From Entry to Leadership: Early-Career Rotation and Leadership in Pakistan's Civil Service*

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March 2026

Abstract

How do states produce generalist leaders from young entrants? I digitize complete career histories for 35 cohorts (1986–2020) of Pakistan's elite administrative civil service and study how early-career geographic rotation shapes access to frontline leadership posts. Identification comes from a gender-specific policy that temporarily allowed some junior officers to remain in their domicile during the initial posting years, creating cohort-by-province variation in early mobility within the same career ladder. Officers exposed to domicile allocation are 33 percentage points less likely to reach frontline roles, despite similar promotion timing, with no evidence that differences in baseline ability, performance, cohort composition, or slot availability drive the result. Domicile allocation reduces rotation breadth, cutting the number of districts served by about 18 percent. A survey of 309 serving officers indicates that mobility is widely viewed as central to meritocratic leadership selection. The findings imply that early-career rotation is a key technology through which personnel systems build generalist capacity and shape who ultimately leads.

JEL Classification Numbers: D73; J24; J45, P43
Keywords: generalist capacity; early-career rotations; internal labor markets; promotions

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I. Introduction

Rotations are a core management practice in large organizations. Firms routinely place new junior officers in varied operational contexts. Rotations across teams and supervisors account for a large share of productivity differences (Best et al., 2019; Limodio, 2021; Fenizia, 2022; Minni, 2023). Moreover, in state bureaucracies, rotation policies are studied as incentive mechanisms for access to desirable postings and advancement, with downstream effects on performance and state capacity (Xu, 2018; Khan et al., 2019; Bergeron et al., 2022). There is a rich theoretical literature on the impact of rotations on career outcomes (e.g., Becker, 1964; Hart and Moore, 2005; Gibbons and Waldman, 2006; Lise et al., 2020). Yet we have limited empirical evidence on the consequences of rotations for civil servants' careers and on their role in building generalist managerial capacity. This paper brings new evidence on both.

This paper examines the role of wider rotation in mid-career assignments of civil servants. A canonical wider rotation design is the non-domicile rule: in the initial years, officers serve in districts outside their province of domicile, where language, local political economy, and administrative practice differ.[*]Non-domicile allocation is a salient feature of large, rule-based bureaucracies such as China's civil service (Xu, 2011), India's administrative service (Iyer and Mani, 2012; Xu et al., 2023), and Pakistan's administrative service (Aman-Rana, 2025). I find that domicile rotation reduces the likelihood of appointment in career streams where coordination is central: appointment into frontline assignments falls by 33 percentage points, with no effect on deputy and ministry streams.[†]These patterns are consistent with a "jack-of-all-trades" hypothesis, in which balanced generalist skills across domains matter more than specialized depth in a single domain.

There are several challenges in empirically examining the consequences of non-domicile rotation on career outcomes and in distinguishing the mechanisms that underlie them. First, allocations are typically endogenous and uniform entry-grade rotation rules applied to all officers are rare; even where such rules exist, identification requires exogenous variation in the rule. Second, studying this requires comprehensive officer-level entry-grade assignment records, including all lateral moves across different districts. Prior work examines vertical mobility (e.g., Bertrand et al., 2020) that speaks to promotion incentives across grades instead of within-grade appointment dynamics. Third, variation in task demands and in how performance is assessed across postings can confound the estimated effect of non-domicile allocation. Fourth, district-level covariates such as political environment, socio-economic conditions, and administrative structure make it difficult to disentangle whether effects reflect rewards to hard postings, appointment in more developed districts, greater visibility, or the wider rotation margin itself. Finally, assessing the impact of entry-grade rotation on next-grade assignment quality at the mid-career margin is challenging in

the absence of pay differences: frontline, deputy, and ministry roles are consequential in distinct ways, and a credible within-grade taxonomy requires an empirical classification.

I digitized administrative data on 1,071 junior officers in Pakistan's elite civil service (1986–2020), which are well suited to studying non-domicile rotation. The setting makes it possible to better confront the identification problems highlighted above. I leverage a rotation policy [‡] that changed the entry-grade non-domicile allocation rule for women officers after induction into the civil service. The policy induced exogenous variation in initial geographic assignment among officers who are otherwise comparable in exam performance, age at entry, and education.[§] This change in the rotation rule generated discrete, cohort-wide shifts that are orthogonal to officers' characteristics, providing rare, system-wide variation in initial assignment at scale. The universe of personnel records allows me to reconstruct complete entry-grade assignment histories and all lateral moves during the first five years. Because these moves do not entail pay changes, cadre switches, or observable patronage, they are homogeneous in value and less prone to confounding.[¶]

I characterize each officer's entry-grade rotations along two margins—the number of postings in the same district and in different districts—and the nature of tasks in these postings. I construct an empirical taxonomy of posts and measure ten-year post-entry transitions across three tracks: frontline general management, deputy management, and specialist or ministry roles.[‖] In addition to the econometric advantages of the data, understanding rotation design in public organizations has first-order implications given the scale of public employment and its role in state capability (Weber, 1978; Simon, 1957; March and Simon, 1958), also see, (Baig et al., 2021; World Bank, 2021). To further understand civil servants' perceptions of mobility and the meritocracy of appointments, I conducted additional analysis using individual survey responses. I surveyed 309 civil servants in April 2025 from cohorts who joined between 1998 and 2010.

I find strong negative effects of domicile allocation on frontline appointments. In my preferred (and most conservative) specifications, domicile-allocation officers' likelihood of frontline leadership roles falls by 33 percentage points relative to non-domicile officers in the same cohorts, while the probability of serving as deputy managers also a district-level role does not change in a statistically meaningful way. Relative to non-domicile officers, domicile-allocation officers' appointments increase by 12 percentage points in ministry roles; the non-domicile group's likelihood instead rises for frontline (district) roles.[**] Results are robust to adding a rich set of controls. Estimates are stable when I collapse frontline and deputy into a single district-service category. Specifications absorb cohort and province fixed effects; adding *Province of Posting* × *Cohort* fixed effects leaves estimates unchanged. Moreover, estimates remain robust when using alternative outcomes and seat-adjusted frontline shares, across functional forms and under alternative inference strategies. The findings show that the margin of wider rotation matters for selection into frontline leadership roles. Frontline quality is unchanged: using test scores as the

quality proxy, I show there is no shift around the policy; the effect is displacement of treated officers, not dilution of the frontline pool.

The granularity of the data makes it possible to separate different channels. Checks on abilities and demographics show that treated and control officers are statistically indistinguishable. This rules out extensive-margin selection and points to allocation, conditional on entry, as the source of downstream differences. Treated and control officers display parallel early-career trajectories. They rotate across posts at similar frequencies, receive comparable performance evaluations, and take about the same time to reach their first promotion. Among officers who reach district leadership, treated and control groups are economically identical. They serve for similar lengths of time, spend comparable durations in post, and are assigned to districts of equivalent quality including high-pressure and more developed regions. This serves as a falsification test: it rules out three alternative explanations for the leadership gap. First, it rules out negative selection that treated officers who make it to leadership are weaker on unobservables. Second, it rules out performance based attrition that treated officers exit faster or are reassigned sooner once in charge. Third, it rules out preferential assignment that treated leaders are cushioned into easier or lower-stakes districts. The absence of any systematic differences among those who reach leadership implies that, conditional on attaining a top post, treated and control officers are managed and deployed equivalently.

Next, I estimate the geographic gap in initial posting between officers who are allocated to non-domicile provinces and those who remain in their province of domicile. I benchmark each officer's first-posting distance using their district of birth as a proxy for origin and measure how far they are placed in their first assignment. I find that post-policy treated officers are posted substantially closer to their district of birth (-1.63 log km, $p = 0.00$), an effect that corresponds to an 80 percent reduction in distance relative to the pre-policy mean. When I proxy domicile with birth district (where domicile is missing), results are similar. These results quantify the magnitude of the shift in geographic allocation between domicile and non-domicile officers at entry. Moreover, I find that post-policy treated officers travel substantially shorter distances across postings in the initial five years (-1.74 log km, $p = 0.02$), an effect corresponding to an 84 percent reduction in mean distance moved during the first five years relative to the pre-policy mean.

I find a strong negative effect of the policy on early-career geographical exposure: the number of distinct political–administrative environments officers experience falls by 0.43 units, an 18 percent decline relative to the pre-policy mean, while overall transfer frequency and district quality remain statistically unchanged. Treated officers therefore move just as often but within a narrower set of institutional contexts. This is the margin along which officers acquire context-specific social, economic, and administrative knowledge the foundation of their generalist capacity. In this organization, a reduction in the number of distinct districts is not simply a drop in postings; it

represents a loss of operational perspective and exposure to varied experiences. The policy thus narrows the environments through which officers learn to navigate heterogeneity, weakening the process by which the system cultivates adaptable, context-sensitive managers. The result is robust to alternative exposure measures (e.g., high diversity and the Early Exposure Index) and to distance defined via great-circle (latitude–longitude) measures. I show that early exposure capital partially mediates the policy's effect on leadership attainment.[††] A one–standard deviation increase raises the likelihood of holding a frontline leadership position by 0.20 points.

In the survey, post-policy officers report a different career environment. They are less likely to say that appointments are meritocratic and less likely to endorse the role of early assignments in leadership appointment. Yet treated and control officers report similar aspirations for senior leadership roles.

A key contribution of the paper is to open the "assignment black box" of the state and measure how early rotations produce managerial capacity. I use universe-wide officer–post histories and a policy shock that altered non-domicile rules to separate where officers are sent (cross-context exposure) from how often they move (transfer volume), two margins that promotion records alone typically cannot disentangle. Aggregating postings over the first five years in service yields a composite measure of early-career assignment portfolios along these channels. This universe-wide panel allows me to move beyond small samples or case-based promotion records and to trace how formal assignment rules shape careers over time.

This study shifts the lens from rotations of mid-career officials to the entry stage of a civil-service career. Most existing work focuses on incumbent managers, where networks, reputations, and discretionary assignments already confound career outcomes. I study a stage-specific production function for managerial capacity, locating the state's leverage in the entry years when policy can shape trajectories at scale. The analysis shows that wider rotations shape appointments to frontline general-management roles, a coordination-intensive margin, in a pattern consistent with the jack-of-all-trades hypothesis and most pronounced in lower-capacity environments that require inter-unit and inter-territorial coordination.

The officers in the study hold significant influence in Pakistan's governance structure: as district administrators, they are the state's primary implementers of local policy and the frontline interface between citizens and government. Their assignments shape how public programs are executed across regions and sectors. Understanding how these assignments are formed and evolve provides a window into the micro-mechanisms of state capacity: how exposure, context, and institutional rules jointly shape who gets to lead, and how the state builds (or loses) its managerial talent pipeline.

A. *Related Literature*

A major question in the economics of organizations is how organizations produce generalist managerial capacity. While there is a rich literature on specialization, team production, and task allocation, we know relatively little about how generalist managers are prepared and developed within internal labor markets. Extensive research has studied mobility within and between organizations or broad career moves such as promotions and exits. By contrast, the internal rotation systems that govern assignments have been studied mostly in theoretical and descriptive studies (e.g. Gibbons and Waldman, 1999; Baker et al., 1994; Gibbons and Waldman, 2006; Gathmann and Schönberg, 2010). This paper is the first to study the role of internal assignment rules in career allocation and appointment of officials, and to show how these rules affect civil servants' careers in the long run.

This paper contributes to three strands of research in the organizational and labor economics of the state. First, it advances a growing literature on the personnel economics of public bureaucracies that examines selection and motivation (Dal Bó et al., 2013; Hanna and Wang, 2017; Callen et al., 2018; Ashraf et al., 2020; Dahis et al., 2025) incentives and management practices (Ashraf and Bandiera, 2018; Rasul and Rogger, 2018; Khan et al., 2019; Bertrand et al., 2020; Bandiera et al., 2021), and monitoring and managerial oversight (Duflo et al., 2012; Dal Bó et al., 2013). Yet the role of assignments in the careers of civil servants has received relatively less attention, especially the process by which younger officers are rotated across posts and exposed to different administrative environments. The closest antecedents are studies using rotation-based identification to measure public-sector performance (Best et al., 2019; Fenizia, 2022; Bergeron et al., 2022). I complement this stream of work by showing how assignment designs in organizations with complex inter-provincial coordination and communication build generalist capital. Second, the paper speaks to research on norms and workplace flexibility, and on how participation and career trajectories respond to mobility frictions and the geography of work (Fernández, 2013; Bernhardt et al., 2019, 2018; Bursztyn et al., 2020; Jayachandran, 2021; Becerra and Guerra, 2023; Ho et al., 2024). A reform targeted at flexibility modified the institution's entry assignment rule, allowing initial postings closer to home (Buchmann and Meyer, 2024). The analysis traces how this adjustment reweighted institutional margins: by altering early geographic exposure, it changed the structure of career assignments and leadership selection while entry ability remained unchanged. The paper provides an institutional account of how shifts in assignment design can operate through distinct margins—the entry margin and the career margin—especially in organizations that rely on rotation-based capability formation. Third, the paper speaks to how organizations produce generalist managerial capital. Classic theories argue that wider experience is central to leadership and entrepreneurial skill formation (Lazear, 2004, 2005; Gibbons and

Waldman, 2006; Gathmann and Schönberg, 2010). Complementary work shows that coordination improves when organizations build a common vocabulary and shared experience, which lowers communication frictions and coordination costs (Garicano, 2000; Dessein, 2002; Weber and Camerer, 2003; Silva, 2007; den Steen, 2010; Stuetzer et al., 2013). I provide the first large-scale evidence from the public sector that early experience in heterogeneous political–administrative environments is a key input into leadership formation.

In the next section, I describe the institutional context and data. Section III presents the baseline empirical specification, and Section IV reports the main results. Section V explores mechanisms. Section VI analyzes rotation diversity and rotation volume and presents evidence from the Early Exposure Index and the survey. Section VII concludes.

II. Empirical Context and Data

The main variables in the analysis come from newly digitized personnel records of Pakistan's elite civil service, the country's central administrative cadre. The data are drawn from approximately 40,000 volumes of official HR files maintained by the federal human resources department, documenting every posting, transfer, and promotion since the mid-1980s. I started this digitization in 2021 and completed it in 2024, constructing a panel of 1,071 officers who entered between 1986 and 2020, covering roughly 80 percent of districts nationwide. The panel records 7,571 rotations about 3,600 at the entry grade and 4,000 at the next grade with job titles, grades, start and end dates, cities, provinces, and ministries, allowing the construction of officer-level career trajectories and rotation measures at fine spatial and temporal granularity. This section describes in detail the structure and construction of the dataset and relevant background information on the organization of the service itself.

A. Background and Natural Experiment

The analysis focuses on how early-career geographic exposure shapes subsequent leadership appointments, I concentrate on postings in the entry grade, where non-domicile rules are centrally enforced. This focus ensures that observed differences in early experiences reflect centralized allocation rather than individual choices about where and when to rotate. Pakistan's elite civil service recruits nationally via a competitive federal exam, followed by multi-stage screening and 14 months of foundational and technical training. I focus on the administrative (general-management) cadre, the post-Independence successor to the colonial service. After training, the central human resource authority allocates each entrant to a province under a non-domicile rule: the first posting must be outside the officer's domicile province. This allocation spans the full administrative geography, creating a cadre with nationwide scope and presence.

Once allocated in a domicile or non-domicile province,[‡‡] new junior officers receive their first command as frontline officers (hereafter, *FLOs*), typically Assistant Commissioners. *FLO*s are generalist administrators with executive, fiscal, and quasi-judicial authority who coordinate district delivery. They then rotate across districts and sub-districts within the allocated province. The design is explicit: early, centrally directed rotations are intended to build generalist managerial capital by exposing officers to varied political–administrative settings (see Figure 1). An early-2000s rotation policy change relaxed the entry-grade non-domicile rule for a subset of junior officers, allowing them to begin service in their home (domicile) province; others continued under the cross-province rule I refer to these as treated (domicile allocation) and control (non-domicile allocation), respectively.

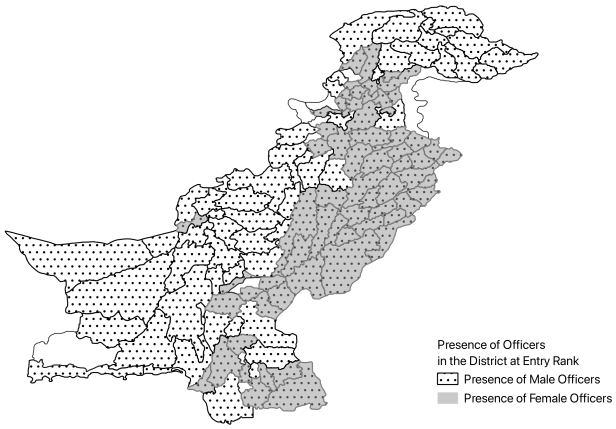


Figure 1: Officers Allocation across Country at Entry Grade

Notes: The figure shows the distribution of younger officers' allocation in the entry across districts and sub-districts (1986-2020).

Over the years from 2000 onward, the share of women in the cohort rises sharply. With total seats fixed, this increase comes almost one-for-one at the expense of men, rebalancing cohort composition by roughly 30–35 percentage points between women and men (see Panel A of Figure A1). Consistent with this, the number of male junior officers falls correspondingly in the same cohorts (see Panel B of Figure A1).

B. Descriptive Statistics: Cohort Composition

Here I describe the overall sample, irrespective of treated status, before turning to differences between treated and control officers. Table 1 summarizes officer demographics and entry characteristics. I observe 1,071 officers entering the civil service between 1986 and 2020. The average entrant is 27.6 years old at joining. About 21 percent belong to the treated group while 79 percent are control officers subject to the non-domicile rule. About 13 percent have an army background, and more than half hold a master's or PhD degree (55 percent) or a STEM background (54 percent). Most officers enter through provincial open merit (86 percent), with 8 percent through national open merit and 6 percent through reserved quotas. The provincial composition reflects the country's population: 57 percent from Punjab, 18 percent from Sindh, 13 percent from Khyber Pakhtunkhwa, 6 percent from Balochistan, 4 percent from Gilgit–Baltistan/FATA, and 2 percent from Azad Jammu and Kashmir. Exam and training scores are high and tightly distributed: average entry exam score is 59.9 percent, general training 70.3 percent, technical training 66.7 percent, and the final progression exam 60.6 percent. These patterns indicate a relatively homogeneous, high-ability entrant pool, consistent across treated groups. The mean cohort size is 34 officers per year.

TABLE 1: Individual Characteristics of Junior Officers

	Mean	SD	p1	p50	p99
Demographics and Background					
Age at joining (years)	27.56	2.47	23.00	27.45	33.00
Women	0.21	0.41	0.00	0.00	1.00
Army background	0.13	0.34	0.00	0.00	1.00
Masters/PhD degree	0.55	0.50	0.00	1.00	1.00
STEM degree	0.54	0.50	0.00	1.00	1.00
Entry Channel					
Provincial open merit	0.86	0.35	0.00	1.00	1.00
National open merit	0.08	0.27	0.00	0.00	1.00
Women/minority quota	0.06	0.25	0.00	0.00	1.00
Province of Domicile					
Punjab	0.57	0.50	0.00	1.00	1.00
Sindh	0.18	0.39	0.00	0.00	1.00
Khyber Pakhtunkhwa	0.13	0.33	0.00	0.00	1.00
Balochistan	0.06	0.24	0.00	0.00	1.00
Gilgit–Baltistan / FATA	0.04	0.19	0.00	0.00	1.00
Azad Jammu and Kashmir	0.02	0.14	0.00	0.00	1.00
Exam and Training Scores (%)					
Entry Exam score	59.89	4.64	50.00	59.53	70.00
General Training score	70.25	7.43	55.00	70.46	85.00
Technical Training score	66.66	6.67	52.00	67.04	80.00
Progression Exam score	60.57	6.41	48.00	61.00	73.00
Other					
Cohort size	34.14	8.77	18.00	37.00	52.00

Notes: The table present summary statistics for 1,071 officers entering the Service between 1986 and 2020. Binary variables are coded 1 if the condition holds. Scores are percentages. Cohort size refers to the number of officers entering in a given year.

Source: Author's calculations.

The next section describes the data, the construction of the longitudinal officer panel, the measurement of assignments and rotations, and how these measures map into the empirical design.

