Lead by Example: The Effect of Supervisors on Police Behavior

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Abstract

Police agencies rely on chain of command to hold officers accountable in their job duties. Using the exogenously-timed rotation of officers within the Dallas Police Department, I document substantial heterogeneity in the enforcement preferences of first-line supervisors. Variation in supervision accounts for 3.4% of the total variation in monthly officer arrests. I find that supervisor preferences can affect arrests for both serious and low-level crimes, however preferences along each dimension are not systematically correlated. I show that, on the margin, low-level arrests induced by supervisors are more likely to be officer-initiated, are concentrated in drug-related crimes, and lead to a substantial increase in officer use of force. Supervisors who value low-level enforcement are more likely to patrol in the field and make their own low-level arrests, effectively leading by example. In contrast, supervisor-induced arrests for serious crimes arise exclusively from activity at 911 calls, where arrests for domestic violence increase significantly. I conclude by demonstrating that performance on the exams used to determine promotions are predictive of supervisor preferences. My findings illustrate that supervision is an important source of discretionary law enforcement practices.

1 Introduction

A primary goal of police reform is aligning the behaviors of police officers with the best interests of the public. However despite decades of reform-focused policies passed across the US — such as Early Intervention Systems, body cameras, and performance monitoring technologies - over a third of Americans still report not trusting police to do what is right in most situations (Quinnipiac, 2023). One of the key difficulties in developing effective reform policy is the lack of traditional workplace incentives within police organizations. Unlike private sector firms (e.g. Lazear, 2018), police departments are unable to motivate their officers using pay, promotion, or the threat of termination. Absent such incentives, policing relies on its rigid top-down command structure. No leadership role has more contact with public-facing

officers than sergeants - the front-line supervisors of policing - who issue command directives, provide officers with advice in uncertain situations, and recommend them for promotion or transfer to specialized units. Given the role occupied by sergeants, a crucial question, then, is whether the incentives that they provide to officers can meaningfully change how those officers interact with the public. To the extent that they do, can supervisors be an effective target for police reform policy?

I study this question in the context of patrol officers, who exercise a high degree of discretion over whether to make an arrest (Weisburst, 2024). I estimate the unique effect of each sergeant on the number of arrests made by officers working under their supervision and show that supervisors vary substantially along this dimension. These estimates imply that changing an officer's supervisor, and, more broadly, changing the stock of supervisors, can meaningfully alter the number of arrests made by a department. Next, I disaggregate the supervisor effects into effects on arrests for serious and low-level crimes. I show that effects along these dimensions are largely independent from one another and operate through distinct forms of officer behavior. I then analyze supervisor patrol activities as a potential channel through which supervisors can affect their subordinates' decisions via behavioral modeling—a form of "leading by example." I conclude by exploring how departments might identify the most effective supervisors by examining the relationship between supervisor characteristics and the different dimensions of their arrest effects.

To identify the effects of a supervisor on arrests, I leverage the mobility of officers within the Dallas Police Department, who frequently switch supervisors throughout their careers. Using a two-way fixed effects model (Abowd et al., 1999), I identify a supervisor effect using changes in the number of arrests made by officers when they transition to or from that supervisor. Officers in Dallas cannot control the timing of these transitions, which are determined by vacancies and predetermined schedule realignments. While my model accommodates sorting on fixed characteristics, I ensure that sorting is not influenced by trends in officer behavior or crime rates, concurrent policy changes, or match quality, which is controlled by the supervisor assignment mechanism in my setting. Even after applying bias corrections to both point estimates (Chetty et al., 2014) and the variance of the fixed effects distribution (Kline et al., 2020), I find substantial differences in officer enforcement behavior under different supervisors. Specifically, moving an officer from a supervisor in the 10th percentile to one in the 90th percentile results in 1.6 additional arrests per month, representing a 42% increase relative to the mean. Replacing the top 10% of supervisors (30 sergeants) with average supervisors would reduce total arrests by 2.5% per officer per year. Estimating an event study around moves, I find that arrests are highly responsive to a change in supervisors and these effects do not atrophy over time, suggesting that sergeants significantly alter the incentives faced by their officers.

I disaggregate total arrests into serious (e.g. violent/property crimes, DWI, fraud) and low-level (e.g. drug possession, warrants, disorderly conduct) categories. I show that when officers move to a supervisor

who induces more arrests, their enforcement increases in comparable sizes along both serious and low-level margins. These composite effects are driven not by a uniform increase in effort among officers working for high-arrest supervisors, but rather substantial heterogeneity in enforcement preferences across supervisors. When separately estimating supervisor effects for low-level and serious arrests, I find for a given supervisor that these effects are largely uncorrelated, implying that harsh enforcement for low-level crimes is not necessarily a downstream consequence of enforcing violent and property crimes. Indeed, I show that changes in low-level and serious effects of supervisors lead to changes in officer behavior along distinct dimensions. When the low-level effect of a supervisor increases by 1 standard deviation, their officers make 54% more drug arrests relative to the mean. Over half of the increase in overall low-level arrests can be attributed to officer-initiated interactions rather than civilian-initiated 911 calls, indicating that officers dedicate substantially more effort into detecting low-level crimes when working with supervisors who have a preference for making such arrests. I find evidence that more low-level interactions result in officers using more force, as use of force incidents increase by 15% when the low-level effect of a supervisor increases by 1 standard deviation.

