidmann et al., 2026). We take manager heterogeneity as the starting point and ask how a portable managerial style is related to experiences before entry into management and how the internal labor market reproduces similar styles after turnover.

A related literature studies how experience shapes subsequent behavior. Macroeconomic conditions, prior professional experience, and family events are associated with later managerial decisions (Dittmar and Duchin, 2016; Ronchi and Smith, 2026; Schoar and Zuo, 2017). Workplace exposure can propagate further. Exposure to supervisors, trainers, or peers can affect the exposed worker's own later behavior (Adger et al., 2026; Frake and Harmon, 2024), affect later advancement into management (Chen et al., 2025; Lyle and Smith, 2014), and appear in how the exposed person subsequently manages others (Frake and Harmon, 2024; Minni et al., 2026). Gudgeon et al. (2026) additionally relate supervisors' own pre-promotion records to their later effects on subordinates. Relative to this work, our contribution is to connect pre-promotion supervisory experience to how the firm assigns managers after promotion and to persistence across repeated manager turnover within one internal labor market. A relationship between future managers and their pre-promotion supervisors need not by itself generate persistence in any particular section because managers rotate across sections; persistence also depends on whom the firm assigns there next. We observe workers under supervisors before promotion, follow those workers after they become managers, and observe successive managers assigned to the same organizational units. We measure managerial style consistently across these stages using the portable manager FE, which also permits a quantitative decomposition of section-level persistence.

Another literature asks what persists after a manager leaves. Persistence can reside in the unit through staffing, routines, or organizational practices that outlast the manager (Adhvaryu et al., 2026b; Bianchi and Giorcelli, 2022; Bloom et al., 2020; Giorcelli, 2019); in workers through job matches or other changes that remain after the manager departs (Minni, 2026); or in the behavior of other managers exposed to the original manager (Minni et al., 2026). Tao and Zhao (2026) document a predecessor-specific route to continuity: firms select CEO successors whose predetermined cultural backgrounds resemble those of outgoing CEOs, consistent with continuity smoothing managerial transitions. Our evidence points to a different margin. Similar portable styles recur across several managerial generations,

and the relationship three replacements later is much stronger than would be implied by repeatedly carrying one-step predecessor-successor resemblance forward. We therefore study persistence anchored in positions and generated through the sequence of managers assigned to them, not only continuity between a departing manager and the immediate successor. This complements work on manager-worker matching within firms (Adhvaryu et al., 2026a).

Finally, the paper speaks to research on working hours. Work hours need not simply reflect workers' unconstrained choices: employer-side constraints and career incentives can shape the hours workers actually work (Kuroda and Yamamoto, 2013; Lachowska et al., 2026; Landers et al., 1996). Management practices and individual managers can also be related to overtime hours and worker outcomes (Okudaira et al., 2024; Okuyama et al., 2026; Tanaka et al., 2025). The importance of long hours can extend beyond individual workers: nonlinear rewards to long hours can also contribute to broader gender inequality (Goldin, 2014). We show that managers are an important within-firm source of variation in subordinates' overtime hours and, more importantly for this paper, that these manager-specific differences become persistent through the internal personnel system. The contribution is therefore not an estimate of the return to long hours, but evidence on how persistent differences in working conditions can be reproduced through managers.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and data. Section 3 defines managerial style, constructs the manager FE used to measure it, and establishes its portability across sections. Section 4 studies the relationship between future managers and their pre-promotion supervisors and the evidence on learning versus initial sorting. Section 5 examines persistence across repeated manager turnover and shows how internal development before promotion and manager assignment after promotion combine to reproduce managerial style. Section 6 studies worker outcomes under managers with different FEs. Section 7 concludes.

2 Institutional Setting and Data

Our setting is a large Japanese manufacturing firm that provided personnel records covering the universe of its regular employees from 2013 through 2025.[2] Three features of its internal labor market are central to the analysis: personnel assignments are centrally administered, manager changes follow a regular annual personnel cycle, and section managers are promoted predominantly from within the firm. The linked records allow us to follow employees from their years as subordinates through promotion into management, identify the supervisors

[2]The same firm's personnel records are used by Okuyama et al. (2026) to study how promotion practices widen the gender pay gap following childbirth.

they worked under before promotion, and then observe the workers they later supervise. We can also follow managers across sections and observe the same sections under successive managers.[3] This section describes these institutional features, the measurement of overtime, and the structure and timing of the data used in the empirical analysis.

2.1 The Firm and Its Personnel System

Employees are organized into sections within three broad departments: manufacturing, sales, and the back office. Each section is headed by a single section manager, and the personnel records identify both that manager and the employees assigned to the section in each month. Below the section-manager rank, employees occupy several non-managerial ranks; acting and assistant managers occupy intermediate positions and are treated separately in the analysis, as described in Appendix A.1. The section is therefore the organizational unit at which we observe a designated manager together with the workers under that manager.

Assignments are centrally administered by the firm's personnel department, which decides both the sections to which workers are assigned and the managers who head them. There is no internal posting or bidding market: workers do not formally apply to work under particular supervisors, and section managers do not recruit or assemble their own teams. Transfers and manager assignments instead occur through the firm's personnel process. This centralized system is consistent with canonical descriptions of personnel administration in large Japanese internal labor markets (Aoki, 1990). Assignments are therefore purposive rather than random, but they are not decentralized choices by workers or section managers.

Manager assignments change on a regular personnel calendar. The firm's fiscal year runs from September through August, and 81.9 percent of datable manager changes in the analytic panel occur in September, with another 12.1 percent in March. Managers rotate across sections, so the same manager can be observed heading different sections over time and the same section under different managers. This mobility allows us to distinguish manager FEs from section FEs. Appendix A.1 reports the rotation calendar and typical manager–section spell lengths in greater detail.

Section managers are promoted predominantly from within the firm rather than hired directly into the rank.[4] The personnel records therefore allow us to observe many future managers before they enter the section-manager rank, identify the sections and supervisors under whom they worked before promotion, and then follow them into management and

[3] Single-firm personnel records have been used to study internal labor markets since Baker et al. (1994). For manager effects estimated in Japanese firm records, see Owan et al. (2014) and Okajima et al. (2021).

[4] Benson et al. (2019) study which workers firms promote into management. We take promotion from within as given and ask how the styles of those promoted are related to their earlier supervisors.

across managerial assignments.

Section moves are common, while moves across departments are less frequent. At promotion into management, 61 percent of newly promoted managers change sections, while 14 percent change departments; Table 1, Panel D reports mobility over longer windows before and after promotion.

2.2 Overtime Institutions and Measurement

Overtime is an economically meaningful margin over which section managers exercise supervisory authority in this firm. Section managers organize the work of their sections and direct or approve the overtime performed by the workers below them, so overtime is not simply a schedule chosen unilaterally by workers. Under the firm's wage rules, the standard full-time workday is 8 hours. Japanese labor law separately limits statutory working time to 8 hours per day and 40 hours per week, with 45 hours per month serving as the ordinary benchmark under the overtime regime.[5]

Our measure is monthly recorded overtime from the firm's payroll records. Working time is recorded through the firm's electronic timekeeping system, and the measure captures paid working time beyond an employee's scheduled hours. For standard full-time workers, scheduled working time is 8 hours per day. The measure is administrative rather than self-reported and does not capture off-the-books or otherwise unrecorded work. It captures the overtime hours worked by subordinates in each section. The overtime hours of section managers themselves are not recorded because employees receiving the managerial allowance are excluded from the payroll overtime records.

2.3 Data and Descriptive Statistics

The personnel records link several administrative and survey sources at the employee level. Monthly payroll records report overtime and compensation, while monthly assignment records identify each employee's section, department, rank, and section manager. Personnel-action records report promotions and transfers, and annual evaluation records provide personnel evaluations. An annual worker survey measures workers' perceived workload, job satisfaction, and other self-reported job outcomes. Together, these records allow us to follow employees

[5]Labor Standards Act (Act No. 49 of 1947), Arts. 32, 36, and 37; Ministry of Labour Notification No. 154 of 1998; and the 2018 Work Style Reform amendments. The statutory overtime caps introduced under the Work Style Reform took effect during our sample period (Burdin et al., 2024; Jiang and Sasaki, 2026). The section–quarter analysis below includes firm-wide quarter fixed effects, which absorb common changes in overtime over time. Appendix A.1 provides further institutional and measurement details.

across assignments and ranks, identify who works under whom, and link those relationships to working hours and worker outcomes. Appendix A.2 provides the exact variable definitions.

The firm's fiscal year runs from September through August and is labeled by the calendar year in which it ends. Payroll and assignment records are observed monthly. For the manager FE estimation in Section 3, we aggregate these records to section–quarter cells. Annual administrative measures are aligned to the August fiscal-year-end snapshot, while worker survey outcomes are measured in January. Appendix A.2 reports the exact timing and covariate construction used in each analysis.

Table 1 summarizes the managers, section–quarter cells, and workers represented in the data used for the analysis. Panel A describes the 406 managers whose FEs are estimated in the main specification. Panel B describes 7,621 section–quarter observations in the largest connected component of the manager–section mobility network, in which managers and sections are linked through observed manager moves, spanning 196 sections, and Panel C describes the non-managerial workers whose hours enter those cells. A section–quarter contains about twenty workers on average. Mean monthly overtime is 23.3 hours when section–quarters are weighted equally and 28.8 hours across pooled worker-months; 10.3 percent of worker-months exceed 45 recorded overtime hours, the ordinary monthly benchmark under Japan's overtime regime. Managers and sections are observed repeatedly across assignments: the average manager heads 1.96 sections over the sample, 62 percent head at least two, and the average section is observed under 4.10 managers. More than half of the managers in the estimation sample are observed entering the rank from below during the sample period. These repeated movements provide the longitudinal support for the analyses that follow.

Assignments change substantially over multi-year horizons. Table 1, Panel D, reports mobility around the first promotion into the section-manager rank. Among future managers observed for the full five years before promotion, about 68 percent change sections and about 18 percent change departments. Promotion is itself a major reassignment point: about 61 percent enter the manager rank in a different section from the one in which they last worked, and about 14 percent in a different department. Mobility continues after promotion, with about 75 percent changing sections and about 23 percent changing departments within five years. The three-year rates show substantial mobility over shorter horizons as well. Table 11 reports the full set of horizons. Thus, promotion is an important reassignment point, but it is one part of a longer career process in which employees continue to move across sections both before and after entering management.

Table 1: Descriptive statistics

Panel A. Managers	
Female	0.03
Age	47.29
Firm tenure (years)	25.81
Sections run over the sample	1.96
Two or more sections over the sample (share)	0.62
Quarters observed as manager	18.67
Promoted into the rank during the sample (share)	0.55
N	406
Panel B. Section–quarters	
Worker count	20.77
Mean monthly overtime (h)	23.28
SD across sections	8.66
SD within section over time	6.23
Share above 45 recorded overtime hours	0.05
Female share among workers	0.30
Managers per section over the sample	4.10
Section–quarter obs. (N)	7,621
Unique sections	196
Panel C. Worker–months	
Female	0.25
Age	36.84
Firm tenure (years)	15.00
Monthly overtime (h), mean (SD)	28.77 (15.44)
Worker–months above 45h overtime (share)	0.10
Unique workers	5,769

Panel D. Mobility around promotion into management

Career window	Section change	Department change	N
Within 5 years before promotion	0.68	0.18	207
Within 3 years before promotion	0.46	0.13	254
At promotion	0.61	0.14	311
Within 3 years after promotion	0.49	0.15	416
Within 5 years after promotion	0.75	0.23	293

Notes: The panels use different units of observation. Panel A contains one observation per manager for the 406 managers whose manager FEs are estimated in the main specification. Manager characteristics are measured in the first quarter observed in the role. Panel B contains one observation per section–quarter in the largest connected component before the first-quarter exclusion used in Equation (1), with each cell weighted equally. The two overtime-dispersion rows report the standard deviation of section means across sections and the standard deviation of within-section deviations over time. The long-hours share is the mean across section–quarters of the within-cell share of worker-months above 45 recorded overtime hours. The 45-hour threshold is the ordinary monthly benchmark under Japan's overtime regime. Panel C pools the worker-months of eligible non-managerial workers, so larger sections receive greater weight. Appendix Table 10 reports Panels B and C separately by department. Panel D reports, for first promotions into the section-manager rank, the share of promotion events with at least one section change and the share with at least one department change during each indicated window. The pre-promotion windows end before the promotion transition and the post-promotion windows begin after it, so the promotion transition enters neither cumulative measure; "At promotion" compares the last assignment before promotion with the first managerial assignment. Each row uses promotion events observed for the full window indicated. Organizational recodings, renamings, and restructurings without a personnel reassignment are not counted as moves. Table 11 reports the full set of horizons and the common-sample comparisons.

3 Measuring Managerial Style

We use "managerial style" to refer to persistent differences in how managers organize and direct subordinate work that are reflected in subordinates' overtime hours and are portable across assignments. This concept does not imply a directly observed preference for long hours, a structural labor-demand parameter, or a particular managerial channel such as staffing, scheduling, work organization, or workplace expectations.

We use a manager fixed effect estimated from subordinates' overtime hours as the empirical counterpart of this concept and ask whether it has the properties that this interpretation requires. Section 3.1 uses manager mobility across sections to estimate the manager FE and establishes that it contains substantial signal and economically meaningful dispersion. Section 3.2 tests whether the manager-specific component is portable across sections and whether it is related to overtime patterns already present in a section before the manager arrives. Appendix B provides additional diagnostics on pre-existing section conditions, manager–section match components, and predetermined workforce composition. Together, these exercises assess whether the manager FE provides a useful empirical measure of managerial style.

3.1 Estimating the Manager Fixed Effect

Let H_{jq} denote mean monthly overtime among the non-managerial workers in section j during quarter q, and let $m(j,q)$ denote the manager assigned to that section–quarter. We estimate the manager component α_m from

$$H_{jq} \;=\; \alpha_{m(j,q)} \;+\; \theta_j \;+\; \tau_q \;+\; u_{jq}, \tag{1}$$

where θ_j is a section fixed effect and τ_q is a quarter fixed effect (Abowd et al., 1999). The section effect absorbs persistent differences across sections, so a section that systematically runs longer hours does not contribute to α_m on that account. The quarter effect absorbs movements common to the firm. The manager and section components are separated by mobility: the rotation system places the same manager in multiple sections and the same section under multiple managers. We refer to the resulting estimate $\hat{\alpha}_m$ as the manager FE.

The manager FE is designed to capture the manager-specific component of subordinates' overtime hours without imposing a particular channel through which managers affect hours. Managers may influence overtime through work allocation, staffing, scheduling, workplace expectations, or other aspects of how the section is managed. We therefore do not condition in the main specification on contemporaneous section size or worker composition, which may

themselves respond to manager assignment. Appendix B.3 instead asks whether workforce composition already in place before the manager arrives accounts for the estimated manager component.

Estimation uses section–quarter cells with a unique designated manager and complete quarterly measurement. Manager changes are concentrated at the annual rotation boundary, and the first quarter of a manager–section pair is a natural transition period in which practices already in place may not yet reflect the incoming manager. Following Fenizia (2022), the main specification therefore excludes the first quarter of each manager–section pair.[6] We then retain the largest connected set of managers and sections linked by mobility. Detailed sample construction and the resulting sample flow are reported in Appendix A.3 and Appendix B.1.

Raw manager FEs are estimated with different precision because managers are observed for different numbers of section–quarters. We therefore use a static empirical-Bayes correction that shrinks noisier estimates toward zero according to their precision (Chetty et al., 2014). The correction is quantitatively modest, so the raw and shrunk distributions are very similar. Appendix B.1 gives the estimator and discusses limited-mobility bias and alternative implementations.[7]

Table 2 summarizes the dispersion and signal in the estimated manager component. Across managers in the connected set, the empirical-Bayes manager FE has a standard deviation of 9.4 hours of monthly overtime per subordinate, and its 90–10 spread is 22.8 hours. The latter is approximately the mean monthly overtime of a section–quarter in this firm, 23.3 hours. Adding the manager FEs to the section and quarter effects raises the adjusted R^2 from 0.725 to 0.826, while the classical joint F against the model without them, computed under homoskedastic errors, is 10.3 ($p < 0.001$).

These results establish that the manager FE contains substantial manager-specific signal and economically meaningful dispersion. But joint significance and in-sample explanatory power are not, by themselves, sufficient evidence that estimated manager effects capture persistent manager-specific heterogeneity. Fee et al. (2013) and Jarosiewicz and Ross (2023) show that such statistics can remain large under placebo or permuted manager assignments. Our interpretation therefore requires evidence that the manager-specific component reappears outside the assignment in which it is measured. Section 3.2 tests this portability directly.

[6] Appendix B.1 shows that the first-quarter exclusion has little effect on the estimated manager FEs: estimates with and without the exclusion correlate 0.991. Selecting the largest connected component before rather than after the exclusion also yields virtually identical estimates.

[7] Limited-mobility bias is a related concern when manager and section effects are identified from mobility (Andrews et al., 2008; Bonhomme et al., 2023). Appendix B.1 explains why we use a homoskedastic sampling-error correction rather than the leave-out variance estimator of Kline et al. (2020); the latter's leave-out requirement retains a substantially smaller comparison sample.

Table 2: Dispersion and signal of the manager FEs

SD of the manager FE, raw	9.62
SD of the manager FE, shrunk	9.4
90–10 spread of the manager FE, shrunk	22.8
Joint F on the manager FEs	10.3 (df 405, 6,143; $p < 0.001$)
Adjusted R^2 without manager FEs	0.725
Adjusted R^2 with manager FEs	0.826

Notes: The table describes the manager FEs from Equation (1) on the section–quarter estimation sample, which excludes the first quarter of each manager–section pair. The three dispersion rows use manager FEs estimated after the exclusion and then restricted to the largest connected component, covering 406 managers. The joint F-statistic and adjusted R^2 rows use the specification estimated after restricting to the largest connected component, on 6,787 section–quarters, 406 managers, and 193 sections. The joint F-statistic is the classical homoskedastic test of the manager FEs against the same model without them. The spread is the difference between the ninetieth and tenth percentiles of the empirical-Bayes manager FEs across managers.

3.2 Portability Across Sections

A central implication of managerial style as defined above is portability. If the manager FE captures a manager-specific component rather than conditions particular to the sections the manager happens to run, a manager who is associated with more overtime hours among subordinates in one set of sections should also be associated with more overtime hours elsewhere. We test this by measuring the same manager separately across sections.[8] For each manager–section spell, we estimate the manager's FE in that focal section and compare it with the same manager's FEs in the other sections the manager has run. For example, if a manager has run sections A, B, and C, when A is the focal section the outcome is the manager FE estimated in A and the predictor is the quarter-weighted mean of the FEs estimated in B and C. We repeat this exercise across manager–section spells. We call the predictor the leave-section-out manager FE. A positive slope means that a manager who has a higher FE in other sections also has a higher FE in the focal section; a slope of zero would imply no portability across sections.

Panel A of Table 3 shows cross-section portability. A one-hour higher leave-section-out manager FE predicts a 0.540-hour higher manager FE in the focal section (SE 0.055). When both sides are measured using spells of at least eight quarters, the slope rises to 0.667 (SE 0.073). For comparison, studies in other manager settings report out-of-sample persistence estimates ranging from 0.37–0.40 for managerial ratings and firm policies to about 0.80 for

[8] An event study around manager turnover, as in Fenizia (2022), compares a section's hours before and after the change and asks whether the difference tracks the incoming manager's style measured elsewhere. That design requires that shocks to a section's hours die out across the change. Fenizia's offices satisfy it, with a residual serial correlation of 0.04. Sections here carry their shocks across the change, with a serial correlation of 0.46 (SE 0.021), so a before-and-after comparison within a section would mix the incoming manager's effect with the section's own drift. Measuring the manager in other sections uses no within-section change over time, so that drift cannot enter.

boss effects on worker output, with store-manager productivity estimates of roughly 0.5–0.85 (Bertrand and Schoar, 2003; Hoffman and Tadelis, 2021; Lazear et al., 2015; Metcalfe et al., 2023). Although these estimates measure different managerial attributes and use different validation designs, our portability estimate is of a similar order of magnitude. Thus, differences associated with the same manager systematically reappear across separate assignments.

Portability could nevertheless arise if the firm systematically assigns managers to sections whose conditions already point toward similar overtime outcomes. Panel B tests whether the incoming manager's leave-section-out manager FE predicts the section's residual overtime level and trend over the preceding four quarters. It does not: the coefficient is 0.000 (SE 0.020) for the pre-arrival level and −0.002 (SE 0.010) for the pre-arrival trend.[9] Appendix B.3 shows that conditioning on predetermined workforce composition likewise leaves the manager FEs and the portability relationship essentially unchanged. Together, these exercises provide little indication that pre-existing section conditions account for the portability result. Appendix B.2 additionally finds only modest departures from additivity at the manager–section match level.