C. Officers Rotations at Entry Grade

I combine multiple sources of administrative and personnel data to construct my main rotation dataset. Here, I briefly describe the main data sources. For the main variables used in the study to measure entry-grade rotation, location, tenure length, and rotation counts, I assemble three independent sources: annual entry lists, initial allocation lists, and curriculum-vitae sets. I then use external sources (quarterly seniority lists and gazette notifications) to resolve discrepancies. For details, see Appendix B.

TABLE 2: Descriptive Statistics: Entry-Grade Rotation Portfolio

	All	Control	Treated
Panel A: Mobility Intensity (Initial 5 Years)			
Average tenure per posting (years)	1.55	1.56	1.52
High diversity assignment (share)	0.52	0.56	0.35
Number of unique districts	2.53	2.62	2.17
Observations	1,071	848	223
Panel B: Geographic and HDI Exposure (Initial 5 Years)			
Share of domicile rotations	0.21	0.09	0.65
Share of non-domicile rotations	0.79	0.91	0.35
Average posting distance from home (km)	517.22	582.07	272.06
Share of Islamabad rotations	0.03	0.02	0.06
Share of low-HDI rotations	0.46	0.56	0.09
Share of medium-HDI rotations	0.47	0.54	0.22
Share of high-HDI rotations	0.40	0.31	0.72
Observations	1,071	848	223

Notes: District Human Development Index is followed from the UNDP 2017 Report. Distances are in kilometer. Sample, (1986 to 2020).

Source: Author's calculations.

Table 2 summarizes early-career rotation dynamics and geographic exposure during the first five years of service. Officers spend on average 1.55 years per posting, rotate across 2.5 unique districts, and about half of their early assignments are classified as high diversity (Panel A). Comparing treated and control officers, the main difference lies in where they rotate, not how often. Treated officers, who are eligible for domicile postings, rotate through 2.17 distinct districts compared to 2.62 for control officers, a gap of roughly 18 percent. Their early-career tenure per posting is similar, indicating no systematic difference in transfer frequency.

Panel B shows the composition of geographic exposure. Treated officers complete about 65 percent of their early-career rotations within the domicile province, compared to only 9 percent for controls. Consequently, their average posting distance from the district of birth is less than half that of the control group (272 km vs. 582 km). Exposure to district conditions also differs markedly: treated officers spend 72 percent of early-career time in districts with high Human Development Index scores and only 9 percent in districts with low Human Development Index scores, while control officers rotate more evenly across the Human Development Index spectrum.

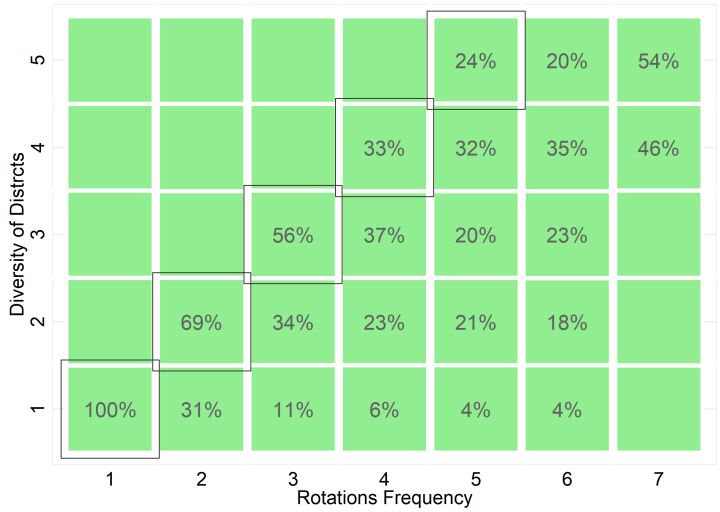


Figure 2: Entry grade Rotation Portfolio: Diversity and Frequency

Notes: Cells plot the share of officers in each diversity–frequency cell during entry grade. Columns are the number of rotations; rows are the diversity of distinct districts (higher = broader geographic exposure). Percentages are row-conditional and rounded to the nearest integer. Sample, (1986 to 2020).

At entry grade, officers' early rotations differ along multiple, quantifiable margins. Early assignments vary on two independent dimensions, frequency and diversity. As shown in Figure 2, cohorts occupy distinct positions in this space: some officers experience many moves within similar environments, while others rotate less frequently but across a wider range of administrative and socio-political contexts. Moving across columns in the figure captures rotation volume: officers at the right-hand side of a given row are transferred repeatedly but often within the same set of districts. Moving up the rows instead captures diversity: officers higher in the grid accumulate experience in a larger number of distinct districts, even if their total number of moves is modest. The figure therefore provides a descriptive map of what "volume" and "diversity" mean in practice and motivates treating them as separate margins of early-career geographic exposure.

D. Officers Appointments at Second Grade

After the first stage of service, promotions follow a standardized rule: nearly all officers advance to the second grade after a fixed number of years. The real selection margin comes not from promotion but from where officers are posted once they reach that grade. Some are appointed to frontline managerial (*FLM*) roles, high visibility district command positions with broad responsibility and stronger career returns, while others move into deputy or ministry positions. These are not separate tracks but assignments drawn from the same pool. To recover the organization's internal logic, I classify second grade appointments into three mutually exclusive

categories: frontline managers (*FLM*), deputy administrators, and ministry or secretariat officers. Nearly all officers pass through the ministry at some point; about 57 percent serve as deputies, and 45 percent hold at least one frontline executive post. This taxonomy captures the three key "destination types" in the bureaucracy, each with distinct responsibility and visibility, and allows me to estimate how differences in early exposure translate into selection into leadership later on.

For this part of the analysis, I restrict the sample to officers whose second grade careers are observed over a comparable window (appointments between 1986 and 2010). In this sample, roughly half of all officers who reach the second grade are ever appointed to an *FLM* post. Conditional on selection, the median *FLM* spell lasts about one year, and most officers spend fewer than two years in such posts across their entire careers. This pattern makes *FLM* appointments a scarce, high stakes assignment rather than a fixed career stream, and it provides a natural setting to study how early geographic rotations shape who eventually gets to lead.

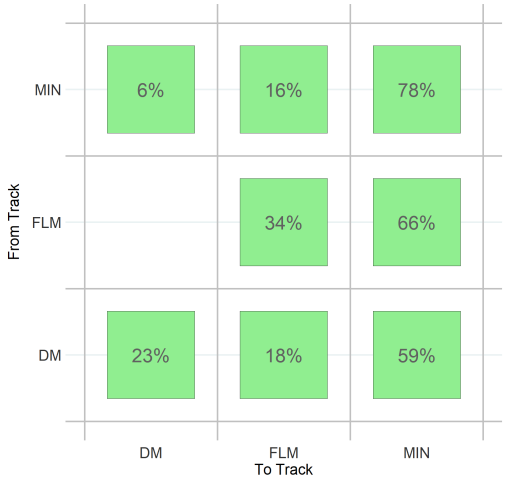


Figure 3: Internal Selection into Leadership Tracks (1986–2010)

Notes: The figure reports empirical one-step transition probabilities across three second grade assignment tracks: *FLM*, deputy managers, and ministry. Rows denote the origin track; columns denote the destination track. Cells are row-conditional probabilities; each row sums to one hundred percent. Percentages are rounded to the nearest integer.

Figure 3 summarizes mobility within the second grade of service, a career stage that typically spans close to a decade. Officers rarely stay in one stream throughout this period. Instead, rotations across frontline leadership, deputy, and ministry assignments are integral to the system's design.

Three patterns stand out. First, the ministry track anchors most careers: 78 percent of those already in ministry remain there in the next spell, and ministry is the modal destination from both frontline leadership (67 percent) and deputy posts (59 percent). Second, frontline leadership is

selective but short lived. Only 34 percent of those who reach an *FLM* position remain there in their next posting; most rotate back to ministry within the same grade. Third, deputy roles are similarly transitory: just 18 percent transition to frontline command, 23 percent stay as deputies, and the rest shift laterally into ministry.

Taken together, these transitions reveal a clear hierarchy of mobility. The ministry serves as the gravitational center of the bureaucracy, while frontline leadership, although prestigious, is temporary. The second grade therefore operates as a decade long carousel of lateral and hierarchical rotations, where selective leadership appointments are brief interludes within careers that are otherwise centered in the ministry.

E. Descriptive Patterns of Entry Grade Rotations

Next, I summarize the raw patterns behind the analysis. The data reveal a clear and internally consistent story. Figure C1 contrasts assignment patterns before and after the policy. Officers exempted from the non-domicile rule (treated) experience fewer rotations across distinct locations and less geographic diversity than those still subject to it (control), and this gap widens after the policy. The total number of postings in the entry grade remains flat across cohorts, but the number of distinct districts served declines by 0.43, an 18 percent drop relative to the pre-policy mean. Officers rotate as often as before, but within fewer unique environments. Spatial data in Figure C2 show that the average distance traveled between postings for treated officers falls from roughly 600 km to under 300 km, indicating a strong localization of assignments around home and neighboring provinces. By contrast, posting length remains stable: Figure C3 shows mean tenure per posting at about 18 months, unchanged across groups and periods. Hence differences in cumulative exposure arise from where officers move, not from how long they remain in post.

Figure C4 visualizes the joint distribution of rotation volume and diversity. Among treated cohorts, the cloud of points contracts downward along the diversity axis while remaining horizontally stable: officers experience fewer distinct contexts for a given number of moves. Controls, by contrast, show no comparable compression. Finally, the Early Exposure Index (*EEI*) in Figure C5 aggregates these dimensions, namely district diversity, distance, and the mix of field districts, into a standardized score. The *EEI* drops sharply for treated cohorts, confirming a broad contraction in the scope of formative experience rather than in effort or time spent in post.

III. Empirical Specification

The baseline specification estimates the impact of the rotation policy on officers' geographic exposure and subsequent leadership outcomes. Identification exploits the interaction of policy timing, treated status, and province: officers entering after the policy faced distinct early-career

mobility rules depending on whether they were subject to domicile or non-domicile allocation. I estimate a two-way fixed effects regression with province p and cohort g between 1986-2017, where treated is defined by eligibility for domicile allocation under the policy. The specification takes the following form:

$$y_{igp} = \alpha + \beta\big(\text{Treat}_i \times \text{Post}_g\big) + \eta\,\text{Treat}_i + \gamma_p + \tau_g + X_i'\delta + \varepsilon_{igp}. \tag{1}$$

Here, y_{igp} is the outcome of interest for officer i from entry cohort g and province p. The variable Treat_i equals one for officers exempted from the non-domicile rule, and Post_g equals one for cohorts entering after the policy. The interaction term $Treat_i \times \text{Post}_g$ thus isolates the differential change in outcomes for treated officers in post-policy cohorts relative to untreated officers from the same province and cohort. Province fixed effects (γ_p) absorb time-invariant differences across provincial cadres such as average district size, development levels, and internal rotation cultures that might otherwise confound policy exposure. Cohort fixed effects (τ_g) capture shocks common to all junior officers in a given year, such as changes in recruitment competitiveness, macroeconomic conditions, or administrative reforms. Notably, the main effect of Post is absorbed by the cohort fixed effects. This combination ensures that the coefficient β is identified from variation across treated and control officers within province–cohort cells, before and after the policy. X_i includes pre-determined officer characteristics, entry exam scores, training results, education, and quota status, measured before the first posting. These controls absorb differences in observable quality or background that might correlate with both treated and later assignments. Standard errors are clustered at the district of first posting with robustness checks allowing two-way clustering by cohort and district of first posting, and cohort level to account for serial correlation in assignment environments. The coefficient β represents the causal effect of the domicile-based flexibility rule on early rotation patterns and on selection into leadership positions. A negative estimate of β indicates that the policy reduced officers' exposure or advancement relative to control officers within the same administrative setting.

The dependent variable varies by analysis stage. At the entry grade, outcomes include (i) the probability of an out-of-province posting and (ii) the number of distinct districts served (rotation diversity). At the second grade, the dependent variable is an indicator for appointment to a frontline managerial (*FLM*) role. Where noted, I standardize outcomes to remove compositional differences.

This full set of fixed effects and covariates addresses the main identification concerns.Province and cohort effects jointly control for any region-specific or national changes in entry standards or administrative policy. Because assignments at entry are centrally determined, the remaining variation in $Treat_i \times \text{Post}_g$ stems solely from differences in rotation rules, not from endogenous

officer appointment. Officer-level covariates X_i further adjust for observable heterogeneity at entry, including exam scores, education, and quota status. Because these characteristics are measured prior to the first posting, they help isolate the effect of the policy from compositional shifts in officer quality. This design thus isolates the policy's effect from both demand-side allocation shocks (changes in province-level needs) and supply-side selection (officer preferences), ensuring that the identifying variation reflects only the exogenous change in the rotation regime.

The coefficient on $Treat_i \times \text{Post}_g$ is a difference-in-differences estimate of the impact of the rotation policy on assignment outcomes. Intuitively, β measures how much the early-career trajectories of officers allowed to remain in their home province diverged from those of officers required to serve outside their province after the policy change. Consider, for example, a province in which both types of officers enter the service. Before the policy, all junior officers were subject to the same non-domicile rotation rule; afterward, only treated officers were exempted and could begin their careers in their home provinces. The coefficient β can therefore be interpreted as the weighted average of (i) the change in early geographic exposure for treated officers relative to their pre-policy counterparts, and (ii) the corresponding change for control officers whose assignment regime remained unchanged.

To examine the dynamics of the policy's effects and test for differential pre-trends, I complement the baseline difference-in-differences with an event-study specification:

$$y_{igp} = \alpha + \sum_{k \neq -1} \beta_k \left(\text{Treat}_i \times \mathbf{1}\{g - g^* = k\} \right) + \eta \, \text{Treat}_i + \gamma_p + \tau_g + X_i' \delta + \varepsilon_{igp}. \tag{2}$$

Here, k indexes the number of entry cohorts relative to the first treated cohort g^*, with $k = -1$ omitted as the reference period. Each coefficient β_k traces the treated–control difference k cohorts before or after the policy change, conditional on province and cohort fixed effects. Pre-policy coefficients ($k < 0$) provide a diagnostic for parallel trends: they indicate whether treated and control officers were on comparable trajectories prior to the change in the rotation rule. Post-policy coefficients ($k \geq 0$) capture the evolution of outcomes as successive cohorts entered under the new assignment regime, revealing both the immediate adjustment in early-career exposure and any persistence into later stages of service.

This formulation serves two purposes. First, it provides a clean visualization of how the policy reshaped the geography of early postings over time, beyond the average effect estimated in the baseline model. Second, it allows for dynamic treated responses that unfold gradually as new cohorts adapt to the revised assignment incentives. In keeping with the identifying logic of the baseline specification, province and cohort fixed effects absorb spatial and temporal shocks.

Identification and robustness. The key identification challenge is that treated status, the exemption from non-domicile allocation was determined by policy timing rather than individual randomization. Post-policy junior officers may differ in composition, or assignment environments could evolve differentially across provinces. Three features of the design, supported by empirical checks, mitigate these concerns and lend credibility to the estimates.

(i) Composition. The first concern is that the policy may have altered who entered the service. In practice, the data show no systematic change in entry composition between treated and control cohorts. Average exam performance, interview scores, education levels, and degree fields remain statistically indistinguishable across groups within province–cohort cells. Cohort size is fixed by design, so the increase in treated junior officers represents a one-for-one substitution rather than an expansion in total seats. Balance tests in narrow cohort windows around the policy confirm stable entry quality and background characteristics, ruling out selection-on-quality as a confounding channel.

(ii) Institutional and regional shocks. The inclusion of province-by-cohort fixed effects ensures that identification comes solely from differential changes between treated and control officers who entered in the same year and were allocated to the same initial province. This specification absorbs contemporaneous shocks—such as local political transitions, recruitment changes, or administrative reforms—that might otherwise bias estimates. The resulting coefficient on treated thus captures the causal impact of the rotation rule within comparable province–cohort environments.

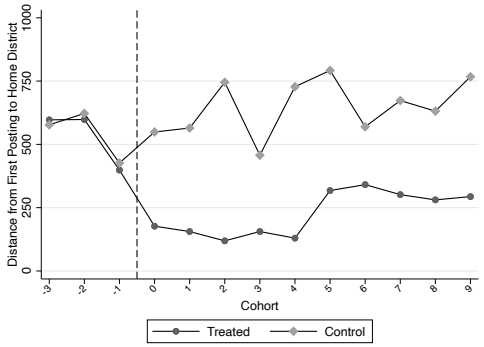
(iii) Dynamics and parallel trends. The event-study specification further validates the identifying assumption. There are no pre trends, indicating that treated and control cohorts followed parallel trajectories prior to the change. After the policy, sharp and sustained divergence appears in two key dimensions: early-career geographic diversity (inter-province exposure and distinct districts served) and the probability of attaining frontline managerial roles in the second grade. These dynamic effects are consistent with the causal interpretation of β as the impact of the rotation policy, rather than the result of contemporaneous institutional or compositional changes.

Taken together, these checks reinforce the core design logic: entry allocation rules are centrally fixed, treated assignment is determined solely by policy timing, and promotions are administered mechanically. The remaining variation in outcomes therefore isolates the causal impact of the rotation policy on the accumulation of managerial capacity, rather than reflecting endogenous appointment or individual preferences.

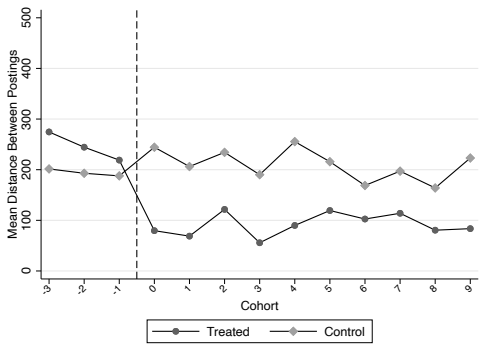
Balance tables in Table A1 confirm that exam performance and baseline demographics remain stable across cohorts, indicating that the sharp change in allocation shares was not accompanied by a shift in the underlying abilities of officers.

IV. Results: Early-Career Exposure and Leadership

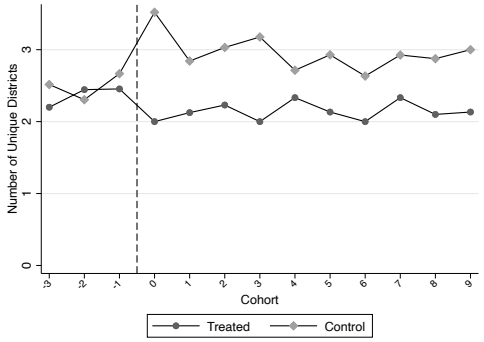
I begin with a graphical description of (unconditional) posting patterns across cohorts around the policy. Officers are divided into two groups based on their exposure to the change: those eligible for domicile allocations under the new rule constitute the treated group, and those whose allocation rule remained unchanged form the control group. The vertical axis in each panel measures a distinct dimension of early-career geographic exposure, and the horizontal axis traces cohorts relative to the first affected one (denoted by the dashed vertical line).



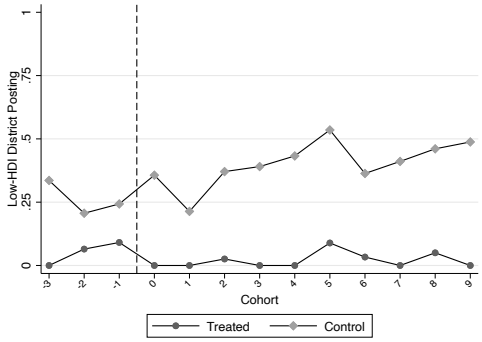
Panel A: First Posting City Mean Distance from Home City (km)



Panel B: Mean Distance between Rotations, first five years (km)



Panel C: Number of Unique Districts Served, in first five years



Panel D: Share of Rotations in Low-HDI Districts, first five years

Figure 4: The Policy Alters Generalist Capital Formation in Entry Grade

Notes: Raw cohort means in entry grade by treated status; dashed line is the first affected cohort. Distances are great-circle kilometers. Sample: cohorts of 1986-2020 entry.

Panel A shows that the mean distance from home to the first posting district drops sharply and persistently for the treated group—by roughly 50 percent—precisely at the policy threshold, while the control group displays no corresponding change. Panel B reveals an analogous pattern in mean distance per rotation: treated officers' average move length declines substantially, whereas

the control group's rotation distances remain flat. These two panels together indicate that the policy substantially reduced early-career geographic mobility for treated officers.