In contrast, as a supervisor's preference for serious arrests grows stronger, officers working for them increase arrests entirely through 911 calls. These effects reflect, in part, enhanced officer effort in responding to calls outside of their beat - as Dallas allows officers to volunteer for calls in the case that officers assigned to the relevant beat are busy. However, officers also increase the *probability* that they make arrests conditional on a call. I find that the serious effect operates in large part through arrests for domestic violence, which increase by 35% when the serious effect of an officer's supervisor increases by 1 standard deviation. In my setting these arrests are highly discretionary, since Texas does not have mandatory arrest laws for domestic violence.

In total, conviction rates increase under supervisors who are high along both dimensions, however it is larger for supervisors with a high serious effect. (Need more in this paragraph). I do not find evidence that changes in supervisor effects along either dimension change crime rates overall within their management jurisdiction.

I then investigate the mechanisms through which supervisors are able to influence their subordinates' behaviors. In particular, I examine the association between the different dimensions of supervisor effects and supervisor activity. Supervisors can choose between staying in the station or patrolling in the field during their shift. Ethnographic evidence suggests that their preference between these two alternatives matters for their ability to incentivize their officers, since a "street sergeant" may be respected more for their knowledge of the officer's work environment than the "station house sergeant" (Van Maanen, 1984). Moreover, field studies have shown that officers place greater value on the types of activities they believe their supervisors prefer, and street sergeants may be able to more easily model their preferred behaviors for officers (Johnson, 2015b).

Consistent with previous evidence, I find that supervisors who induce low-level arrests are more likely to be street sergeants, responding to calls independently and making their own arrests. Importantly, the arrests that these supervisors make are often for low-level crimes, suggesting that supervisors who encourage such arrests do so through leading by example and demonstrating the importance of these arrests through their own behavior. As a supervisor's low-level effect increases, they are also more likely to be assigned to their subordinates' calls, enabling them to give direct guidance to their officers in uncertain situations. However, I do not find evidence that supervisors with a large effect on serious arrests differ systematically in either direction along my measures of field activity. While data limitations prevent me from observing all of the channels through which supervisors can influence their subordinates, anecdotal evidence suggests that supervisors who value serious arrests can provide officers with direct advice on how to handle situations through radio correspondence Brooks (2021).

I investigate the correlates of supervisor preferences along both dimensions by comparing the distribution of supervisor effects across pre-promotion characteristics, leveraging detailed personnel records including newly obtained data on the exams used to determine promotion to sergeant. While I find no significant differences across race, gender, or age at the time of the exam, I find evidence that the low-level effect distribution for above-average scorers on the promotional exam is shifted to the left of the same effect distribution for below-average scorers. Moreover, high scorers have significantly more spread in their serious arrest effect compared to low scorers. Together, these findings suggest that the best promotion candidates tend place lower weight on low-level arrests compared to marginal candidates, however their involvement in serious arrests is significantly more variable. between an officer's score on the promotional exam and their propensity to induce low-level arrests as a sergeant.

Overall, my results suggest that policy targeting first-line supervision in policing may be effective at modifying officer behavior. However, the construction of this policy depends critically on the objectives of the department. For example, if a department wanted to reduce discretionary enforcement for low-level crimes, my results suggest that training targeted toward marginal promotees who are lower on the promotion could be particularly effective.

This paper contributes to two key strands of literature. First, it advances research on the determinants of police discretion, particularly in the form of institutional features, by examining the role of supervision in police outcomes (Mummolo, 2018). While previous studies have shown that variation in top-level police command can explain broad tactical strategies such as stops and searches (e.g. Bacher-Hicks and De La Campa, 2020; Kapustin et al., 2022), my findings reveal that first-line supervisors play a more significant role in explaining selective enforcement via arrests. My work complements recent studies by Frake and Harmon (2023) and Gudgeon et al. (2023), who demonstrate that supervisors influence subordinate behavior based on their prior exposure to misconduct and their race, respectively. Despite the different institutional contexts of our studies, a shared conclusion is that supervision crucially shapes police

outcomes. Ignoring supervisors in discussions of officer preferences risks overstating the importance of individual officers (Weisburst, 2024). By estimating supervisor-specific effects, I capture the rich heterogeneity across supervisor characteristics, enabling a more comprehensive assessment of policies targeting supervisors.

In addition to contributing to the literature on supervision and management, this paper complements research on the influence of peers in shaping police discretion (e.g. Rivera, 2022; Holz et al., 2023). While much of the work on peer effects has focused on relationships formed during an officer's early-career training period (Adger et al., 2022), I demonstrate that supervisors, as peers, exert influence deep into an officer's career. Outside of economics, my findings contribute to a long-standing debate within criminology about the ability of sergeants to shape police behavior (Van Maanen, 1984; Brown, 1988). A substantial body of observational fieldwork has documented correlations between supervisor behaviors and officer decisions (Engel, 2000, 2001, 2002; Engel and Worden, 2003; Johnson, 2011, 2015b,a; Ingram et al., 2014), but to my knowledge, this paper is the first to establish a causal connection between specific supervisors and the behaviors of the officers they manage. Notably, my findings that high-arrest supervisors are often the most active in the field align with earlier observational work by Engel (2000).

Second, I contribute to work within labor economics on the importance of managers (Bertrand and Schoar, 2003; Lazear et al., 2015; Adhvaryu et al., 2020). My findings illustrate that even in settings where managers have limited direct oversight of their employees, they can still play a meaningful role in shaping job outcomes. This work extends the recent literature on the effects of managers in the public sector (Bloom et al., 2015; Rasul and Rogger, 2018; Fenizia, 2022) by providing estimates of manager effects for public-facing "street-level bureaucrats." While the "output" in policing is less directly tied to specific employee behaviors than in other public bureaucracies, my results show that management remains crucial in the operation of public sector organizations.