These results support using the manager FE as an empirical measure of managerial style. The measure contains substantial manager-specific signal, is portable across sections, and shows little relationship with conditions already present before manager arrival. We therefore use "managerial style" in the remainder of the paper to refer to the portable manager-specific component of subordinates' overtime hours captured by the manager FE.

4 Pre-Promotion Supervisors and Future Managerial Style

This section asks how managers' styles are related to the supervisors under whom they worked before promotion. Because the firm fills its section-manager assignments almost entirely through internal promotion, we observe future managers while they are still subordinates and identify their pre-promotion supervisors. Table 1, Panel D, shows that these workers are reassigned across sections before promotion.

We proceed in three steps. Section 4.1 documents a strong relationship between managers and their pre-promotion supervisors. Resemblance alone, however, can reflect either learning while working under a supervisor or initial sorting of workers with pre-existing differences into particular supervisory environments. Section 4.2 uses supervisor changes that occur after the worker is already in the section to distinguish these explanations. Section 4.3 considers the main remaining concerns about selection and contemporaneous organizational change.

[9] Appendix B.2 reports a related turnover-based diagnostic: pre-event overtime growth does not jointly predict the incoming-minus-outgoing change in manager FE in the primary specification ($p = 0.132$).

Table 3: Portability of the manager FE across sections

	Slope on the leave-section-out manager FE	Managers
A. Outcome: manager FE in this section		
All spells	0.540	233
	(0.055)	
Spells with eight or more quarters on each side	0.667	91
	(0.073)	
B. Placebo outcome: the section's overtime before the manager arrives		
Level over the four prior quarters, net of the predecessor	0.000	224
	(0.020)	
Trend over the four prior quarters, hours per quarter	−0.002	224
	(0.010)	

Notes: Each observation is a manager–section spell. The regressor is the leave-section-out manager FE, defined as the quarter-weighted mean of the manager's spell fixed effects in the manager's other sections. Spell fixed effects are estimated from a section-by-quarter model with department-by-quarter time effects, excluding the focal manager's own observations when estimating the nuisance components. Panel A uses the manager FE estimated in the focal section as the outcome. Panel B uses the focal section's residual overtime over the four quarters before the manager arrives: either its mean net of the predecessor's FE or its linear trend in hours per quarter. Standard errors are clustered by manager and reported in parentheses.

4.1 Resemblance to the Pre-Promotion Supervisor

We begin by asking whether managers resemble the supervisors under whom they worked before promotion. We define the pre-promotion supervisor as the manager of the section in which the future manager worked in the fiscal year before entering the manager rank. We observe 204 promotion events for which both FEs are available. We regress the future manager's FE on the pre-promotion supervisor's FE, with one observation per promotion event and standard errors clustered by pre-promotion supervisor. These coefficients describe the relationship among observed promotions; they are not causal effects.

Table 4 shows a positive relationship. Using all-section manager FEs, a one-hour higher FE for the pre-promotion supervisor is associated with a 0.918-hour higher FE for the future manager (SE 0.054). Among the 123 future managers promoted into a different section, the slope is 0.868 (SE 0.072). The relationship therefore does not depend on the future manager immediately taking over the section in which the two worked together.

The all-section measures may nevertheless share variation from the section in which the pair worked together. The supervisor's FE is estimated partly from outcomes in that section, and both FEs may contain local shocks or manager–section components not fully absorbed by the additive fixed effects. We therefore measure both managers outside the section they shared, using the leave-section-out manager FE defined in Section 3.2. Among

the 88 promotion pairs for which this measure can be constructed for both managers, the slope using all-section manager FEs is 0.871 (SE 0.102). Using leave-section-out manager FEs on the same pairs lowers the slope to 0.539 (SE 0.098). Excluding the shared section therefore attenuates the relationship, but a substantial positive resemblance remains.[10]

Figure 1 shows that the observations also cluster by pre-promotion department. Departments are a relatively persistent layer of the firm's assignment structure, and department assignment is itself chosen by the firm. Table 4, Panel B therefore uses the department comparison to locate where the resemblance appears rather than to define a preferred specification. On the common leave-section-out sample, the pooled all-section slope is 0.871 (SE 0.102) and the corresponding within-department slope is 0.590 (SE 0.157). Using leave-section-out manager FEs, the pooled slope is 0.539 (SE 0.098) and the within-department slope is 0.333 (SE 0.127). The attenuation indicates that part of the overall resemblance is associated with department assignment, while the positive within-department relationship shows that the resemblance is not confined to differences across departments.

The resemblance remains observational: it is consistent with learning, but it can also arise from initial sorting into supervisory environments. Section 4.2 therefore turns to supervisor changes after the worker is already in the section to distinguish these explanations.

4.2 Evidence from Supervisor Changes

Related work shows that workplace exposure can affect workers' later behavior, including how exposed workers subsequently manage others (Frake and Harmon, 2024; Minni et al., 2026).[11] We exploit supervisor changes that occur while the worker remains in the same section. This allows us to ask whether a change in supervisory exposure predicts the managerial style the worker later displays.

We use "learning" broadly to mean that experience under a supervisor affects the managerial style a worker later displays; we do not distinguish among imitation, specific management practices, changes in norms or beliefs, or related channels. Initial sorting, by contrast, allows workers with pre-existing differences to be assigned to supervisors or sections in ways that generate resemblance even if supervisory experience has no effect on their later managerial style.

We restrict attention to future managers who remain in the same section while their

[10] Appendix C.1 reports additional measurement checks. The relationship remains positive after removing overlapping workers and organizational settings from the two manager-FE estimation samples. Split-sample IV gives a similar leave-section-out estimate (0.574, compared with 0.546 under OLS).

[11] Related work also studies how supervisors and mentors affect subordinates' subsequent careers (Chen et al., 2025; Lyle and Smith, 2014), how workers learn from coworkers (Jarosch et al., 2021), and how workers learn managerial practices from peers (Shue, 2013).

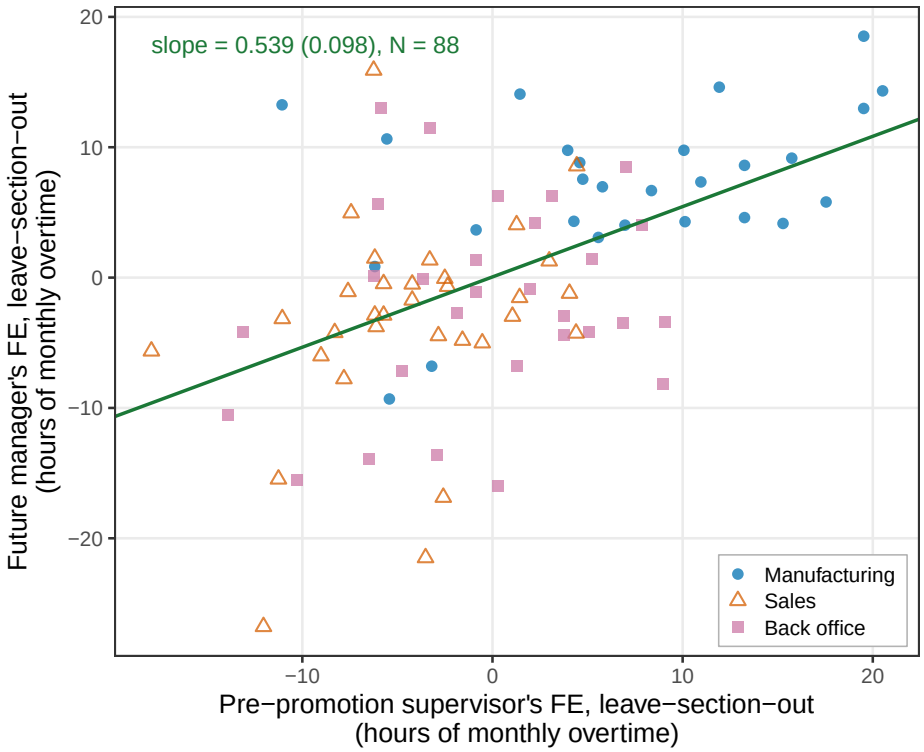


Figure 1: Supervisor–future-manager FEs

Notes: Each point is one of the 88 promotion pairs for which both leave-section-out manager FEs are defined. Marker shape and color identify the future manager's pre-promotion department. The future manager's FE excludes the section in which the pair worked together; the supervisor's FE excludes both that shared section and the future manager's later section. Both are measured in hours of monthly overtime. The line is the pooled fitted relationship corresponding to Table 4, with a slope of 0.539 and a standard error of 0.098 clustered by pre-promotion supervisor. Department coding is descriptive: department assignment is itself part of the firm's personnel process.

supervisor is replaced through the firm's rotation process.[12] For future manager i, let $o(i)$ denote the outgoing supervisor and $n(i)$ the incoming supervisor, and define

$$D_i = \widehat{\alpha}^{(-i)}_{n(i)} - \widehat{\alpha}^{(-i)}_{o(i)}, \tag{2}$$

where both supervisor FEs are re-estimated after removing i's own person-months. The future manager's own overtime therefore enters neither component of D_i.[13] We then estimate

$$\widehat{\alpha}_i = \delta D_i + \lambda_{c(i)} + \varepsilon_i, \tag{3}$$

[12] Cullen and Perez-Truglia (2023) and Minni (2026) also use within-firm manager rotation to compare the same workers under different managers.

[13] The focal switch section remains in this baseline construction. Appendix C.3 reconstructs the supervisor change using FEs estimated outside that section. The relationship remains positive, but this construction sharply reduces the sample and substantially changes the measured supervisor variation, so the coefficient magnitudes are not directly comparable.

Table 4: Resemblance to the pre-promotion supervisor

	Slope on the pre-promotion supervisor's FE		
	Pooled relationship	Within pre-promotion department	Promotion events
Panel A. Supervisor–future-manager resemblance			
All promotions, all-section manager FEs	0.918		204
	(0.054)		
Different-section promotions, all-section manager FEs	0.868		123
	(0.072)		
Leave-section-out sample, all-section manager FEs	0.871		88
	(0.102)		
Leave-section-out manager FEs (Figure 1)	0.539		88
	(0.098)		
Panel B. Pooled and within-department resemblance			
All-section manager FE	0.871	0.590	88
	(0.102)	(0.157)	
Leave-section-out manager FE	0.539	0.333	88
	(0.098)	(0.127)	

Notes: One observation per promotion event. Each row regresses the future manager's FE on the pre-promotion supervisor's, with supervisor-clustered standard errors in parentheses. In *Panel A* the first three rows measure both manager FEs over all sections, the third on the promotion pairs of the fourth, which measures both outside the section the two shared (Figure 1); no controls enter. *Panel B* uses the same 88 promotion pairs as the final two rows of Panel A. The first column reports the pooled relationship. The second conditions on the pre-promotion department, so the coefficient is identified from variation within the firm's three broad departments. Pre-promotion department is the department of the section in which the future manager worked under the supervisor before promotion. This comparison describes where the resemblance appears in the assignment structure; it is not a preferred causal specification.

where $\widehat{\alpha}_i$ is the empirical-Bayes-shrunk manager FE the worker later displays after entering management and $\lambda_{c(i)}$ denotes promotion-cohort fixed effects. The coefficient δ therefore relates the change in supervisor FE at the switch to the manager FE the worker later displays.

The identifying idea comes from the timing of the supervisor change. Let T_i denote the worker's latent managerial type: the style the worker would later display if supervisory experience did not change it. Initial sorting may relate T_i to the section or supervisor to which the worker was assigned before the switch. The timing assumption instead concerns the subsequent supervisor change. Let $\widetilde{D}_i$ and $\widetilde{T}_i$ denote the population residuals from projecting D_i and T_i on the promotion-cohort indicators and any pre-switch controls included in the specification. The timing assumption is

$$\mathrm{Cov}(\widetilde{D}_i, \widetilde{T}_i) = 0. \tag{4}$$

In words, after partialling out the controls in the specification, the subsequent change in supervisor FE is uncorrelated with the worker's latent managerial type. Thus, the design does not require the worker's initial assignment to be random.

Under the timing assumption, pure initial sorting predicts a zero coefficient on D_i. The

worker may have been selectively assigned before the switch, but that pre-existing type is unrelated to the subsequent change in supervisor FE. Learning predicts a different pattern: if experience under supervisors affects later managerial style, a shift toward a higher-FE supervisor should be associated with a higher FE when the worker later becomes a manager. A positive δ therefore favors learning over pure initial sorting under the timing assumption.[14]

The institutional setting makes the timing assumption plausible, but institutional practice alone cannot verify the assumption. Initial worker assignments can reflect information about the individual worker. A later supervisor rotation, in contrast, replaces the section head for the whole team rather than assigning a supervisor specifically to one subordinate who is already there. To assess whether the data are consistent with the timing assumption, we examine whether information measured before the switch predicts the subsequent change in supervisor FE.

Panel A of Table 5 reports the estimates. With promotion-cohort fixed effects, the coefficient is 0.74 (SE 0.25). Adding the section's pre-switch overtime and staffing conditions gives 0.86 (SE 0.22), and additionally controlling for the future manager's own pre-switch overtime trend gives 0.89 (SE 0.24). The positive relationship is therefore not explained by the observed pre-switch conditions of the section or by the worker's own pre-existing overtime trajectory. All three estimates also remain positive, with point-centered sensitivity intervals above zero, when Appendix G re-estimates the manager-FE objects in the resampling sensitivity analysis.

The switch result provides the key distinction from the resemblance in Section 4.1. Initial sorting can generate a relationship between a worker and the supervisor already in place when the worker is assigned. Here, by contrast, the worker is already in the section when the supervisor changes, and the managerial style the worker later displays moves in the direction of that subsequent change. Under the timing assumption, this is the pattern predicted by learning but not by pure initial sorting.

Panel B asks whether information available before the switch predicts the subsequent supervisor change. In the future-manager switch sample, recorded worker characteristics and pre-switch section conditions do not jointly predict D_i ($p = 0.224$). The same worker records do, however, strongly predict the FE of the supervisor already in place before the switch (the outgoing supervisor; $p < 0.001$). Observable sorting into supervisory environments is therefore apparent before the switch, while within this future-manager sample the subsequent supervisor change is not jointly predicted by the recorded pre-switch information.

The switch evidence favors learning over pure initial sorting under the timing assumption.

[14]Appendix C.2 also shows that initial matching to a similar outgoing supervisor would, if anything, work against a positive switch coefficient, because the outgoing supervisor enters D_i with a negative sign.

The sample contains 78 future managers, so the estimate is more informative about the direction of the relationship than about its precise magnitude. Section 4.3 considers the main remaining concerns: broader organizational changes accompanying supervisor replacement, selection created by conditioning on eventual promotion, and latent selection not captured by the observed pre-switch controls.

4.3 Interpretation: Learning and Selection

The supervisor-switch result favors learning over pure initial sorting under the timing assumption in Section 4.2. We now consider three remaining questions: whether the relationship instead reflects broader organizational changes that accompany supervisor replacement, whether conditioning on eventual promotion generates the positive relationship, and what forms of latent selection could still account for the result.

A supervisor replacement may coincide with broader changes in the section. If changes in supervisor FE systematically coincide with changes in coworker composition or staffing, the switch coefficient could reflect both the new supervisor and those contemporaneous changes. Appendix C.3 examines recorded changes in coworker composition and headcount around the switch and re-estimates the switch relationship after excluding events with the largest coworker turnover. These checks do not capture unobserved changes in tasks or other operating conditions. In the recorded data, however, larger supervisor-FE changes are not systematically accompanied by larger team changes, and the switch estimate remains positive after the highest-churn events are excluded.

Table 5: Supervisor-switch estimates and pre-switch predictability

	$\hat{\delta}$	(SE)	N
A. Future manager's later FE on the supervisor change			
Promotion-cohort effects	0.74	(0.25)	78
+ section pre-switch conditions	0.86	(0.22)	76
+ future manager's own pre-switch overtime trend	0.89	(0.24)	76
B. Predictability of the supervisor change from pre-switch information			
Change vs. records and section conditions (p)	0.224		
Outgoing supervisor's FE level vs. records (p)	< 0.001		

Notes: The sample is future managers whose supervisor was replaced while they stayed in the same section, before their own promotion. D is the incoming supervisor's FE minus the outgoing supervisor's, both re-estimated after removing the future manager's own person-months. The focal switch section remains in this baseline construction; Appendix C.3 reports a diagnostic that instead estimates both supervisors' FEs outside the switch section. *Panel A* reports $\hat{\delta}$, the coefficient on D in a regression of the future manager's later FE. The rows are cumulative: all carry promotion-cohort effects; the second adds the section's pre-switch conditions; the third retains all preceding terms and adds the future manager's own pre-switch overtime trend. All controls are measured before the switch. Standard errors, in parentheses, cluster on the switch event. The first row has 74 switch-event clusters, the last two 72. *Panel B* reports, with the same cohort effects, the p-value of a joint test that the future manager's records and the section's pre-switch conditions predict D, and of a test that the records predict the outgoing supervisor's FE.

Conditioning on eventual promotion could also induce selection. The positive relationship between the change in supervisor FE and the worker's later manager FE could arise even without learning if workers with higher pre-existing managerial types were more likely to be promoted after experiencing a larger increase in supervisor FE. Because pre-existing managerial type is not observed directly, Appendix C.3 constructs a predicted managerial-type index from personnel information recorded before the switch.[15] The index uses prior overtime, evaluations, rank, tenure, education, hiring route, demographics, and related personnel records to predict the manager FE a worker would later display. We apply this index to a broader sample of pre-promotion workers who experience a supervisor replacement, including workers who are not subsequently promoted, and ask whether larger increases in supervisor FE are more likely to be followed by promotion among workers with higher predicted managerial types. The data provide no evidence of this pattern. This diagnostic therefore provides no support for promotion selection as an explanation for the positive relationship between changes in supervisor FE and workers' later manager FEs.

The remaining concern is selection on information about the worker that is not recorded in the personnel data. To assess how strong such selection would have to be, Appendix C.3 applies the sensitivity analysis of Cinelli and Hazlett (2020) to the fully controlled switch specification. The exercise asks how strongly a single omitted factor would have to be related to both the supervisor change and the worker's later manager FE, after removing the variation explained by the included controls, to reduce the estimated switch coefficient to zero. The resulting robustness value is 0.37: such a factor would have to explain 37 percent of the residual variation in both variables. For comparison, the largest partial R^2 attained by any single recorded benchmark is 0.11. The confounding required to account for the estimated relationship would therefore have to be substantially stronger than any single observed benchmark.

Across these analyses, the supervisor-switch evidence favors learning over pure initial sorting under the timing assumption. The additional selection diagnostics are consistent with this interpretation. Section 5 turns to whether the firm subsequently assigns managers in ways that make similar managerial styles recur in the same sections.

[15] We estimate the prediction model by lasso on workers who are later observed as managers, using 22 pre-switch personnel variables as candidate predictors of their later manager FE, and apply the fitted model to workers in the broader pre-promotion sample. Tuning uses five folds defined by pre-promotion-supervisor clusters.

5 Personnel Assignment and Reproduction of Managerial Style

Section 4 showed that supervisory experience before promotion helps shape the managerial styles workers later display, with the supervisor-switch evidence favoring learning over sorting. Because the firm develops most section managers internally but rotates them across sections after promotion, this alone does not generate persistence in any particular position. Section 5 asks how internal development and subsequent assignment combine to produce such persistence. Section 5.1 shows that the recurrence is tied to the position rather than merely to the immediate predecessor; Section 5.2 links this recurrence to managers' pre-promotion supervisors; Section 5.3 interprets the two stages together as reproduction of managerial style.

5.1 Persistence Across Repeated Manager Turnover

We begin by asking whether managerial styles persist when one manager replaces another in the same section. For each succession pair, we construct leave-section-out manager FEs from the managers' other sections and exclude any other section they both managed. Thus, neither FE uses outcomes from the section in which the succession occurs.

Table 6 shows substantial resemblance across successive managers. A one-hour higher leave-section-out manager FE for the outgoing manager is associated with a 0.576-hour higher FE for the incoming manager (SE 0.062). More importantly, this relationship remains 0.549 two replacements later and 0.494 three replacements later. On a common set of four-manager succession chains, the relationship three replacements later is 0.443, compared with 0.147 from mechanically iterating the adjacent-turnover relationship. The recurrence therefore attenuates far less than would be implied by repeatedly carrying predecessor-successor resemblance forward one transition at a time.[16]

This distinction matters for interpreting succession. Resemblance to the immediate predecessor can be economically valuable even if a position has no persistent demand for a particular managerial type. A change in leadership may disrupt established expectations, coordination, and organizational practices, so appointing a similar successor can reduce the adjustment costs of transition. Tao and Zhao (2026), for example, interpret firms' tendency to select CEO successors who are culturally closer to outgoing CEOs as such a smoothing mechanism. This predecessor-specific mechanism predicts resemblance from one manager to

[16]Appendix G shows that the persistence estimates remain positive and well away from zero in a resampling sensitivity analysis that re-estimates the manager-FE objects while also resampling the downstream section clusters.

the next. By itself, however, it does not explain why managers remain similar after several intervening replacements. In our data, the longer-horizon relationship is much stronger than would be implied by repeatedly carrying one-step resemblance forward. The evidence therefore points to the managerial position, rather than only the immediate predecessor, as the more persistent anchor of the recurrence.[17]

Figure 2 uses random reassignment to ask where in the assignment process this position-level recurrence appears (Young, 2019). Randomly reassigning incoming managers across all sections turning over in the same year produces a benchmark centered near zero. Preserving the set of managers assigned to each department and year and randomizing only their section assignments raises the benchmark mean to 0.257. Because department assignment is itself chosen by the firm, this second exercise holds fixed one layer of the assignment process rather than removing it. The shift indicates that part of the recurrence reflects which managerial types are assigned to each department. The realized slope of 0.576 remains substantially higher, indicating additional persistence in which managers the firm assigns to particular sections within departments.