Panel C depicts the number of unique districts served during the entry grade. Following the policy, the treated group experiences a discrete and sustained decline in district diversity, while the control group continues along a stable trajectory. Panel D shows a similar divergence in the share of rotations in low-HDI districts: treated officers see a marked drop in exposure to low-HDI areas, whereas control officers' exposure remains unchanged.

I highlight three patterns in these plots, as they are strongly suggestive of a causal relationship between domicile flexibility and early-career geographic exposure. First, cohorts affected by the policy—those newly eligible for domicile allocations exhibit an immediate and sustained reduction in both the average distance from home and the mean distance per rotation. In contrast, control cohorts that remain under the prior non-domicile rule show no corresponding change in these outcomes. Second, exposure margins that the policy does not directly affect, such as rotation intensity among control officers, remain flat before and after the policy, implying that my estimates are not confounded by generic cohort turnover or "new batch" effects. Third, there are no discernible pre-policy trends in any of the four panels, indicating that treated and control groups were on parallel trajectories before the change in assignment rules.

The patterns observed in Figure 4 provide strong evidence that the policy which allowed domicile allocations in the entry grade, generated a discrete decline in early-career geographic mobility for the treated group. These patterns validate the identification assumptions underlying the difference-in-differences estimator of the policy's effect on generalist capital formation in specification (1).

A. Rotation and Leadership

In Table 3, I estimate the effect of the policy on early-career appointments using specification (1) with cohort and province fixed effects and standard entry controls. Outcomes are measured at the officer level: column 1 reports frontline managerial (*FLM*) attainment, column 3 Deputy District Executive (*DDE*) attainment, and column 5 the share of time in secretariat/desk roles during the entry grade. In all cases, the *Treat* × *Post* interaction isolates the policy's differential impact on the treated. I find a negative and significant effect of the policy on frontline managerial attainment: the probability that a treated officer attains an *FLM* post falls by about 33 percentage points. By contrast, time in desk roles increases by 12–15 percentage points, and DDE attainment does not change following the policy. Adding controls (even-numbered columns) leaves these magnitudes essentially unchanged.See Figure A3.

TABLE 3: Frontline, Deputy and Ministry Appointments Share in Second Grade

Dependent variable	*FLM* Appointment		*DM* Appointment		Ministry Appointment Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.000	-0.011	0.046	0.102	0.028	-0.004
	(0.080)	(0.085)	(0.094)	(0.099)	(0.051)	(0.053)
Treat × *Post*	-0.331***	-0.331***	0.040	-0.013	0.117**	0.150**
	(0.090)	(0.095)	(0.100)	(0.103)	(0.059)	(0.061)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Mean DV	.398	.398	.5651	.565	.638	.638
Observations	968	968	968	968	968	968
R^2	.221	.253	.094	.124	.142	.175

Notes: Columns (1)–(2) report attainment of frontline managerial positions (*FLM*), columns (3)–(4) report attainment of Deputy Manager (DM), and columns (5)–(6) report the share of time spent in ministry appointments. Each specification reports OLS estimates of the interaction term *Treat* × *Post*, controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and standard controls (entry-exam scores and demographics) where indicated. The post policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post-Reform* identifies the differential post-reform change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district; robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure B1 provides descriptive context for interpreting the main results by comparing posting distances across Deputy Manager (DM) and Frontline Manager (*FLM*) roles. Both positions are district-level assignments that involve substantial travel and geographic dispersion, making them directly comparable in terms of field exposure and distance intensity.

The figures show that average posting distances are large for both roles, underscoring their similarly demanding geographic scope. Distances decline after the policy for both DM and *FLM* appointments, but the relative gap between the two roles remains essentially unchanged. The shaded trends in Figure B1 and the mean differences with confidence intervals in Figure B2 illustrate that while overall posting distances contract over time, both roles continue to require substantial inter-district mobility.

In Table A4, I examine the intensive margin of early-career exposure using specification (1) with cohort and province fixed effects. The policy leads treated officers to spend about 11–12 percentage points more time in non-field posts and 10–12 percentage points less time in field service, while the share of assignments in higher-capacity districts rises by 15–17 percentage points. These magnitudes are stable when controls are added, indicating a reallocation of work intensity away from field exposure toward administrative and high-capacity settings

TABLE 4: *FLM* with Seat Adjusted Specification in Second Grade

Dependent variable	*FLM* Appointment						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat	0.000	-0.011	0.018	-0.010	-0.067	-0.011	-0.020
	(0.080)	(0.085)	(0.086)	(0.085)	(0.089)	(0.080)	(0.083)
Treat × *Post*	-0.331***	-0.331***	-0.358***	-0.325***	-0.561***	-0.331***	-0.420***
	(0.090)	(0.095)	(0.101)	(0.094)	(0.122)	(0.093)	(0.103)
Cohort FE	✓	✓		✓	✓	✓	✓
Province FE	✓	✓		✓	✓	✓	✓
Standard controls		✓	✓	✓	✓	✓	✓
Province × *Cohort* FE			✓				
Seat-adjusted (capacity)				✓			
Linear Gender Trends					✓		
Mean DV	.398	.398	.398	.341	.398	.398	.449
Observations	968	968	950	968	968	968	665
R^2	0.221	0.253	0.303	0.214	0.258	0.253	0.218

Notes: The dependent variable is an indicator for *FLM* attainment. Each column reports OLS estimates of the interaction term *Treat* × *Post*, controlling for a treated indicator (*Treat*) and the fixed effects listed. Column (1) is the baseline; column (2) adds standard controls including entry-exam scores and demographics; column (3) includes *Province* × *Cohort* fixed effects; column (4) uses a seat-adjusted outcome; column (5) adds linear treated-specific trends; column (6) is as in column (2) but clusters standard errors by *Cohort* × *Province*; and column (7) restricts the sample to cohorts ≤ 2010 to ensure a balanced follow-up window. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The sample consists of officers who achieved second grade in cohorts 1986–2017. "Mean DV" reports the mean of the dependent variable. Unless otherwise noted, standard errors are clustered by first-posting district; robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table 4, I present the effect of the policy on frontline managerial (*FLM*) appointments using specification (1) with cohort and province fixed effects. The dependent variable is an indicator for *FLM* at second grade, and columns vary specification and sample to probe robustness. Across all columns, treated officers in post-policy cohorts are markedly less likely than the control in the same allocation–*Province* × *Cohort* cells to obtain the frontline seat. The baseline in column (1) shows a decline of about 33 percentage points relative to a pre-policy mean near 40 percent. Adding controls (col. 2) leaves the estimate essentially unchanged. Saturating with allocation-*Province* × *Cohort* fixed effects (col. 3), scaling by local seat capacity (col. 4), allowing group-specific linear trends (col. 5), clustering at *Cohort* × *Province* (col. 6), and restricting to early cohorts for balanced follow-up (col. 7) all deliver very similar magnitudes.

Robustness.—I probe four concerns, and the findings hold. If the pattern reflected entry

composition rather than policy exposure, we would observe shifts in pre-determined traits around the policy. We do not: exam grades and education profiles remain flat across cohorts, indicating that cohort quality and selection are stable. Broader institutional changes during the 2000s—such as training expansions, record digitization, or quota adjustments—also cannot explain the result: absorbing *Cohort* × *Province* shocks and adding rich officer covariates leaves the core coefficients virtually unchanged. Nor is this a general slowdown in advancement: there is no comparable decline outside frontline (*FLM*) roles, and tenure length and posting distances within other tracks remain similar. Finally, the estimates are stable when excluding provinces with atypical assignment practices, dropping cohorts one at a time, or restricting the sample to a narrow policy window. Across a wide range of specifications, samples, outcome definitions, and estimation strategies, the results consistently indicate that the decline in *FLM* appointment is not an artifact of sample composition, outcome construction, or estimation choice. Table R1 tests whether the results are driven by any single province. Columns sequentially exclude Balochistan (a small, institutionally distinct province), other smaller provinces (AJK, FATA, KPK), and the two largest provinces (Punjab and Sindh). Across all specifications, the coefficient on *Treat* × *Post* remains negative, statistically significant, and similar in magnitude to the baseline estimate. This suggests that the estimated effect is not driven by any one region's institutional dynamics or scale. A natural concern is that changes in the composition of entering cohorts might mechanically generate the observed decline. Table R2 includes the treated share of each cohort–province cell as a control. The interaction coefficient remains large and significant across specifications, indicating that compositional shifts do not explain the result. Table R3 replaces the standardized outcome with a binary indicator for ever attaining an *FLM* position. The *Treat* × *Post* coefficient remains negative and significant in both the linear probability model and nonlinear specifications (logit and probit). Table R4 further adjusts for variation in seat supply by subtracting expected appointment based on cohort–province seat availability. The results remain unchanged, confirming that the decline in appointment probability is not explained by changing vacancy patterns. Table R5 verifies that the findings are not cohort-specific: sequentially excluding one entry cohort at a time yields highly stable estimates. Finally, Table R6 re-estimates the main difference-in-differences specification using three alternative dependent variables—a binary appointment indicator, a seat-adjusted appointment measure, and the baseline standardized index. In all cases, the *Treat* × *Post* interaction remains negative, statistically significant, and of similar magnitude. Together, these robustness exercises provide strong support for the validity of the main result: the policy significantly reduced the likelihood that domicile-allocated officers advanced into frontline managerial roles. The effect persists when removing influential provinces, controlling for cohort composition, redefining outcomes, adjusting for seat supply, and excluding specific cohorts. This convergence of evidence across specifications underscores the robustness of the main finding.

V. Exploring Mechanisms

The results above provide robust evidence that policy impacted the careers of treated officers. Thus creating multiple channels that could affect leadership outcomes. I now investigate these potential underlying mechanisms. The following analysis isolates these channels to identify where in the career pipeline the divergence originates. I address this concern on three fronts. First, I show that treated and control officers are comparable on observable characteristics—exam performance, demographics, age, and prior experience indicating that the policy did not alter the composition of junior officers. Second, I examine other margins of career progression beyond geographic rotation, time to promotion, performance evaluations on assignments longer than 12 months, and appointment to other district-level operational (*FLO*) posts and show that these dimensions evolve similarly for both groups.

A. Change in Officer's Quality

I begin by examining whether the policy changed who entered the civil service. This test addresses the possibility of extensive-margin selection that later differences in career outcomes might reflect shifts in entrant quality or background rather than changes in assignment design. Using the same difference-in-differences specification as in Equation (1), I regress entry characteristics including STEM background, age at joining, and provincial origin—on the interaction term *Treat* × *Post*. Table 5 reports the results. Across all specifications, the *Treat* × *Post* coefficients are small and statistically indistinguishable from zero. The share of junior officers with STEM degrees, their average age at entry, and the fraction originating from Sindh remain stable across cohorts. Point estimates range from 0.02 to 0.05, quantitatively negligible relative to pre-policy means. Columns (3)–(6) further reinforce this conclusion: the estimates remain close to zero when examining age at joining and provincial origin, confirming that the policy did not alter the observable composition of new officers. These results indicate that entry quality and demographics are stable across the policy, ruling out selection at entry as an explanation for subsequent differences in assignment outcomes. The absence of compositional change also implies that neither the supply of applicants nor the recruitment criteria of the state shifted materially. The policy therefore did not alter the equilibrium matching between candidates and available seats. The adjustment instead occurs post-entry, through the allocation of assignments conditional on entry.

TABLE 5: Policy Effect on Entry Composition

Dependent variable	STEM Degree		Age of Joining		From Sindh	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	-0.119	-0.012	-0.003	-0.002	-0.017	0.032
	(0.093)	(0.087)	(0.003)	(0.003)	(0.065)	(0.038)
Treat × *Post*	0.047	0.024	0.003	0.003	-0.058	-0.036
	(0.109)	(0.100)	(0.003)	(0.003)	(0.080)	(0.040)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓		
Controls		✓		✓		✓
Mean DV	.549	.549	.002	.002	.185	.185
Observations	968	968	968	968	968	968
R^2	.053	.294	.006	.016	.014	.958

Notes: The dependent variables are: columns (1)–(2) an indicator for holding a STEM degree, columns (3)–(4) an indicator for age at joining ≥30, and columns (5)–(6) an indicator for being born in Sindh. Columns (2), (4), and (6) additionally include baseline controls including entry-exam scores, training scores, education, and age at entry. Each column reports OLS estimates of the effects of *Treat*, *Post*, and their interaction (*Treat* × *Post*) on entry grade characteristics. All specifications include cohort fixed effects; province fixed effects are included where possible but omitted in columns (5)–(6) due to collinearity with the Sindh indicator. Each cell reports the estimated coefficient with standard errors in parentheses, clustered by first-posting district. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B. Change in Work Load and Performance

The next step is to test whether the policy affected observable performance or field load in the entry grade. If officers who stayed closer to home faced lighter tasks or slower advancement, differences in frontline appointments could reflect weaker early signals rather than narrower exposure. I estimate the same difference-in-differences specification as in Equation (1), using three outcomes: the number of field postings, average evaluation score, and time to promotion. All regressions include cohort and province fixed effects and baseline controls for entry-exam scores, education, and age. The evidence points to stable early-career conditions. Officers in treated and control cohorts face similar workloads, receive comparable evaluations, and progress through the promotion ladder at the same pace. The policy therefore did not operate through lighter field duties, weaker appraisal standards, or slower career advancement.

Direct performance measurement is limited: the only systematic metric is the formal evaluation

record, observed mainly in the entry grade and heavily top-coded ("Very Good/Excellent"), leaving little cross- or within-officer variation. I therefore use these evaluations narrowly to verify that treated and control officers are comparable on observed performance at entry while relying on assignment measures to capture the meaningful variation in early exposure.

Table 6 reports officer-level regressions with cohort and province fixed effects (baseline controls in the even columns). Columns (3)–(4) show no change in average evaluations across cohorts—the *Treat* × *Post* estimates are small and imprecise. Columns (5)–(6) likewise show no systematic change in time-in-grade prior to second grade. Columns (1)–(2) report modest, imprecise negatives in frontline/district exposure. Read together, the near-zero effects on evaluations and grade length falsify performance/effort explanations.

TABLE 6: Policy Effect on District level Assignment Intensity and Performance

Dependent variable	Number Field Postings		Average Evaluation		Time to Promotion	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	-0.567**	-0.529**	-0.010	-0.011	1.672	1.998
	(0.227)	(0.215)	(0.044)	(0.046)	(2.988)	(3.407)
Treat × *Post*	0.228	0.218	-0.013	-0.019	-0.654	-0.624
	(0.230)	(0.223)	(0.049)	(0.049)	(3.189)	(3.424)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Mean DV	3.303	3.303	1.432	1.432	69.642	69.642
Observations	968	968	968	968	968	968
R^2	.109	.122	.144	.158	.246	.256

Notes: The dependent variables are: columns (1)–(2) the number of field postings during entry grade, columns (3)–(4) average evaluation coded 2 for "outstanding", 1 for "very good", and 0 otherwise, and columns (5)–(6) time to promotion (months) during entry grade. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*) on these entry grade posting characteristics. Columns (2), (4), and (6) additionally include baseline controls (entry-exam scores, training scores, education, and age at entry). All specifications include cohort and province fixed effects. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In sum, the evidence, including no detectable shifts in entry composition and stable early-career workload, evaluations, and promotion timing, suggests that selection at entry and early-performance channels are unlikely to be the primary margins of adjustment. While some

compositional changes are mechanically inevitable because the size of the relevant subset of entrants increases, these shifts do not appear systematic along observed dimensions. I cannot observe informal references or other unrecorded reputational signals at entry, and underlying preferences may in principle be a concern, but the overall posting rule does not change over this period. Together with the stability and precision of the estimated coefficients around zero on formal observables, this allows me to rule out even modest changes along these dimensions as first-order drivers of the results.

C. Generalist Appointments Quality

Among officers who reach a frontline managerial post, treated and control groups are indistinguishable across every observable dimension of career quality and deployment. They are promoted at the same rate, serve comparable durations in second grade, and spend nearly identical time in post. Their assigned districts are also statistically equivalent in quality: they govern regions with similar economic development, administrative complexity, and population size, including both high-pressure urban centers and peripheral or hardship districts. This invariance holds across a range of specifications, whether outcomes are measured in months, tenure shares, or district-level characteristics. These results serve as a strong falsification test for alternative explanations of the leadership gap observed in Table 7.

First, these finding rule out negative selection. If treated officers who advance to leadership were weaker along unobservable dimensions such as motivation, competence, or political connectivity we would expect to observe differences in assignment quality, performance ratings, or duration once they reached command. The equivalence in post characteristics and tenure length indicates that the subset of treated officers who attain leadership is not compositionally different from their control counterparts. Second, they rule out performance based attrition. Treated officers are not removed from frontline posts earlier, reassigned more frequently, or shifted into lower-profile districts at higher rates. The distribution of tenure lengths and reappointment probabilities is identical across groups. This suggests that the policy's effect does not operate through diminished on-the-job performance or weaker retention once officers are promoted. Third, they rule out preferential or protective assignment. Treated officers are not systematically placed in safer, less politically exposed, or less operationally demanding districts. On average, they are equally likely to manage high-stakes postings such as provincial capitals or districts with complex administrative portfolios. This eliminates the possibility that observed differences in leadership attainment reflect compensatory behavior by the organization that is, shielding certain officers from demanding environments rather than restricting their access to leadership positions. Together, these findings imply that conditional on attaining a leadership role, treated and control officers are

managed, evaluated, and deployed equivalently. The organization does not differentiate between them once they cross the promotion threshold. Consequently, the observed divergence in leadership attainment must originate before the first promotion in the process that determines who gets prepared for leadership, not how those leaders are subsequently treated.

Economically, this points to a clear mechanism: the policy did not alter incentives or management of officers once they reached senior positions; instead, it changed the distribution of formative experience specifically, early exposure to diverse administrative contexts that determines who becomes ready for frontline command. The gap in leadership outcomes therefore reflects differences in the production of managerial capital, not differences in how the organization allocates or evaluates leaders ex post. The results, visualized in Figure B3, provide a complementary falsification test: early-career exposure determines selection into frontline roles, but conditional on appointment, treated and control officers display similar managerial intensity and quality.

TABLE 7: Conditional on Leadership, Assignment Intensity and Quality

Dependent variable	Log Experience before becoming *FLM*	Log *FLM* Tenure Duration	Share of Time as *FLM*	Number of *FLM* postings	High-HDI Posting city as *FLM*
	(1)	(2)	(3)	(4)	(5)
Treat	-0.175*	-0.334	-0.027	-0.407***	0.007
	(0.092)	(0.267)	(0.061)	(0.104)	(0.124)
Treat × *Post*	-0.017	-0.063	-0.065	-0.335*	-0.147
	(0.128)	(0.314)	(0.070)	(0.178)	(0.157)
Cohort FE	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Mean DV	4.579	3.142	.336	1.584	.186
Observations	387	387	387	387	387
R^2	0.392	0.110	0.307	0.208	0.162

Notes: The dependent variables are measures of *FLM* assignment characteristics conditional on attaining an *FLM* position. Column (1) examines the logarithm of experience before becoming *FLM* (months). Column (2) examines the logarithm of *FLM* tenure duration (months). Column (3) examines the share of time spent as *FLM* during second grade. Column (4) examines the number of *FLM* postings. Column (5) examines an indicator for being posted in a high-HDI city as *FLM*. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and standard controls (entry-exam scores, training scores, education, and age at entry). The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Among officers who reach frontline leadership, we see parallel tenure, identical time-in-post, and assignments to districts of comparable pressure and development; all differences are small and precisely estimated around zero. This triple null falsifies (i) negative selection among promoted treated officers, (ii) performance-based churn once in charge, and (iii) cushioned or easier postings for treated leaders. Hence the margin of adjustment is pre-promotion: the policy altered the formation of managerial capital.

VI. District Diversity in First Five Years

For many decades, Pakistan's elite civil service has followed a formal rotation policy requiring new junior officers to begin their careers outside their province of domicile. The rule's intent is pedagogical rather than punitive: by placing officers in unfamiliar administrative and political contexts, the state seeks to build a generalist corps capable of managing heterogeneous environments across the federation. Early exposure to diverse institutional settings, in districts that vary in political alignment, resource endowments, and social composition, is viewed as essential training for higher managerial responsibility.

High-frequency movers who stayed within similar contexts often became quick at executing established routines but acquired little new knowledge with each transfer. By contrast, officers who had fewer but more distinct assignments built a broader repertoire of problem-solving skills: they learned to work with new political actors, interpret unfamiliar local constraints, and adapt policy execution to different institutional environments. Over time, this diversity margin became central to the production of generalist managerial capability within the cadre.