2 Supervision in Policing

Sergeants are the first level of management within policing. Within each branch of a police department, officers are divided into units, each of which is led by a Sergeant. In patrol, which often makes up the largest sector of a police department and which I focus on in this paper, Sergeants are assigned a unit of 6 to 8 officers to patrol within an area of the city. In Dallas, the location of this study, Sergeants are in charge of 1 of 35 sectors within the city. There is at least one Sergeant assigned for each sector on each of the three watches, or shifts (which correspond, roughly, to overnight, day, and evening).[1] Sergeants are responsible for overseeing officer behavior and ensuring that officers are compliant with the department's bylaws and high-level command priorities. As the frontline managers who have the most direct contact

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[1]For especially large or crime-ridden sectors, there may be more than one sergeant assigned. Each of these sergeants manages their own unit of officers.

with officer on the ground, they have a number of informal interactions with officers that have potential to shape their officers' actions and perspectives. In conversations with Dallas Police Department Sergeants, this informal channel has been emphasized, with one supervisor I spoke to emphasizing his duty to "lead by example" for his officers.

At the beginning of each day, sergeants conduct a roll call where they take attendance and give location assignments to their officers. During this period, they also communicate their patrol priorities. Officers spend they day patrolling their assigned beats and responding to calls for service. Supervisors are expected to be available to assist officers and provide guidance in the field if a request is made through police radio. In cases where officers are uncertain about the appropriate actions, including whether a custodial arrest is appropriate, they may consult a supervisor directly. While officers may receive field assistance from other supervisors within their division, direct supervisors are typically assigned their officers' calls if available. While monitoring the radio, sergeants can choose whether to spend time in the office or conduct patrol activities in the field. Supervisors in the field have discretion to make arrests and traffic stops as they see fit, however this may be considered undesirable since it creates extra work for their officers. [2]

Outside of direct field interaction, supervisors have several channels for monitoring and modifying their subordinates' behaviors. Supervisors are expected to review and approve use of force and arrest reports. They are expected to review patterns in consensual searches and citation writing for their officers and spot check body-worn camera footage. They conduct performance reviews and write recommendations for promotion. Additionally, they can document formal disciplinary action for violating department procedures or commendations for exemplary behavior (Rim et al., 2024). Given their direct contact with officers, Sergeants also have a wide scope for informal interactions that provide advice or models for ideal behavior. Sergeants may communicate their preferences for enforcement with the officers that they oversee. Some examples based on anecdotal conversations include Sergeants telling their subordinates that they dislike report-writing or that they have few constraints in making arrests.

It is not uncommon for officers to occasionally work in a different sector on their watch. In those cases, the officer's primary supervisor would not change, however they would receive roll call from the supervisor in charge of the sector in which they work on that day. Their regular supervisor would still be in charge of monitoring trends in their activity and holding the officer accountable for actions taken in the field. Moreover, outcomes that affect officer careers in the long-term, like promotional recommendations, are only conducted by the regular supervisor. In conversations with members of the Dallas Police Department, one supervisor that I spoke with stated that the actions of his officers were reported back to him

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[2]Anecdotally, the decision to spend time in the office or in the field comes down the supervisors' preferences. One former sergeant that I spoke to mentioned that he disliked sitting in the office so he spent as much time as he could in the field. This dichotomy between active "street" sergeants and "station house" sergeants has been documented previously in ethnographic and observational studies (see: Van Maanen, 1984; Engel, 2001)

whenever they worked outside of his sector. Even when officers work in their primary sector, they will have a different supervisor on days in which the regular supervisor is off work. The relief supervisor will either be a floating supervisor, whose primary job is to cover supervisors on their days off, or a supervisor from another sector.

Patrol officers change supervisors frequently throughout their career, and they have limited discretion to determine who they are. An officer's supervisor will change if the officer is reassigned to a different sector within their division or if their current unit receives a new supervisor. Reassignments occur because of officer or supervisor vacancies that are generated by promotion, retirement, death, or transfers into specialized units. Unlike most other large police departments, which allow officers to select into vacancies based on their tenure in the department, Dallas does not have a similar procedure to fill vacancies. [3] Instead, officer vacancies may be filled within a division and watch at the discretion of command staff. When Sergeant vacancies occur, other Sergeants can interview for the opening, but the final transfer decision is unrelated to seniority.

Once a year, command staff determines staffing needs within each of the patrol divisions, shifts, and days of the week. If it is determined that large scale staffing changes are needed, then the Chief can implement a Patrol Bid, which allows Sergeants and officers to choose their division, shift, and day-off groups in descending order according to time in rank. Since the bid can only occur once per year[4], this limits the ability of officers to sort on *trends* in crime or behavior, which is crucial to the identification strategy that I discuss in Section 4. More importantly, the bid does not determine the sector in which they work. These assignments are up to the discretion of Division Commanders, meaning that officers have no scope to select their supervisory assignments, even if they may have some ability to select where and when they work. [5]

3 Data

This project uses several administrative datasets obtained via FOIA request from the Dallas Police Department and Dallas County District Attorney's Office, covering June 2014 to July 2019. I combine information on police incidents, personnel, officer activity, and downstream court outcomes in order to construct a monthly panel of supervisory assignments for patrol officers that links officer enforcement activity to each of their supervisors throughout the sample.

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[3]See Ba et al. (2021) for a discussion of this assignment procedure in the Chicago Police Department and its implications for officer sorting between high and low crime districts.

[4]In my sample, it happens 3 out of the 5 years

[5]Internal Patrol Bid guidelines that were obtained via FOIA request reveal that officers are told explicitly that they do not have a say over their supervisors.