The random-reassignment exercises show where persistence across multiple managerial generations appears within the firm's assignment structure. Similar portable styles recur across several managerial generations, and this recurrence operates partly through department assignment and partly through assignment to particular sections within departments. The firm therefore repeatedly places related managerial types in the same managerial positions rather than merely replacing one manager with a similar immediate successor. Appendix D.1 provides the detailed construction and shows that the pattern appears for both lateral rotations and managers entering the rank through promotion.

5.2 Internal Development of Managers

Section 5.1 shows that particular managerial positions repeatedly receive managers with related portable styles. Where do these styles come from? In this firm, most section managers are promoted from within, so the managerial workforce is largely developed inside the organization rather than hired from an external market. Workers spend years under supervisors before entering management. Section 4 showed that supervisory experience helps shape the styles they later display as managers, with the supervisor-switch evidence favoring learning over initial sorting. The managerial styles available for subsequent assignment are

[17] One possibility is that the firm preserves a predecessor's style more strongly when it views that predecessor favorably. Appendix D.5 tests this implication. Succession persistence is not detectably stronger following managers with higher personnel evaluations, whether evaluation enters continuously or the sample is split at the median.

Table 6: Persistence across repeated manager turnover

Panel A. Full samples

Replacements apart	Observed slope (SE)	Events
One	0.576 (0.062)	336
Two	0.549 (0.068)	245
Three	0.494 (0.079)	155

Panel B. Common four-manager chains

Replacements apart	Observed slope (SE)	Mechanical benchmark	Observed − benchmark [95% bootstrap CI]	Chains	Sections
One	0.527 (0.110)	–	–	99	63
Two	0.391 (0.092)	0.278	0.113 $[-0.090, 0.278]$	99	63
Three	0.443 (0.107)	0.147	0.297 $[0.074, 0.472]$	99	63

Notes: Panel A regresses the incoming manager's leave-section-out manager FE on the outgoing manager's using all available pairs at each horizon. Panel B estimates all three horizons on the same 99 four-manager succession chains in 63 sections. For each comparison, the focal section and every other section held by both compared managers are excluded from both manager FEs. The mechanical benchmark is the square or cube of the one-replacement slope estimated on the same chains. Bracketed intervals are section-cluster bootstrap percentile intervals over $B = 1{,}999$ draws, within which all three slopes are re-estimated. The leave-section-out manager FEs are constructed directly from the spell fixed effects and do not use the empirical-Bayes shrinkage applied to the global manager FEs. Standard errors in parentheses are clustered by section.

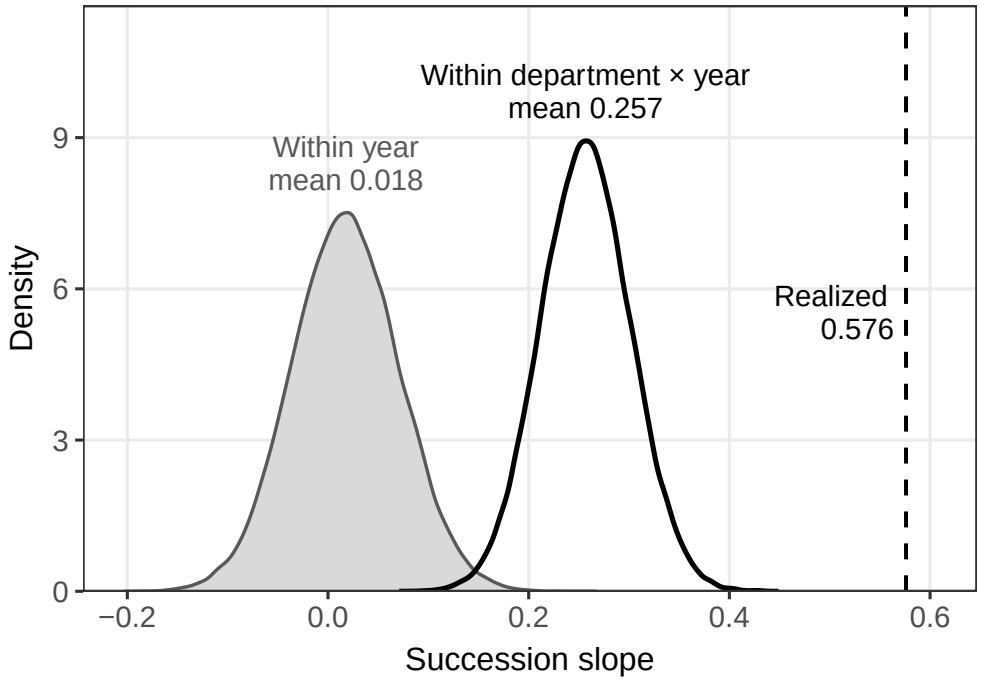


Figure 2: Random-reassignment benchmarks

Notes: The figure uses the 336 adjacent manager turnovers in Table 6. Incoming managers are randomly reassigned either across all sections turning over in the same year or across sections turning over in the same department and year, over $B = 49{,}999$ draws. For each counterfactual pair, the two manager FEs are reconstructed using the same section exclusions as in Table 6; pairs with no remaining section are dropped from that draw. The two distributions have means 0.018 and 0.257 and standard deviations 0.053 and 0.045, respectively. The dashed vertical line marks the realized slope of 0.576. For each benchmark, p is the share of reassignment draws with a slope at least as large as the realized slope; both are <0.001.

therefore themselves partly developed inside the firm.

To connect this internal development to the recurrence documented in Section 5.1, we ask how much of the incoming manager's FE is associated with the manager's pre-promotion supervisor. Consider an adjacent manager turnover. We use leave-section-out manager FEs throughout. Let α_{out}, α_{in}, and α_{sup} denote the FEs of the outgoing manager, the incoming manager, and the incoming manager's pre-promotion supervisor. We first define the section-level persistence slope b by

$$\alpha_{\text{in}} = a + b\alpha_{\text{out}} + u_{\text{in}}. \tag{5}$$

Section 4.1 estimates that a one-hour higher FE for the pre-promotion supervisor is associated with a 0.539-hour higher FE for the future manager. Let $\beta_F = 0.539$ denote this supervisor–future-manager slope. We write

$$\alpha_{\text{in}} = \beta_F \alpha_{\text{sup}} + v_{\text{in}}, \tag{6}$$

where the first term is the component of the incoming manager's FE associated with the pre-promotion supervisor relationship and v_{in} is the remainder. If b_{sup} is the slope of the pre-promotion supervisor's FE on the outgoing manager's FE and b_{rem} is the corresponding slope for v_{in}, then

$$b = \beta_F b_{\text{sup}} + b_{\text{rem}}. \tag{7}$$

The first term is the supervisor-linked component of section-level persistence.

The decomposition can be constructed for 153 of the 336 adjacent turnovers in Table 6, primarily because many incoming managers entered the managerial rank before the personnel records begin. On this sample, the persistence slope is 0.506. Table 7 shows that 0.267 is supervisor-linked and 0.240 remains. Thus, about half of section-level persistence in this sample is associated with the supervisory histories through which the firm developed its incoming managers.

This result provides a quantitative connection between internal development and subsequent assignment. The firm develops most of its managers internally, supervisory experience helps shape the styles they later bring into management, and the assignment process places managers with related styles into positions where those styles recur. The decomposition may also reflect some remaining selection, but the supervisor-switch evidence in Section 4 makes learning the leading interpretation. We therefore interpret the supervisor-linked component as pointing to a role for learning during managers' internal development in the persistence documented in Section 5.1.

Finally, Appendix D.2 details the construction of the decomposition, including the leave-out restrictions and sample accounting. Appendix D.3 asks whether the supervisor-linked component is driven by the most direct cases, in which the outgoing manager had previously supervised the incoming manager before promotion. The component remains positive after excluding those cases.

Table 7: Decomposition of section-level persistence across adjacent manager turnover

	Estimate
Incoming manager's FE (b)	0.506 [0.329, 0.659]
Supervisor-linked component ($\beta_F b_{\text{sup}}$)	0.267 [0.152, 0.407]
Remaining component (b_{rem})	0.240 [0.036, 0.428]
Share supervisor-linked ($\beta_F b_{\text{sup}}/b$)	0.526 [0.287, 0.903]
Pre-promotion supervisor's FE (b_{sup})	0.494 [0.331, 0.655]
Supervisor–future-manager slope (β_F, fixed)	0.539 (0.098)
Turnover events	153

Notes: The sample consists of 153 adjacent manager turnovers, of the 336 in Table 6, for which the incoming manager's pre-promotion supervisor has a usable leave-section-out manager FE. Brackets report 95 percent two-sample cluster bootstrap percentile intervals, 999 replications, resampling sections in the decomposition sample and pre-promotion supervisors in the Section 4.1 sample. The point decomposition fixes β_F at the Section 4.1 estimate of 0.539. In the bootstrap, β_F is re-estimated within each resample of the Section 4.1 sample, so the percentile intervals incorporate uncertainty in that estimated relationship. The manager FEs themselves are held fixed across replications. The standard error of the fixed supervisor–future-manager slope is in parentheses. Appendix G reports a separate resampling sensitivity analysis that re-estimates the manager-FE objects. Components may not sum exactly to the reported persistence slope because of rounding.

5.3 Interpretation: Reproduction

Sections 4 and 5 describe a personnel process through which managerial style can be reproduced across generations. Supervisory experience before promotion helps shape the styles workers later bring into management, and the firm subsequently assigns managers so that related portable styles repeatedly appear in the same positions. This interpretation is closely related to Van den Steen (2010), in which managerial influence persists across generations through learning, screening, and internal succession. Our setting adds an explicit assignment step: because managers rotate across sections, developing managers with related styles is not sufficient for persistence in any particular position. Persistence also depends on where those managers are assigned.

This personnel-based channel differs from persistence through changes embedded in

the organizational unit after a manager leaves. Management practices, staffing, routines, and other organizational arrangements can survive the individuals who introduced them (Adhvaryu et al., 2026b; Bianchi and Giorcelli, 2022; Bloom et al., 2020; Giorcelli, 2019). Learning from supervisors is itself a form of persistence from one managerial generation to the next, but it resides in the people who later become managers rather than in the unit they once worked in. Because those managers subsequently move across sections, persistence in a particular position additionally depends on where the firm assigns them. Unit-level legacies and this personnel-based channel may also interact: earlier managers may leave persistent changes in workers, routines, or operating conditions that affect which managerial types are subsequently well suited to similar sections.

This raises the question of why particular positions repeatedly receive particular managerial types. A natural possibility is persistent manager-position matching. Different managerial types need not be uniformly better across organizational environments. Bandiera et al. (2020), for example, show that the productivity consequences of CEO behavioral types depend on the CEO-firm match rather than simply ranking one type as better for all firms. Within firms, Adhvaryu et al. (2026a) show that managers and production lines are systematically matched, with assignment reflecting organizational objectives beyond a simple ranking of managerial quality. Stable differences in tasks, workforces, or other operating requirements could therefore make particular managerial types better suited to particular kinds of sections, leading related types to be assigned repeatedly to similar positions.

The firm also need not assign managers on managerial style itself. Firms can match managers to positions using experience or other attributes that are relevant to the position; for example, Custódio and Metzger (2014) document systematic CEO-firm matching on prior functional experience. If such attributes are correlated with managerial style, the recurrence we observe could arise as a consequence of assignment on those attributes. Our evidence therefore identifies a persistent assignment pattern, but not the characteristic on which the firm is matching managers to positions.

Nor do we identify the firm's objective in sustaining this assignment pattern. Managers with higher FEs do not systematically receive higher evaluations, pay, or promotion, so the observed personnel outcomes do not reveal a simple firm-wide preference for higher-FE styles (Appendix D.4). The available operating records also do not establish whether assigning related managerial types to particular positions raises firm performance (Appendix F). The evidence thus shows how internal development and assignment reproduce managerial style, while leaving open why the firm favors particular manager-position combinations and whether those assignments are productive.

6 Worker Outcomes

We now ask whether the managerial differences measured by the manager FE are consequential for workers. Section 6.1 studies exit following manager arrival. Section 6.2 examines compensation, evaluation and promotion, workload, and job satisfaction.

6.1 Worker Exit

We first ask whether the arrival of a manager with a higher manager FE is followed by higher exit from the firm among workers in the section. Both field-experimental and non-experimental evidence shows that managers can materially affect employee retention (Friebel et al., 2022; Hoffman and Tadelis, 2021). Leaving the firm is consequential in this internal labor market, where careers are largely internal and exit is uncommon (cf. Kambayashi and Kato, 2026). The personnel records do not classify separation reasons or observe workers' outside alternatives.

As in the supervisor-switch design in Section 4.2, the exit analysis uses manager changes generated by the firm's personnel rotation. For each September manager succession, we take the eligible non-managerial workers present in the section when the incoming manager takes over as the baseline worker sample. For each monthly horizon through 24 months after manager arrival, the outcome indicates whether the worker has separated from the firm by that horizon. We exclude workers close to mandatory retirement.[18] We use the same baseline workers at every horizon and restrict the succession events to those for which the personnel records extend at least 24 months after manager arrival. Section fixed effects compare workers present at successive manager arrivals within the same section, year fixed effects absorb firm-wide shocks, and predetermined worker characteristics adjust for differences in the workers present when each manager arrives. For worker i present in section j when manager $m(j,t)$ arrives in year t, we estimate

$$\text{exit}_{ijt} \;=\; \beta\,\hat{\alpha}^{\text{EB}}_{m(j,t)} \;+\; X_i'\gamma \;+\; \theta_j \;+\; \tau_t \;+\; \varepsilon_{ijt}, \tag{8}$$

where X_i includes demographics, pre-arrival overtime, the preceding personnel evaluation, and an indicator for whether the worker was assigned to the section in the same September rotation as the incoming manager rather than already working there. Because the arriving manager may rotate again during the outcome window, β relates the initial manager assignment to exit over the subsequent window rather than requiring continuous exposure to the same

[18] The firm has a mandatory retirement age of 60. Appendix E.1 describes the age restriction used to exclude scheduled retirement from the exit outcome.

manager for the full 24 months. The identifying assumption is that, conditional on section fixed effects and predetermined covariates, the arriving manager's FE is unrelated to changes in the exit propensity of the workers present when the manager arrives.

Figure 3 shows how the exit difference evolves after manager arrival. The coefficients are scaled to a one-standard-deviation increase in the manager FE across managers, corresponding to about 9.3 hours of monthly overtime per subordinate. A one-standard-deviation higher manager FE is followed by a 0.97-percentage-point higher probability of exit from the firm within 12 months (SE 0.61), relative to a base rate of 3.8 percent. By 24 months, the difference is 1.78 percentage points (SE 0.75), relative to a base rate of 7.3 percent. Thus, a one-standard-deviation higher manager FE is followed by an increase in 24-month exit of roughly one-quarter of the baseline exit rate. The monthly profile generally rises with the horizon, indicating that the 24-month difference accumulates over time rather than being concentrated only around the manager transition.

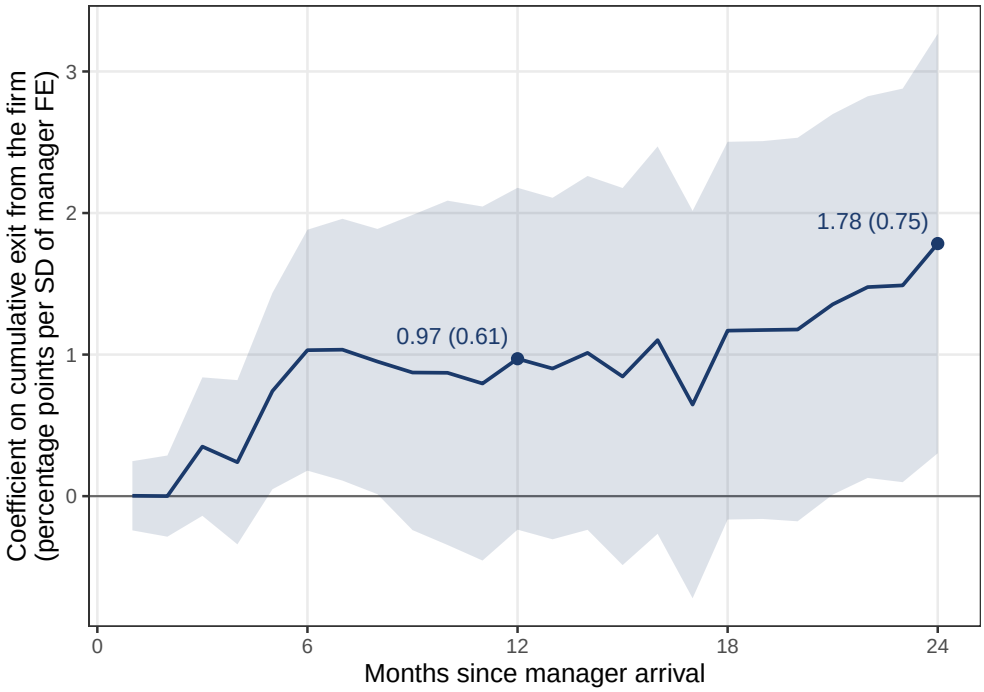


Figure 3: Worker exit following manager arrival

Notes: Each point reports a separate regression of exit from the firm by the indicated month after manager arrival on the arriving manager's EB-shrunk manager FE, scaled to one SD and expressed in percentage points; the band is the 95 percent pointwise confidence interval. Base exit rates are 3.8 percent at 12 months and 7.3 percent at 24 months; the estimate for months 13–24 is 0.81 percentage points (SE 0.56). All horizons use 8,060 worker-events from 435 succession events. Controls are section and year fixed effects, pre-arrival overtime and its square, preceding evaluation, demographics, and an indicator for assignment to the section in the same rotation as the incoming manager. Standard errors are two-way clustered by worker and incoming manager.

A concern is that managers with higher FEs may be assigned to sections in which exit was already likely to rise. Appendix E.1 adds the section's recent exit history and, separately,

department-by-year fixed effects to absorb common department-year shocks. The 24-month estimate is no smaller in either exercise. These checks provide little indication that observable pre-existing exit trajectories or common department-year shocks account for the result.[19]

6.2 Compensation and Other Worker Outcomes

Most workers nevertheless remain with the firm. We therefore examine compensation, evaluation and promotion, workload, and job satisfaction among workers observed under managers with different FEs. Manager rotations allow us to compare the same worker under managers with different FEs. For each outcome observed in at least two fiscal years, we estimate

$$Y_{it} \;=\; \delta\, \hat{\alpha}^{\mathrm{EB}}_{m(j(i,t),t)} \;+\; \mu_i \;+\; \theta_{j(i,t)} \;+\; \tau_t \;+\; \varepsilon_{it}, \tag{9}$$

where μ_i, $\theta_{j(i,t)}$, and τ_t are worker, section, and fiscal-year fixed effects. Worker fixed effects absorb persistent differences across workers, while section fixed effects absorb persistent differences across sections. We do not condition on rank because promotion is itself an outcome of interest. Identification therefore comes from changes in the manager FE to which a worker is exposed over time, conditional on section and fiscal-year effects. Such changes arise when the manager of a worker's section turns over and when the worker moves across sections headed by different managers. The identifying assumption is that these changes are unrelated to other time-varying determinants of the worker outcomes.

Table 8 reports estimates scaled to a one-standard-deviation increase in the manager FE across managers, again corresponding to about 9.3 hours of monthly overtime per subordinate. Panel A of Table 8 reports compensation. Under a manager with a one-standard-deviation higher FE, total monthly pay is 16.57 thousand yen higher (SE 1.37) and hours-linked pay is 19.13 thousand yen higher (SE 1.14). Because recorded overtime determines overtime compensation in payroll, the additional recorded hours are remunerated. The larger increase in hours-linked pay than in total pay indicates that other pay components move partly in the opposite direction. For pay outcomes, the focal worker's own overtime contributes to both the manager FE and hours-linked pay. Appendix E.2 therefore re-estimates the manager FE without that worker's observations. The compensation estimates become somewhat smaller but remain positive and precisely estimated.