A. District Diversity and Quality in Entry Grade

Early assignments translate into very different exposure depending on where officers rotate. Two margins matter: the number of unique districts an officer serves (distinct political–administrative contexts) and the frequency of moves (how often they are transferred). In the raw data, variation in unique districts strongly tracks subsequent frontline-manager appointments, while variation in frequency does not. For example, Figure C6 links these early differences to later outcomes: officers whose portfolios span a wider range of districts are substantially more likely to reach frontline managerial posts. The relationship is steep and monotonic: variety, not volume, predicts advancement in district intensive roles. Motivated by this pattern, I isolate these margins in the empirical design treating unique districts as the formative exposure channel and test whether changes in early assignment practice that reduce unique-district exposure weaken the pipeline into frontline managerial roles.

TABLE 8: Rotation Diversity and Volume in Initial Five Years

Dependent variable	Number of Unique Districts		Number of Postings	
	(1)	(2)	(3)	(4)
Treat	-0.139	-0.169	0.044	0.082
	(0.173)	(0.175)	(0.174)	(0.173)
Treat × *Post*	-0.412*	-0.430**	-0.200	-0.208
	(0.209)	(0.205)	(0.182)	(0.184)
Cohort FE	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
Controls		✓		✓
Sample: Entry grade (first 5 years)	✓	✓	✓	✓
Mean DV	2.62	2.62	3.567	3.567
Observations	968	968	968	968
R^2	0.097	0.136	0.107	0.120

Notes: Columns (1)–(2) measure the number of unique districts in which an officer serves, while columns (3)–(4) measure the total number of postings completed in the first five years. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*) on these entry grade posting characteristics. Columns (2) and (4) additionally include baseline controls (entry-exam scores, training scores, education, and age at entry). All specifications include cohort and province fixed effects. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district; robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8 shows how the policy reshaped early-career mobility portfolios in the entry grade. Column 1 uses the count of unique districts served; Column 3 uses the number of postings. All specifications include cohort and province fixed effects with baseline controls. Two main results follow; First, the policy substantially reduces district diversity: the *Treat* × *Post* coefficient is about −0.43 districts off a mean of 2.62, a decline of 17%. Second, there is no corresponding change in the number of postings; coefficients of about −0.20 are small in magnitude and imprecisely estimated. The results identify a margin of adjustment: postings become more geographically concentrated rather than more or less frequent. Early careers span fewer distinct districts, with no systematic change in rotation intensity. This pattern is consistent with one of the mechanisms through which the policy weakens *FLM* attainment. By reducing contextual breadth, a key jack-of-all-trades input, the assignment system is less likely to produce officers with the broad experience profile associated with frontline leadership. To complement the district-count evidence, Table A6 examines the geographic radius of initial assignments. I compare first-posting distance between officers allocated to non-domicile provinces and those who remain in their

province of domicile. Each officer's first-posting distance is benchmarked using their district of birth as a proxy for origin. Post-policy treated officers are posted substantially closer to their district of birth (−1.63 log km, $p = 0.00$), an effect that corresponds to an 80 percent reduction in distance relative to the pre-policy mean. Results are similar when I proxy domicile with birth district where domicile is missing. These estimates quantify the magnitude of the shift in geographic allocation between domicile and non-domicile officers at entry and show that the decline in district diversity reflects a sharp contraction in the radius of early careers rather than a simple change in province labels. Consistent with this, post-policy treated officers also travel substantially shorter distances across postings in the initial five years (−1.74 log km, $p = 0.02$), an effect corresponding to an 84 percent reduction in mean distance moved relative to the pre-policy mean. I then examine whether district quality is a relevant margin of adjustment. In Table A5, I split districts into terciles of the HDI distribution and computing the share of the first five years spent in each. The Treat coefficients show that, even before the policy, treated officers were already tilted toward better-resourced districts and away from harder districts relative to controls. The *Treat* × *Post* coefficients indicate that the policy slightly amplifies this pre existing difference but does not generate a new reallocation: treated officers are not systematically removed from hard districts, nor are they disproportionately steered into high visibility districts compared with controls. Economically, the policy does not introduce a new reward or penalty for difficult or high visibility posts across groups; rather, the main margin of adjustment remains how many distinct districts officers pass through, making district quality an unlikely central channel for the decline in frontline managerial appointments.

I next plot cohort-based event studies for the variables discussed above around the policy. The horizontal axis measures event time in entry cohorts relative to the cutoff ($t = 0$ is the first post-policy cohort; $t = -1$ the last pre-policy cohort). The vertical axis reports cohort averages with 95% confidence bands for: (i) distance from home province, (ii) rotation diversity (distinct districts), (iii) total kilometers rotated, (iv) the share of postings across development contexts, and (v) tenure. The *Treated* series (officers whose mobility rule changes at $t = 0$) is plotted against a *Control* series that remains under the status-quo rule. I use a symmetric window (e.g., $[-3, +9]$); tails are truncated where cohorts have not fully reached second grade. At the cutoff, the treated exhibit discrete shifts in geographic assignment, distance from home falls, rotation diversity declines, and postings tilt away from lower-development contexts while the control shows no comparable breaks within the same province–cohort cells. Outcomes mechanically orthogonal to mobility, and all control series, display no discontinuity, ruling out generic cohort/calendar effects. Pre-policy trajectories for treated and control are flat and parallel within the window. Taken together, these patterns validate the *Cohort* × *treated* difference-in-differences design and support interpreting the post-policy divergence as the causal effect of the mobility rule on

early-career exposure and downstream assignments (see Figure A2).

B. Early Exposure Index in Entry Grade

A single metric risks confounding distinct channels through which early-career assignments may operate. The policy could, in principle, shift (i) the number of unique administrative contexts an officer encounters, (ii) the geographic radius of rotations, (iii) the developmental or institutional quality of districts, or (iv) rotation intensity itself. I therefore use two complementary measures: a High Diversity indicator (equal to 1 if an officer serves in at least two unique districts in the entry grade) and a continuous Early Exposure Index (*EEI*) that equally averages (i) the count of unique districts, (ii) composite distance between rotations, and (iii) heterogeneity of district development across postings, all measured over the initial five years in the entry grade. The first measure is assumption-light; the second aggregates richer variation and guards against threshold artifacts. Across specifications controlling for cohort and province-of-allocation fixed effects and conditioning on baseline demographics and training scores, both measures deliver the same result. The regression results show that this cross-context margin is an operative mechanism (Table 9).

TABLE 9: Effect of Policy on Generalist Capital — Multiple Measures of Rotation Diversity

Dependent variable	High Diversity		*Early Exposure Index (EEI)*	
	(1)	(2)	(3)	(4)
Treat	-0.001	-0.022	-0.307	-0.332
	(0.076)	(0.077)	(0.219)	(0.221)
Treat × *Post*	-0.290***	-0.286***	-0.762***	-0.797***
	(0.099)	(0.096)	(0.265)	(0.263)
Cohort FE	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
Controls		✓		✓
Mean DV	.555	.555		
Observations	968	968	968	968
R^2	.099	.131	.179	.219

Notes: Columns (1)–(2) report a high-diversity indicator equal to 1 if the number of unique districts is at least two and 0 otherwise, and columns (3)–(4) report the *Early Exposure Index (EEI)*, defined as the standardized equal-weighted average of three standardized components: the number of distinct districts served, the log of the mean great-circle distance between consecutive posts within the first five years after entry, and an indicator for posting in mixed-HDI districts. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*) on rotation diversity during entry grade. Columns (2) and (4) additionally include baseline controls (entry-exam scores, training scores, education, and age at entry). All specifications include cohort and province fixed effects. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The sample consists of officers who achieved second grade in cohorts 1986–2017. "Mean DV" reports the mean of the dependent variable. Standard errors, reported in parentheses, are clustered by first-posting district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The effects are statistically meaningful and precise. The probability of holding a high diversity portfolio declines by 0.29 SD. The *EEI* falls by 0.76–0.80 SD relative to the control group, implying a substantial reduction of the early career portfolio. Two points follow. First, frequency within similar settings does not substitute for unique-district exposure. Repeated transfers inside a narrow administrative corridor add little information; a single move into a new district adds a lot. Second, convergence between a binary threshold and a composite index rules out a measure-choice story: the mechanism is not an artifact of functional form or weights. Reweighting the *EEI* components leaves signs and magnitudes intact, and omitting any one component barely moves coefficients. See Table A7 for the individual impact of each component of the index.

TABLE 10: Early Exposure Capital Mediates the Effect on Frontline Appointments

Dependent variable	Baseline (no *EEI*)	Baseline + *EEI*	*EEI* as sole predictor
	(1)	(2)	(3)
Treat	-0.011	0.055	
	(0.085)	(0.085)	
Treat × *Post*	-0.331***	-0.171*	
	(0.095)	(0.095)	
EEI		0.201***	0.216***
		(0.024)	(0.020)
Cohort FE	✓	✓	✓
Province FE	✓	✓	✓
Controls	✓	✓	✓
Mean DV	.398	.398	.398
Observations	968	968	968
R^2	.253	.378	.373

Notes: Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*) on *FLM* appointment. Column (1) excludes the *Early Exposure Index (EEI)*, column (2) includes EEI as a covariate, and column (3) reports the effect of *EEI* alone. The *EEI* is defined as the standardized equal-weighted average of three standardized components: the number of distinct districts encountered, the log of the mean great-circle distance between consecutive posts within the first five years after entry, and an indicator for posting in mixed-HDI districts. All specifications include cohort and province fixed effects and baseline controls including entry-exam scores, training scores, education, and age at entry. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The sample consists of officers who achieved second grade in cohorts 1986–2017. "Mean DV" reports the mean of the dependent variable. Standard errors, reported in parentheses, are clustered by district of first posting. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Having shown that the policy contracts early exposure portfolios, I next ask whether this loss of exposure helps explain the decline in frontline managerial (*FLM*) appointments. Table 10

implements a simple mediation exercise by introducing the Early Exposure Index (*EEI*) into the baseline promotion equation and comparing how the *Treat* × *Post* effect changes. This allows me to assess whether early cross-context experience is an important channel linking the policy to later leadership outcomes.

Table 10 tests whether the contraction in early exposure explains the drop in *FLM* appointments. Column (1) reports the baseline model without any exposure controls; the treated-by-post coefficient is about −0.33, consistent with earlier results. In Column (2), I add the *EEI*, an equal-weighted composite of unique districts served, initial rotation distance, and development-context heterogeneity. The treated-by-post coefficient shrinks to about −0.17 (roughly half the baseline), while *EEI* enters large and positive (0.20, $p = 0.00$). Column (3), which includes only *EEI*, shows that officers with higher early exposure are significantly more likely to reach *FLM* positions. The attenuation of the treated-by-post coefficient after adding *EEI* suggests that around half of the policy effect on *FLM* operates through early exposure capital. Posting volume does not explain *FLM* appointments once *EEI* is included. The evidence is consistent with a production-function channel in which reductions in distinct-district exposure limit cross-context learning and are associated with a weaker pipeline into frontline leadership, even when overall mobility remains high.

Table E1 quantifies how much of the frontline-appointment gap is associated with assignment diversity. These numbers are suggestive; standard mediation requires that unobserved factors affecting assignments do not also directly affect promotion. With that caveat, the indirect (ACME) effect operating through assignment diversity accounts for about 35% of the treated-by-post gap. Rotation volume and performance evaluations each account for less than 5%. A robustness check using the balanced sample (Table E2) yields similar magnitudes, indicating that these shares are not driven by sample composition or cohort balance. See Appendix E for details.

Across subgroups, the pattern is similar. The policy reduces distinct-district exposure and *FLM* appointments, with the largest effects where feasible rotations are naturally limited. In Punjab, effects are close to zero. In regions coefficients on both outcomes are negative, consistent with fewer viable cross-district moves. Splitting by exam ability, both above and below median officers lose exposure and frontline roles, with somewhat larger declines below the median; the pattern is therefore not simply a differences in abilities. By age at entry, younger and older officers look similar, which makes a life-cycle maturity explanation less likely. Taken together, these results support a training-technology interpretation: where officers serve early in their careers matters for later leadership, over and above selection on talent, posting frequency, or timing.

TABLE 11: Heterogeneity in Policy Effects by Region, Ability, and Age

Dependent variable	*FLM* Appointment	Unique Districts Served
	(1)	(2)
Panel A: Region heterogeneity (Treat × Post by province of origin)		
Treat × *Post* × *Punjab*	0.001	0.079
	< 0.01	(0.32)
Treat × *Post* × *KPK*	-0.167	-0.447
	(-1.32)	(-1.28)
Treat × *Post* × *Sindh*	-0.065	-0.235
	(-0.49)	(-0.59)
Panel B: Ability heterogeneity (Treat × Post by exam scores)		
Treat × *Post* × *High-ability* (above median)	-0.312**	-0.341
	(-3.34)	(-1.50)
Treat × *Post* × *Low-ability* (below median)	-0.355***	-0.519*
	(-3.80)	(-2.40)
Panel C: Age at entry heterogeneity (Treat × Post by age threshold)		
Treat × *Post* × *Younger* (≤25)	-0.360**	-0.443
	(-3.04)	(-1.88)
Treat × *Post* × *Older* (>25)	-0.326***	-0.406
	(-3.72)	(-1.93)
Controls (entry exams, training, education, age)	Yes	Yes
Province & Cohort FE	Yes	Yes
Observations	968	968
R^2	0.221	0.097

Notes: The dependent variables are: (1) an indicator for *FLM* attainment and (2) the number of unique districts served. Panel A reports *Treat* × *Post* effects by province of birth (Punjab, KPK, Sindh). Panel B splits the sample by ability (above vs. below median exam score), and Panel C splits by age at entry (≤ 25 vs. >25). Each cell reports OLS estimates of the indicated interaction term, with standard errors in parentheses. All specifications include baseline controls (entry-exam scores, training, education, and age) and province and cohort fixed effects. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The sample consists of officers who achieved second grade in cohorts 1986–2017. "Mean DV" reports the mean of the dependent variable. Standard errors are clustered by first-posting city.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C. Perception Survey

To complement the administrative records, I fielded an original survey of 309 serving officers from the elite civil service in April 2025 across all four provinces (instrument, sampling, and

implementation in Appendix S). The survey was administered to trainees in civil-service academies that host participants from multiple occupational groups. Although the survey sample cannot be matched one-to-one to the officers in the administrative dataset, it is informative about broader perceptions among civil servants. The questionnaires were explicitly designed with careers in the elite administrative service in mind, and it was stated that the scenarios and questions were specific to this institutional context. Invitations were distributed via training academies with single–use links; participation was voluntary and non–incentivized, and respondents could skip items or withdraw at any time. The questionnaire elicited: (i) perceived assignment constraints and opportunity structure; (ii) evaluations of posting quality (remoteness, hardship, proximity); and (iii) career aims and attitudes toward frontline versus specialists tracks. Table S3 summarizes characteristics of the 309 officers in the survey sample. About one-fifth belong to the treated group, and roughly 70 percent entered service after the policy change. Half of respondents are from Punjab, followed by Khyber Pakhtunkhwa (18 percent) and Sindh (15 percent), mirroring the national cadre composition. The sample spans all career stages: 41 percent are in the second grade, 31 percent in the third, and 28 percent in senior grades (BPS 20–22). Comparing treated and control officers, most of the representation at higher grades lies among controls, consistent with their longer career exposure before the reform. Overall, the survey captures a balanced cross-section of officers by region and seniority.

The survey shows that treated and control officers share many views about what makes a good career and a good district leader. In Table S4, both groups agree that high priority roles require strong management skills and that early assignments are important for later outcomes. Treated officers are somewhat more likely to report that they follow a different rotation track, which they interpret as a distinct opportunity structure rather than a lack of talent. In Table S5, officers in both groups value exposure to different districts, while treated officers describe a context that is more locally anchored, consistent with the policy design. Table S6 and Table S7 indicate that challenging posts are widely seen as useful for learning, and that treated officers accumulate experience that is more concentrated in similar locations. Finally, Table S8 shows that overall aspirations remain high. Most officers continue to express interest in meaningful leadership positions. Overall, the survey points to two assignment models that share similar career aspirations and motivations. For comparability with the administrative analysis, I estimate the same specifications and summarize coefficients in Figure 5.

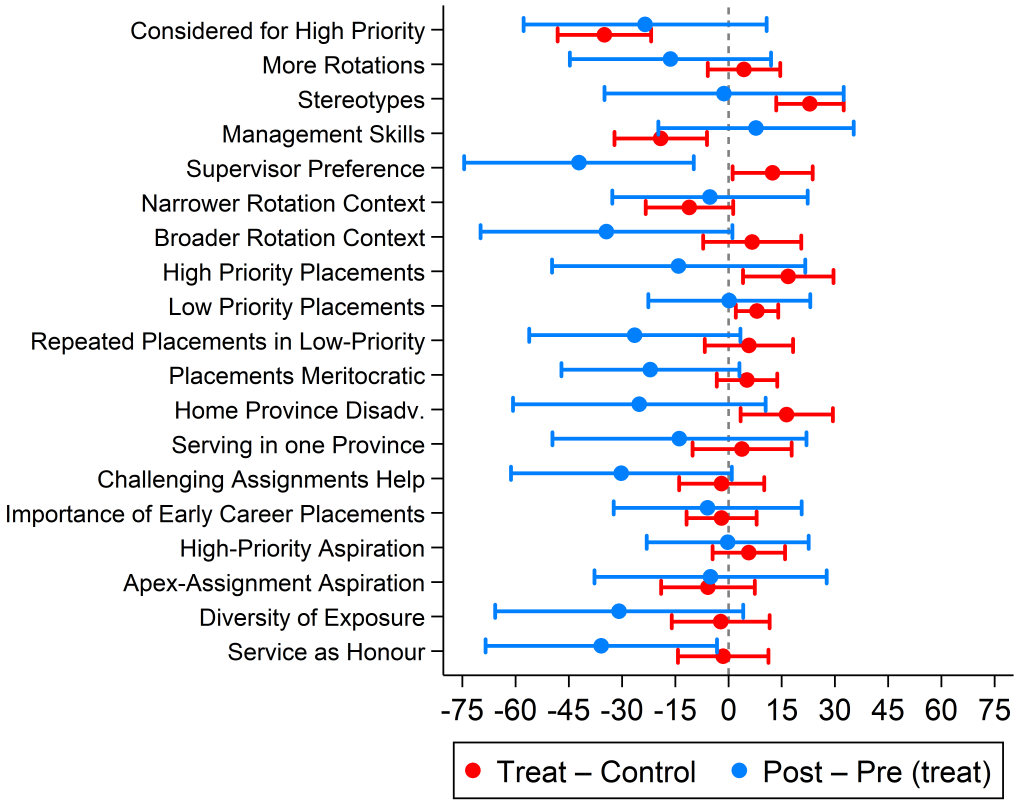


Figure 5: Perception about Assignments Before and After the Policy

Notes: Points show differences in agreement (percentage points) for each statement; whiskers denote 95% confidence intervals. Positive values indicate stronger agreement for the first group in each contrast. The vertical dashed line marks zero

There are three main patterns. First, perceived constraints align with the margins that matter for selection into frontline responsibility. Stated beliefs about managerial capability and the role of supervisor gender are very similar across cohorts, which is consistent with changes in opportunity structure rather than changes in underlying preferences. Second, reported exposure tracks the administrative first stage. Earlier cohorts more often describe a diverse district portfolio as an important component of leadership development, whereas later cohorts report fewer such experiences and place greater emphasis on skills acquired through other channels. Third, career trajectories adjust in a way that is consistent with a smaller role for generalist capital. While treated officers place less weight on geographic mobility, their stated aspirations for high-priority frontline and apex roles are comparable to those of untreated officers.

D. External validity

Many large organizations, public and private, use intensive early career assignments to broaden the skills of younger workers. Learning models treat rotation as the firm's optimal response to uncertainty about worker abilities across tasks: by moving junior staff across roles, the principal learns their comparative advantage and can allocate them more efficiently (Ortega, 2001). The mechanism is both informational and developmental. Second, the geography of assignment shapes discretion and independence. Using Chinese data on audit inspections, Chu et al. (2021) show that hometown auditors are systematically more lenient toward local governments, consistent with assignment geography affecting behavior as well as appointment. Third, assignments also matter through who junior officers work under. Using US Army administrative and evaluation data, Lyle and Smith (2014) show that exposure to high-performing senior officers raises junior officers' early promotion probabilities, with stronger effects when the mentoring relationship lasts longer. Taken together, this evidence indicates that assignment rules are a key determinant of how leadership-relevant skills accumulate over the career. A unifying implication is that, across otherwise disparate institutional settings, rotation policies perform a dual role in the production of leadership: these management practices both generate the experiences through which managerial capability is formed and provide information that enables organizations to identify and allocate talent.