3.1 Supervisor Assignments

Police supervisors in Dallas are linked to the sector and watch in which their officers are assigned. Dallas only maintains assignment data at the level of patrol divisions, meaning that they do not keep records of supervisory assignments. However, the Computer Aided Dispatch (CAD) system that is used to allocate officers to police incidents stores the daily sector and watch assignments of responding officers (and supervisors) who are working that call. I combine the universe of CAD entries with division assignments and promotion histories to construct monthly supervisor assignments for patrol officers from June 2014 to July 2019. These assignments exclude officer spells in specialty units whose assignment indicator does not match a geographic sector. Sergeants cannot be reliably assigned for these units and, based on conversations with DPD, these units perform distinct duties from regular patrol officers. Specifically, I assign officers to the sector-watch in which they are assigned on the most days within the month and assign each sector-watch the supervisor who is observed with that assignment on the most days. This assignment construction yields a panel of 2,067 officers, 388 sergeants, 15,355 officer-sergeant spells, and 61,166 officer-month observations.

For the average sector-watch, I observe the assigned supervisor in CAD on 9.1 unique days within the month, suggesting that it is unlikely that I am consistently selecting fill-in supervisors who are more active than the regularly-assigned one. I focus on patrol officers whose primary job duties are responding to civilian-initiated calls, since their assignments are most reliably recorded through CAD data. This gives me a sample of 61,166 officer-months.

By aggregating assignments to the monthly level, I focus on the effects of an officer's regularly assigned supervisor, as opposed to any supervisor who they may receive daily instruction from on a temporary sector assignment. Doing so means that the effects I estimate will derive from a combination of instruction in the field and administrative practices. In order to ensure my estimates are consistent with the effects driven by permanent supervisors, I subject the sample to two filters. First, I require that spells last at least 2 consecutive sample months. This minimizes any assignment errors that would be generated from officers working in a temporary assignment that has relatively more activity than their permanent one, in which case officer arrests may be erroneously credited to the wrong supervisor. If the underlying supervisor did not change, then these errors would attenuate the variance in supervisor fixed effects This eliminates 5,747 spells, 19% of which are single months with no assigned Sergeant. Next, I filter out the remaining 866 spells in which the sergeant cannot be determined. I show in Figure 2 that these sample restrictions do not meaningfully change my estimates.

To facilitate identification of supervisor and officer fixed effect, I remove any officers and sergeants who only appear together, as well as any officer-sector watch and sergeant-sector watch pairs that only appear together as well as any levels of the fixed effects that only appear once in the data. I also require that officers appear in the data in at least 5 separate months. These restrictions eliminate 310 officer-supervisor

spells, yielding an analysis sample of 1,805 officers, 347 supervisors, 8,432 officer-supervisor spells and 49,923 monthly officer observations. In order to study trends around officer moves, I also construct a balanced event study sample. I define an event as two chronological spells involving the same officer but different supervisors. I require the duration of the spells to be at least 6 months prior to the switch and at least 4 months after the switch.

I supplement the panel of supervisory assignments with data on individual officer activity from several sources. I use officer identifiers in the universe of arrest reports to count the number of arrests made by each officer in each month of my sample. I match each arrest to all of the charges listed at the time of apprehension and partition arrests into two categories: serious and low-level. Serious arrests include any crimes against person or property that are tracked by the FBI (i.e., index crimes). These crimes include murder, rape, robbery, aggravated assault, theft, burglary, and arson. I also classify several non-index crimes that have high social costs as serious: simple assaults, any form of domestic violence, sexual assault, fraud, and DWI. All other crimes are classified as low-level.[6] Low-level crimes are primarily for outstanding warrants, disorderly conduct, and drug possession, though they include a range of offenses that are generally victimless or considered "Crimes Against Society" by the FBI's NIBRS classification. Arrests may contain multiple charges, so I use the most severe charge to classify each arrest (i.e. an arrest with any serious charge is classified as serious).

In order to evaluate the quality of each arrest, I link them to court outcomes using records obtained from the Dallas County District Attorney's office.[7] I use the court records to classify the conviction status of each of the arrest charges. Conviction occurs if the arrest is matched to a court case that does not result in a dismissal. Convictions thus include plea bargains as well as those administered by a judge or jury. If a charge does not match to court data, I consider it to have been dismissed. I define conviction at the arrest level as the arrestee being convicted of any of the charges related to the arrest.

I extract 911 calls from CAD data in order to separately evaluate civilian-initiated and proactive police encounters.[8] An arrest is considered proactive if it does not originate from a 911 call. Additionally, I merge use of force reports and civilian complaints to the involved officers and the month of occurrence. Finally, I link officers and supervisors to internal personnel records that contain demographic information, tenure and promotion history, shift, day-off group, and bureau assignments, disciplinary action, and promotional

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[6]The classifications used are similar to those used by Rivera (2022), with the inclusion of DWI as a serious arrest being the primary difference.

[7]Specifically, I use the name of the arrestee and the offense date to match an arrest to a case within the universe of cases disposed within Dallas County from 2014 to 2020. I first match arrests to all court cases with the same offense date. Then I use Jaro-Winkler distance to calculate the similarity of the first and last names of the matched defendants. If an arrest has a matching case with first and last names that perfectly match (i.e. Jaro-Winkler score equalt to 1), I keep only that case. For all other arrests, I keep a match if it has a Jaro-Winkler score of 0.9 or above. This matching technique is similar to the one used by Adger et al. (2022) and allows for some spelling errors in the arrest report while still being conservative about the name similarity required for a match.

[8]I use the cleaning procedure described by **?** to isolate 911 calls in CAD.

exam performance for Sergeants exams given in 2012, 2014, and 2018.