Panel B reports evaluation and promotion. Evaluation is 0.065 standard deviations higher

[19] Appendix E.1 also instruments the all-section manager FE with a leave-section-out FE constructed from the manager's other sections. The estimate remains positive and similar in magnitude on the same sample, although less precise. Appendix G reports a joint worker-resampling sensitivity check in which the manager FEs are re-estimated rather than held fixed. Along the worker sampling dimension, re-estimation changes the dispersion little relative to the corresponding fixed-FE exercise.

under a manager whose FE is one standard deviation higher (SE 0.035).[20] We do not, however, detect a corresponding increase in promotion: the promotion estimate is 0.58 percentage points lower (SE 0.43), relative to a base rate of 3.83 percent. Appendix E.2 confirms that the evaluation measure itself is strongly related to advancement: a one-standard-deviation higher evaluation predicts a 5.07-percentage-point higher probability of promotion in the following fiscal year in a specification with worker fixed effects. Thus, although the evaluation measure is strongly related to advancement in general, the somewhat higher evaluations under managers with higher FEs are not accompanied by detectably higher promotion.[21]

Panel C reports five worker-reported outcomes.[22] The pattern is consistently less favorable under managers with higher FEs: workers report lower job satisfaction, less willingness to seek promotion, a weaker sense that effort is rewarded by the firm, heavier workload, and greater work difficulty. A one-standard-deviation higher manager FE is associated with 0.187 standard deviations higher workload (SE 0.024) and 0.073 standard deviations lower job satisfaction (SE 0.028). The sharpened false-discovery-rate q-values of Anderson (2008) are below 0.05 for all five outcomes, ranging from 0.001 to 0.021.

The higher exit documented in Section 6.1 could change which workers remain observed in Table 8. Appendix E.2 examines this concern in three ways. First, it re-estimates the specifications among workers who remain with the firm through the last complete fiscal year. Second, it uses inverse-probability-of-firm-retention weighting based on predetermined worker characteristics and prior personnel records. Third, it asks how large the outcome difference between leavers and stayers would have to be for selection bias alone to explain the entire baseline estimate. Overall, the pattern in Table 8 is robust to these selective-attrition checks.[23]

The outcome pattern is not simply that managers with higher FEs are associated with more recorded overtime. The additional recorded hours are compensated, and evaluations are somewhat higher, but we detect no corresponding increase in promotion. Workers also report less favorable working conditions and career perceptions across the five survey measures, and Section 6.1 shows that a higher manager FE is followed by higher exit from the firm. These results show that the managerial differences reproduced through the internal personnel

[20] Supervisors differ systematically in how they evaluate subordinates (Frederiksen et al., 2020; Kanayama et al., 2025).

[21] Long working hours predict promotion in other settings (Frederiksen et al., 2026; Kato et al., 2016). Using personnel records from the same firm, Okuyama et al. (2026) also show that longer hours are positively related to evaluation and promotion, especially at lower ranks.

[22] Related work links manager- or supervisor-driven differences in working conditions to job satisfaction, workload, health, and separations (Frankort and Avgoustaki, 2026; Kuroda and Yamamoto, 2018; Okudaira et al., 2024).

[23] Appendix G reports the corresponding worker-resampling sensitivity check; re-estimating the manager FEs changes the sampling dispersion little.

Table 8: Worker outcomes

Outcome	Coefficient on the manager FE (per SD)	N	q
Panel A. Compensation			
Total wage	16.57 (1.37)	39,001	–
Hours-linked pay	19.13 (1.14)	39,001	–
Panel B. Evaluation and promotion			
Evaluation	0.065 (0.035)	36,826	–
Promotion	−0.58 (0.43)	36,120	–
Panel C. Worker-reported outcomes			
Job satisfaction	−0.073 (0.028)	21,334	0.012
Promotion willingness	−0.065 (0.027)	11,038	0.013
Effort is rewarded by firm	−0.096 (0.034)	15,244	0.010
Workload	0.187 (0.024)	27,859	0.001
Work difficulty	0.035 (0.020)	27,813	0.021

Notes: Each coefficient is scaled to an increase of one manager-level standard deviation in the manager's EB-shrunk all-section manager FE. Estimates are from Equation (9), with worker, section, and fiscal-year fixed effects, on non-managerial workers. Total pay and hours-linked pay are measured in thousands of real yen per month. The evaluation outcome is the firm's role-based personnel evaluation, standardized to unit variance. Promotion indicates movement to a higher rank in the following fiscal year and is reported in percentage points. Worker-reported outcomes are standardized to unit variance and are coded so that higher values mean more satisfied, more willing to seek promotion, more rewarded for effort, heavier work volume, and more difficult work. Panels A and B cover fiscal years 2014–2025, with promotion observed through 2024; Panel C covers the survey years in which each item is available. Standard errors are two-way clustered by worker and manager and reported in parentheses. The q column reports sharpened false-discovery-rate q-values (Anderson, 2008), computed jointly over the five worker-reported outcomes in Panel C.

system correspond to economically meaningful differences in workers' working conditions and outcomes. They do not determine whether workers are better or worse off overall or identify the net return of these styles to the firm.

7 Conclusion

This paper shows how an internal labor market can reproduce managerial style across successive managers. Future managers resemble the supervisors they worked under before promotion, and the supervisor-switch evidence favors learning over pure initial sorting if subsequent supervisor changes are unrelated to workers' pre-existing managerial types. After promotion, related portable styles recur in the same managerial positions across multiple replacements, with persistence much stronger than would be implied by carrying predecessor-successor resemblance forward one transition at a time. These patterns are connected: about half of section-level persistence in the decomposition sample is associated with incoming managers' pre-promotion supervisors. The evidence therefore shows how internal development before promotion and assignment after promotion can combine to reproduce managerial style inside an organization.

The broader implication is that organizational persistence need not reside only in changes

embedded in a unit after a manager leaves. The evidence favoring learning points to another route: managerial influence can persist through people who later become managers, while assignment determines where their styles subsequently appear. A managerial style can therefore remain manager-specific and portable while related styles continue to recur in the same position because the internal personnel system repeatedly assigns managers with those styles there. Replacing an individual manager need not substantially change how a position is managed if the personnel system continues to develop and assign managers in ways that reproduce similar styles. The worker outcomes also show that the managerial differences being reproduced correspond to economically meaningful differences for workers: managers with higher FEs are followed by more worker exit, while workers report systematically less favorable working conditions and career perceptions.

Several questions remain open. We study one large manufacturer and one dimension of managerial style, measured through subordinates' overtime hours. The data do not identify the specific content of what workers learn from supervisors, which features of managerial positions make particular managerial types more suitable, or the net value of these assignments to the firm. Nor can we determine whether the firm targets managerial style itself or assigns managers on other attributes correlated with style. An important next step is to determine whether similar reproduction occurs for other dimensions of management and in other internal labor markets, and when reproducing existing styles helps or hinders organizational performance and worker welfare.

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A Setting, Measurement, and Samples

A.1 Institutional Details and Measurement

The personnel records distinguish non-managerial workers from section managers and from intermediate acting and assistant-manager positions. Non-managerial workers occupy staff, team-leader, and senior-team-leader ranks, while section managers independently head sections. Acting and assistant managers occupy intermediate positions but do not independently head a section. The payroll data do not record overtime for employees receiving the managerial allowance, including section managers themselves. The overtime measure used in the paper is therefore constructed from eligible non-managerial workers.

Manager changes follow a regular annual personnel cycle. In the analytic panel, 81.9 percent of datable manager changes occur in September, at the boundary of the firm's September-to-August fiscal year, and another 12.1 percent occur in March. The concentration differs across departments: 95.3 percent of datable changes in sales occur in September, compared with 73.7 percent in manufacturing. Completed manager–section spells cluster at 4, 8, 12, and 16 quarters, consistent with rotations organized around the annual personnel cycle. The personnel department also described a norm of limiting time in the same managerial assignment to roughly four years.

Section managers are promoted predominantly from within the firm. We identify entry into the section-manager rank from the personnel records. For each future manager, we define the pre-promotion supervisor using the August snapshot at the end of the fiscal year preceding the worker's first quarter in the manager rank. The pre-promotion supervisor is the section manager heading the worker's section at that snapshot. Because the records follow employees continuously across ranks and assignments, we observe the future manager's section and supervisor before promotion and the subsequent entry into management. This definition underlies the supervisor–future-manager relationship studied in Section 4.

Under the firm's wage rules, the standard full-time workday is eight hours. Work beyond an employee's scheduled hours is recorded for payroll purposes, including overtime within statutory hours, overtime beyond statutory hours, and overtime worked on holidays. Some employees work shorter scheduled hours, so recorded overtime can include paid hours beyond scheduled hours that remain within statutory working hours. The resulting measure therefore captures paid working time beyond the employee's scheduled hours rather than total time spent working.

Working time is recorded through the firm's electronic timekeeping and payroll systems. The payroll records do not capture off-the-books or otherwise unrecorded work. The overtime measure used in the paper is therefore an administrative measure of recorded, paid hours

beyond scheduled working time for non-managerial workers.

A.2 Variables and Timing

The firm's fiscal year runs from September through August and is labeled by the calendar year in which it ends; fiscal 2019, for example, runs from September 2018 through August 2019. Payroll and assignment records are observed monthly. For the manager FE estimation, monthly subordinates' overtime hours are aggregated to section–quarter cells, with quarters indexed by their starting calendar month. Annual administrative measures, including compensation, evaluations, rank, and workers' own overtime, are aligned to the August fiscal-year-end snapshot. Survey outcomes are aligned to the January snapshot, when the worker survey is administered. When a specification uses a worker's monthly overtime around an annual snapshot, we use the mean of the snapshot month and the preceding two months.

Compensation is measured at the August fiscal-year-end snapshot in thousands of real yen per month. Total pay is the sum of time pay, rank pay, age-based pay, and other pay components. Hours-linked pay is the firm's time-pay component and includes overtime compensation. The evaluation records contain two measures: a role-based evaluation, which assesses performance relative to the requirements of the employee's role, and an achievement evaluation, which assesses attainment of assigned targets. These measures are used as outcomes or predetermined covariates depending on the analysis. For Section 4, promotion into management is defined as the first observed entry into the section-manager rank. In the worker- and manager-outcome analyses, promotion is defined as movement to a higher rank in the following fiscal year.

The January worker survey records five self-reported job outcomes: job satisfaction, willingness to seek promotion, whether the worker reports that effort is rewarded by the firm, perceived workload, and work difficulty. Survey responses used in the analysis are standardized to unit variance. Individual items are not available in every survey year.

Several analyses use worker and section characteristics measured before a personnel event. Recorded worker characteristics include demographics, firm tenure, education, hiring route, rank, prior overtime, and personnel evaluations. Hiring route distinguishes new-graduate and mid-career hires; a residual category covers other or unclassified entry routes. Predetermined section characteristics include prior overtime, section size, and worker composition.

A.3 Sample Construction and Additional Descriptive Statistics

We construct the section–quarter panel from the monthly assignment and payroll records using a common set of eligibility rules. Secondees and employees assigned overseas are

excluded. We code a section's department from the department labels of its non-managerial workers, using the modal department within each section-month and then aggregating to the section–quarter level. This avoids treating occasional person-level department labels as section reassignments. No section in the resulting analytic panel changes its modal department within the analyzable period. A manager is linked to a section only through that manager's main assignment; concurrent assignments do not make the manager the head of an additional section. A quarter is retained when the section contains at least one eligible non-managerial worker in each of its three months and has a unique designated manager in the quarter's final month. We then average monthly subordinates' overtime hours to the section–quarter level.

Estimating manager and section FEs requires managers and sections to be connected through observed manager mobility. Before the first-quarter exclusion, the unrestricted section–quarter panel contains 31 connected components. Its largest component contains 415 managers, 196 sections, and 7,621 section–quarter observations (Table 9). Within this component, 253 managers are observed heading more than one section and 181 sections are observed under more than one manager. It also contains 602 manager-to-manager transitions within sections. This mobility provides the network support for estimating relative manager and section FEs. The 30 components outside this pre-exclusion largest component contain little additional mobility-based identifying variation: 18 consist of one manager in one section, one contains one manager across two sections, and the 11 components with multiple managers provide 31 within-component manager contrasts in total. The main manager-FE specification then excludes the first quarter of each manager–section pair, recomputes the connected components, and retains the largest resulting component; Appendix B.1 reports the resulting estimation sample.

The composition of the analytic panel differs substantially across the firm's three departments. Table 10 shows that manufacturing sections are considerably larger and have higher overtime than sales and back-office sections, while back-office sections are the smallest and have the lowest overtime. Manager turnover is observed in all three departments. These differences provide descriptive context for analyses that pool departments or compare assignments within department and year; we do not treat the departments as comparable operating environments.

B Managerial-Style Measurement and Validation

This appendix provides additional validation of the manager FE used in Section 3. Appendix B.1 gives construction and estimation details, B.2 examines sorting and additivity, and B.3 examines predetermined workforce composition.

Table 9: Manager–section mobility in the largest connected component

	Largest connected component
Managers	415
Sections	196
Managers observed in more than one section	253
Sections observed under more than one manager	181
Manager-to-manager transitions within section, by department	602
Manufacturing	187
Sales	163
Back office	252
Section–quarter observations	7,621

Notes: The table describes the largest connected component of the manager–section mobility network before the first-quarter exclusion used in the main manager-FE specification. A manager-to-manager transition is counted when one observed manager-section spell is immediately followed by another manager in the same section, with no intervening observational gap. Appendix B.1 describes the first-quarter exclusion and the resulting manager-FE estimation sample.

Table 10: Section and worker descriptives by department

Panel B. Section–quarters

	(1) Full	(2) Manuf.	(3) Sales	(4) Back office
Worker count	20.77	51.03	9.68	6.61
Mean monthly overtime (h)	23.28	30.57	24.05	17.80
SD across sections	8.66	7.23	5.74	7.18
SD within section over time	6.23	5.26	6.38	6.73
Share above 45 recorded overtime hours	0.05	0.12	0.01	0.02
Female share among workers	0.30	0.18	0.25	0.41
Managers per section over the sample	4.10	4.45	3.98	3.97
Section–quarter obs. (N)	7,621	2,292	1,997	3,332
Unique sections	196	51	56	89

Panel C. Worker–months

	(1) Full	(2) Manuf.	(3) Sales	(4) Back office
Female	0.25	0.22	0.25	0.43
Age	36.84	36.18	38.19	39.20
Firm tenure (years)	15.00	14.59	15.86	16.43
Monthly overtime (h), mean (SD)	28.77 (15.44)	31.52 (15.23)	24.26 (10.74)	18.04 (14.44)
Worker–months above 45h overtime (share)	0.10	0.13	0.01	0.03
Workers	5,769	4,498	860	1,961

Notes: Column (1) repeats the corresponding statistics from Table 1; columns (2)–(4) report them separately by department. Panel B gives section–quarter statistics with each cell weighted equally. Panel C pools worker-months of eligible non-managerial workers, so larger sections receive greater weight. Workers who change departments during the sample are counted in each department in which they are observed.

Table 11: Mobility across sections and departments around promotion into management

Horizon	Section change	Department change	N
Panel A. Stage-specific samples			
Before promotion			
One year	4.6% (14/303)	1.7% (5/303)	303
Three years	46.5% (118/254)	13.0% (33/254)	254
Five years	67.6% (140/207)	18.4% (38/207)	207
Ten years	84.1% (53/63)	31.7% (20/63)	63
After promotion			
One year	3.1% (16/516)	1.2% (6/516)	516
Three years	49.0% (204/416)	14.9% (62/416)	416
Five years	74.7% (219/293)	22.9% (67/293)	293
Ten years	100.0% (44/44)	36.4% (16/44)	44
Panel B. Reassignment at promotion			
Same section	38.9% (121/311)		311
Different section, same department	47.6% (148/311)		311
Different department	13.5% (42/311)		311
Any section change	61.1% (190/311)		311
Panel C. Horizon-specific common samples			
Before promotion			
One year	4.7% (13/274)	1.8% (5/274)	274
Three years	47.4% (81/171)	11.7% (20/171)	171
Five years	71.8% (51/71)	15.5% (11/71)	71
After promotion			
One year	1.5% (4/274)	0.7% (2/274)	274
Three years	38.6% (66/171)	15.8% (27/171)	171
Five years	78.9% (56/71)	26.8% (19/71)	71

Notes: The unit of observation is a first promotion into the section-manager rank. A cell reports the share of promotion events with at least one section change, or at least one department change, within the stated horizon, followed by the count and the sample size. The mobility definition is the one used in Table 1, Panel D. A section is identified by its section code together with its name and its position in the hierarchy, and three organizational recodings are not counted as moves: a change in a parent code alone, a renaming under an unchanged section code, and a unit-wide renumbering in which a whole unit moves to a new code. The department of a section-month is the department code recorded for most of that unit's members in that month, so a department move is also a section move. The windows before promotion end before the promotion transition and the windows after promotion begin after it; Panel B reports that transition on its own, comparing the last assignment before promotion with the first managerial assignment. In Panel A each horizon and side uses every promotion event observed for that full window, so the samples differ across rows. In Panel C both windows of a horizon are observed for the same events, and the shares before and after promotion are computed on that common sample. Panel C stops at five years because a ten-year common sample would require twenty years of consecutive monthly records around a promotion, which exceeds the length of the panel.

B.1 Construction and Estimation Details

Starting from the section–quarter panel constructed in Appendix A.3, which requires a unique designated manager and complete quarterly measurement, we estimate the manager FEs on the connected manager–section network. Manager and section fixed effects are comparable only within sets connected by observed manager mobility. The main specification excludes the first quarter of each manager–section pair. Manager rotations are concentrated at the

annual rotation boundary, making this first observation a transition period in which the incoming manager may not yet fully characterize the section's overtime. After this exclusion, we recompute the manager–section network and retain its largest connected component, leaving 6,787 section–quarter observations, 406 managers, and 193 sections. The exclusion removes 10.4 percent of otherwise usable section–quarter observations and has little effect on the estimates: manager FEs estimated with and without the exclusion correlate 0.991. As a robustness check, selecting the largest component before rather than after the exclusion yields virtually identical estimates. Among managers in the post-exclusion component, the two manager-FE vectors correlate above 0.9999; the standard deviation of their difference is 0.02 hours and the largest absolute difference is 0.11 hours. The manager FEs used for the joint-F and adjusted-R^2 rows of Table 2 and in downstream regressions are estimated after the first-quarter exclusion on the resulting largest connected component. The dispersion rows in Table 2 use FEs estimated after the exclusion and then restricted to that component.

Because raw manager FEs are estimated with different precision, we apply a static empirical-Bayes correction. For manager m,

$$\hat{\alpha}_m^{\mathrm{EB}} \;=\; \lambda_m \, \hat{\alpha}_m, \qquad \lambda_m \;=\; \frac{\hat{\sigma}_\alpha^2}{\hat{\sigma}_\alpha^2 + \hat{\sigma}_\varepsilon^2 / n_m}, \tag{A1}$$

where n_m is the number of section–quarter observations used to estimate manager m, $\hat{\sigma}_\varepsilon^2$ is the residual variance from Equation (1), and $\hat{\sigma}_\alpha^2$ is the cross-manager variance corrected for sampling error using the homoskedastic correction of Andrews et al. (2008). We use this correction rather than the leave-out variance estimator of Kline et al. (2020) because the latter requires the manager–section network to remain connected after removing each match and therefore retains a much smaller sample. On the corresponding comparison panel before singleton removal, the largest connected component contains 410 managers; the leave-one-match-out connectivity requirement then retains 264. How the correction is implemented has essentially no effect on shrinkage: the median empirical-Bayes weight is 0.986 under the plug-in correction above and 0.984 when the manager, section, and quarter components are bias-corrected jointly (Gaure, 2014). Consistent with this similarity, Table 2 shows nearly identical dispersion for the raw and empirical-Bayes manager FEs.

The portability analysis measures each manager separately across sections. For each manager–section spell, we estimate a focal-section effect from that spell and construct the manager's leave-section-out manager FE from the manager's spells in all other sections, weighting those spell effects by the number of quarters they contribute. The spell-level construction absorbs department-by-quarter shocks so that common time-varying changes within the firm's broad departments do not contribute to the cross-section portability

relationship. The section and department-by-quarter effects used to construct the focal spell effect are estimated without the focal manager's own observations. The focal section therefore contributes to the left-hand-side spell effect but not to the leave-section-out manager FE used to predict it.

B.2 Sorting and Additivity

The first concern is that manager changes may be systematically related to overtime changes already under way in the receiving section. The firm could assign managers with higher FEs to sections whose overtime is already rising, or a manager change could follow a temporary overtime shock whose subsequent reversal is then attributed to the incoming manager. Both mechanisms imply that the change in manager FE at turnover should be related to the section's pre-event overtime trajectory.