VII. Conclusion

Bureaucrats constitute a core component of the state's administrative capacity (Evans and Rauch, 1999; Besley and Persson, 2010; Finan et al., 2017). Through their assignments, governments determine where managerial expertise is deployed, the environments in which officers accumulate experience, and the leadership pipeline available for frontline roles.

I study a rotation policy that exempted a subset of entrants from the mandatory non-domicile rule. The policy generated sharp variation in early-career geographic exposure for officers who were otherwise comparable in entry criteria, ability, and training. This variation enables me to examine how initial assignment diversity shapes subsequent career trajectories. The analysis draws on a universe of personnel records that document every posting and transfer, merged with geocoded district characteristics. These data allow a detailed examination of the formative stage of bureaucratic careers. The rotation design under study can be viewed as altering access to heterogeneous administrative and political environments. Exposure to diverse districts is associated with acquiring the coordination and management experience that the organization rewards in promotions and frontline leadership roles.

To recap, the analysis shows how internal assignment rules shape early-career postings and long-run career trajectories. Officers subject to the post-policy flexibility rule are, at entry, posted to districts roughly 80 percent closer to their home districts and rotate through about 18 percent fewer distinct administrative contexts. At the first promotion grade, they are about one-third less likely to be appointed to frontline managerial posts, with no corresponding differences in deputy or ministry appointments. In this setting, early exposure refers to the portfolio of formative assignments officers receive in their initial years: rotations across distinct districts, variation in socio-economic and administrative environments, and time spent in frontline field roles versus headquarters posts. These are the observable assignment inputs that the organization systematically rewards in promotions and leadership appointments. They map naturally into what the literature describes as generalist managerial capital coordination and leadership experience accumulated through managing heterogeneous contexts but the analysis does not attempt to recover a latent ability parameter. Instead, it exploits the policy-induced shift in these observable assignment inputs to show that assignment diversity, rather than rotation frequency per se, is the key margin through which early-career rotation design shapes the leadership pipeline.

Many open questions remain. First, the study does not examine service delivery or performance outcomes. These generalist positions lack comparable performance measures. Second, a promising direction is to study assignment complementarities within teams and hierarchies, such as whether exposure diversity among subordinates enhances coordination for district leaders or dilutes task specific expertise. Finally, the institutional framework conceptualized here can be extended to other bureaucratic reforms to understand the trade-off between flexibility and career outcomes. These remain important areas for future work.

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Supplementary Data and Appendix For Online Publication

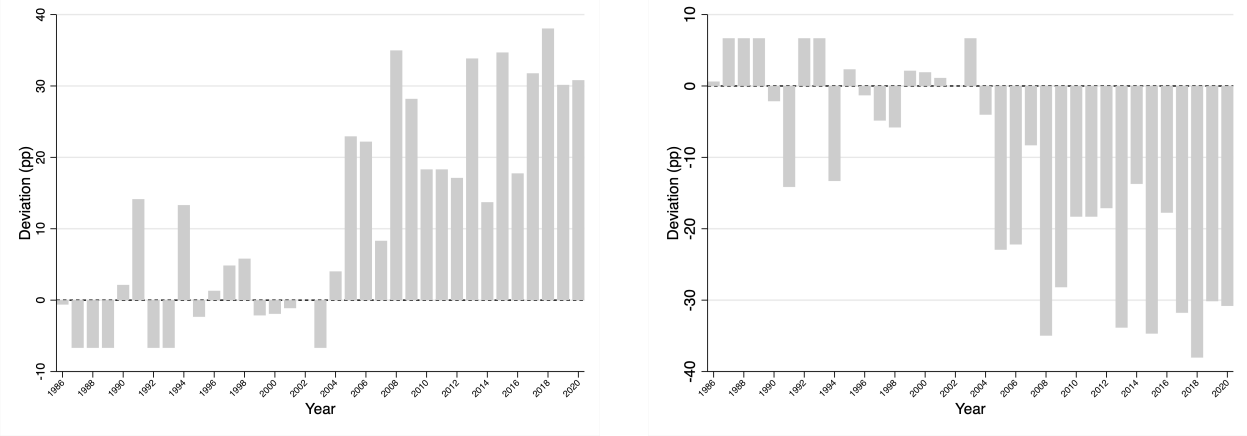
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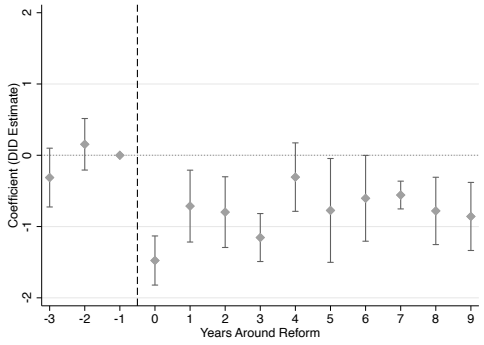
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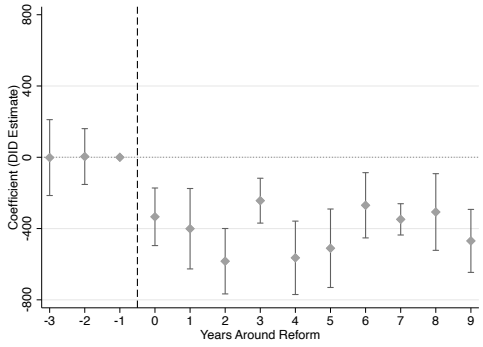
Panel A *Panel B*

Figure A1: Treated and Control Deviation from Pre-Policy Mean

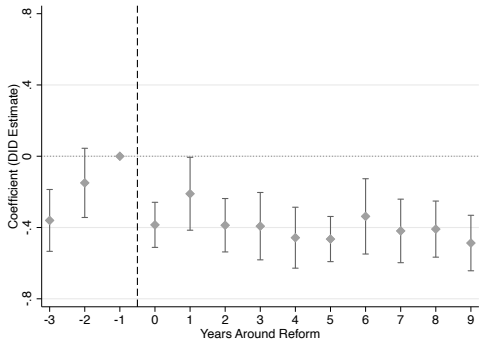
Notes: Panel A plots the deviation (in percentage points) of treated junior officers' share relative to the pre-policy mean. Panel B plots the corresponding deviation for control junior officers. Bars represent annual deviations, and the dashed line at zero indicates the baseline (pre-policy) mean.



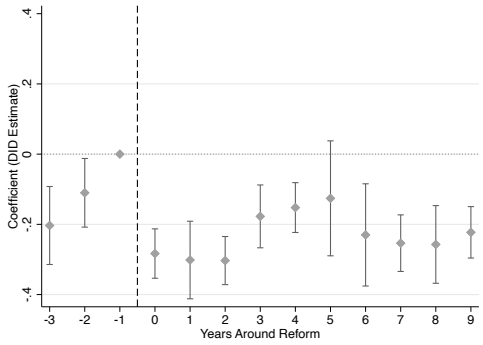
Panel A: Number of Distinct Districts Served in Entry Grade



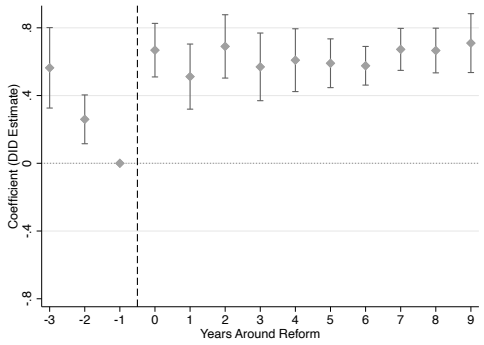
Panel B: First Appointment City distance from Home districts (km)



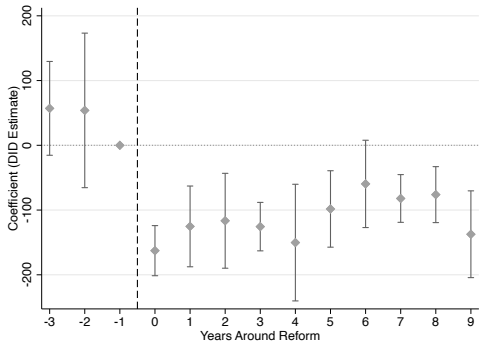
Panel C: Share of Assignments in Low-HDI



Panel D: Share of Assignments in Medium-HDI



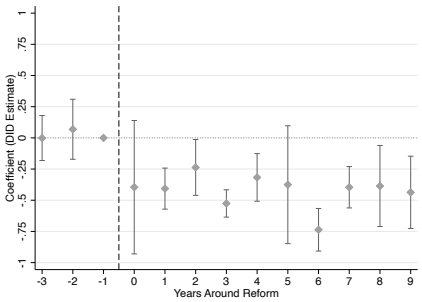
Panel E: Share of Assignments in High-HDI



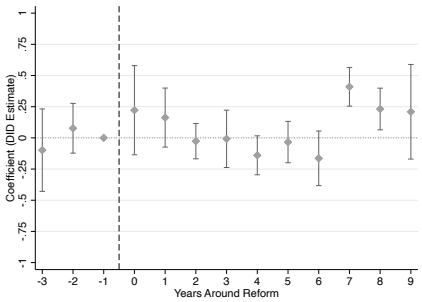
Panel F: Mean distance in km between Consecutive Postings

Figure A2: Officer Presence at entry grade (first five years) by HDI Category and Spatial Distance

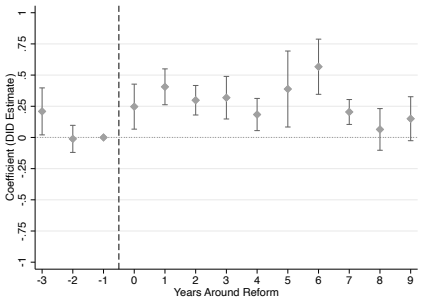
Notes: The figure presents event study estimates of the policy effect on officer posting patterns during entry grade rotations. Each panel displays coefficients and 95% confidence intervals from separate regressions estimating differential changes for treated officers relative to control officers across cohorts. All specifications include cohort and province fixed effects with standard controls. The vertical line indicates policy implementation year. Sample: cohorts entering in years 1986–2017.



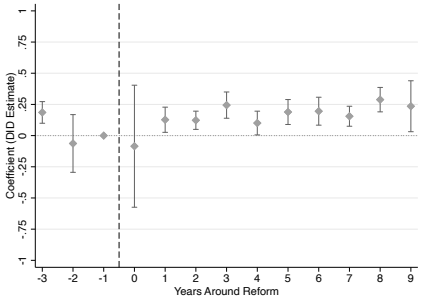
Panel A: FLM Appointment



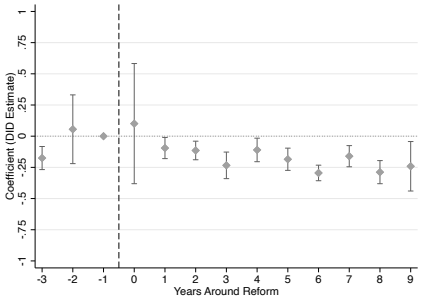
Panel B: DM Appointment



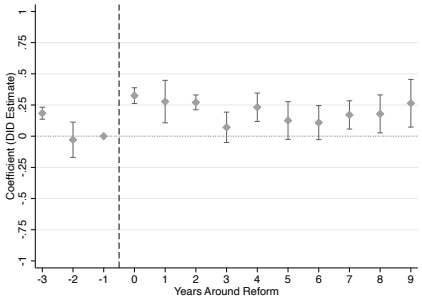
Panel C: Share of Ministry Appointments



Panel D: Time Spent in Ministry Assignments



Panel B: Time Spent in District Positions (FLM or DM)



Panel F: Share of Assignments in Progressive cities

Figure A3: Appointments and Share of Time Across Three Assignments Before and After the Policy

Notes: The figure presents event study estimates of the policy effect on career progression and position attainment during second grade rotations. Each panel displays coefficients and 95% confidence intervals from separate regressions estimating differential changes for treated officers relative to control officers across cohorts. The vertical line indicates the policy implementation year. Sample: cohorts entering in years 1986–2020.

Additional Tables

TABLE A1: Pre- and Post-Policy Means by Treated and Control and DiD Estimates

	Pre-T	Pre-C	Post-T	Post-C	Sample	*Treat* × *Post*
Panel A: Entry Characteristics						
Entry Exam Score	60.526	60.346	58.646	60.008	1071	-1.542 (0.134)
General Training Score	72.991	67.573	73.742	70.904	1071	-2.580 (0.103)
Technical Training Score	69.517	65.815	68.250	66.534	1071	-1.986 (0.178)
Age at entry	26.154	27.805	27.166	27.586	1071	1.231** (0.025)
From Lahore	0.360	0.188	0.308	0.141	1071	-0.005 (0.954)
From Sindh	0.160	0.178	0.141	0.205	1071	-0.046 (0.595)
From High HDI (city of birth)	0.680	0.289	0.460	0.258	1071	-0.189* (0.064)
From Medium HDI (city of birth)	0.280	0.541	0.379	0.557	1071	0.082 (0.457)
From Low HDI (city of birth)	0.000	0.051	0.056	0.046	1071	0.060 (0.211)
GB/FATA and AJK Origin	0.000	0.036	0.035	0.026	1071	0.044 (0.250)
KPK/Balochistan origin	0.160	0.208	0.172	0.236	1071	-0.016 (0.862)
From Punjab and Sindh	0.840	0.756	0.793	0.736	1071	-0.026 (0.783)
Panel B: entry grade						
non-domicile allocation	2.440	2.962	0.904	3.236	1071	-1.810*** (0.000)
Field posting length out of total time in entry grade	0.211	0.242	0.541	0.518	1071	0.053 (0.270)
Avg. tenure per posting (yrs)	1.390	1.441	1.376	1.262	1071	0.165* (0.083)
Transfer frequency in entry grade	106.640	67.710	94.823	54.967	1071	0.926 (0.964)
High HDI posting	0.141	0.072	0.278	0.075	1071	0.134*** (0.002)
Medium HDI Posting	0.082	0.061	0.225	0.099	1071	0.105*** (0.007)
Low HDI Posting	0.003	0.022	0.007	0.076	1071	-0.049** (0.044)
Number of posts during time in entry grade	3.600	3.487	3.096	3.383	1071	-0.400* (0.098)
Number of unique districts	2.400	2.555	2.131	2.709	1071	-0.423* (0.061)
Panel C: second grade						
DM Role	0.520	0.792	0.591	0.689	1071	0.173 (0.347)
FLM Role	0.320	0.508	0.131	0.826	1071	-0.507*** (0.009)
Field posting length out of total time in second grade	0.059	0.080	0.100	0.176	1071	-0.055** (0.031)
Islamabad-level post	0.800	0.322	0.288	0.200	1071	-0.390** (0.028)
Avg. tenure per posting (yrs)	1.969	1.684	1.479	1.323	1071	-0.128 (0.376)
Transfer frequency in second grade	106.640	67.407	96.630	53.742	1071	3.655 (0.859)
High HDI posting	0.182	0.177	0.236	0.166	1071	0.065* (0.079)
Medium HDI Posting	0.032	0.047	0.072	0.082	1071	0.005 (0.822)
Low HDI Posting	0.000	0.008	0.001	0.024	1071	-0.015 (0.137)

Notes: The table reports sample means by treated and control. The last column shows the coefficient on *Treat x Post* from separate OLS regressions of each row outcome on indicators for Treated, Post, and their interaction. Panels A-C group entry characteristics, entry grade outcomes, and second grade outcomes, respectively.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A2: Robustness of Inference to Alternative Clustering of Standard Errors — Main Outcomes

Dependent variable		Estimated standard error (SE) clustering at:			
	Coefficient estimate	District of First Posting	District of Birth	Cohort × Province	Cohort × District of First Posting
	(1)	(2)	(3)	(4)	(5)
Panel A: Unique Districts (count)					
Treat	-0.169	(0.175)	(0.101)*	(0.136)	(0.173)
Treat × *Post*	-0.430	(0.205)**	(0.125)***	(0.165)**	(0.193)**
Panel B: Number of Postings					
Treat	0.082	(0.173)	(0.165)	(0.119)	(0.164)
Treat × *Post*	-0.208	(0.184)	(0.201)	(0.149)	(0.182)
Panel C: FLM Attainment					
Treat	-0.012	(0.085)	(0.080)	(0.080)	(0.087)
Treat × *Post*	-0.331	(0.095)***	(0.070)***	(0.093)***	(0.095)***
Panel D: DDE Attainment					
Treat	0.102	(0.099)	(0.068)	(0.081)	(0.109)
Treat × *Post*	-0.013	(0.103)	(0.089)	(0.095)	(0.118)
Panel E: Secretariat / Desk Share					
Treat	-0.004	(0.053)	(0.046)	(0.059)	(0.055)
Treat × *Post*	0.150	(0.061)**	(0.048)***	(0.068)**	(0.062)**
Panel F: High Diversity					
Treat	-0.022	(0.077)	(0.069)	(0.058)	(0.083)
Treat × *Post*	-0.286	(0.096)***	(0.075)***	(0.074)***	(0.094)***
Panel G: EEI					
Treat	-0.332	(0.221)	(0.168)*	(0.196)	(0.211)
Treat × *Post*	-0.797	(0.263)**	(0.167)***	(0.217)***	(0.230)***

Notes: The table reports coefficient estimates for the *Treat* indicator and its interaction with the post–policyindicator (*Treat* × *Post*) across a set of dependent variables. Column (1) presents the point estimates. Columns (2)–(5) report the corresponding standard errors computed under alternative clustering schemes: (i) district of first posting, (ii) district of birth, (iii) *Cohort* × *Province*, and (iv) *Cohort* × *District* of first posting. All regressions include cohort fixed effects, province fixed effects, and the full set of baseline controls (exam scores and demographics). The post-policy main effect is absorbed by cohort fixed effects and is therefore not reported. The sample is restricted to officers entering between 1986 and 2017. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3: Frontline Managers Appointment- Robustness to Alternative Sample

Dependent variable	*FLM* Appointment					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.000	-0.020	0.012	-0.018	0.076	-0.020
	(0.080)	(0.083)	(0.084)	(0.083)	(0.120)	(0.079)
Treat × *Post*	-0.418***	-0.420***	-0.464***	-0.410***	-0.200	-0.420***
	(0.098)	(0.103)	(0.107)	(0.102)	(0.184)	(0.119)
Cohort FE	✓	✓		✓	✓	✓
Province FE	✓	✓		✓	✓	✓
Standard controls		✓	✓	✓	✓	✓
Province × *Cohort* FE			✓			
Seat-adjusted (capacity)				✓		
Linear Gender Trends					✓	
Observations	665	665	656	665	665	665
R^2	.179	.218	.259	.181	.220	.218

Notes: The dependent variable is an indicator for *FLM Appointment*. Each column reports OLS estimates of the interaction term *Treat* × *Post*, controlling for a treated indicator (*Treat*) and the fixed effects listed. Column (1) is the baseline specification. Column (2) adds standard controls (entry-exam scores and demographics). Column (3) includes *Province* × *Cohort* fixed effects. Column (4) uses a seat-adjusted outcome (capacity). Column (5) adds linear treated-specific trends. Column (6) is as in column (2) but clusters standard errors by *Cohort* × *Province*. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The sample is trimmed to officers who have entered in the third grade and have completed all assignment in second grade (1986-2010). Unless otherwise noted, standard errors are clustered by first-posting district; robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A4: Effect of the Policy on Second Grade Assignment: Intensive Margin

Dependent variable	Time in Non-Field		Time in Field (*FLM* or *DM*)		High-HDI Share (Second grade)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.074	0.042	-0.057	-0.028	0.089*	0.061
	(0.046)	(0.050)	(0.051)	(0.054)	(0.050)	(0.051)
Treat × *Post*	0.079	0.112**	-0.099*	-0.123**	0.153***	0.166***
	(0.050)	(0.054)	(0.054)	(0.058)	(0.053)	(0.053)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Sample: Entry grade (first 5 years)	✓	✓	✓	✓	✓	✓
Mean DV	.634	.634	.342	.342	.734	.734
Observations	968	968	968	968	968	968
R^2	.107	.137	.124	.143	.219	.235