Summary statistics for the full unrestricted data, the analysis sample, and the balanced event study sample are given in Table 3. Both the event study and analysis samples are similar in observable characteristics to the unrestricted data, suggesting that estimates are unlikely to be biased by sample selection decisions. Notably, officers are highly mobile and supervisors are exposed to a large number of officers within the sample. The average officer has just under 4 unique supervisors and the average supervisor manages over 20 officers. This density within the managerial network is vital for my empirical strategy, since supervisor fixed effects can only be identified within groups of officers and supervisors who are connected by moves (Abowd et al., 2002). In my data, all of the observations are within the largest connected set.

4 Empirical Strategy

I estimate the effects of each of the 347 supervisors in my sample on officer arrests. To do so, I follow the two-way fixed effects approach pioneered by Abowd et al. (1999) and since used to identify manager effects in a variety of settings (Benson et al., 2019; Frederiksen et al., 2020; Fenizia, 2022; Metcalfe et al., 2023). The model takes the following form:

$$y_{it} = \theta_i + \psi_{J(i,t)} + x'_{it}\beta + \nu_{it}, \tag{1}$$

where y_{it} is the number of arrests made by officer i in year-month t, θ_i is an officer fixed effect, and $\psi_{J(i,t)}$ is a fixed effect for officer i's supervisor in month t. The time-varying control vector x_{it} includes sector by watch fixed effects in order to net out differences in officer enforcement that arise because of the different times of day and locations in which officers work. By including sector by watch fixed effects, I identify the effect of the sergeant using changes in enforcement patterns within an officer's career, relative to the typical enforcement levels within the sector-watches that they patrol. Since sector-watch and supervisor assignments overlap, separate identification of supervisor and sector-watch fixed effects requires that all sector-watches in my sample are managed by multiple supervisors. I also include fixed effects for the day-off group of the officer in order to control for changes in an officer's scheduled days that coincide with changes in their supervisory assignments. Additionally, I include a second degree polynomial of officer tenure.

Supervisor fixed effects are identified by officers who change supervisors. Specifically, supervisors in this model are only credited for *changes* in the behavior of officers who switch to them. For ψ_j to identify the causal effect of supervisor j, I require that mobility of officers between supervisors is as-good-as random, conditional on officer fixed effects and the controls. In other words, supervisory assignments need to be uncorrelated with determinants of officer behavior that are not present in the model. However,

the model allows officers to sort to supervisors based on the permanent components of officer effects θ_i and supervisor effects $\psi_{J(i,t)}$. Thus, if officers who have a preference for making arrests tend to work with supervisors who encourage officers to make them, the identifying assumptions would not be violated. Following Card et al. (2013), I consider three forms of endogenous mobility that would lead to violations of the identifying assumptions.

First, supervisor assignments must be uncorrelated with trends in both officer behavior and crime within the assigned sector. For example, if supervisors who are more lenient toward low-level arrests are more likely to be assigned officers whose preference for making arrests is increasing over time, then the model would erroneously attribute gains in arrests to the supervisor and overstate the importance of supervisors overall. Moreover, if officers were moved systematically to neighborhoods whose demand for police enforcement was increasing, then I would also overstate the variation in supervisor effects. Second, I require that changes in an officer's supervisor do not coincide with unobserved shocks to their enforcement behavior. In this context, one may be particularly worried about departmental policy changes that coincide with an officer's move, such as hot-spot policing within an officer's new sector. Endogenous movements of this form would also lead me to overstate the importance of supervisors. Finally, my identification assumes that officers do not sort to supervisors based on comparative advantage or match quality.

Anecdotally, the nature of supervisory assignments in Dallas makes endogenous movements unlikely. There are two primary sources of variation in supervisory assignments. First, once a year the Chief may initiate a Patrol Bid, which realigns patrol officers and supervisors based on preferences that are prioritized according to time in rank. Officers and supervisors have the ability to choose their patrol station, watch, and day-off group in the bid. After the bid, sector assignments - which determine an officer's supervisor - are given at the discretion of station commanders. Second, officers or supervisors may switch sector assignments based on vacancies that arise from retirements, promotions, separations, and transfers into non-patrol assignments. Compared to other large police departments which allow officers to bid for these openings[9], officers in Dallas have limited discretion when it comes to filling vacancies. Officer positions are filled at the discretion of command staff and supervisor positions are filled from a pool of interviewers. In neither case are more senior officers given priority for openings. Because officers cannot control the timing of vacancies or the Patrol Bid, they have limited ability to adjust their behavior in anticipation of a switch or sort on crime trends within locations.

Because officers may change location, shift, or days off when they switch supervisors, inclusion of sector-watch and day-off group fixed effects means that the supervisor effects are identified net of these contemporaneous changes. To the extent that there are other concurrent changes, such as a new

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[9]See Ba et al. (2021), who specify a model of the mechanism used to fill vacancies in the Chicago Police Department. Because vacancies in their context are filled using officer seniority, officers are able to sort continuously between districts based on trends in crime.

department-wide or location-specific policy or changes in an officer's peers, the supervisor fixed effects will identify a combination of supervisor effects and effects from other sources. However, policy changes are unlikely to be a driving factor in supervisory moves. Hot-spot initiatives, for example, are generally carried out by sub-teams within the patrol division which are not included in my sample (cite from Dallas website). From a conversation I had with Major Stephen Bishopp, supervisory movements outside of the ones described above are "sporadic, completely at random."