Following Fenizia (2022), we regress the incoming-minus-outgoing change in manager FE on the section's pre-event overtime levels and growth rates,

$$\begin{aligned}\hat{\alpha}_{m_{\text{in}}(j)} - \hat{\alpha}_{m_{\text{out}}(j)} \;&= \delta_0 \;+\; \delta_1' \bar{X}_j \;+\; \sum_{e=-3}^{-1} \rho_e \, H_{j,t^\star+e} \\ &\quad + \sum_{q=1}^{2} \gamma_q \, g_{j,q} \;+\; \nu_j,\end{aligned} \tag{A2}$$

on the balanced $[-3, 0]$ event sample. Here $H_{j,t}$ is section j's mean monthly overtime per subordinate in quarter t; $g_{j,1} = (H_{j,t^\star-1} - H_{j,t^\star-2})/H_{j,t^\star-2}$ and $g_{j,2} = (H_{j,t^\star-1} - H_{j,t^\star-3})/H_{j,t^\star-3}$. $\bar{X}_j$ contains department indicators and baseline section overtime. Standard errors are clustered by section. We begin with 577 turnover events in the post-exclusion largest connected component that are balanced over $[-3, 0]$, with the same outgoing manager in quarters $[-3, -1]$. One event lacks a full-panel FE for one of the two managers, and two events have a zero denominator in $g_{j,1}$ or $g_{j,2}$, leaving 574 events before the event-specific leave-out. For each turnover event, we re-estimate the manager FEs after removing the outgoing manager's three pre-event section–quarters. The pre-event overtime variables on the right-hand side therefore do not also enter the manager-FE estimates on the left-hand side. This event-specific leave-out further reduces the usable sample to 545 events.

Table 12 reports the results. The pre-event growth rates do not jointly predict the incoming manager FE ($p = 0.681$) or the incoming-minus-outgoing change in manager FE ($p = 0.132$). The latter is the more direct diagnostic for the manager-FE design because differencing removes the section's persistent component. The full set of regressors does predict the change in manager FE, but this is driven by standing department and section

characteristics rather than by the pre-event growth block. Thus, the primary specification provides little evidence that manager turnover systematically assigns higher-FE managers to sections whose overtime was already rising.

The conclusion is similar under several alternative constructions. Using manager FEs estimated without the event-specific leave-out gives a growth-block p-value of 0.168, and replacing quarter effects with department-by-quarter effects gives $p = 0.194$. We therefore read the exercise as showing little systematic relationship between manager-FE changes and pre-event overtime growth.

Table 12: Pre-event predictors of incoming manager FEs and manager-FE changes

	(1) $\hat{\alpha}_{m_{\text{in}}}$	(2) $\hat{\alpha}_{m_{\text{in}}} - \hat{\alpha}_{m_{\text{out}}}$
Sales dummy	-10.411^{***}	-1.966^{***}
	(1.314)	(0.485)
Back-office dummy	-6.580^{***}	-2.728^{***}
	(1.749)	(0.525)
Section baseline OT	-0.053	-0.173^{***}
	(0.092)	(0.042)
OT $t-1$	0.121	-0.119
	(0.086)	(0.087)
OT $t-2$	0.221^{**}	0.163^{**}
	(0.098)	(0.078)
OT $t-3$	-0.006	0.037
	(0.092)	(0.097)
Growth rate OT -1q	0.222	1.252^{*}
	(0.558)	(0.645)
Growth rate OT -2q	-0.956	1.405
	(1.306)	(1.448)
N events	545	545
Sections	172	172
Adj. R^2	0.367	0.052
p-value (all RHS)	$< 10^{-38}$	$< 10^{-7}$
p-value (growth rates)	0.681	0.132

Notes: The sample starts from turnover events in the post-exclusion largest connected component that are balanced over quarters $[-3, 0]$, with the same outgoing manager in quarters $[-3, -1]$. After excluding one event without the required full-panel manager FE and two with a zero growth-rate denominator, 574 events remain before the event-specific leave-out; 545 remain in the estimation sample. Manager FEs are estimated separately for each event after removing the event's three pre-event section–quarters from the fixed-effect estimation. Column (1) uses the incoming manager FE as the outcome; column (2) uses the incoming-minus-outgoing change in manager FE. Regressors include department indicators, baseline section overtime, pre-event overtime levels, and the one- and two-quarter pre-event overtime growth rates. Department indicators are relative to manufacturing; baseline section overtime is the section's mean in its first fiscal year. The estimation sample spans 172 sections. Standard errors are clustered by section. The reported p-values test either all right-hand-side variables jointly or the two pre-event growth-rate coefficients jointly. Significance: $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.10$.

The second concern is that the additive manager-plus-section specification may mask systematic manager–section match components. If a manager generates unusually high overtime only in particular sections, the estimated manager FE may partly reflect those matches rather than a component that is portable across assignments.

Figure 4 examines whether departures from additivity vary systematically with the estimated manager and section components. We group manager–section cells by quartiles of the manager FE and section FE and plot the mean residual from Equation (1) in each of the resulting 16 bins. If systematic complementarity were tied to these components, the residuals would display an organized pattern across the grid. For example, positive complementarity would generate positive residuals when high-FE managers are paired with high-FE sections and negative residuals in systematically mismatched cells. The figure shows no such organized pattern: the bin means fluctuate around zero, and the largest deviation is 11 percent of one manager-FE standard deviation. A fully saturated manager-by-section specification raises the unadjusted R^2 from 0.842 to 0.870. These diagnostics do not rule out idiosyncratic match effects that average out within the bins, but they provide little indication that systematic manager–section match components account for the variation attributed to manager FEs.

B.3 Predetermined Section Composition

A third concern is that manager FEs may partly capture workforce composition that predates the manager's arrival. Sections with different headcount or worker composition may have systematically different overtime, and managers may be assigned nonrandomly across such sections. We do not condition on contemporaneous workforce composition in the main specification because staffing may itself respond to manager assignment. Instead, we re-estimate Equation (1) after adding section headcount, mean subordinate age, and female share, each measured in the last observed quarter before the incoming manager takes over. These measures are fixed before the manager can affect staffing in the section.

A qualifying pre-arrival quarter is observed for 622 of 875 manager–section spells (71 percent). Each composition variable is centered at its mean among these covered spells; for uncovered spells, the centered variables are set to zero and a common missing-pre-arrival indicator is included. We require the pre-arrival quarter to fall within four quarters before the manager arrives. As a sensitivity check, we also drop uncovered spells and obtain the same qualitative conclusions among managers observed under both constructions.

Panel B of Table 13 confirms that predetermined workforce composition is related to overtime. The largest association is with headcount: ten additional workers predict about half an hour less monthly overtime per subordinate. Mean subordinate age is also weakly

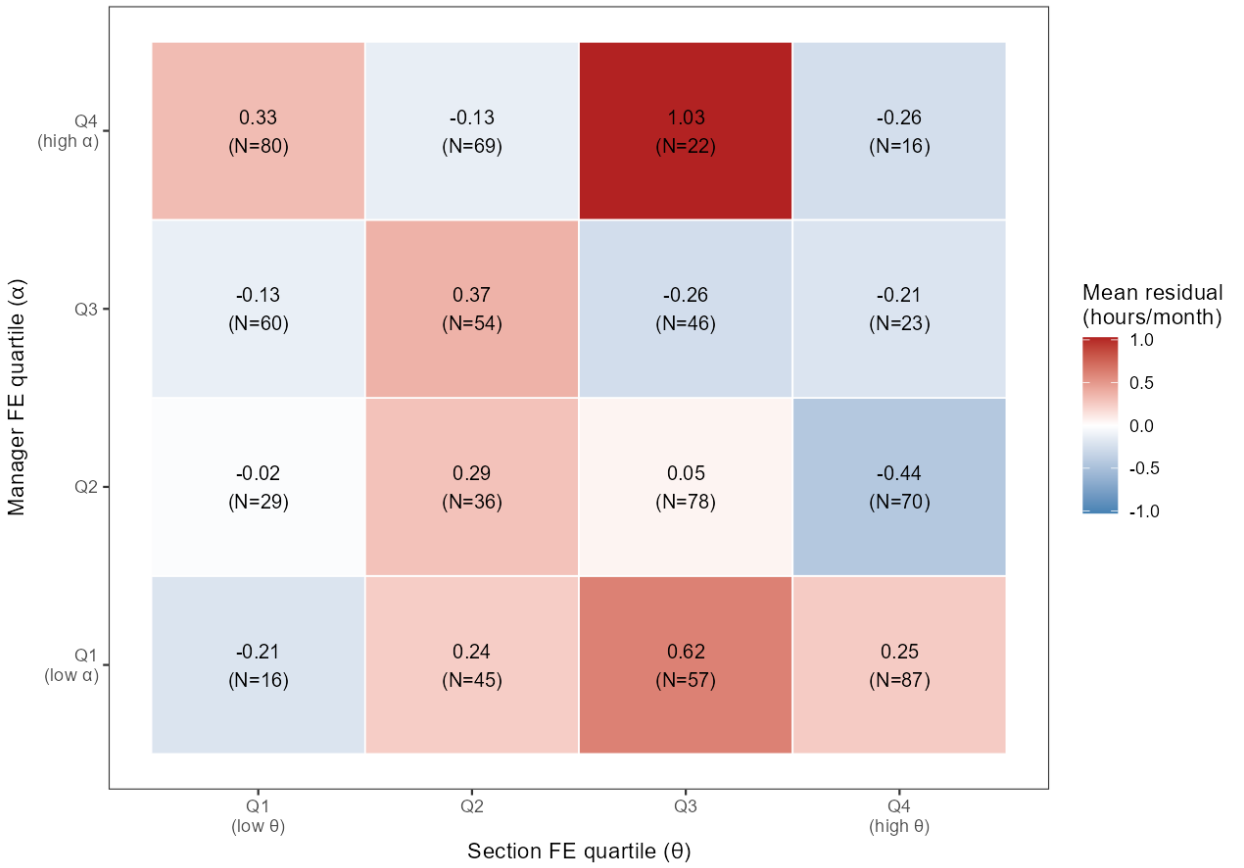


Figure 4: Manager–section additivity diagnostic

Notes: The figure plots mean residuals from Equation (1) across 16 bins defined by quartiles of the manager FE and section FE. The residuals are estimated on the 6,787 section–quarters covering 406 managers and 193 sections after excluding the first quarter of each manager–section pair and retaining the largest resulting connected component. Residuals are first averaged within manager–section cells and then across cells in each bin. Manager-FE and section-FE quartiles are defined on the same estimation sample. The N shown in each bin is the number of manager–section cells. Cell shading reports the mean residual in hours of monthly overtime per subordinate.

associated with overtime, while female share is not precisely related to it. The question is whether these observable differences account for the estimated manager component. Panel A shows that they do not. The composition-adjusted manager FEs correlate 0.994 with the baseline estimates, and their empirical-Bayes standard deviation is 9.88 hours, compared with 9.41 hours at baseline.

Panel A also recomputes the two relationships most directly tied to the interpretation of the manager FE. The cross-section portability slope changes from 0.540 to 0.574, while the supervisor–future-manager slope changes from 0.918 to 0.904. Thus, predetermined workforce composition predicts overtime to some extent, but conditioning on it barely changes either the manager FEs or the relationships that motivate their interpretation as managerial style.

Table 13: Manager FEs adjusted for predetermined section composition

Panel A. Baseline and composition-adjusted estimates

	(1) Baseline	(2) Composition-adjusted
Manager-FE SD (EB-shrunk, h/month)	9.41	9.88
Correlation with the baseline manager FE	—	0.994
Cross-section portability slope	0.540	0.574
	(0.055)	(0.050)
Supervisor–future-manager slope	0.918***	0.904***
	(0.054)	(0.050)
Managers (SD)	406	406
Spells (portability)	573	573
Promotion events (supervisor–future-manager)	204	204

Panel B. Predetermined composition coefficients

	Coefficient (SE)
Pre-arrival headcount	−0.053*** (0.018)
Pre-arrival mean age	−0.154* (0.087)
Pre-arrival female share	1.67 (1.77)
Section–quarter obs. (N)	7,561
Covered spells	622 of 875 (71%)

Notes: Panel A compares the baseline manager FE with a manager FE estimated after conditioning on predetermined section composition: headcount, mean subordinate age, and female share, each measured in the section's last observed quarter before the manager arrives. Each composition variable is centered at its mean among covered spells; uncovered spells are set to zero on these centered variables and enter with a common missing-pre-arrival indicator. Coverage requires a pre-arrival quarter within four quarters before manager arrival. The manager-FE standard deviation and correlation are computed across the 406 managers in the connected set. The portability row regresses the focal-section spell FE on the leave-section-out manager FE over 573 spells, with standard errors clustered by manager. The supervisor–future-manager row regresses the future manager's FE on the pre-promotion supervisor's FE over 204 promotion events, with standard errors clustered by supervisor. Panel B reports the coefficients on the three predetermined composition variables from the adjusted manager-FE specification, together with the number of section–quarter observations and covered spells. The composition-adjusted regression underlying Panel B is estimated on the full section–quarter panel after the first-quarter exclusion; the manager-FE comparisons in Panel A are then restricted to the 406 managers in the connected set. Standard errors are in parentheses. Significance markers are not shown for the cross-section portability row. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C Pre-Promotion Supervisors: Measurement and Selection

This appendix reports measurement and selection checks for Section 4. Appendix C.1 audits the supervisor–future-manager relationship, C.2 formalizes the supervisor-switch design and timing assumption, and C.3 examines remaining selection, measurement, and team-change concerns.

All Section 4 samples are constructed from a common promotion-event file that fixes the future manager, pre-promotion supervisor, pre- and post-promotion sections, and inclusion

status for each analysis. Observability requirements differ across the exercises, so the resulting samples are not random subsamples of all promotions. In particular, samples requiring a usable pre-promotion supervisor FE disproportionately include later promotion cohorts because more supervisor history is observed. We report the relevant sample restrictions with each analysis.

C.1 Measurement Checks for the Supervisor–Future-Manager Relationship

Two measurement concerns could affect the supervisor–future-manager relationship in Section 4.1. First, the two manager FEs may be mechanically related because their estimation samples contain overlapping workers or organizational settings. Second, sampling error in the estimated manager FEs may attenuate the relationship, particularly for leave-section-out measures. Table 14 examines both concerns.

Panel A removes potential sources of mechanical overlap. These exercises use promotions into a different section and compare each modified estimate with the unmodified estimate on the same sample. Removing the future manager's own observations from the pre-promotion supervisor's estimation sample leaves the slope essentially unchanged, at 0.879 rather than 0.871. Removing every subordinate observed under both managers gives a slope of 0.846. In the strongest separation, we remove from each manager's estimation sample every worker observed under the other manager and all sections run by both managers. After reconstructing the connected set, the slope remains 0.812, compared with 0.855 on the same 110 promotion pairs. The positive relationship therefore survives even when the two manager FEs are constructed from nonoverlapping workers and organizational settings.

These exercises address a different issue from the leave-section-out comparison in Section 4.1. The overlap checks ask whether common observations mechanically enter the two estimated manager FEs. The leave-section-out measure instead asks whether the relationship remains when both managers are measured outside the section in which they worked together.

Panel B examines sampling error using split-sample instrumental variables. Within each manager–section cell, we randomly divide section-quarters into two halves, estimate the manager FE separately in each half, and instrument one half-sample estimate with the other. For promotions into a different section, the all-section OLS slope is 0.884 and the split-sample IV estimate is 0.892. Split-sample IV gives a similar leave-section-out estimate (0.574, compared with 0.546 under OLS). The median first-stage F-statistics are 305 and 173.

Appendix G reports a resampling sensitivity analysis that re-estimates the manager-FE objects; the Table 4 estimate remains well away from zero under this exercise.

Table 14: Measurement audits of the supervisor–future-manager resemblance

Panel A. Mechanical overlap			
	Audited slope	Same-sample baseline	Promotion events
Exclude promoted worker's own observations	0.879 (0.070)	0.871 (0.073)	123
Remove shared subordinates	0.846 (0.075)	0.871 (0.073)	123
Full separation of workers and sections	0.812 (0.078)	0.855 (0.080)	110
Panel B. Sampling error			
	OLS	Split-sample IV	Median first-stage F	Promotion events
All-section, different-section promotions	0.884 (0.076)	0.892 (0.083)	305	122
Leave-section-out	0.546 (0.114)	0.574 (0.120)	173	84

Notes: One observation per promotion event, with promotion-cohort effects and standard errors clustered on the pre-promotion supervisor in parentheses. Panel A and the first row of Panel B use promotions into a different section. The leave-section-out row uses all promotion pairs for which the leave-section-out measures are defined. Each Panel A row re-estimates the slope beside the unmodified slope on the same promotion pairs: the first removes the promoted worker's own person-months from the pre-promotion supervisor's cells, the second every subordinate the two managers share, the third every worker observed in the other manager's cells together with every cell in a section both managers have run, after which the connected set is rebuilt. Panel B deals each manager's section-quarters at random into two halves inside each manager-by-section cell and instruments one half's estimate with the other's, reporting the median over 40 draws. Panel A uses the empirical-Bayes shrunk manager FEs used in the main resemblance analysis. Panel B uses the unshrunk manager FEs required for the split-sample IV exercise; the corresponding OLS slope therefore differs slightly from the Panel A baseline. The split-sample rows use 122 of the 123 different-section promotion pairs and 84 of the 88 leave-section-out promotion pairs, because both half-sample manager FEs must be estimable.

Separately from these measurement checks, conditioning on pre-promotion department gives the same across-versus-within-department comparison on the larger promotion samples. Because department assignment is itself part of the firm's personnel process, these are within-department comparisons rather than preferred causal specifications. Using all 204 promotions, the all-section slope falls from 0.918 to 0.805 (SE 0.059); among the 123 different-section promotions, it falls from 0.868 to 0.710 (SE 0.094). Positive point estimates therefore remain within department, although their magnitude is attenuated.

These checks do not indicate that mechanical overlap or sampling error accounts for the positive supervisor–future-manager relationship.

C.2 Supervisor-Switch Design and Timing Assumption

This subsection gives the formal identification argument behind the supervisor-switch design in Section 4.2. For future manager i, define

$$D_i = \widehat{\alpha}_{n(i)}^{(-i)} - \widehat{\alpha}_{o(i)}^{(-i)}, \tag{A3}$$

where $n(i)$ and $o(i)$ are the incoming and outgoing supervisors and both supervisor FEs are re-estimated without i's own pre-promotion person-months. Let T_i denote the worker's latent managerial type: the managerial style the worker would later display if supervisory experience did not alter it. Under pure initial sorting, write the worker's later estimated

manager FE as

$$Y_i = T_i + e_i, \tag{A4}$$

where e_i is estimation error.

The design does not require the worker's initial assignment to be random. Workers may be sorted into sections or supervisors before the switch. To match the regression estimand, we state the timing assumption in linear-projection form. Let Z_i collect the promotion-cohort indicators and any additional pre-switch controls included in the specification, and let $\widetilde{D}_i$ and $\widetilde{T}_i$ denote the population residuals from projecting D_i and T_i on Z_i. The timing assumption is

$$\mathrm{Cov}(\widetilde{D}_i, \widetilde{T}_i) = 0. \tag{A5}$$

Thus, after partialling out the controls used in the regression, the subsequent supervisor change is orthogonal to the worker's latent managerial type. We also assume that the corresponding residual in the estimation error of the worker's later manager FE is orthogonal to $\widetilde{D}_i$.

Let $\widetilde{Y}_i$ and $\widetilde{e}_i$ denote the residuals from projecting Y_i and e_i on Z_i, so that $\widetilde{Y}_i = \widetilde{T}_i + \widetilde{e}_i$. Under pure initial sorting,

$$\operatorname{plim} \widehat{\delta} = \frac{\mathrm{Cov}(\widetilde{Y}_i, \widetilde{D}_i)}{\mathrm{Var}(\widetilde{D}_i)} = \frac{\mathrm{Cov}(\widetilde{T}_i, \widetilde{D}_i) + \mathrm{Cov}(\widetilde{e}_i, \widetilde{D}_i)}{\mathrm{Var}(\widetilde{D}_i)} = 0. \tag{A6}$$

This zero probability limit applies to the supervisor-switch coefficient, not to the cross-sectional resemblance in Section 4.1. Initial sorting can generate that resemblance by relating workers' latent managerial types to the supervisors to whom they are initially assigned, while the supervisor-switch coefficient remains zero when the subsequent supervisor change satisfies the timing assumption.

A related form of initial sorting also does not generate the positive coefficient estimated in Section 4.2. If workers with higher latent managerial types were initially matched to supervisors with higher FEs, this would tend to raise the outgoing supervisor's FE. Because D_i is defined as the incoming supervisor's FE minus the outgoing supervisor's FE, such matching enters D_i with the opposite sign. Initial matching to a similar outgoing supervisor would therefore tend to attenuate or reverse, rather than generate, a positive supervisor-switch relationship.

C.3 Remaining Threats to the Supervisor-Switch Design

The remaining checks address three concerns: selection into the future-manager sample, measurement of the switch relationship, and contemporaneous changes in the team around supervisor replacement.