Notes: Columns (1)–(2) report the dependent variable *time spent in non-field posts*, columns (3)–(4) report *time spent in field assignments (FLM or DM)*, and columns (5)–(6) report the dependent variable *share of postings in high-HDI districts at second grade*. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and baseline controls (entry-exam scores, training scores, education, and age at entry) where indicated. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors, reported in parentheses, are clustered by first-posting district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

TABLE A5: Posting District Quality in Initial Five years (Entry Grade)

Dependent variable	High-HDI Share		Med-HDI Share		Low-HDI Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.333***	0.322***	-0.144***	-0.143***	-0.189***	-0.178***
	(0.052)	(0.054)	(0.041)	(0.044)	(0.037)	(0.039)
Treat × *Post*	0.223***	0.249***	-0.053	-0.052	-0.169***	-0.196***
	(0.080)	(0.078)	(0.059)	(0.059)	(0.047)	(0.048)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Sample: Entry grade (first 5 years)	✓	✓	✓	✓	✓	✓
Mean DV	.495	.495	.233	.233	.272	.272
Observations	968	968	968	968	968	968
R^2	.269	.280	.087	.095	.187	.200

Notes: Columns (1)–(2) report the dependent variable: share of time spent in high-HDI districts, columns (3)–(4) report;share of time spent in medium-HDI districts, and columns (5)–(6) report: share of time spent in low-HDI districts. Each column reports OLS estimates of *Treat*, *Treat* × *Post*, cohort fixed effects, province fixed effects, and (where indicated) baseline controls (entry-exam scores and demographics). The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district; robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

TABLE A6: Geographical Spread of Initial Five Years Postings

Dependent variable	Log Distance of First District from Home (km)		Log Mean Distance Traveled (first 5 years, km)		Avg. Tenure per Posting (months)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	-0.748	-0.620	0.004	-0.073	-2.677**	-2.654*
	(0.545)	(0.526)	(0.625)	(0.595)	(1.278)	(1.438)
Treat × *Post*	-1.460***	-1.599***	-1.700**	-1.743**	4.468***	4.781***
	(0.507)	(0.510)	(0.753)	(0.768)	(1.517)	(1.590)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Sample: Entry grade (first 5 years)	✓	✓	✓	✓	✓	✓
Mean DV	5.632	5.632	4.428	4.428	7.100	7.100
Observations	968	968	968	968	968	968
R^2	0.231	0.242	0.082	0.124	0.085	0.109

Notes: Columns (1)–(2) report the logarithm of the distance between an officer's first posting and home district; columns (3)–(4) report the logarithm of the mean distance traveled between consecutive postings during the first five years; columns (5)–(6) report the average tenure per posting (in months). Each column reports OLS estimates including *Treat*, *Treat* × *Post*, cohort fixed effects, province fixed effects, and (where indicated) baseline controls (entry-exam scores, training scores, education, and age at entry). The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district; robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

TABLE A7: Early Exposure Index (Initial five years) Components

Dependent variable	*Early Exposure Index* (EEI)	Number of Unique Districts	Mix-HDI	Log Mean Distance Traveled (km, first 5 years)
	(1)	(2)	(3)	(4)
Panel A: Composite Index				
Treat	-0.304			
	(0.211)			
Treat × *Post*	-0.814***			
	(0.257)			
Panel B: Components				
Treat		-0.142	-0.265**	-0.005
		(0.169)	(0.106)	(0.601)
Treat × *Post*		-0.447**	-0.351**	-1.782**
		(0.201)	(0.142)	(0.780)
Cohort FE	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Mean DV		2.62	.687	4.428
Observations	968	968	968	968
R^2	.216	.133	.251	.121

Notes: Columns (1)–(4) report dependent variables measuring evaluator diversity and exposure during entry grade. The dependent variable in Panel A is the Early Exposure Index (*EEI*), defined as the standardized equal-weighted average of three components: the number of distinct districts encountered, the log of the mean great-circle distance between consecutive posts within the first five years after entry, and an indicator for posting in mixed-HDI districts. Panel B reports each of these three components separately: number of unique districts, share of postings in mixed-HDI districts, and log mean distance traveled (km) within five years. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and baseline controls (entry-exam scores, training scores, education, and age at entry) where indicated. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors, reported in parentheses, are clustered by first-posting district.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A8: Variable Definitions — (Tables 3–6)

Table	Columns	Variable	Definition
Table 3	Column 1	*FLM* Attainment	Indicator = 1 if second grade is to a *FLM* role.
Table 3	Column 2	*DM* Attainment	Indicator = 1 if any DM post in second grade.
Table 3	Column 3	Ministry Appointment Share	Fraction of second grade months in ministry assignments.
Table 4	Columns 1 – 7	*FLM* Attainment	Indicator = 1 if second grade is to a *FLM* role.
Table 5	Columns 1 – 2	STEM Degree	Indicator = 1 if highest pre-entry qualification is STEM (science, tech., engineering, math).
Table 5	Columns 3 – 4	Age at Joining $\geq$ 30	Indicator = 1 if age at entry $\geq$ 30.
Table 5	Columns 5 – 6	From Sindh	Indicator = 1 if province of origin is Sindh.
Table 6	Columns 1 – 2	Number Field Postings	Total number of field roles (FLM or DM) during time in entry grade.
Table 6	Columns 3 – 4	Average Evaluation	Standardized average of annual performance evaluations received in entry grade.
Table 6	Columns 5 – 6	Time to Promotion	Total days in entry grade before promotion to second grade.

TABLE A9: Variable Definitions — (Tables 7–11)

Table	Columns	Variable	Definition
Table 7	Column 1	Log Experience Before Becoming *FLM*	ln total months of experience accumulated before first *FLM* post.
Table 7	Column 2	Log *FLM* Tenure Duration	ln average months per *FLM* posting.
Table 7	Column 3	Share of Time as *FLM*	Fraction of second grade time spent as *FLM*.
Table 7	Column 4	Number of *FLM* Rotations	Count of distinct *FLM* assignments in second grade.
Table 7	Column 5	High-HDI Posting City as *FLM*	Indicator = 1 if any *FLM* assignment in a high-HDI district.
Table 8	Column 2	Number of Unique Districts	Count of distinct districts served in first five years (breadth).
Table 8	Column 2	Number of Postings	Count of postings served in first five years.
Table 9	Columns 1 – 2	High Diversity	Indicator = 1 if number of unique districts ≥ 2, else 0.
Table 9	Columns 3 – 4	Early Exposure Index (*EEI*)	Equal-weighted average of standardized components: (i) distinct distrcits (ii) average distance between postings (km), (iii) share of postings in mixed-HDI districts.
Table 10	Columns 1-3	Early Exposure Index (*EEI*)	Continuous mediator capturing early-career exposure diversity (definition above).
Table 11	Column 1	*FLM* Role	Indicator = 1 if *FLM* post in second grade.
Table 11	Column 2	Unique Districts Served	Count of districts served in first five years (breadth) for subgroup analyses.
Table 11	–	Region heterogeneity (*Treat* × *Post* by province of origin)	Interactions testing province-of-origin differences (Punjab, KPK, Sindh) in effects on *FLM* or breadth.
Table 11	–	Ability heterogeneity (*Treat* × *Post* by exam scores)	Interactions splitting by exam performance (above/below median).
Table 11	–	Age at entry heterogeneity (*Treat* × *Post* by age threshold)	Interactions splitting by age at entry (≤ 25 vs > 25).

Appendix B:Additional Figures and Tables

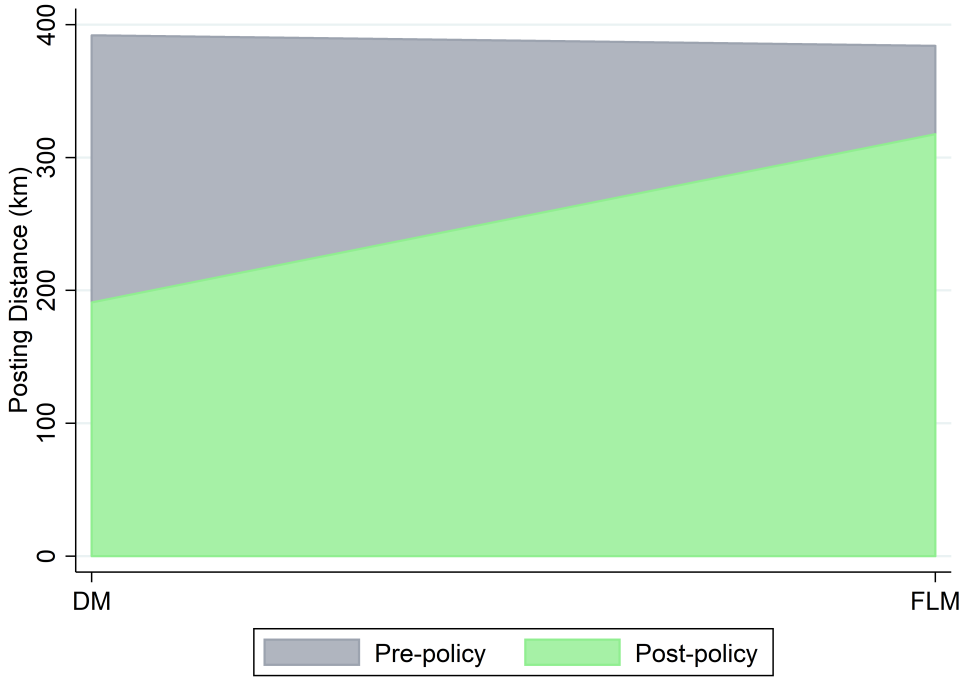
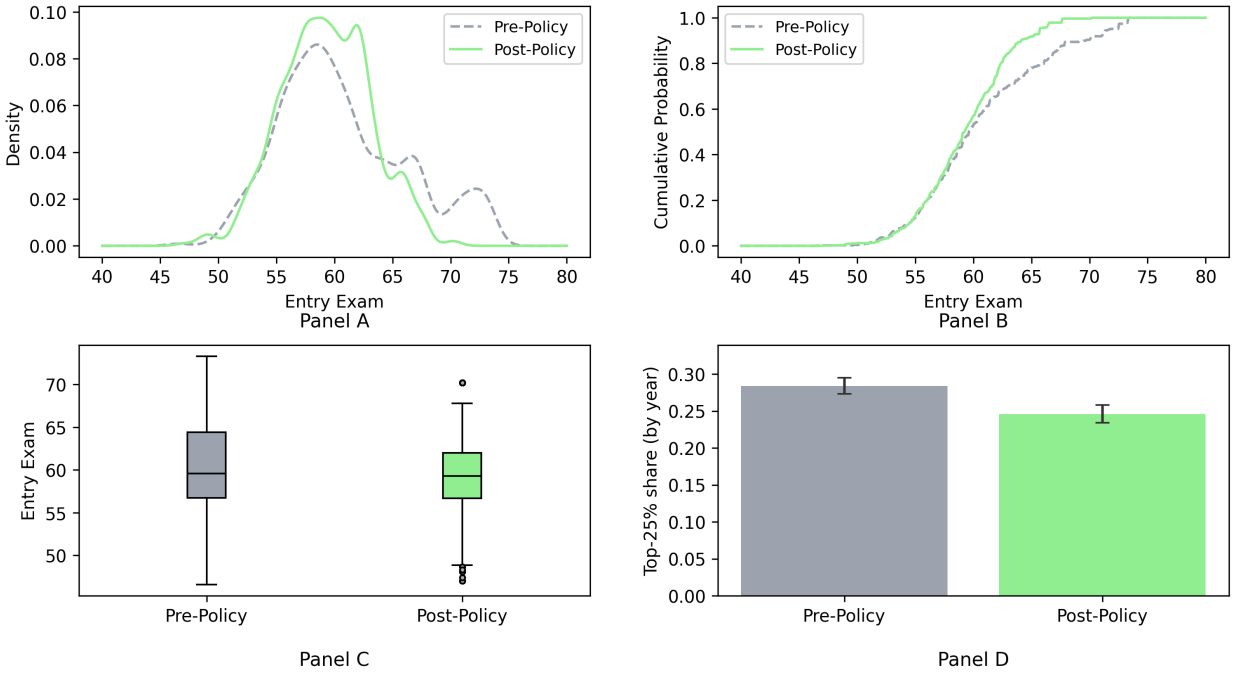
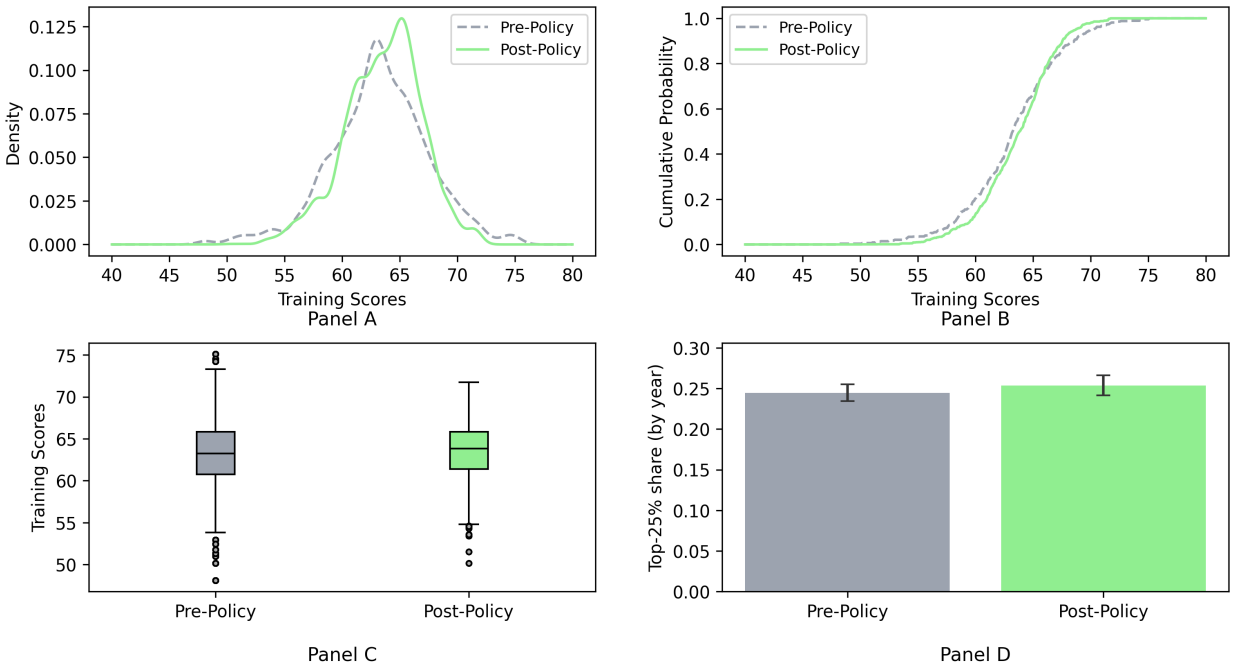


Figure B1: Distance-Preference Channel

Notes: The figure compares average posting distances for control and treated officers across Deputy Manager (*DM*) and *FLM* roles, before and after the policy. Distances decline post-policy for both roles, but the *DM–FLM* gap remains unchanged, indicating no gender-specific "stay-close" response. The evidence rules out proximity preference as a driver of treated officers' reallocation.



Panel A: Entry Exam: distribution, CDF, boxplot, fraction.



Panel B: Training Scores: distribution, CDF, boxplot, fraction.

Figure B2: Pre- vs Post-Policy ability and training panels.

Notes: The unit of observation is officer-year. The figure stacks two *2* × *2* panels: Panel A plots *Entry Exam* scores; Panel B plots *Training Scores*, defined as the simple average of the General Training and Technical Training test scores. Within each *2* × *2* panel: Panel A shows Gaussian-kernel density estimates; Panel B shows empirical CDFs; Panel C shows boxplots of raw scores; Panel D shows the share above the within-year 75th percentile ($z > 0.674$) with 95% confidence intervals.

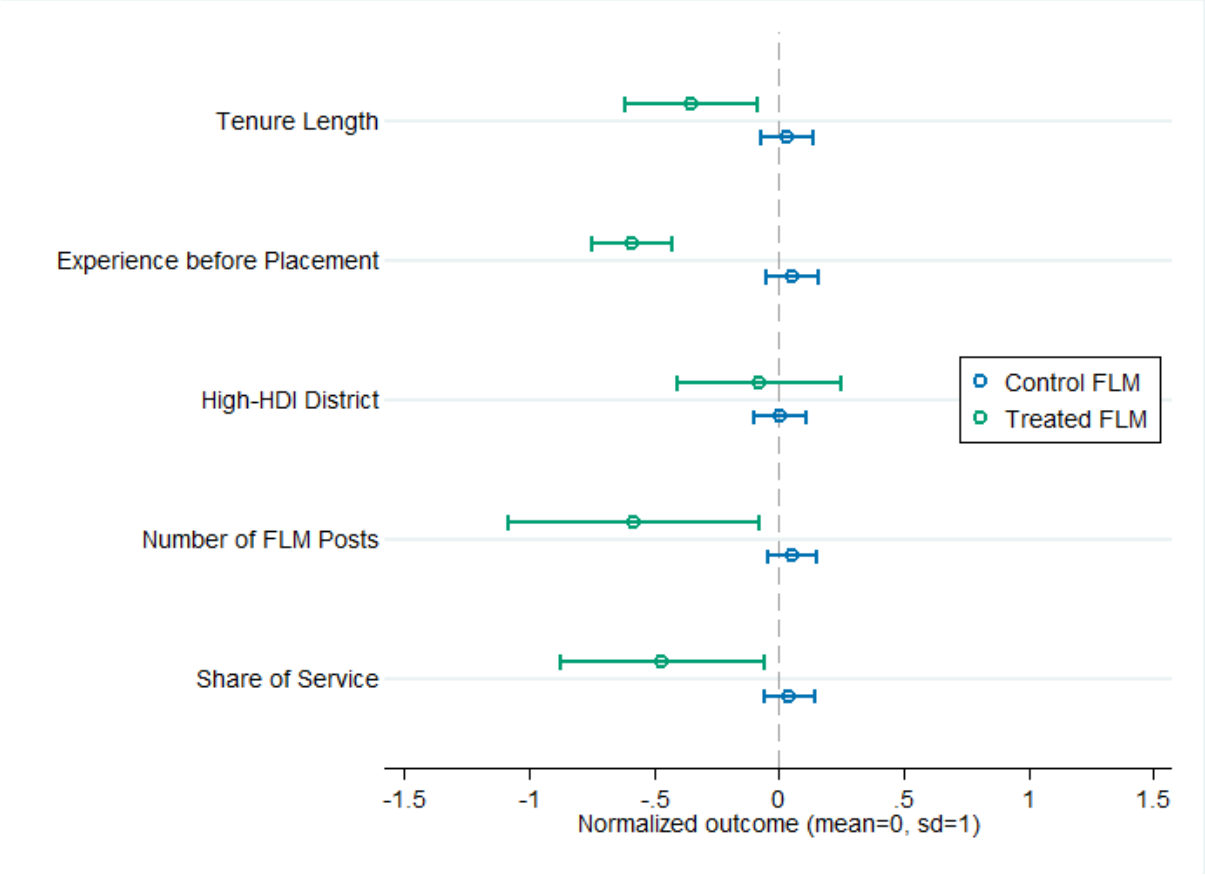


Figure B3: Conditional on Leadership Appointment, Treated and Control Careers

Notes: The figure presents normalized performance outcomes (mean = 0, SD = 1) are shown for control and treated *FLM* across the following dimensions: months before obtaining a *FLM* position, tenure length, share of service, number of *FLM* posts, and serving in a high-HDI district.

Appendix C: Career Architecture at First Promotion

Data construction. I assemble a longitudinal officer panel (1986–2020) by linking three primary administrative sources: (i) annual entry registers, (ii) initial allocation notifications, and (iii) curriculum vitae. Records are linked using service number, cohort, standardized names, and event dates; ambiguous matches are adjudicated using posting notifications, and external reconciliations rely on quarterly seniority lists.

Individual CVs. Source files were collected as scanned personnel folders. Missing rotation files were retrieved in person and digitized. All files were then digitized and harmonized on assignments, rotations, titles, grades, dates, cities, provinces, and ministries, reconciling conflicts across sources.