I rigorously test the validity of the identification assumptions in Section 5. However, in the spirit of Card et al. (2013), one can also conduct simple nonparametric event studies around officer moves in order to assess the variation in the data that is leveraged for identification. In Figure 9, I provide such event studies by splitting supervisors into terciles according to the average number of arrests made by officers whom they manage during the sample. I then plot officer arrest paths separately by the terciles that they transition to and from. Arrests are residualized by officer fixed effects and the control vector using within-supervisor variation, as in Chetty et al. (2014).[10] I use the sample of switches that are balanced 2 months prior to the move and 2 months after the move.

Figure 9 exhibits a few substantial patterns. First, following an officer switch to a new supervisor, their arrests change suddenly and persistently. This is consistent with the fixed effects specification, in which the supervisor's effect "turns on" once the officer moves and does not degrade over time. Second, while there is evidence of fluctuations in officer behavior prior to a switch, these movements do not appear to be systematically related to the direction of the officer's switch. If, for example, officers whose arrests are trending upwards are more likely to move to supervisors who tend to manage high-arrest officers, then I would overestimate the fixed effects for high arrest supervisors. This does not appear to be the case in Figure 9. Officers who move from a supervisor in the lowest tercile to one in the highest tercile are trending similarly to those who move down from the highest to the lowest tercile.

Third, Figure 9 suggests that officer-supervisor match quality is not an important determinant of moves. Sorting on match quality implies that officers tend to move to supervisors with whom they have a comparative advantage (or, in the case of negative sorting, disadvantage) in making arrests. One implication of sorting on match quality is asymmetry in the effect of upward and downward moves. As shown by Card et al. (2013), in the presence of an endogenous match effect η_{ij}, the expected difference in arrests as a result of the move to a high supervisor ($j = 2$) in period t from a low supervisor ($j = 1$) in period $t-1$ is given by:

$$E[y_{it} - y_{it-1}] = \psi_2 - \psi_1 + E[\eta_{i2} - \eta_{i1}|J(i,t) = 2, J(i,t-1) = 1], \tag{2}$$

whereas the same expectation for an equal move in the opposite direction is given by:

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[10]In practice, this means that I estimate $\hat{\theta}_i$ and $\hat{\beta}$ by estimating equation 1. I then calculate $y_{it} - \hat{\theta}_i - x'_{it}\hat{\beta}$ using these estimates.

$$E[y_{it} - y_{it-1}] = \psi_1 - \psi_2 + E[\eta_{i1} - \eta_{i2}|J(i,t) = 1, J(i, t-1) = 2]. \tag{3}$$

Under positive (negative) assortative matching, both of the match quality difference terms will be positive (negative). In the extreme, this would result in all movers increasing (decreasing) arrests on average. Even in an intermediate case, asymmetry in the sense that $\psi_2 - \psi_1 \neq \psi_1 - \psi_2$ would imply misspecification of the model. Upon visual inspection of Figure 9, it is clear that officers who move to a higher arrest sergeant increase their arrests on average while those who move to a lower arrest sergeant decrease arrests. Moreover, moves in the opposite direction appear to be symmetric: a move, for example, from the 3rd tercile to the 1st appears to be equal and opposite in magnitude to a move from the 1st tercile to the 3rd. Reassuringly, moves within the same tercile do not appear to produce average changes in either direction. In the case of comparative advantage matching, one would expect moves within a tercile to still produce changes since the match quality would change in a systematic way. I additionally verify the symmetry across moves in Appendix Figure ??, which plots the average change in residual arrests for upward moves against the average change in residual arrests for downward moves in the opposite direction. The points line up roughly along the -45 degree line.[11]

Since consistency in the two-way fixed effect model requires that the number of observations tends to infinity within each officer-supervisor pair, the raw fixed effects are likely to be estimated with error even if the identification assumptions are satisfied. This error will be more severe for supervisors with few in-sample observations. To obtain consistent estimates of the supervisor fixed effects, I adapt Empirical Bayes shrinkage procedures first developed by Morris (1983) that have been commonly used in the teacher value-added literature (e.g. Kane and Staiger, 2008; Chetty et al., 2014). Specifically, I bootstrap the estimation of equation 1 in order to obtain estimates of the variance in supervisor fixed effects that can be attributed to the true, signal variance, σ_ψ, and the variance attributable to sampling error, σ_ϵ.[12] I then multiply each of the raw fixed effects by the ratio of signal variance to total variance, $\frac{\hat{\sigma}_\psi}{\hat{\sigma}_\psi + \hat{\sigma}_\epsilon}$, in order to obtain the Empirical Bayes estimates of supervisor effects. Further details are given in Appendix .1. I perform the same procedure for officer fixed effects.

4.1 Estimating the Variation Explained by Supervisors

In addition to individual supervisor fixed effects, I am also interested in the contribution of supervisors and officers to variation in enforcement outcomes.

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[11]Splitting sergeants into terciles provides relatively few cases to study for symmetry. In Appendix Figure 8, I perform the same nonparametric event study analysis by splitting sergeants into quartiles. The conclusions of Figure 9 and Appendix Figure ?? hold similarly in this case.