We first examine promotion-related selection. The main switch regression is observed only for workers who eventually become managers. Conditioning on promotion could induce a positive relationship between D_i and latent managerial type if both affect the promotion decision. The relevant concern is not simply whether D_i predicts promotion, but whether the relationship between D_i and promotion differs with the worker's pre-existing type.

Panel A of Table 15 uses a broader pre-promotion sample consisting of workers at the two ranks below manager who were already in the section before a supervisor change and remained there across the change. The sample contains 1,355 worker-events and has a one-year promotion rate of 3.0 percent. A one-standard-deviation increase in D_i is associated with a -0.60-percentage-point change in subsequent promotion probability (SE 0.45). Adding a predicted managerial-type index, constructed from personnel information recorded before the supervisor switch, gives a coefficient of -0.45 (SE 0.45) on D_i, while the interaction between D_i and the predicted managerial-type index is 0.12 percentage points (SE 0.51). The index uses the worker's pre-switch personnel records to proxy for the pre-existing managerial type that could enter the firm's promotion decision. These estimates provide no evidence that more positive supervisor changes increase subsequent promotion, either on average or disproportionately for workers with higher recorded types.

Selection may also operate through information about the worker that is not recorded in the personnel data. Following Cinelli and Hazlett (2020), Panel B quantifies how strong a single omitted factor would have to be to reduce the fully controlled switch coefficient to zero. The robustness value is 0.37: such a factor would have to explain 37 percent of the residual variation in both the supervisor change and the future manager's later FE. The largest partial R^2 attained by any single recorded benchmark on either side is 0.11. This comparison quantifies the strength of latent selection required to account for the estimate; it does not establish that such selection is absent.

Panel C examines whether the switch result depends on using managerial observations from the section in which the supervisor change occurred. Its first row reproduces the primary stay-in-section estimate from Table 5 for reference. Re-estimating the future manager's later FE after excluding all managerial observations from the focal switch section leaves 63 of the 78 future managers and gives a coefficient of 0.60 (SE 0.29), compared with 0.74 (SE 0.25) in the primary specification. Among the 49 future managers later promoted into a different section, the corresponding estimate is 0.77 (SE 0.35), compared with 0.68 (SE 0.38) using the

Table 15: Supervisor-switch diagnostics and sensitivity to selection

Panel A. Promotion in the broader pre-promotion sample	
	Estimate (SE)
D, without type index	−0.60 (0.45)
D, with type index	−0.45 (0.45)
Predicted managerial-type index	−1.28 (0.61)
$D\times$ predicted managerial-type index	0.12 (0.51)
Worker-events	1,355
Panel B. Sensitivity to latent selection	
	Value
Cinelli–Hazlett robustness value	0.37
Largest partial R^2 of a recorded benchmark	0.11
Panel C. Measurement of the future manager's later FE	
	$\hat{\delta}$ (SE) N
Primary stay-in-section sample (Table 5)	0.74 (0.25) 78
Primary sample, later manager FE excluding focal switch section	0.60 (0.29) 63
Promoted into a different section	0.68 (0.38) 49
Same sample, later manager FE excluding focal switch section	0.77 (0.35) 49

Notes: D is the incoming supervisor's FE minus the outgoing supervisor's, one value per switch event. *Panel A* is a linear probability model of promotion one fiscal year ahead on D, the standardized predicted managerial-type index (estimated by lasso from pre-switch personnel records among workers later observed as managers and applied to the broader pre-promotion sample), their interaction and the worker's qualification grade, tenure, age and sex, with period fixed effects, in percentage points per standard deviation of D and of the index. The sample consists of workers at the two ranks below manager who were in the section before its supervisor changed and remained across the change, and the promotion rate in the estimated sample is 3.0 percent. *Panel B* describes the last row of Panel A of Table 5, the specification with promotion-cohort effects, the section's pre-switch conditions and the future manager's own pre-switch overtime trend: the share of residual variation in both D and the future manager's later FE that one unobserved factor would have to explain to account for that estimate — the robustness value of Cinelli and Hazlett (2020), with q set to one — and the largest such share any single recorded control attains on either side. *Panel C* regresses the future manager's later FE on D with promotion-cohort effects. The first row of Panel C reproduces the first row of Table 5 for reference. Rows two and four rebuild that later manager FE from managerial observations outside the section in which the supervisor change occurred. Standard errors, in parentheses, are clustered on the switch event in Panels A and C.

all-section FE. The positive relationship therefore does not depend on measuring the future manager in the focal switch section.

Table 16 addresses a separate measurement concern: the supervisor change D_i is itself estimated partly from outcomes in the section in which the switch occurs. We therefore reconstruct the incoming and outgoing supervisors' FEs after removing the focal switch section, in addition to removing the future manager's own person-months. This focal-section-out supervisor change can be constructed for 45 of the 78 future managers, with 44 entering the promotion-cohort regression. The coefficient is 0.58 (SE 0.24) with promotion-cohort effects. Adding the section's pre-switch conditions lowers the estimate to 0.32 (SE 0.21).

The smaller focal-section-out coefficient is not a clean estimate of what happens when

focal-section shocks are removed, because this construction also changes both the sample and the variation used to measure the supervisor change. On the same 44 surviving events, the coefficient using the baseline supervisor-change measure is 1.22. The baseline and focal-section-out supervisor-change measures correlate only 0.35, and the standard deviation of the focal-section-out measure is about 1.9 times as large. The attenuation therefore cannot be attributed solely to removing variation from the focal section.

Table 16: Supervisor-switch estimates with supervisor FEs measured outside the switch section

Supervisor-switch estimates, original and focal-section-out D_i

	Original D_i, full sample (1)	Original D_i, surviving sample (2)	Focal-section-out D_i, surviving sample (3)
Promotion-cohort effects	0.743	1.222	0.578
	(0.253)	(0.327)	(0.240)
+ section pre-switch conditions	0.857	1.050	0.318
	(0.218)	(0.264)	(0.214)
+ future manager's own pre-switch overtime trend	0.891	1.060	0.343
	(0.243)	(0.279)	(0.211)
Future managers	78 / 76	44	44
Clusters	74 / 72	41	41

Notes: Each row reports the coefficient on the supervisor change in Equation (3). Column 1 reports the baseline Table 5 specification on the full switch sample. Column 2 uses the same baseline supervisor-change measure on the events for which the focal-section-out measure can be constructed. Column 3 reconstructs both the incoming and outgoing supervisor FEs after removing the focal switch section, in addition to the future manager's own person-months. The rows add controls cumulatively as indicated. Standard errors, in parentheses, are clustered on the switch event. Of the 78 future managers, 45 have a focal-section-out supervisor-change measure: 13 are lost because the incoming supervisor has no observations outside the focal section, 13 because the outgoing supervisor has none, six because the two supervisors become disconnected after the focal section is removed, and one because the outgoing supervisor becomes a singleton. The promotion-cohort regression uses 44 of the 45 surviving events.

The third concern is that supervisor replacement may coincide with changes in the team around the future manager. The switches sit in the firm's annual personnel cycle, so coworkers can move in the same month. Panel A of Table 17 compares month boundaries across the firm with and without a change of supervisor. Mean coworker churn is 0.029 at boundaries without a supervisor change and 0.203 at boundaries with one, and the adjusted difference is 0.057 (SE 0.009). Supervisor replacement is accompanied by more coworker turnover in the firm, so the concern is empirically relevant.

Panel B asks whether the size of that team change is related to D_i within the 78 future managers that identify Equation (3). Each team-change measure is the outcome and D_i is the regressor; no team-change measure enters Equation (3) itself. The coefficient of coworker churn on D_i is 0.0056 (SE 0.0058) with promotion-cohort effects. The outflow, inflow and headcount measures behave the same way. Larger supervisor changes do not appear to be

systematically accompanied by larger recorded team changes in this sample.

Panel C re-estimates Equation (3) after removing the events with the largest recorded team changes. The high-churn threshold is the pre-specified cutoff of 0.3333, the 75th percentile of churn at September personnel-cycle boundaries, fixed before any restricted estimate was computed. Excluding the high-churn events lowers the fully controlled estimate from 0.89 to 0.72, which stays positive and lies inside the unrestricted interval; the estimate with promotion-cohort effects alone moves from 0.74 to 0.71. These tests use what the personnel records measure, which is coworker composition and headcount. They do not observe contemporaneous changes in tasks, workload allocation, budgets, clients or other operating conditions, so they address the recorded part of the concern rather than establishing that no organizational change accompanies a supervisor replacement.

Table 17: Contemporaneous team changes at supervisor switches

Panel A. Coworker turnover at supervisor-switch boundaries

	Mean or estimate (SE)
Mean coworker churn, no supervisor switch	0.029
Mean coworker churn, supervisor switch	0.203
Adjusted switch difference	0.057 (0.009)

Panel B. Contemporaneous team change and the supervisor FE change

	Promotion-cohort effects
Coworker churn	0.0056 (0.0058)
Coworker outflow	0.0036 (0.0040)
Coworker inflow	0.0022 (0.0043)
Absolute percentage headcount change	−0.0014 (0.0029)

Panel C. Supervisor-switch estimates on restricted event sets

	Promotion-cohort effects		Full pre-switch controls	
	$\hat{\delta}$ (SE)	N	$\hat{\delta}$ (SE)	N
Full sample	0.743 (0.253)	78	0.891 (0.243)	76
Excluding high-churn events	0.705 (0.291)	62	0.724 (0.256)	61

Notes: A section's coworker roster is the set of subordinates recorded in that section in a month; in the switch sample it excludes the future manager. Churn is one minus the Jaccard overlap of the rosters in the month before the incoming supervisor arrives and in the incoming supervisor's first month. Outflow and inflow are the corresponding one-sided rates, and the headcount measure is the absolute change in roster size as a share of the earlier roster. *Panel A* compares consecutive month boundaries in sections with a single recorded section manager in both months: 25,454 boundaries in 236 sections, of which 674 carry a supervisor change. The adjusted difference is the coefficient on a supervisor-change indicator, with fixed effects for the month of the boundary and for the department and standard errors clustered by section. *Panel B* takes each team-change measure as the outcome and D as the regressor on the 78 future managers of Table 5; the column carries promotion-cohort effects. No team-change measure enters Equation (3). *Panel C* reports $\hat{\delta}$ from Equation (3) on restricted event sets. Its first column is the first row of Panel A of Table 5 and its second column that panel's last row, which carries the section's pre-switch conditions and the future manager's own pre-switch overtime trend. High churn is churn above 0.3333, the 75th percentile of churn at September personnel-cycle boundaries, fixed before these estimates were computed. Standard errors, in parentheses, are clustered on the switch event in Panels B and C.

D Persistence of Managerial Style Across Manager Turnover

This appendix provides construction details and robustness checks for Section 5. Appendix D.1 covers repeated turnover and random reassignment, D.2 the supervisor-linked decomposition, D.3 robustness and heterogeneity, and D.4–D.5 manager-side rewards and predecessor evaluations.

D.1 Repeated Manager Turnover and Random Reassignment

Table 6 follows sections through successive manager spells. A spell is a maximal consecutive period in which the same manager runs the same section. The full spell construction yields 620 pairs of successive manager spells, of which 593 are adjacent-quarter manager turnovers with estimated manager FEs for both managers. There are 442 pairs two replacements apart and 287 pairs three replacements apart. Requiring both managers to have leave-section-out manager FEs reduces these samples to 391, 285, and 176 pairs. We then remove pairs for which the two manager FEs cannot be measured after excluding every other section held by both managers. This leaves the 336, 245, and 155 pairs reported in Table 6.

For each pair, both managers are measured using leave-section-out manager FEs. For a succession in section s, each manager's FE is constructed from the other sections that manager has run, so section s contributes to neither measure. If the two managers have also run another section in common, that section is excluded from both measures as well. The succession regression therefore compares manager FEs measured outside the focal section and every other section shared by the pair.

Table 18 reports the within-department-year exercise separately by the source of the incoming manager. We classify an incoming manager as a lateral rotation if the manager was already in the section-manager rank in the quarter before taking over the section, and as a promotion if the manager was below that rank in the preceding quarter and is in the section-manager rank when taking over the section. For lateral rotations, the realized slope is 0.578 and the mean randomized slope is 0.262. For promotions, the corresponding slopes are 0.504 and 0.224. The realized slope exceeds the mean randomized slope in both groups, although the promotion sample is substantially smaller.

Appendix G reports a separate resampling sensitivity analysis that re-estimates the manager-FE objects.

Table 18: Random reassignment within department and year, by source of incoming manager

			Random reassignment			
	Events	Realized slope	Mean	SD	Realized − mean	p
Lateral rotation	248	0.578	0.262	0.054	0.316	<0.001
		(0.069)				
Promotion	83	0.504	0.224	0.091	0.280	0.002
		(0.126)				

Notes: The sample is drawn from the 336 adjacent turnovers in Table 6. A lateral rotation is an incoming assignment of a manager already in the section-manager rank in the preceding quarter; a promotion is an assignment in which the incoming manager is below that rank in the preceding quarter and is in the section-manager rank in the takeover quarter. Rows split events by whether the incoming manager arrives by lateral rotation or promotion; 5 events with neither label are omitted. Incoming managers are permuted across turnover sections within department and year over $B = 49{,}999$ draws. A pool with a single turnover retains its incoming manager. For each counterfactual pair, both manager FEs are reconstructed excluding the focal section and any other section held by both managers; pairs with no remaining section are dropped from that draw. The realized slope regresses the incoming manager's leave-section-out manager FE on the outgoing manager's, with standard errors clustered by section. Random mean and SD describe the distribution of the permuted slopes, and p is the share of draws with a slope at least as large as the realized slope.

D.2 Supervisor-Linked Decomposition: Construction

Table 7 augments each adjacent turnover with the incoming manager's pre-promotion supervisor. We use leave-section-out manager FEs for the outgoing manager, the incoming manager, and the pre-promotion supervisor. The outgoing and incoming managers' FEs follow the construction in Appendix D.1. For the pre-promotion supervisor, we exclude the focal succession section, the section in which the supervisor and future manager worked together before promotion, and every section held by the outgoing manager. When the pre-promotion supervisor is the outgoing manager, we set $\alpha_{\text{sup}} = \alpha_{\text{out}}$. Thus, when the two are different people, the supervisor's FE is measured outside both the pre-promotion section linking the pair and the sections used to measure the outgoing manager.

Let α_{out}, α_{in}, and α_{sup} denote the leave-section-out FEs of the outgoing manager, the incoming manager, and the incoming manager's pre-promotion supervisor. The persistence slope b is defined by

$$\alpha_{\text{in}} = a + b\alpha_{\text{out}} + u. \tag{A7}$$

The slope b_{sup} describes how the incoming manager's pre-promotion supervisor is related to the outgoing manager:

$$\alpha_{\text{sup}} = a_{\text{sup}} + b_{\text{sup}}\alpha_{\text{out}} + u_{\text{sup}}. \tag{A8}$$

Let $\beta_F = 0.539$ denote the leave-section-out supervisor–future-manager slope estimated in Section 4.1. We define the part of the incoming manager's FE not associated with the pre-promotion supervisor through that relationship by

$$\alpha_{\text{in}} - \beta_F\alpha_{\text{sup}} = a_{\text{rem}} + b_{\text{rem}}\alpha_{\text{out}} + u_{\text{rem}}. \tag{A9}$$

Because

$$\alpha_{\text{in}} = \beta_F \alpha_{\text{sup}} + (\alpha_{\text{in}} - \beta_F \alpha_{\text{sup}}), \tag{A10}$$

substituting the two regressions above gives

$$\alpha_{\text{in}} = (\beta_F a_{\text{sup}} + a_{\text{rem}}) + (\beta_F b_{\text{sup}} + b_{\text{rem}})\, \alpha_{\text{out}} + (\beta_F u_{\text{sup}} + u_{\text{rem}}). \tag{A11}$$

Hence,

$$b = \beta_F b_{\text{sup}} + b_{\text{rem}}. \tag{A12}$$

The first term is the supervisor-linked component reported in Table 7, and the second is the remaining component. Once β_F is fixed at the Section 4.1 estimate, the two components sum to the persistence slope by construction. As a diagnostic for using the Section 4.1 estimate in this decomposition, re-estimating the supervisor–future-manager relationship at the Table 7 event level gives a slope of 0.527 (SE 0.080; $N = 153$), close to the Section 4.1 estimate of 0.539.

The decomposition requires observing the incoming manager before entry into management and constructing a usable leave-section-out FE for the pre-promotion supervisor. Of the 336 adjacent turnovers in Table 6, 153 satisfy these requirements. Among the 183 excluded turnovers, 116 involve incoming managers whose promotion is left-censored by the beginning of the panel, 47 have an identified pre-promotion supervisor but no usable leave-section-out FE for that supervisor, 12 lack an identified pre-promotion section, and 8 lack an identified pre-promotion supervisor. The persistence slope is 0.506 (SE 0.081) in the 153 included turnovers and 0.648 (SE 0.088) in the 183 excluded turnovers. Their difference is −0.142 (SE 0.115; $p = 0.217$). The decomposition sample therefore has a smaller point estimate of persistence, but the difference is not statistically distinguishable from zero.

D.3 Robustness and Heterogeneity

This subsection examines whether the decomposition is sensitive to the direct supervisor–outgoing-manager link, the leave-out construction, the succession horizon, and the source of the incoming manager.

Appendix Table 19 first excludes turnovers in which the incoming manager's pre-promotion supervisor is the same person as the outgoing manager. This removes the cases with the most direct link between the pre-promotion supervisor and the succession pair. Among the remaining 116 turnovers, the persistence slope is 0.513, of which 0.195 is supervisor-linked and 0.318 is the remaining component.

We next vary how the pre-promotion supervisor's FE is constructed. Omitting the

pre-promotion-section exclusion gives a persistence slope of 0.521, with a supervisor-linked component of 0.266 and a remaining component of 0.254. Omitting the exclusion of sections held by the outgoing manager gives a nearly identical persistence slope, with components of 0.278 and 0.245. Demeaning the outgoing-manager FE, incoming-manager FE, pre-promotion-supervisor FE, and remaining component within the fiscal year of the turnover gives similar results: the persistence slope is 0.506, with 0.262 supervisor-linked and 0.244 remaining.

The same decomposition can also be applied at longer succession horizons. Two replacements apart, the persistence slope is 0.469, with 0.258 supervisor-linked and 0.211 remaining. Three replacements apart, the corresponding estimates are 0.542, 0.228, and 0.313.

Finally, we split the turnovers with a distinct pre-promotion supervisor by whether the incoming manager was below the section-manager rank in the preceding quarter (a promotion) or was already in that rank (a lateral arrival). Among promotions, the persistence slope is 0.605, with 0.261 supervisor-linked and 0.344 remaining. Among lateral arrivals, the corresponding estimates are 0.480, 0.179, and 0.301. The promotion sample is substantially smaller.

Table 19: Robustness and heterogeneity of the supervisor-linked decomposition

Specification	N	Persistence b	Supervisor-linked	Remaining component	Share linked
Baseline	153	0.506	0.267	0.240	0.526
		[0.329, 0.659]	[0.152, 0.407]	[0.036, 0.428]	[0.287, 0.903]
Pre-promotion supervisor ≠ outgoing manager	116	0.513	0.195	0.318	0.381
		[0.327, 0.682]	[0.092, 0.329]	[0.126, 0.493]	[0.196, 0.664]
No pre-promotion-section exclusion	159	0.521	0.266	0.254	0.512
		[0.339, 0.661]	[0.139, 0.406]	[0.046, 0.428]	[0.278, 0.862]
No outgoing-manager-held-section exclusion	158	0.523	0.278	0.245	0.531
		[0.356, 0.666]	[0.153, 0.423]	[0.046, 0.423]	[0.287, 0.891]
Demeaned within turnover fiscal year	153	0.506	0.262	0.244	0.518
		[0.349, 0.672]	[0.143, 0.396]	[0.055, 0.439]	[0.271, 0.864]
Two replacements apart	128	0.469	0.258	0.211	0.550
		[0.322, 0.598]	[0.143, 0.399]	[0.033, 0.366]	[0.319, 0.907]
Three replacements apart	102	0.542	0.228	0.313	0.422
		[0.353, 0.764]	[0.095, 0.392]	[0.131, 0.510]	[0.202, 0.701]
Promotion, supervisor ≠ outgoing	37	0.605	0.261	0.344	0.431
		[0.378, 0.862]	[0.118, 0.446]	[0.099, 0.580]	[0.207, 0.777]
Lateral, supervisor ≠ outgoing	78	0.480	0.179	0.301	0.373
		[0.250, 0.693]	[0.074, 0.315]	[0.065, 0.511]	[0.165, 0.796]

Notes: Each row applies Equation (7) to the indicated sample or specification. The persistence slope b regresses the incoming manager's leave-section-out manager FE on the outgoing manager's. The supervisor-linked component is β_F times the slope of the pre-promotion supervisor's leave-section-out manager FE on the outgoing manager's, with β_F fixed at the Section 4.1 estimate. The remaining component is the slope from regressing $\alpha_{\text{in}} - \beta_F \alpha_{\text{sup}}$ on the outgoing manager's FE. The share is the supervisor-linked component divided by b. The first row uses the sample of Table 7. The next rows vary the supervisor-FE construction, the succession horizon, or the origin of the incoming manager. One turnover with a distinct pre-promotion supervisor is omitted from the final two rows because the incoming manager is classified as neither a promotion nor a lateral arrival. Brackets report 95 percent two-sample cluster-bootstrap percentile intervals from 999 replications, resampling sections in the indicated Section 5 sample and pre-promotion supervisors in the Section 4.1 sample.