Officer biographical files. From personnel records, training files, and published directories, demographic characteristics were digitized and harmonized: gender, age at entry, province/district of origin, and education. These demographics were then linked to the assignment panels for rotations and promotions to study how background interacts with assignment rules.

Gazette notifications. Gazette notifications on appointments, transfers, promotions, and retirements provide additional checks on exact notification and effective dates. These anchor event timing and resolve conflicts between CVs and seniority lists.

Training and demographics. Training scores were collected from three different training institutions and harmonized with officers' characteristics. Exact merges track within-officer changes.

Validation. Constructed rotations/grades are cross-checked against *seniority lists* snapshots at quarter boundaries; promotion dates are verified against *Seniority Lists*.

TABLE C1: Descriptive Statistics: Assignment Outcomes by Stage

	entry grade	second grade
Average tenure per posting (years)	1.55	1.50
Transfer frequency (per year)	0.77	0.65
Share of field rotations	0.94	0.43
Share of Islamabad rotations	0.03	0.09
Share of low-HDI rotations	0.46	0.27
Share of medium-HDI rotations	0.47	0.32
Share of high-HDI rotations	0.40	0.71
Observations (officers)	1,071	1,071

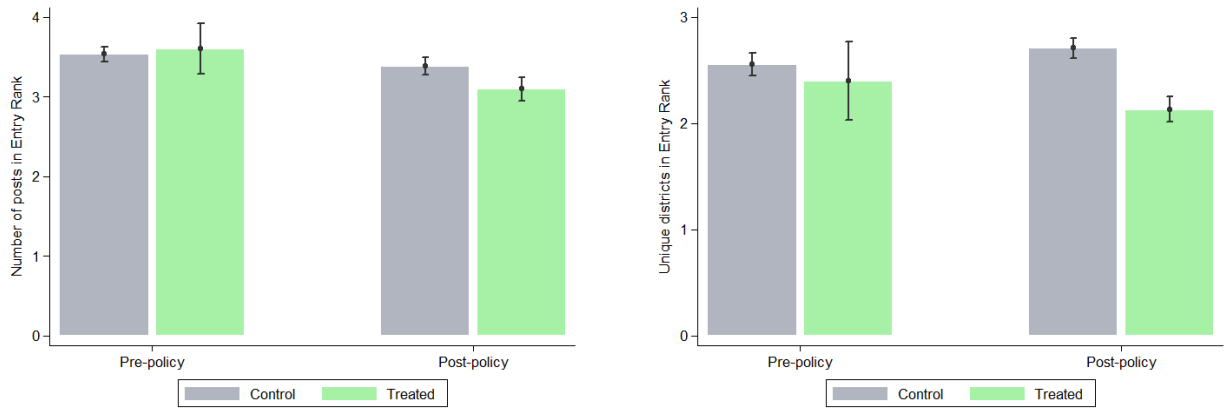
Exposure measure. To measure the breadth of early-career exposure, I construct an *Early Exposure Index* (EEI) that aggregates three predetermined dimensions of diversity in postings. Formally, for officer o:

$$EEI_o = \frac{1}{3}\sum_{k=1}^{3} Z(X_{ok}),$$

where X_{ok} are the three components of evaluator exposure: (i) the number of distinct districts in entry grade, (ii) the average distance in kilometers of officer's postings in initial 5 years, and (iii) the share of postings in mixed-HDI districts.

The *Early Exposure Index* (EEI) is defined as the equal-weighted average of the three standardized components:

$$EEI_o = \frac{1}{3}\Big(Z(\text{Districts}_o) + Z(\text{Distance}_o) + Z(\text{MixHDI}_o)\Big).$$



Rotation volume

Rotation diversity

Figure C1: Rotation Volume and District Diversity in Entry Grade

Notes: The figure presents the decomposition of Rotation volume and diversity by treatment status and pre- and post-policy years. Panel A reports the mean number of postings; Panel B reports the mean number of unique districts, for control and treated groups pre- and post-policy. Means with 95% confidence intervals. Sample: cohorts entering in years 1986–2015.

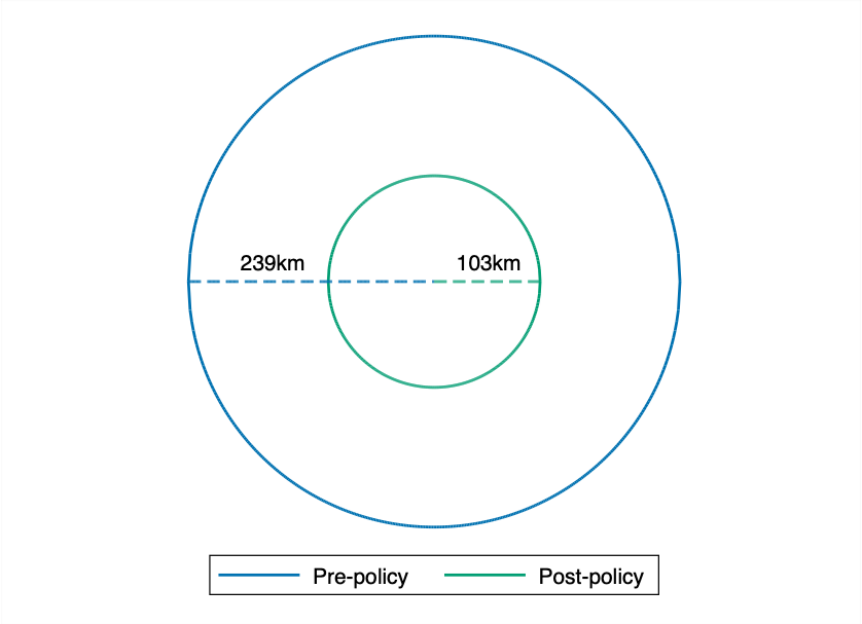
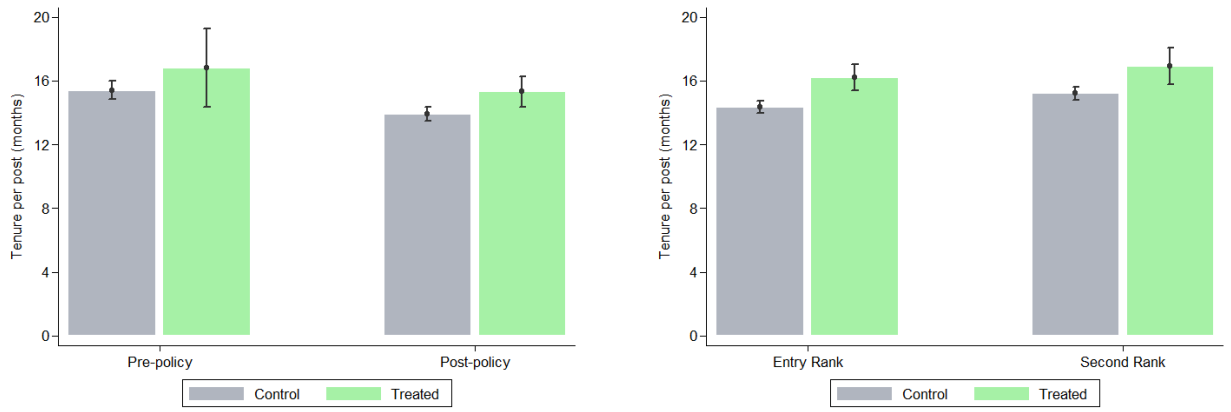


Figure C2: Mean Distance Between Postings in Initial Five Years (Treated Officers, Pre- vs Post-Policy)

Notes: Radii show mean great-circle distance (km) between consecutive postings in the first five years. Blue (outer): pre-policy cohorts; green (inner): post-policy cohorts. Labels show means (nearest km). Sample: cohorts entering in years 1986–2017.

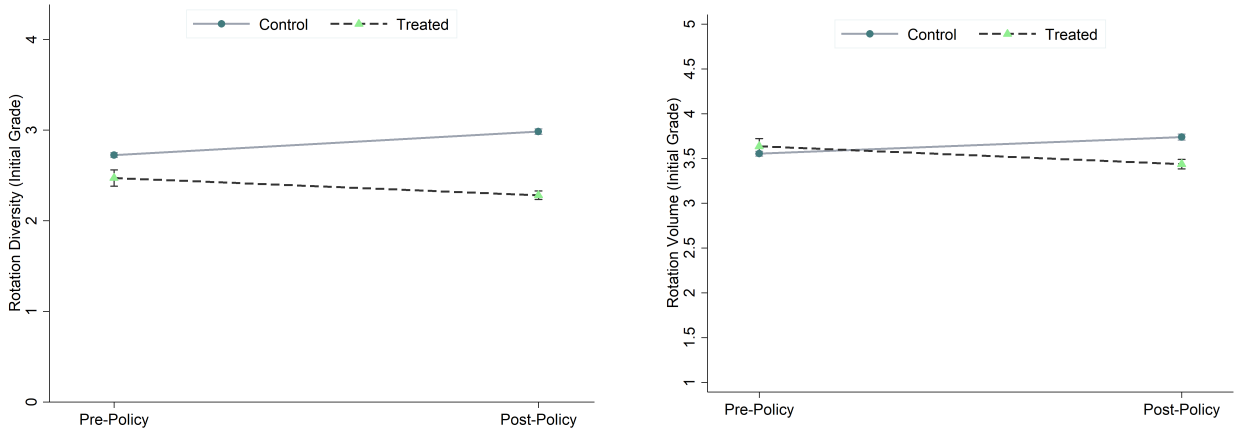


Entry grade, pre vs post

Entry vs Second grade

Figure C3: Average Tenure per Posting (Months), by Treated status, Policy Period, and Grade

Notes: Panel A: mean months per posting in entry grade, pre- and post-policy. Panel B: mean months per posting in entry grade vs second grade, by treatment status. Means with 95% CIs. Sample: cohorts entering in years 1986–2017.



Rotation diversity

Rotation volume

Figure C4: Rotation Diversity and Volume in Entry Grade, by Treated status and Policy Period

Notes: The figure presents difference in means for Control and Treated officers in Rotation diversity and volume before and after policy implementation year. Markers show group means; lines connect pre- and post-policy means; whiskers are 95% CIs. Sample: cohorts entering in years 1986–2017.

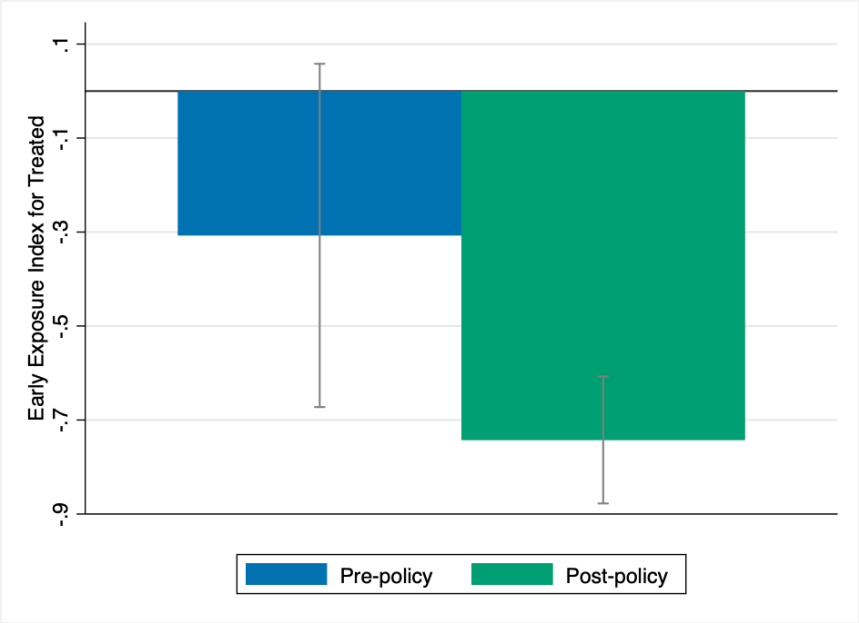


Figure C5: Early-Career Exposure Index Pre- and Post-Policy

Notes: Mean of a standardized index (mean = 0, s.d. = 1) combining diversity, distance, and field intensity in entry grade. Bars: group means; whiskers: 95% CIs. Sample: cohorts entering in years 1986–2017.

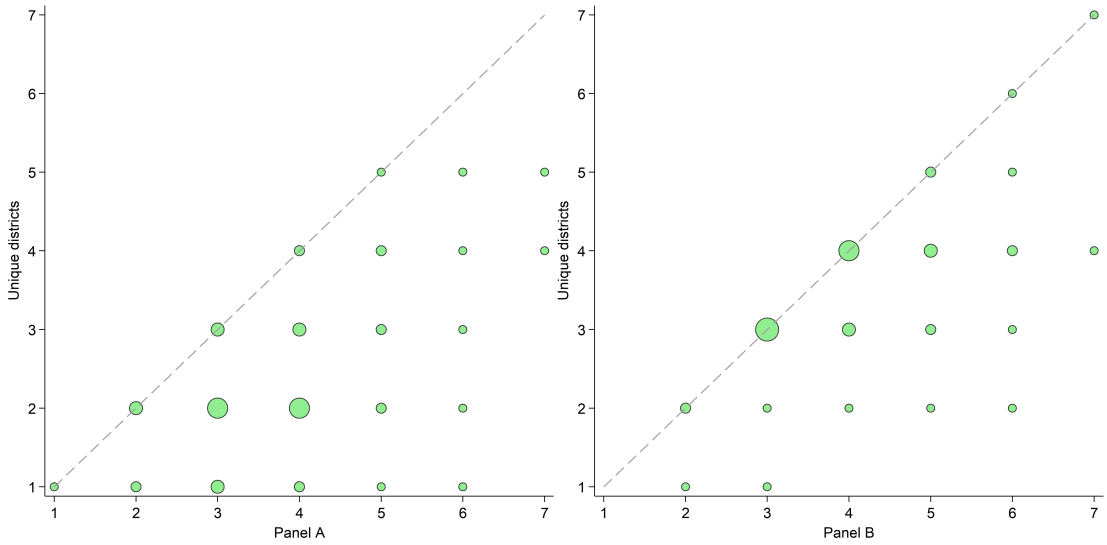


Figure C6: Rotation Diversity vs Volume in Entry Grade and Subsequent Frontline Leadership

Notes: Bubbles are cells of postings (x-axis) and unique districts (y-axis) in entry grade; bubble size is proportional to the number of officers. The 45° line marks equal volume and diversity. Pre- and post-policy cohorts are shown within the same graphic. Association with subsequent *FLM* appointment is discussed in the text. Sample: cohorts entering in years 1986–2010.

Appendix S: Perception-Based Survey

I conducted a one-wave, cross-sectional survey of officers in second to fourth grade and above. The survey was distributed through secure, single-use links sent to a defined official roster; 309 officers completed it. Each link could only be used once, and no duplicate submissions were possible. The survey reached officers across all provinces and grades. Responses are analyzed by cohort, gender (treated), province, and grade composition. Treated group constitutes about 20 percent of respondents; roughly 72 percent entered service after policy.

I estimate a simple officer-level difference-in-differences regression. I estimate:

$$y_i = \alpha_0 + \alpha_1 \,\text{Treat}_i + \alpha_2 \,\text{Post}_i + \alpha_3 \big(\text{Treat}_i \times \text{Post}_i\big) + \lambda_p + \varepsilon_i, \tag{3}$$

where y_i is a belief index or a top-box indicator, Treat_i equals one for treated, Post_i equals one for officers entering after policy, and λ_p are province fixed effects. Standard errors are clustered by province (seven clusters). This specification compares control and treated before and after the policy within provinces. It is descriptive rather than causal: beliefs are measured long after entry, and responses reflect both experience and retrospective interpretation.

TABLE S1: Variable construction and Survey measures

Variable	Components
Panel A: Perceptions of DE Posting and Selection	
Consideration Gap	Treated officers are considered for District Executive (DE) posts at the same rate as control officers.
Mobility Factor	Availability for geographic mobility is the main determinant of women's DE assignments.
Stereotypes Constraint	Selection-board stereotypes are the biggest constraint to women reaching DE roles.
Crisis Management	Treated officers are always or often as effective as control officers at crisis handling in DE.
Supervisor Preference	I feel more comfortable working under a Control/Treated officer (vs. no preference/depends).
Panel B: How DE Shortlists Are Made	
High Ability, Narrow Mobility	An officer who ranks in the top three in trainings but has experience in only one district is likely to be shortlisted for a DE post.
Breadth Of Experience	Holding training grade fixed, serving in multiple districts (ideally including at least one remote posting) increases the likelihood of DE shortlisting.
Panel C: Assignment Returns and System Merit	
DE Track Premium	Serving in district authority roles significantly boosts career progression.
Non-Executive Posting	Lower-visibility ministries are disadvantaged relative to DE roles for promotion.
Low-Visibility Tenure	Continuous service in low-visibility ministries harms career prospects.
System Meritocratic	The posting system is largely meritocratic (ability and exposure dominate).
Home-Province Disadvantage	Women with early home-province careers face disadvantages in assignments.
Rotation Breadth	Rotations across administrative contexts build trust and lead to better postings.
Hard Assignments Help	Hard, frontline assignments—such as district management—improve long-run outcomes.
Early Assignment Matter	Early-career postings (first 5 years) shape long-term assignments and leadership opportunities.
Panel D: Career Aspirations and Mobility Beliefs	
High-Visibility Aspiration	Aspires to serve in a high-priority or high-visibility role.
Apex-Assignment Ambition	Aims to be selected for an apex assignment (secretary-level) and lead a major department.
Mobility a Prerequisite	Geographic mobility is essential for advancement.
Service As Honour	It is not a sacrifice but an honour to serve in the civil services of Pakistan.

TABLE S2: Survey answer options (by variable)

Variable	Possible answers
Panel A: Perceptions of DE Posting and Selection	
Consideration Gap	1 Yes, equally 2 No, less frequently 3 Not sure
Mobility Factor	1 Job performance 2 Availability for mobility 3 Gender norms/assumptions 4 Not sure
Stereotypes Constraint	1 Personal preferences 2 Job performance 3 Selection-board stereotypes 4 Other
Crises Management	1 Always equally effective 2 Often equally effective 3 Sometimes less effective 4 Often less effective 5 No experience with a Treated boss
Supervisor Preference	1 Control supervisor 2 Treat supervisor 3 No preference 4 Depends on the person
Panel B: How DE Shortlists Are Made	
High Ability, Narrow Mobility	1 Yes 2 No 3 Depends
Breadth of Experience	1 Yes 2 No 3 Depends
Panel C: Assignment Returns and System Merit	
DE Track Premium	1 Significantly boosts promotion chances 2 Slightly helps 3 No difference vs. ministries 4 Don't know
Non-Executive Posting	1 Equally considered 2 Slightly disadvantaged 3 Significantly disadvantaged 4 Don't know
Low-Visibility Tenure	1 Strongly agree 2 Agree 3 Neither 4 Disagree 5 Strongly disagree
System Meritocratic	1 Mostly merit 2 Some merit 3 Mostly networks/patronage 4 Personal preferences drive postings 5 Prefer not to say
Home-Province Disadvantage	1 Equal long-run chances 2 Structural disadvantages 3 Faster due to network stability 4 No clear pattern 5 Don't know
Rotation Breadth	1 Trusted with high-stakes/leadership postings 2 Seen as overburdened, slower growth 3 Perceived as ambitious (no clear effect) 4 Not sure
Hard Assignments Help	1 Strongly agree 2 Agree 3 Neither 4 Disagree 5 Strongly disagree
Early Assignments Matter	1 Very influential 2 Somewhat influential 3 Not very influential 4 No influence
Panel D: Career Aspirations and Mobility Beliefs	
High-Visibility Aspiration	1 Yes 2 No 3 Not sure
Apex-Assignment Ambition	1 Reach apex assignment and lead a major department 2 Prefer a relaxed assignment 3 Prefer geographic stability 4 Prefer not to say
Mobility a Prerequisite	1 Essential—mobility is key 2 Somewhat important 3 Not important—can rise locally 4 Not sure
Service as Honour	1 True 2 Comes with significant personal sacrifices 3 No opinion

TABLE S3: Summary Statistics of Civil Servants in Survey Sample

Variable	Mean	SD	Sample
Panel A: Demographics and Cohort			
Treat	0.20	0.40	309
Post	0.72	0.45	309
Treat × *Post*	0.17	0.38	309
Panel B: Province of Domicile			
Punjab	0.50	0.50	309
Sindh	0.15	0.35	309
Khyber Pakhtunkhwa	0.18	0.39	309
Balochistan	0.09	0.29	309
GB/FATA	0.05	0.22	309
Azad Jammu and Kashmir (AJK)	0.01	0.11	309
Panel C: Grade			
Second grade	0.41	0.49	309
Third grade	0.31	0.46	309
Fourth grade or above	0.28	0.45	309
Panel D: Grade by treatment status			
Second grade (treated)	0.13	0.33	309
Third grade (treated)	0.05	0.21	309
Fourth grade or above (treated)	0.03	0.17	309
Second grade (control)	0.28	0.45	309
Third grade (control)	0.26	0.44	309
Fourth grade or above (control)	0.25	0.43	309

Notes: Means represent the fraction of respondents with value = 1. *Post* refers to officers who entered after the policy was implemented.