[12]For the bootstrap, I follow the procedure outlined by Best et al. (2023). I obtain residuals $\hat{v}_{it}$ and randomly resample them, stratifying by supervisor-officer pair in order to preserve the match structure of the data. I then re-estimate the supervisor fixed effects. I repeat this process 1000 times and use the distribution of fixed effect estimates for each officer to calculate $\hat{\sigma}_\psi$ and $\hat{\sigma}_\epsilon$.

$$Var(y_{it}^*) = Var(\theta_i) + Var(\psi_{J(i,t)}) + 2Cov(\theta_i, \psi_{J(i,t)}) + Var(\nu_{it}), \tag{4}$$

$$y_{it}^* = y_{it} - x_{it}\hat{\beta}. \tag{5}$$

I focus on variation in pair-level average residualized arrests, since variation within an officer-supervisor pairing is uninformative for estimates of supervisor fixed effects. Arrests are residualized by the controls, with $\hat{\beta}$ estimated using within-supervisor and within-officer variation from the full model in equation 1.[13]

While the Empirical Bayes procedure outlined in the previous section reduces measurement error in the estimated fixed effects, the variance components may still be biased if there are too few officer movers in the data relative to the number of supervisors - the well-known *limited mobility bias* problem (Andrews et al., 2008). This would overinflate the variance of supervisor fixed effects - causing us to conclude that supervisors have more of an impact than they actually do - and bias the covariance negatively due to the inverse correlation of measurement errors between the officer and supervisor effects. This bias has been shown to be severe in the context of firm-worker networks (Bonhomme et al., 2023). Compared to other contexts, the mobility network in my data is incredibly dense. Over 85% of officers in my sample switch supervisors and the entire sample is connected by officer moves. Thus limited mobility bias may not be a large concern in this context.

Nonetheless, I adopt the bias-correction strategies developed by Andrews et al. (2008) and Kline et al. (2020) to for the estimates of the variance components. The Andrews et al. (2008) method relies on a derivation of the bias term that requires homoskedastic errors, whereas Kline et al. (2020) - KSS, herafter - derive the bias term under unrestricted heteroskedasticity. The KSS bias term is a linear combination of each observation's variance weighted by each observation's influence on the plug-in variance estimator. The KSS bias-corrected variance terms take the form of leave-out estimators that rely on model parameters computed when leaving out the *i*-th observation. Due to the leave-out nature of the KSS estimator, it can only be used on the leave-one-out connected set, which is the set of officers and and supervisors who remain connected when any one officer is removed. The leave-one out connected set only removes 3 supervisors and 3 officers from my data. In Section 5, I show that each of the bias-correction methods provide similar estimates of the variance and covariance components.

5   Results

Figure 3 plots the density of the raw and shrunken supervisor fixed effects. Reassuringly, shrinkage reduces the variation and increases the mass around 0. One standard deviation in the unshrunken fixed

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[13]This follows from Chetty et al. (2014), who highlight that sorting of officers/supervisors to sector-watches and day-off groups would create bias in the estimates $\hat{\beta}$ if supervisor effects were omitted.

effects equates to 0.9 additional arrests per month, whereas a standard deviation for the shrunken distribution is equivalent to 0.66 arrests. Though relatively symmetric around the average supervisor, the fixed effects distribution has a heavy left tail, implying the existence of a disproportionate amount of low-enforcement supervisors. The estimates suggest that moving from a high arrest supervisor to a low arrest supervisor makes a sizeable difference in the enforcement behavior of officers: taking an officer whose supervisor is in the 10th percentile of the effect distribution and placing them with a supervisor in the 90th percentile would lead to 1.6 more arrests per month (42% relative to the mean).

I present the results of the variance decomposition in Table 1, including the decompositions using raw fixed effects, the Bayes-shrunken fixed effects, and both bias-correction methods. As expected, the raw fixed effects overstate the contribution of supervisors to variation in officer arrests - on the order of 1.5 percentage points. All three of the adjustment methods that I use produce similar estimates of the contribution of supervisors and officers to enforcement outcomes. In the preferred KSS specification (Columns (7) and (8)), I find that variation in supervisors explains 3.39% of the total variation in officer arrests. Officers, on the other hand, account for nearly three-fourths of the variation in arrests. This is to be expected given that police interactions are highly variable and arrests are ultimately made according to the judgment call of each officer.

Given that I estimate effects on the actions of individual workers, the amount of variance attributed to managers is necessarily smaller than estimates that have been obtained by looking at office level outcomes (Fenizia, 2022, e.g.). Compared to contexts in which worker-level outcomes are attributed to variation in management, my estimates imply a slightly smaller role for management and a significantly larger role for workers. The percentage of variance attributable to managers in my context is slightly smaller than estimates obtained by Adhvaryu et al. (2020) in the context of worker productivity within Indian garment factories (7.3% to 3.4%). However, relative to the routinized work environment in their study, officers in my context explain a significantly larger portion of the variation than do workers in a factory line (5.4% to 72.3%).[14]

In the fourth row of Table 4, I report the covariance between supervisor and officer effects. I find evidence across all specifications of high arrest officers sorting to low arrest supervisors. One interpretation of this result is that officers who make a lot of arrests prefer to work under relatively uninvolved supervisors which may lead to less scrutiny over their behavior. However, the magnitude of sorting is small and accounts for no less than -1.51% of the total variation across each specification. Consistent

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[14]My model implicitly assumes that supervisor effects are completely private. If I instead assumed public manager effects, as is done by Lazear et al. (2015) in their fixed effects specification of manager and worker effects within the IT industry, my estimates would imply that a standard deviation change in the manager effects distribution is responsible for 5.1 more arrests per month($\sqrt{0.378} * 8.31$, where 8.31 is the average team size), an effect which is 1.8 times larger than a standard deviation in individual officer distribution. This is slightly smaller than Lazear et al. (2015)'s estimates, which are 2.6 times the size of the standard deviation in worker effects.

with institutional practices that limit officers' abilities to select specific supervisors, sorting-even on fixed characteristics-appears to be limited.