D.4 Observed Manager-Side Rewards

Section 5 does not identify why the firm sustains the observed assignment pattern. One possibility is that managers associated with more overtime hours among subordinates receive higher observed rewards from the firm. We therefore examine whether a manager's all-section FE predicts that manager's evaluation scores, pay, or advancement. These regressions describe firm-wide associations in the personnel records; they do not identify the firm's objective or the incentives governing assignment.

Appendix Table 20, Panel A, shows no systematic positive relationship between a higher manager FE and these outcomes. The point estimates for role-based evaluation and promotion are negative, while the wage and achievement-evaluation estimates provide no evidence of a positive reward pattern. The strongest relationship is for advancement: a one-standard-deviation higher manager FE is associated with a 0.72-percentage-point lower annual probability of promotion above the section-manager rank (SE 0.32). We interpret these associations descriptively rather than as causal effects of managerial style on the manager's own career.

Panel B confirms that the evaluation measures are informative about subsequent personnel outcomes. Higher evaluations, particularly the role-based evaluation, predict subsequent promotion and pay growth in the personnel records. The absence of an observed reward does not imply that such incentives are absent or identify why the firm sustains these assignment patterns.

D.5 Predecessor Evaluations and Succession Persistence

One possible reason for succession persistence is that the firm may place greater value on continuity after managers it evaluates favorably. We examine whether the relationship between outgoing and incoming managers' leave-section-out manager FEs is stronger when the outgoing manager received a higher personnel evaluation before turnover. We use the most recent evaluation from the fiscal term before the incoming manager takes over, following the same lag-one timing convention used elsewhere in the personnel data. Both the role-based and achievement evaluations are observed for all 336 adjacent turnover events in Table 6. The role-based evaluation is the primary measure, and the achievement evaluation provides a secondary measure. Each evaluation is standardized on the manager-year evaluation frame used in Appendix D.4.

We estimate the Table 6 succession relationship after adding the outgoing manager's evaluation and its interaction with the outgoing manager's FE. The interaction asks whether succession persistence varies with the predecessor's evaluation. Table 21 shows no detectable heterogeneity. With the role-based evaluation, the interaction is 0.024 (SE 0.058, $p = 0.679$),

Table 20: Manager FEs, observed manager outcomes, and subsequent personnel outcomes

Panel A. The manager's FE and the manager's own outcomes

Outcome	Slope on the manager's FE (per SD)	N
Own total wage (thousand yen)	1.52 (2.94)	2,319
Own role-based evaluation (SD)	−0.066 (0.038)	2,305
Own achievement evaluation (SD)	0.016 (0.024)	2,309
Promotion above section rank (percentage points)	−0.72 (0.32)	2,101

Panel B. The manager's own evaluation scores, promotion and pay

	(1)	(2)
	Fiscal-year FE	+ own fixed effect
Promotion above section rank in the next fiscal year (pp)		
Role-based evaluation	2.96 (0.56)	2.18 (0.83)
Achievement evaluation	0.32 (0.57)	−0.15 (0.64)
Base rate (pp)	2.86	2.73
Observations	2,096	2,053
Managers	404	361
Change in log total pay to the next fiscal year (percent)		
Role-based evaluation	1.31 (0.16)	0.84 (0.26)
Achievement evaluation	0.30 (0.38)	0.23 (0.39)
Mean change (percent)	1.37	1.39
Observations	2,304	2,266
Managers	404	366

Notes: Both panels run on the managers of the analysis set, followed from each manager's first month at section-manager rank. *Panel A* regresses the manager's own outcome on that manager's EB-shrunk all-section FE, scaled to one manager-level standard deviation, across managers, with fiscal-year fixed effects and controls for sex, age, tenure, education, and two baseline evaluation measures. For managers whose promotion into the section-manager rank is observed, the evaluation controls are measured in the fiscal year before promotion; for managers already in the rank at the beginning of the panel, they are measured in FY2013, the year before the first observed manager period. The wage row is the August month in real thousands of yen, deflated as in Table 8; the two evaluation rows are in standard deviations of the manager frame's own score distribution; the promotion row is the annual probability of moving to a title above section rank, whose base rate is 2.86 percent. *Panel B* replaces the manager FE with the manager's own evaluation scores, entering the role-based and achievement evaluations together, each standardized on the same manager frame. The Panel B specifications do not include the Panel A controls. Its first column carries fiscal-year fixed effects and its second column adds the manager's own fixed effect. Promotion rows are in percentage points; the total-pay row is the change in log total pay between consecutive fiscal years, in percent. Base rates and mean changes are taken on the estimation sample of the column above them. Standard errors clustered by manager in parentheses throughout. Managers' own overtime is not recorded and does not enter the manager FE, and the personnel records carry no bonus variable.

implying succession slopes of 0.557, 0.581, and 0.605 at evaluations of minus one standard deviation, the mean, and plus one standard deviation. With the achievement evaluation, the interaction is −0.001 (SE 0.063, $p = 0.992$). Splitting the sample at the median gives the same conclusion: the role-based slopes are 0.531 below or at the median and 0.638 above it, a difference of 0.107 (SE 0.113, $p = 0.343$); the corresponding achievement-evaluation difference is 0.027 (SE 0.117, $p = 0.821$).

The timing result is also similar when the evaluation is required to come from at least two fiscal terms before turnover: the role-based interaction is 0.051 (SE 0.057, $p = 0.376$).

These estimates provide no evidence that succession persistence is stronger following more highly evaluated predecessors, although the confidence intervals do not rule out moderate heterogeneity.

Table 21: Succession persistence by outgoing manager evaluation

Panel A. Continuous evaluation interaction

				Slope at		
Evaluation measure	N	FE × evaluation	p-value	−1 SD	Mean	+1 SD
Role-based	336	0.024 (0.058)	0.679	0.557	0.581	0.605
Achievement	336	−0.001 (0.063)	0.992	0.576	0.575	0.574

Panel B. Median split

	Below/at median		Above median			
Evaluation measure	N	Slope	N	Slope	Difference	p-value
Role-based	179	0.531 (0.087)	157	0.638 (0.075)	0.107 (0.113)	0.343
Achievement	184	0.564 (0.089)	152	0.591 (0.081)	0.027 (0.117)	0.821

Notes: The sample is the 336 adjacent manager turnovers in Table 6. Both the outgoing and incoming manager FEs use the same leave-section-out construction as Table 6, excluding the focal section and every other section held by both managers. Panel A estimates the succession relationship after adding the outgoing manager's standardized evaluation and its interaction with the outgoing manager's FE. The reported interaction is the coefficient on that product, and the three slope columns report the implied succession slope at the indicated evaluation level. Panel B divides turnover events at the median of the indicated evaluation; ties are included in the below-or-at-median group. Evaluations are the most recent role-based or achievement evaluation from the fiscal term before the incoming manager takes over and are standardized on the manager-year evaluation frame used in Appendix D.4. Standard errors, in parentheses, are clustered by section. Reported p-values are two-sided tests of the interaction in Panel A and of the slope difference in Panel B.

E Worker Outcomes

This appendix provides supporting analyses for the worker outcomes in Section 6. Appendix E.1 describes the exit-sample construction and examines the identifying assumption and manager-FE measurement. Appendix E.2 examines selective attrition, a focal-worker leave-out construction, and the relationship between personnel evaluations and subsequent promotion.

E.1 Worker Exit: Sample Construction and Identification

Worker sample at manager arrival. The exit design defines the worker sample at the September snapshot when a new manager arrives. Workers already in the section before the rotation are incumbents, while workers assigned to the section in the same rotation as the manager are identified by a co-arrival indicator included in the specification. Internal transfers do not count as exits. We restrict the sample to workers below age 59 at arrival and count a separation only if it occurs before age 59. The personnel records do not classify separation reasons. For each monthly horizon from 1 through 24 months after manager arrival, we define the outcome as whether the worker has separated from the firm by that horizon. All horizons use the same baseline worker sample and are restricted to manager succession events for which the personnel records extend at least 24 months after manager arrival, so the monthly profile does not reflect changes in the underlying sample across horizons. Because manager arrivals occur in September, calendar composition is aligned across succession events.

Manager spells and the timing of exit. Each monthly cumulative-exit outcome is related to the manager assigned at arrival rather than requiring continuous exposure to that manager throughout the full 24-month window. Of the 437 manager succession events for which the personnel records extend at least 24 months after arrival, 435 enter the regression sample; the other two are dropped as section fixed-effect singletons after applying the regression covariates. Among these 437 events, 75.1 percent of arriving managers remain the section's unique manager after 12 months and 51.7 percent after 24 months. The median post-arrival spell lasts 30 months, and the arriving manager covers 84.3 percent of the 24-month window on average. Figure 3 shows that the cumulative exit difference generally rises with the horizon rather than appearing only at manager arrival.

Leave-section-out measurement check. The all-section manager FE uses overtime from the focal section, so exit-induced changes in workforce composition or subsequent section overtime could mechanically enter the regressor. Table 22 therefore reconstructs the manager

FE using only the manager's other sections. On the common sample, the all-section estimate is 2.44 percentage points (SE 1.01). Using the standardized leave-section-out FE directly gives 1.23 percentage points (SE 0.75). Because the leave-section-out FE is an imperfect proxy for the all-section measure, this direct estimate is attenuated by measurement error. Using the leave-section-out FE as an instrument for the all-section FE gives an estimate of 3.15 percentage points (SE 1.85), close to the common-sample all-section estimate. The first-stage slope is 0.391 (SE 0.039; $F = 100.2$). This exercise checks whether the exit result depends on using focal-section outcomes in the manager FE; it does not change the identifying assumption about manager assignment.

Table 22: Worker exit with all-section and leave-section-out manager FEs

	(1)	(2)	(3)	(4)
	All succession events	Succession events whose manager has a leave-section-out FE		
	All-section manager FE	All-section manager FE	Leave-section-out manager FE	IV: all-section manager FE instrumented by leave-section-out manager FE
Exit from the firm within 24 months, percentage points per SD	1.78 (0.75)	2.44 (1.01)	1.23 (0.75)	3.15 (1.85)
First stage: all-section manager FE on leave-section-out manager FE				
Slope			0.391 (0.039)	
Cluster-robust F			100.2	
N worker-events	8,060	7,062	7,062	7,062
Succession events	435	365	365	365

Notes: The outcome is separation from the firm within 24 months of manager arrival. Each coefficient is reported in percentage points per one-standard-deviation increase in the relevant EB-shrunk manager FE. Column (1) repeats the Figure 3 specification. Columns (2)–(4) restrict the sample to succession events whose manager is also observed in another section, so that a leave-section-out manager FE can be constructed; these columns use the same sample and controls. Column (3) uses the manager FE estimated from the manager's other sections. The all-section and leave-section-out manager FEs are each standardized by the standard deviation of their own distributions, so the coefficients in columns (2) and (3) are not on the same measurement scale. Column (4) instruments the standardized all-section manager FE with the standardized leave-section-out manager FE. The first-stage slope and cluster-robust F-statistic are reported below the outcome estimates. Standard errors are two-way clustered by worker and incoming manager.

Exit identification diagnostics. The main concern is time-varying manager assignment: managers with higher FEs could be assigned to sections in which exit was already likely to rise. Table 23 examines this concern at the 24-month horizon in two ways. Panel A adds the section's recent exit history on the subsample for which both pre-arrival windows are observed. The estimate is 2.38 percentage points on this subsample before the history controls. Replacing the lagged exit rates with lagged departure counts and log section headcounts yields an estimate of 2.68 percentage points (SE 0.91). Panel B adds department-by-year fixed effects to the section fixed effects. The estimate is 1.99 percentage points (SE 0.86), compared with 1.78 in the primary specification. These checks do not indicate that observable pre-existing exit trajectories or department-level shocks account for the 24-month result. The interpretation nevertheless remains conditional on the identifying assumption in Section 6.1.

Table 23: Worker-exit identification diagnostics

Panel A. Section exit-history controls	
	Exit within 24 months
Baseline, history subsample	2.38 (0.93)
+ lag-1 section exit rate	2.42 (0.88)
+ lag-1 and lag-2 section exit rates	2.49 (0.86)
Lagged departure counts and log headcounts	2.68 (0.91)
N worker-events (succession events)	7,469 (399)
Panel B. Department × year shocks	
	Exit within 24 months
Primary specification	1.78 (0.75)
Section FE + department × year FE	1.99 (0.86)
N worker-events (succession events)	8,060 (435)

Notes: Coefficients are percentage-point changes in exit from the firm per one-standard-deviation increase in the arriving manager's EB-shrunk manager FE. Panel A uses the subsample for which two pre-arrival section exit rates are observed. Panel A first adds lagged exit rates. The final row replaces those rates with lagged departure counts and log section headcounts. Panel B compares the primary specification with one that replaces year fixed effects with department-by-year fixed effects (department of the worker's current section) while retaining section fixed effects. All other controls follow Figure 3 unless indicated. Standard errors are two-way clustered by worker and incoming manager and are reported in parentheses.

E.2 Robustness of Worker Outcomes

Selective attrition. Because managers with higher FEs are followed by higher exit, changes in the composition of workers who remain could affect the estimates in Table 8. Table 24 examines this concern in three ways. First, we re-estimate the within-worker specifications on employees who remain in the firm through the last complete fiscal year. Second, we use stabilized inverse-probability-of-firm-retention weights based on predetermined worker characteristics and personnel records, estimating retention separately for the August administrative panel and the January survey panel. The retention models include the manager FE, age and tenure quadratics, sex, education, rank, own overtime in the three months up to the snapshot, the previous fiscal year's role evaluation and average monthly overtime, and department and fiscal-year fixed effects. Missing lagged evaluation and overtime values are set to zero and accompanied by missing-value indicators. This reweights observed worker-years for differential firm retention predicted by observables while leaving the Equation (9) fixed-effect specification unchanged.

Third, we report a break-even calibration for selection on outcomes not captured by the retention model. For each outcome, we calculate the leaver-stayer gap that would be required for selection bias alone to explain the entire baseline estimate, given the 24-month exit differential in Figure 3. This exercise is deliberately transparent rather than a formal bound: it maps the arrival-based exit differential into the worker-year outcome panel and

therefore should be read as a scale calculation.

Panel A shows that the compensation estimates are essentially unchanged under both attrition checks. The total-wage coefficient is 16.57 thousand yen in the baseline specification, 15.88 among workers who remain through the end of the panel, and 16.45 under IPCW. The corresponding estimates for hours-linked pay are 19.13, 18.94, and 18.56 thousand yen. The break-even leaver-stayer gaps are 928.7 thousand yen in absolute value for total wage and 1,072.2 thousand yen for hours-linked pay, which are large relative to the corresponding baseline monthly means reported in the final column.

Panel B shows a similarly stable pattern for evaluation and promotion. The evaluation coefficient changes from 0.065 in the baseline to 0.081 in the survivor sample and 0.053 under IPCW. The promotion estimates are -0.58, -0.77, and -0.53 percentage points, respectively. Explaining the baseline estimates through selective attrition alone would require a leaver-stayer gap of 3.62 standard deviations in evaluation or 32.4 percentage points in promotion, compared with a baseline promotion rate of 3.83 percent.

Panel C shows that the worker-reported outcomes are also little changed by the attrition adjustments. The IPCW estimates retain the baseline signs and are generally close in magnitude; the largest change is for work difficulty, whose coefficient falls from 0.035 to 0.024. The break-even calibration provides a direct scale comparison. For the sense that effort is rewarded and workload, the required leaver-stayer gap exceeds the full response-scale range reported in the final column, while the gap for job satisfaction is close to the full range. The required gaps for promotion willingness and work difficulty are smaller but remain sizable relative to their response scales. The calibration therefore requires substantial outcome selection to account for the baseline survey estimates.

Removing the focal worker from the manager FE. For pay outcomes, the focal worker's own overtime contributes to both the manager FE and hours-linked pay. Table 25 therefore re-estimates each manager FE after excluding that worker's observations. The total-wage coefficient declines from 16.57 to 13.52 thousand yen, and the hours-linked-pay coefficient from 19.13 to 16.39 thousand yen. Both remain positive and precisely estimated. The estimates for evaluation, promotion, workload, and job satisfaction change little.

Evaluation and subsequent promotion. Table 8 shows higher evaluation under managers with higher FEs but no detectable increase in promotion. Table 26 asks whether the evaluation measure itself predicts promotion in the following fiscal year. A one-standard-deviation higher role-based evaluation predicts a 5.14-percentage-point higher promotion probability; adding worker fixed effects gives 5.07 percentage points. The corresponding estimates for the

Table 24: Sensitivity of worker-outcome estimates to selective attrition

Outcome	Baseline	Survivors to end	IPCW	Break-even leaver-stayer gap	Reference scale
Panel A. Compensation					
Total wage	16.57 (1.37)	15.88 (1.34)	16.45 (1.38)	−928.7	Mean = 332.6
Hours-linked pay	19.13 (1.14)	18.94 (1.11)	18.56 (1.18)	−1,072.2	Mean = 62.3
Panel B. Evaluation and promotion					
Evaluation	0.065 (0.035)	0.081 (0.036)	0.053 (0.035)	−3.62	SD = 1
Promotion	−0.58 (0.43)	−0.77 (0.49)	−0.53 (0.44)	32.4	Base = 3.83
Panel C. Worker-reported outcomes					
Job satisfaction	−0.073 (0.028)	−0.068 (0.030)	−0.068 (0.027)	4.11	Range = 4.33
Promotion willingness	−0.065 (0.027)	−0.064 (0.026)	−0.065 (0.026)	3.64	Range = 4.24
Effort is rewarded by firm	−0.096 (0.034)	−0.118 (0.035)	−0.103 (0.037)	5.36	Range = 3.77
Workload	0.187 (0.024)	0.193 (0.025)	0.180 (0.024)	−10.50	Range = 5.49
Work difficulty	0.035 (0.020)	0.034 (0.021)	0.024 (0.021)	−1.95	Range = 5.17

Notes: The *Baseline* column repeats Table 8. *Survivors to end* re-estimates the same specifications on workers who remain in the firm through the last complete fiscal year and is a descriptive sample restriction, not an attrition correction. *IPCW* reports the same fixed-effect regressions using stabilized inverse-probability-of-firm-retention weights. Retention is defined from August to August for administrative outcomes and January to January for worker-reported outcomes as presence anywhere in the firm's affiliation roster one year later. The retention models use predetermined worker characteristics, prior personnel records, the current manager FE, department fixed effects, and fiscal-year fixed effects; retirement-age transitions are treated as administrative censoring. *Break-even leaver-stayer gap* is $d^* = -\beta/\Delta p$, where β is the Baseline coefficient and Δp is the 24-month exit differential per one-standard-deviation increase in the manager FE from Figure 3. It is the leaver-minus-stayer outcome difference that, under this calibration, would be required for selection bias alone to explain the entire Baseline estimate. This is a calibration rather than a bound. The reference column reports the baseline-sample mean for the two pay outcomes, one standard deviation for the standardized evaluation measure, the baseline promotion rate, and the full response-scale range expressed in standardized units for each worker-reported outcome. Coefficients in the first three columns use the units and scaling of Table 8. Standard errors in parentheses are two-way clustered by worker and manager; for IPCW, they treat the estimated retention weights as fixed.

Table 25: Worker outcomes with the focal worker excluded from the manager FE

Outcome	Coefficient on the manager FE (per SD)	N
Panel A. Compensation		
Total wage	13.52 (1.39)	38,985
Hours-linked pay	16.39 (1.15)	38,985
Panel B. Evaluation and promotion		
Evaluation	0.057 (0.034)	36,810
Promotion	−0.60 (0.43)	36,108
Panel C. Worker-reported outcomes		
Job satisfaction	−0.070 (0.028)	21,321
Workload	0.166 (0.023)	27,844

Notes: The table re-estimates the Table 8 specifications for total wage, hours-linked pay, evaluation, promotion, job satisfaction, and workload after excluding the focal worker's own observations from the manager FE. The specification, units, scaling, and standard errors otherwise follow Table 8. For each outcome, the sample is the corresponding Table 8 sample restricted to observations for which the manager FE excluding the focal worker's own observations is defined.

achievement evaluation are much smaller. Thus, the evaluation measure used in Table 8 is strongly related to subsequent promotion even though workers under managers with higher FEs do not experience a detectable increase in promotion.