TABLE S4: Responses about Career Appointments

Dependent variable	Considered for High Priority	More Rotations	Stereotypes	Management Skills	Supervisor Preference
	(1)	(2)	(3)	(4)	(5)
Treat	-0.163***	0.143***	0.215***	-0.250***	0.477***
	(0.023)	(0.036)	(0.031)	(0.052)	(0.079)
Post	0.020	-0.030	-0.026	-0.050	0.012
	(0.031)	(0.063)	(0.037)	(0.056)	(0.038)
Treat × *Post*	-0.258***	-0.107**	0.041	0.116*	-0.407**
	(0.054)	(0.038)	(0.060)	(0.054)	(0.110)
Province FE	✓	✓	✓	✓	✓
Observations	309	309	309	309	309
R^2	0.111	0.039	0.100	0.045	0.059

Notes: Each column reports a separate regression with province fixed effects. Standard errors are clustered at the province level; the number of clusters is 7 in all columns. The interaction of interest is *Treat* × *Post*. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

TABLE S5: Responses about System Fairness and Remote Experience

Dependent variable	Narrower Rotation Context	Broader Rotation Context
	(1)	(2)
Treat	-0.071	0.320**
	(0.129)	(0.116)
Post	-0.010	-0.091**
	(0.075)	(0.032)
Treat × *Post*	-0.023	-0.283**
	(0.172)	(0.105)
Province FE	✓	✓
Observations	309	309
R^2	0.042	0.036

Notes: Each column reports a separate regression with province fixed effects. Standard errors are clustered at the province level; the number of clusters (provinces) is 7 for all regressions. The interaction of interest is *Treat* × *Post*. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

TABLE S6: Responses about Career Progression and Meritocracy

Dependent variable	High Priority Placements (1)	Low Priority Placements (2)	Repeated Placements in Low-Priority (3)	Placements Meritocratic (4)
Treat	0.263	0.083	0.255***	0.227***
	(0.221)	(0.124)	(0.027)	(0.057)
Post	0.007	0.016	-0.023	-0.012
	(0.104)	(0.017)	(0.041)	(0.039)
Treat × *Post*	-0.122	-0.002	-0.227**	-0.195**
	(0.213)	(0.169)	(0.082)	(0.064)
Province FE	✓	✓	✓	✓
Observations	309	309	309	309
R^2	0.037	0.034	0.020	0.027

Notes: Each column reports a separate regression with province fixed effects. Standard errors are clustered at the province level; the number of clusters (provinces) is 7 for all regressions. The interaction of interest is *Treat* × *Post*. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

TABLE S7: Responses about Mobility and Rotations

Dependent variable	Home Province Disadv. (1)	Serving in One Province (2)	Challenging Assignments Help (3)	Importance of Early Career Placements (4)
Treat	0.362***	0.146	0.247***	0.032
	(0.040)	(0.117)	(0.018)	(0.042)
Post	-0.027	-0.035	-0.040*	-0.007
	(0.054)	(0.038)	(0.020)	(0.065)
Treat × *Post*	-0.214***	-0.103	-0.272***	-0.068
	(0.046)	(0.070)	(0.025)	(0.086)
Province FE	✓	✓	✓	✓
Observations	309	309	309	309
R^2	0.044	0.010	0.039	0.031

Notes: Each column reports a separate regression with province fixed effects. Standard errors are clustered at the province level; the number of clusters (provinces) is 7 for all regressions. The interaction of interest is *Treat* × *Post*. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

TABLE S8: Responses about Motivation and Identity

Dependent variable	High-Priority Aspiration (1)	Apex-Assignment Aspiration (2)	Diversity of Exposure (3)	Service as Honour (4)
Treat	0.092	-0.007	0.272**	0.270***
	(0.102)	(0.094)	(0.086)	(0.037)
Post	0.094	0.096	0.036	-0.008
	(0.082)	(0.066)	(0.072)	(0.049)
Treat × *Post*	-0.083	-0.123	-0.337***	-0.342***
	(0.115)	(0.127)	(0.070)	(0.052)
Province FE	✓	✓	✓	✓
Observations	309	309	309	309
R^2	0.034	0.035	0.017	0.050

Notes: Each column reports a separate regression with province fixed effects. Standard errors are clustered at the province level; the number of clusters (provinces) is 7 for all regressions. The interaction of interest is *Treat* × *Post*. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix R: Robustness Check of Main Result

This appendix provides additional robustness checks and supplementary material. In particular, I report alternative specifications, expanded control sets, and event-study estimates that complement the main results.

This section presents a set of robustness exercises that complement the baseline specification in Table 5. The goal is to ensure that the negative effect after policy on the standardized value of treated officers in achieving *FLM* positions is not an artifact of sample composition, outcome definition, or estimation choices.

In Table R1 I re-estimate the baseline specification excluding provinces that might disproportionately influence results. First, I exclude Balochistan, which contributes relatively few observations and has distinct institutional dynamics. Next, I exclude other small provinces. Finally, I exclude Punjab, the largest province, and Sindh to confirm that results are not mechanically driven by scale. Across all specifications, the estimated negative policy effect for treated remains stable.

To rule out compositional effects, I control for the treated share of each *Cohort* × *Province* cell. Results in table Table R2 show that the post-policy drop in leadership appointments is not explained by changes in cohort composition.

To verify that the results are not driven by the standardization of the dependent variable, I re-estimate the main specification using a binary outcome equal to one if the officer ever attained the *FLM* in table Table R3. Column (1) reports estimates from a linear probability model (OLS), while Columns (2) and (3) report average marginal effects from logit and probit models, respectively. All specifications include cohort and province fixed effects as well as the same set of controls used in the baseline regression.

One concern is that the decline in leadership for treated may simply reflect variation in the availability of *FLM* seats across provinces and cohorts. To address this, I normalize attainment by seat supply. Specifically, for each *Cohort* × *Province* cell, I compute the average annual number of second grade *FLM* positions available in the first five years of eligibility. Dividing by the number of entrants in the same *Cohort* × *Province* yields the expected probability of attaining a *FLM* post, denoted seat $share_{cp}$. I then construct an excess attainment measure for each officer:

$$FLM_{excessi} = FLM_{everi} - seat_share_{cp}$$

.

Re-estimating the baseline specification with $FLM_{excessi}$ as the dependent variable yields results consistent with the baseline. The *Treat* × *Post* penalty is not explained by mechanical changes in

seat supply.

To verify that results are not driven by a single influential cohort, I re-estimate the baseline reduced-form specification while sequentially excluding one entry cohort at a time. The *Treat* × *Post* coefficient remains stable in both sign and magnitude across all specifications. This demonstrates that the results are not an artifact of a particular cohort.

Re-estimate the main difference-in-differences specification (*Treat* × *Post*), with *Cohort* × *Province* fixed effects, baseline controls, and clustered SEs at the birth-district level) using the following three dependent variables:

1. Ever attained *FLM* (binary): = 1 if officer ever holds a *FLM* post at second grade.
2. Seat-adjusted *FLM* attainment (excess): attainment minus expected seat share in the officer's *Cohort* × *Province* cell.
3. Standardized *FLM* index (baseline): standardized (z-scored) measure of *FLM* attainment, consistent with the baseline specification.

TABLE R1: Robustness check Excluding Provinces

Dependent variable	*FLM* Attainment					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	-0.006	-0.010	-0.010	-0.030	0.064	-0.015
	(0.086)	(0.085)	(0.085)	(0.090)	(0.177)	(0.088)
Treat × *Post*	-0.349***	-0.324***	-0.329***	-0.290***	-0.456**	-0.328***
	(0.097)	(0.095)	(0.095)	(0.095)	(0.195)	(0.102)
	(0.139)	(0.143)	(0.143)	(0.152)	(0.311)	(0.153)
Cohort & Province FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Dropped Province	Balochistan	AJK	FATA	KPK	Punjab	Sindh
Mean DV	.390	.398	.397	.384	.471	.383
Observations	914	947	961	818	412	788
R^2	0.251	0.249	0.253	0.253	0.318	0.250

Notes: Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and baseline controls (entry-exam scores, education, and age at entry). The specifications are identical across columns; each column drops one province from the baseline sample. Column (1) excludes Balochistan, column (2) excludes AJK, column (3) excludes FATA, column (4) excludes KPK, column (5) excludes Punjab, and column (6) excludes Sindh. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors, reported in parentheses, are clustered by district of first posting. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE R2: Robustness Check: Cohort Composition

Dependent variable	*FLM* Attainment					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.001	-0.012	-0.010	-0.066	-0.012	-0.020
	(0.080)	(0.085)	(0.085)	(0.089)	(0.080)	(0.083)
Treat × *Post*	-0.323***	-0.323***	-0.325***	-0.553***	-0.323***	-0.418***
	(0.093)	(0.099)	(0.098)	(0.123)	(0.095)	(0.107)
Share Treat in Cohort × *Province*	-0.093	-0.080	0.003	-0.052	-0.080	-0.016
	(0.153)	(0.153)	(0.143)	(0.149)	(0.130)	(0.226)
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Standard controls		✓	✓	✓	✓	✓
Seat-adjusted (capacity)			✓			
Linear Gender Trends				✓		
Mean DV	.398	.398	.341	.398	.398	.449
Observations	968	968	968	968	968	665
R^2	0.222	0.253	0.214	0.258	0.253	0.218

Notes: The dependent variable is an indicator for *FLM* attainment. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), the share of treated officers in *Cohort* × *Province*, and the fixed effects listed. Column (1) is the baseline; column (2) adds standard controls (entry-exam scores and demographics); column (3) uses a seat-adjusted outcome; column (4) adds linear treated-specific trends; column (5) is as in column (2) but clusters standard errors by *Cohort* × *Province*; and column (6) restricts the sample to cohorts ≤ 2010 to ensure a balanced follow-up window. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. "Mean DV" reports the mean of the dependent variable. The sample consists of officers who achieved second grade in cohorts 1986–2017. Standard errors are clustered by first-posting district; robust standard errors are in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE R3: Robustness Check: Binary Outcomes

Dependent variable	*FLM* Attainment		
	OLS	Logit (AME)	Probit (AME)
Treat	-0.011	-0.025	-0.011
	(0.085)	(0.433)	(0.254)
Treat × *Post*	-0.331***	-2.135***	-1.291***
	(0.095)	(0.513)	(0.301)
Cohort FE	✓	✓	✓
Province FE	✓	✓	✓
Controls	✓	✓	✓
Mean DV	.398	.398	.398
Observations	968	901	901
R^2	0.253		

Notes: The dependent variable is a binary indicator for *FLM* attainment. Each column reports estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and standard controls (entry-exam scores, education, and age at entry). Column (1) reports coefficients from a linear probability model estimated by OLS. Columns (2) and (3) report average marginal effects (AMEs) from logit and probit models, respectively. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The estimation sample in columns (2)–(3) excludes cohort–province cells with no within-cell variation in the outcome (perfect prediction in nonlinear models). Standard errors are clustered by first-posting district; robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE R4: Seat-Adjusted Outcomes

Dependent variable	Seat-Adjusted *FLM* Attainment					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.001	-0.010	0.018	-0.062	-0.010	-0.018
	(0.080)	(0.085)	(0.086)	(0.089)	(0.080)	(0.083)
Treat × *Post*	-0.325***	-0.325***	-0.358***	-0.543***	-0.325***	-0.410***
	(0.089)	(0.094)	(0.101)	(0.119)	(0.094)	(0.102)
Cohort FE	✓	✓		✓	✓	✓
Province FE	✓	✓		✓	✓	✓
Standard controls		✓	✓	✓	✓	✓
Province × *Cohort* FE			✓			
Linear Gender Trends				✓		
Mean DV	.341	.341	.341	.341	.341	.393
Observations	968	968	950	968	968	665
R^2	0.181	0.214	0.255	0.219	0.214	0.181

Notes: The dependent variable is an indicator for *FLM* attainment. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), the share of treated officers in *Cohort* × *Province*, and the fixed effects listed. Column (1) is the baseline. Column (2) adds standard controls (entry-exam scores and demographics). Column (3) includes *Province* × *Cohort* fixed effects. Column (4) adds linear treated trends. Column (5) is as in column (2) but clusters standard errors by *Cohort* × *Province*. Column (6) restricts the sample to cohorts ≤ 2010 to ensure a balanced follow-up window. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. The sample consists of officers who achieved second grade in cohorts 1986–2017. "Mean DV" reports the mean of the dependent variable. Standard errors are clustered by first-posting district; robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE R5: Leave-One-Cohort-Out Analysis

Dependent variable	*FLM* Attainment									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat	0.008	-0.036	-0.013	-0.011	-0.008	-0.010	-0.013	-0.009	-0.012	-0.008
	(0.096)	(0.106)	(0.110)	(0.086)	(0.085)	(0.084)	(0.085)	(0.085)	(0.086)	(0.086)
Treat × *Post*	-0.347***	-0.311***	-0.321***	-0.329***	-0.330***	-0.336***	-0.322***	-0.328***	-0.326***	-0.305***
	(0.108)	(0.115)	(0.112)	(0.098)	(0.096)	(0.095)	(0.094)	(0.095)	(0.094)	(0.096)
Cohort & Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Dropped Cohort	-3	-2	-1	0	1	2	3	4	5	6
Mean DV	.396	.422	.396	.387	.393	.383	.387	.399	.393	.387
Observations	814	868	811	940	942	925	928	932	927	928
R^2	0.290	0.250	0.297	0.244	0.254	0.241	0.245	0.258	0.255	0.241

Notes: The dependent variable is an indicator for *FLM* attainment. Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and standard controls (entry-exam scores, education, and age at entry). The specifications are identical across columns; each column drops one entry cohort relative to the policy cutoff. Column (1) drops cohort 3, column (2) drops cohort 2, column (3) drops cohort 1, column (4) drops cohort 0, column (5) drops cohort 1, column (6) drops cohort 2, column (7) drops cohort 3, column (8) drops cohort 4, column (9) drops cohort 5, and column (10) drops cohort 6. The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. Standard errors are clustered by district of first posting. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE R6: Alternative Outcomes

Dependent variable	*FLM* Attainment	Seat-Adjusted *FLM*	Standardized *FLM* Index
	(1)	(2)	(3)
Treat	-0.011	-0.010	-0.023
	(0.085)	(0.085)	(0.174)
Treat × *Post*	-0.331***	-0.325***	-0.675***
	(0.095)	(0.094)	(0.193)
Cohort × *Province* FE	✓	✓	✓
Controls	✓	✓	✓
Mean DV	.398	.341	
Observations	968	968	968
R^2	0.253	0.214	0.253

Notes: The dependent variables are alternative measures of *FLM* attainment. Column (1) uses a binary outcome for ever attaining a *FLM* post, column (2) uses a seat-adjusted *FLM* attainment measure (excess relative to capacity), and column (3) uses a standardized *FLM* index (baseline specification). Each column reports OLS estimates of the interaction of a treated indicator with a post-policy indicator (*Treat* × *Post*), controlling for a treated indicator (*Treat*), cohort fixed effects, province fixed effects, and standard controls (entry-exam scores, education, and age at entry). The post-policy main effect is absorbed by cohort fixed effects and therefore not reported; the coefficient on *Treat* × *Post* identifies the differential post-policy change for treated officers relative to untreated officers. Standard errors are clustered by district of first posting. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE R7: *FLM* Attainment: Post-Policy

Dependent variable	*FLM* Attainment	
	Treat	Control
	(1)	(2)
Post	-0.184*	0.184***
	(0.092)	(0.054)
Province FE	✓	✓
Observations	190	778
R^2	0.053	0.053

Notes: The dependent variable is an indicator for *FLM* appointment. Column (1) reports OLS estimates of a post-policy indicator for treated officers, and column (2) reports estimates for control officers. Both specifications include province fixed effects. Standard errors are clustered by first-posting district. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix E: Mediation Analysis

Mediation with Multiple Channels

To quantify the mechanisms, I extend the causal mediation framework (Imai et al., 2010a,b) to allow for multiple mediators: assignment diversity (D), volume of postings (V), and performance evaluations (P). Let T_i denote the policy (*Treated* × *Post*) and Y_i the executive outcome. A first-order decomposition is:

$$\frac{d\,\mathbb{E}[Y_i]}{dT_i} = \underbrace{\frac{\partial \mathbb{E}[Y_i]}{\partial D_i}\frac{d\,\mathbb{E}[D_i]}{dT_i}}_{\text{ACME}_D} + \underbrace{\frac{\partial \mathbb{E}[Y_i]}{\partial V_i}\frac{d\,\mathbb{E}[V_i]}{dT_i}}_{\text{ACME}_V} + \underbrace{\frac{\partial \mathbb{E}[Y_i]}{\partial P_i}\frac{d\,\mathbb{E}[P_i]}{dT_i}}_{\text{ACME}_P} + \underbrace{R}_{\text{ADE / residual}} . \tag{3}$$

I report both mediator-specific ACMEs and a joint ACME including (D, V, P) simultaneously to avoid double-counting. These estimates should be interpreted as *reduced-form gauges* of the assignment channel, not structural parameters.

TABLE E1: Mediation Analysis of policy Effect on Frontline Leadership

Mediator set in model	Indirect (ACME) (1)	Direct (ADE) (2)	Total Effect (3)	Share Mediated (%) (4)
Diversity (unique districts)	−0.101***	−0.198***	−0.303***	33.3
	(0.025)	(0.032)	(0.048)	(4.8)
Volume (no. postings)	−0.009·	−0.294***	−0.303***	3.0
	(0.005)	(0.047)	(0.049)	(1.4)
Performance evals	−0.003·	−0.301***	−0.303***	1.1
	(0.004)	(0.048)	(0.049)	(1.1)
Joint (D, V, P)	−0.112***	−0.184***	−0.303***	36.8
	(0.028)	(0.033)	(0.048)	(5.8)

Notes: Table reports a mediation decomposition of the effect of the exemption policy on frontline attainment. The indirect effect (ACME) equals the product of the policy's effect on the mediator (path a) and the mediator's effect on DC attainment (path b). The direct effect (c') is the residual policy effect controlling for the mediator. The total effect (c) is the reduced-form policy effect. Standard errors are clustered at the city level, 110 clusters for each column. The number of observations is 993, while number of bootstrap replications is 1,000 for each column. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E2: Mediation Analysis of policy Effect on Frontline Leadership (Trimmed Sample)

Mediator set in model	Indirect (ACME)	Direct (ADE)	Total Effect	Share Mediated (%)
	(1)	(2)	(3)	(4)
Diversity (unique districts)	-0.138^{***}	-0.098^{***}	-0.234^{***}	58.8
	(0.052)	(0.037)	(0.073)	(12.8)
Volume (no. postings)	$-0.009^{\cdot}$	-0.226^{***}	-0.234^{***}	3.8
	(0.008)	(0.071)	(0.075)	(2.6)
Performance evals	$0.006^{\cdot}$	-0.242^{***}	-0.234^{***}	-2.6
	(0.005)	(0.073)	(0.074)	(2.4)
Joint (D, V, P)	-0.143^{***}	-0.090^{**}	-0.234^{***}	61.0
	(0.052)	(0.040)	(0.072)	(11.2)

Notes: Table reports mediation decomposition of the effect of the exemption policy on *FLM* attainment. Indirect effect is the product of the effect of the policy on the mediator (path a) and the effect of the mediator on *FLM* attainment (path b). Direct effect (c') is the residual effect of the policy controlling for the mediator. Total effect (c) is the reduced-form policy effect. Standard errors are clustered at the city level, 93 clusters for each column. The number of observations is 714, while number of bootstrap replications is 1,000 for each column. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.