Table 26: Personnel evaluations and subsequent promotion

	(1)	(2)
	Fiscal-year FE	+ worker fixed effects
Realized promotion in the next fiscal year (pp)		
Role-based evaluation (per SD)	5.14 (0.21)	5.07 (0.26)
Achievement evaluation (per SD)	0.16 (0.19)	1.10 (0.23)
Base rate (pp)	4.10	4.08
Observations	34,016	33,570
Workers	5,044	4,598

Notes: The outcome is promotion to a higher rank in the following fiscal year, in percentage points. The two personnel evaluation measures are entered jointly and standardized to unit variance. Column (1) includes fiscal-year fixed effects; column (2) additionally includes worker fixed effects. Standard errors are clustered by worker and reported in parentheses. Base rates are calculated on each column's estimation sample.

F Firm-Side Operating Outcomes

F.1 Operating-Record Coverage

The firm keeps annual operating records of standard wholesale value and hours worked for the units that make and sell its products. Standard wholesale value is measured at the firm's standard wholesale prices rather than realized sales revenue. The records do not report cost, margin, profit, value added, or quality. The source documentation also notes that price revisions and reorganizations of sales areas, customers, and manufacturing items can affect comparisons over time. The operating records and personnel records are maintained in separate administrative systems, and their organizational units do not always correspond one-to-one. We therefore map the operating-record units to personnel sections using the August organizational hierarchy for each fiscal year, which also determines the section manager. The operating records cover fiscal years 2016–2025. This yields 645 section–fiscal-year cells in 72 sections with a unique August manager and an estimable manager FE, involving 159 managers. Of these sections, 32 are in manufacturing and 40 in sales; no back-office section has an operating record. The linked operating-record sample therefore covers only part of the organization and reflects both the coverage of the operating system and the ability to map its units to personnel sections. It does not provide a firm-wide measure of productive returns.

The linked sections are also a selected subset of the manager FE estimation sample. Of the 193 sections in that sample, 72 can be linked to operating records. As Panel A of Table 27 shows, linked sections are substantially larger, with 34.5 subordinates on average compared with 13.7 among unlinked sections, and their subordinates work more overtime, 27.8 versus 21.0 hours per month. Their managers also have somewhat higher manager FEs on average, 0.113 standard deviations compared with −0.057. Within sales, however, the ordering reverses: managers of linked sales sections average −0.633 standard deviations compared with −0.199 among the rest of the department. Any analysis using these records should therefore be read as evidence for a selected set of relatively large manufacturing and sales sections, rather than as a firm-wide productivity estimate.

F.2 Standard Wholesale Value and Hours

The operating records report three outcomes linked by the accounting identity $\log(\text{standard wholesale value}) = \log(\text{standard wholesale value per hour}) + \log(\text{hours})$. Because the three regressions use the same sample, specification, and regressor, the corresponding coefficients satisfy the same identity. Standard wholesale value per hour should be interpreted with care: the manager FE is constructed from subordinates' overtime

hours and therefore directly relates to the labor-input denominator.

The hours measure is not identical to the payroll measure underlying the manager FE. The personnel and operating records are maintained in separate administrative systems and measure labor input for different purposes. Thus, even when an operating-record unit can be linked to a personnel section, the recorded total hours need not coincide. The manager FE is estimated from average monthly overtime among eligible non-managerial subordinates, whereas the operating records report aggregate section-level hours. The two hours measures are strongly correlated in logs (0.97), but the operating records contain more hours overall and their exact worker coverage is not documented. The hours column should therefore be interpreted as an aggregate labor-input outcome rather than as a direct counterpart to the overtime measure used to construct the manager FE.

The standard-wholesale-value estimates are too imprecise to determine whether managers with higher FEs are associated with higher operating value. In the unweighted specification, a one-standard-deviation higher manager FE is associated with 1.5 percent higher standard wholesale value pooled across manufacturing and sales (SE 5.3). The corresponding estimates are 11.8 percent in manufacturing (SE 7.9) and 4.4 percent in sales (SE 7.8).

The aggregate-hours estimates are also imprecise. The pooled point estimate is 6.2 percent (SE 7.3), so its imprecision should not be read as evidence that higher manager FEs leave aggregate labor input unchanged. The department estimates differ: in manufacturing, recorded hours rise by 22.2 percent (SE 15.9) and standard wholesale value per hour falls by 10.4 percent (SE 9.6), whereas in sales recorded hours move by -4.1 percent (SE 4.9) and standard wholesale value per hour rises by 8.6 percent (SE 4.9). These department-specific decompositions describe how the accounting identity is realized in the observed sample, but they do not by themselves identify productivity effects.

Weighting section–year cells by recorded hours leaves the basic standard-wholesale-value conclusion unchanged: the pooled estimate is 5.5 percent (SE 4.9), with similarly imprecise department estimates.

These additional estimates do not overcome the more fundamental limitations documented in Appendix F.1. The operating records cover only a selected subset of sections, standard wholesale value is not realized sales revenue, and the records do not contain cost, margin, profit, value added, or quality. The available evidence therefore does not establish whether any productive benefits offset the higher worker exit documented in Section 6.1. The net firm return to managerial styles associated with higher manager FEs remains unidentified.

Table 27: Manager FE, standard wholesale value, and hours in sections linked to operating records

Panel A. Sections with and without linked operating records

	Linked	Not linked	Difference	p
Manager FE estimation frame				
Sections	72	121		
August-manager frame				
Section–fiscal-year cells	645	1,360		
Sections	72	222		
Managers	159	346		
manager FE (SD units)	0.113	−0.057	0.169	< 0.001
manager FE, sales sections only	−0.633	−0.199	−0.434	< 0.001
Subordinates per section	34.5	13.7	20.8	< 0.001
Subordinates' overtime hours (hours/month)	27.8	21.0	6.8	< 0.001
Subordinates' work hours (hours/month)	182.4	170.5	11.9	< 0.001
Female share (percent)	25.4	31.9	−6.5	< 0.001
Shorter-hours share (percent)	2.1	9.2	−7.0	< 0.001
Manufacturing share (percent)	45.7	18.8	26.9	< 0.001
Sales share (percent)	54.3	14.2	40.1	< 0.001

	log(standard wholesale value)	log(standard wholesale value per hour)	log(hours)	N	Sections
Panel B. Unweighted					
Pooled	0.015	−0.047	0.062	645	72
	(0.053)	(0.050)	(0.073)		
Manufacturing	0.118	−0.104	0.222	295	32
	(0.079)	(0.096)	(0.159)		
Sales	0.044	0.086*	−0.041	350	40
	(0.078)	(0.049)	(0.049)		
Panel C. Weighted by section hours					
Pooled	0.055	−0.052	0.107	645	72
	(0.049)	(0.047)	(0.075)		
Manufacturing	0.086	−0.049	0.135	295	32
	(0.063)	(0.059)	(0.096)		
Sales	0.054	0.054*	−0.001	350	40
	(0.038)	(0.029)	(0.036)		

Notes: The unit throughout is the section–fiscal-year cell with a unique August section manager whose manager FE is in the connected set, and the manager FE is the EB-shrunk fixed effect in connected-set standard deviations. *Panel A.* A cell is linked if an operating-record unit can be mapped to the personnel section and reports positive standard wholesale value and positive hours for that fiscal year. The panel counts sections on two frames, named by the two sub-headings: the manager FE estimation frame is the 193 sections of Table 2, and the August-manager frame, which every row below the second heading uses, is wider. Subordinates per section, overtime, work hours, female share, and shorter-hours share are fiscal-year means of the quarterly section panel, with overtime and work hours in monthly hours per subordinate; department shares are taken from the August snapshot. Differences are linked minus not linked, p-values are two-sided Welch t-tests, and the count rows are not tested. *Panels B and C.* The dependent variables are the logs of standard wholesale value, standard wholesale value per hour, and hours worked, taken from the firm's operating records and summed to the cell; hours are the hours recorded in those operating records. The three logs satisfy log(standard wholesale value) = log(standard wholesale value per hour) + log(hours) cell by cell, and all three regressions use the same sample, fixed effects, weights, and regressor, so the coefficients in each row sum exactly. Each cell of the two panels is an OLS regression of the log outcome on the manager FE with section and fiscal-year fixed effects, on the 645 cells in 72 sections that are linked to operating records, have a unique August section manager, and have a connected-set manager FE. Standard errors in parentheses are clustered by section, with p-values from a t distribution on (sections − 1) degrees of freedom. Panel C weights cells by section hours in the operating records. Significance: $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

G Sensitivity to Estimation Uncertainty in Manager FEs

G.1 A Nonstandard Two-Step Inference Problem

The regressions in Tables 3–7 use estimated manager FEs as outcomes, regressors, or both. In Tables 3–6, the reported clustered standard errors treat these generated objects as fixed. Table 7 instead uses a two-sample cluster bootstrap that re-estimates the supervisor–future-manager slope and the decomposition slopes, but it likewise holds the manager FEs fixed. This is a two-step inference problem in the broad sense of Murphy and Topel (1985), but our setting is less standard because the first-stage objects are high-dimensional network fixed effects and the downstream analyses use leave-out versions of them. Network connectivity also affects the precision of the estimated fixed effects and inference in subsequent regressions that use them (Jochmans and Weidner, 2019).

Related applications do not use a single correction for these problems. They instead use different combinations of shrinkage, leave-out or split-sample constructions, instrumental-variable or reliability adjustments, inverse-variance weighting, connected-set restrictions, and conventional robust or clustered inference (Benson et al., 2019; Chetty et al., 2014; Jacob et al., 2010; Lazear et al., 2015; Metcalfe et al., 2023; Weidmann et al., 2026). Fenizia (2022), in a closely related manager-FE setting, bootstraps downstream event-study standard errors because manager quality is a generated regressor. Lazear et al. (2015), by contrast, explicitly note that their boss-clustered standard errors are too small because the boss measures are generated regressors and leave that uncertainty uncorrected. These different choices reflect that the appropriate treatment depends on how the estimated effects are constructed and reused downstream.

We therefore do not treat the analysis below as a general correction for generated-regressor uncertainty. Its narrower purpose is to assess whether the paper's downstream results are sensitive to re-estimating the manager-FE objects rather than treating their realized estimates as fixed.

G.2 Conditioning on the Realized Network

Manager FEs in our setting are identified through the realized manager–section network. The precision of network fixed effects depends on the connectivity of that network, and relevant theory commonly conditions on the realized network or design matrix when deriving their sampling properties (Jochmans and Weidner, 2019; Kline et al., 2020). We therefore condition the sensitivity analysis on the realized manager–section network and the connected set used

in the paper.

This choice keeps the empirical object being perturbed fixed across draws. Reconstructing the connected set in each draw could change which managers and sections are jointly identified, the normalization under which their FEs are comparable, and the observations entering a downstream regression. Such variation would mix uncertainty in estimating manager FEs for a given identified network with uncertainty over which units are identified at all. Our sensitivity analysis asks the narrower question: conditional on the realized organizational network and connected set, how much do the downstream results change when the manager-FE objects are re-estimated?

This conditional treatment of the connected sample is also consistent with close manager-FE applications that estimate and normalize fixed effects within connected sets and conduct subsequent analyses within those sets (Metcalfe et al., 2023; Weidmann et al., 2026). These applications do not, however, establish a resampling rule requiring the connected set to remain fixed. Holding it fixed here is a choice about the inferential target of our sensitivity analysis.

G.3 Resampling Sensitivity Design

Within each section, we resample worker–section blocks with replacement. A block contains all monthly observations for a worker while that worker is in the section. This preserves dependence across months within a worker's spell in a section while perturbing the subordinate observations used to construct the manager FEs. The organizational network, connected set, and definitions of the downstream samples are held fixed.

The first-stage reconstruction follows the specification used by each downstream analysis. For Tables 3, 4, 6, and 7, the leave-out samples and the corresponding fixed-effect operators are held fixed, while the manager-FE values are recomputed from the resampled cell outcomes; these refits do not apply empirical-Bayes shrinkage. Table 5 instead refits its manager FEs re-estimated after removing the focal worker's own person-months in each draw, while holding its shrinkage parameters fixed. When a worker resample produces a non-finite section-quarter mean or fewer than three distinct months for a cell, the other exercises retain that cell's observed mean; this occurs for about 2.3 percent of cells per draw. In the Table 5 refits of this measure, thin cells are instead dropped. In 61 of the 999 Table 5 draws, this leaves one or two of the 78 future managers without an estimable manager-FE object.

We then resample the downstream clusters used by each analysis: managers in Table 3, pre-promotion supervisors in Table 4, switch events in Table 5, and sections in Table 6. Table 7 combines the section sample used for the persistence and pre-promotion-supervisor

slopes with the separate pre-promotion-supervisor sample used to estimate the supervisor–future-manager relationship. The first-stage worker resample and downstream resample are therefore two distinct perturbations rather than a single joint draw from a common sampling unit.

This distinction matters because there is no common resampling unit that naturally nests both stages of all the FE-on-FE analyses. Fenizia (2022) provides a useful contrast. Her replication package uses a common office-level bootstrap weight when re-estimating the leave-out manager effects and the downstream event-study regression. In our setting, the first-stage manager FEs are estimated through the worker–section network, whereas the relevant downstream units are managers, pre-promotion supervisors, switch events, or sections depending on the analysis. A direct analogue of Fenizia's joint office-level resampling is therefore not available across Tables 3–7.

We do not interpret the resulting two-layer procedure as a formally established bootstrap for the sampling distribution of our estimator. Bootstrap validity in fixed-effect settings depends on the estimator and the resampling scheme. Higgins and Jochmans (2024), for example, establish validity for a recursive parametric bootstrap in a different fixed-effect setting; their result does not establish validity of the nonparametric block resampling used here. We instead use the procedure as a resampling sensitivity analysis and examine its behavior directly where informative internal diagnostics are available.

G.4 Internal Diagnostics

We use two diagnostics to assess the behavior of the resampling sensitivity analysis where the design permits them. These diagnostics do not establish formal validity of the procedure.

First, we hold the manager-FE objects fixed and apply the same downstream resampling used in the sensitivity analysis. For Table 6, the resulting standard deviations are 1.03, 1.01, and 1.04 times the corresponding section-clustered standard errors across the three succession horizons. For the persistence slope and pre-promotion-supervisor slope in Table 7, the corresponding ratios are 0.93 and 1.05 relative to the fixed-FE two-sample bootstrap dispersion used in Table 7. Thus, for these specifications, the downstream resampling reproduces the dispersion of the reported fixed-FE inference closely.

Second, for Table 6 and the section-sample part of Table 7, we can partially couple the first-stage perturbation to the downstream section draw by perturbing only sections selected in that draw and leaving other sections at their observed cell values. This construction is not a valid alternative bootstrap, because observations in undrawn sections remain in the first-stage estimation. It is useful only as a diagnostic of the independent layering in the

baseline sensitivity analysis. For Table 6 and the two section-sample slopes in Table 7, the standard deviation from the baseline two-layer procedure is 9 to 16 percent larger than under this coupled variant. At the same time, the coupled standard deviation remains 1.27 to 1.59 times the corresponding fixed-FE dispersion in the specifications for which the fixed-FE diagnostic above is well calibrated.

These comparisons indicate that pairing separate first-stage and downstream perturbations can increase dispersion somewhat, but does not account for most of the difference between the fixed-FE and FE-reestimated exercises in the specifications where this comparison is feasible. No analogous coupling diagnostic is available for Tables 3–5, whose downstream units do not provide a nesting structure for the first-stage worker–section resampling. We therefore do not use the coupling results to validate the procedure for those tables.

A complementary Monte Carlo exercise examines the Table 5 design. The simulations preserve the observed panel structure, manager–section network, connected set, and promotion-event structure while generating data with known downstream relationships. Across five calibrated designs, the resampling standard deviation is 1.01 to 1.16 times the Monte Carlo standard deviation of the estimator. By contrast, the displacement of the resampling distribution from the original estimate is negatively related to the realized estimation error, with correlations ranging from -0.55 to -0.92. Using that displacement as a bias correction reduces root mean squared error in only one of the five designs. We therefore use the dispersion of the resampling distribution, centered on the reported estimate, as a descriptive sensitivity measure. Neither this Monte Carlo exercise nor the diagnostics above establish formal coverage of the resulting sensitivity intervals.

G.5 Sensitivity of the FE-on-FE Estimates

Table 28 applies the resampling sensitivity analysis to the main FE-on-FE results in Tables 3–7. For Tables 3–6, the fixed-FE reference is the clustered standard error reported in the main analysis. For Table 7, it is the standard deviation from the two-sample bootstrap underlying that table's own fixed-FE intervals. The resampling standard deviation instead allows the relevant manager-FE objects to vary under the procedure described above.

We do not interpret the difference between these two dispersion measures as a correction to the reported standard errors. The two-layer resampling procedure is not a formally validated estimator of the full sampling distribution, and the internal coupling diagnostic is available only for a subset of the specifications. The comparison instead asks whether conclusions based on the realized manager-FE estimates are fragile when those objects are repeatedly reconstructed from perturbed worker-level data.

The conclusions are not fragile to re-estimating the manager FEs. The resampling dispersion is larger than the fixed-FE reference in every row, but all thirteen point estimates remain positive and the lower endpoint of each point-centered sensitivity interval remains above zero. This includes the portability estimates in Table 3, the supervisor–future-manager relationship in Table 4, all three supervisor-switch specifications in Table 5, persistence through three manager replacements in Table 6, and the persistence slope, pre-promotion-supervisor slope, supervisor-linked component, and remaining component in Table 7.

We do not construct the same symmetric sensitivity interval for the share of persistence associated with the supervisor-linked component in Table 7. The share is a ratio, and in a small number of resampling draws its denominator, the overall persistence slope, is close to zero. We therefore assess the decomposition through the sensitivity results for the supervisor-linked component and the overall persistence slope rather than through a symmetric interval for the ratio.

Table 28: Sensitivity of FE-on-FE estimates to re-estimating manager FEs

Main-table result	Estimate	Fixed-FE uncertainty	Resampling SD	Sensitivity interval
Table 3				
All spells	0.540	0.055	0.109	[0.324, 0.755]
At least eight quarters on each side	0.667	0.073	0.124	[0.419, 0.914]
Table 4				
Leave-section-out supervisor–future-manager slope	0.539	0.098	0.149	[0.241, 0.838]
Table 5				
Promotion-cohort effects	0.74	0.25	0.32	[0.10, 1.38]
+ section pre-switch conditions	0.86	0.22	0.32	[0.22, 1.50]
+ future manager's own pre-switch overtime trend	0.89	0.24	0.34	[0.21, 1.57]
Table 6				
Adjacent managers	0.576	0.062	0.116	[0.347, 0.805]
Two replacements apart	0.549	0.068	0.118	[0.315, 0.783]
Three replacements apart	0.494	0.079	0.128	[0.241, 0.748]
Table 7				
Incoming manager's FE (b)	0.506	0.085	0.137	[0.233, 0.780]
Pre-promotion supervisor's FE (b_{sup})	0.494	0.084	0.123	[0.251, 0.738]
Supervisor-linked component	0.267	0.068	0.106	[0.056, 0.478]
Remaining component	0.240	0.098	0.113	[0.014, 0.465]

Notes: This table reports a resampling sensitivity analysis, not corrected standard errors. The realized manager–section network and connected set are held fixed. Within each section, worker–section blocks are resampled and the relevant manager-FE objects are recomputed as described in Appendix G.3; the downstream clusters are then resampled. For Tables 3–6, Fixed-FE uncertainty is the clustered standard error reported in the corresponding main analysis. For Table 7, it is the standard deviation from the FE-fixed two-sample bootstrap used for that table's intervals. Resampling SD is the standard deviation across the two-layer sensitivity draws. The sensitivity interval is centered on the reported estimate with half-width equal to the resampling SD times the 97.5th percentile of the relevant t distribution. These intervals are descriptive sensitivity measures and are not confidence intervals with established coverage. The Table 7 share is omitted because its resampling dispersion is unstable when the estimated persistence slope is close to zero in a small number of draws.

G.6 Worker-Outcome Regressions

The worker-outcome regressions in Section 6 permit a different diagnostic because the outcomes are directly observed and the manager FE enters on the right-hand side. We use

the same worker resample in both stages: in each draw, the manager FEs are either held fixed or re-estimated from the resampled first-stage data, and the worker-outcome regression is estimated using that same worker draw.

As a benchmark, holding the manager FEs fixed produces standard deviations between 0.96 and 1.03 times the corresponding worker-clustered standard errors. Re-estimating the manager FEs changes these standard deviations by comparatively little: the ratio of the FE-reestimated to FE-fixed standard deviation ranges from 0.91 to 1.16 across the eight estimates: worker exit at 12 and 24 months, total pay, hours-linked pay, evaluation, promotion, workload, and job satisfaction. Thus, along the worker sampling dimension, re-estimating the manager FEs does not systematically increase the sampling dispersion of the Section 6 estimates.

This check addresses the worker dimension only. The main Section 6 specifications use two-way clustering by worker and manager, and we do not have a validated joint resampling procedure that reproduces that two-way inference while also re-estimating the manager FEs. We therefore use this exercise as a sensitivity check rather than as a replacement for the reported two-way clustered standard errors.