One way to contextualize the magnitudes of the supervisor fixed effects is through a simple counterfactual where I replace supervisors at the top of the arrest distribution with one from the median, either through training programs or demotion and promotion of new supervisors. Specifically, my estimates suggest that replacing the top 10% of supervisors with median supervisors would reduce the number of monthly arrests by 2.5% per officer. (Explain this more/compare to Chetty?)

5.1 Diagnostic Checks

In this section, I use my estimates of the sergeant fixed effects to evaluate the validity of the identifying assumptions. I begin by considering endogenous mobility based on trends in officer behavior. One might be concerned that are officers are reassigned to supervisors on the basis of recent changes in arrests. For example, officers may heterogeneously adopt different policing techniques that are learned through mandated training and, either through choice or commander decisions, be reassigned to a sergeant whose policing style matches their changing techniques. On the other hand, if an opening within a low-enforcement supervisor's unit arises following a retirement, one might be worried that an officer with increasing enforcement levels who is generating civilian complaints would be more likely to be assigned to that vacancy in order to reduce any negative impact on the department. In either case, the supervisor fixed effects would systematically biased.

In order to test for the presence of endogenous sorting based on trends, I examine the presence of heterogeneity in trends prior to a supervisor move using an event study. Specifically, I estimate a model of arrest behavior around the time of a move:

$$y_{et} = \alpha_e + \sum_{k \neq 0} [\pi_0^k D_{et}^k + \pi_1^k D_{et}^k (\Delta \hat{\psi}_e)] + x_{et}' \beta + \epsilon_{et}. \tag{6}$$

Here, e indexes a switching event - uniquely determined by the officer i and the switch month T - and k indexes months relative to the switching month. I include the model controls (tenure, sector-watch fixed effects, and day-off group fixed effects) in order to adjust for time trends and an event fixed effect, α_e, in order to control for differences in baseline arrest rates prior to the switch.

The parameters of interest are the π_1^k's, which capture heterogeneity in the effect of being k periods before/after a switch that depends on the change in enforcement capacity between the old and new supervisor. I test for endogenous reassignment by evaluating the pre-move event study coefficients. The event study model also nests a test for general misspecification of the supervisor effects, since the empirical model given in equation 1 implies that a supervisor switch results in an instantaneous and non-degrading change in arrests. In order to allow for a comprehensive analysis of pre-trends, I estimate equation 6 using the event study sample, so that $k \in [-5, 4]$.

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I plot the event study coefficients in Figure 1. Reassuringly, there is no evidence of heterogeneous trends in arrest behavior prior to an officer changing supervisors. An F-test of joint significance of the pre-move coefficients yields a p-value of 0.8473 (see Appendix Table **??**). Moreover, following a switch to a high-arrest supervisor, an officer's arrests immediately increase and remain elevated throughout the duration of the panel, in line with the insights from the nonparametric event study in Figure 9.[15]

While it does not appear that officers are assigned to supervisors on the basis of trends in their arrest behavior, one may still be concerned that supervisory switches are more likely to occur in areas in which crime is rising, which would result in officers making more arrests after a move, but not because of their new supervisor. This would happen if either officers are moved to areas in which crime tends to be rising or if supervisors tend to be replaced in areas in which crime is rising. This may be particularly concerning in light of assignment mechanisms used in other cities that create the opportunity for officers to sort on trends (e.g. Ba et al., 2021). As described in Section 4, the unique institutional practices of the Dallas Police Department go against sorting on crime trends.

To show that crime trends are not correlated with supervisory switches, I collapse the data to the sector-watch by month level and calculate the average change in supervisor effect separately for officers switching in and out of that assignment each month. I then regress the average incoming and outgoing supervisor changes on the log of 911 calls in the 5 months prior to the switches. If supervisor effect estimations are driven by trends in crime, then we would expect high crime in the months prior to a switch to predict higher changes in the supervisor effects for incoming officers and lower changes for outgoing officers. In Appendix Table 10, I report results for this test. Joint F tests cannot reject the null hypothesis that pre-period crimes are uncorrelated with changes in incoming or outgoing officers' supervisors and the point estimates are economically insignificant across the board.

(Put this test in the appendix) First, I explicitly evaluate heterogeneity in crime trends within an officer's post-switch sector using an event study design similar to the one in equation 6. In particular, I assess whether trends in 911 calls within the sector that an officer occupies after a supervisory switch differ by the magnitude of the switch. I use 911 calls as a measure of crime rather than crime reports since crime reporting will be endogenous to police activity (Weisburd, 2021).[16] I estimate the following model for switching events balanced around [-5, 4]:

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[15]The size of the effect after moving is also close to 1, which is reassuring since the π_1^k's are interpreted as the change in arrests following a move to a supervisor who induces one more arrest per month relative to the previous supervisor. To the extent that the estimates are below 1, this reflects measurement error that arises due to using estimated objects and a smaller subset of the data in which officers make slightly fewer arrests relative to the full sample.

[16]To the extent that aggressive policing can also affect the public's willingness to contact the police, 911 calls may also be partially endogenous to police activity (Ang et al., 2024). Nonetheless, I take the stance that, because crime reports will be generated by proactive policing and 911 calls are civilian initiated, the latter is a more appropriate indicator for crime in my context, as the reporting biases are likely to be smaller.

$$log(911Calls)_{SW(e),t} = \alpha_{SW(e)} + \sum_{k \neq 0} [\pi_0^k D_{et}^k + \pi_1^k D_{et}^k (\Delta \hat{\psi}_e)] + \epsilon_{et}. \tag{7}$$

The function SW returns the sector-watch that the officer is assigned to in event e after switching supervisors. I control for pre-switch differences in the level of crime using a fixed effect for the sector-watch, $\alpha_{SW(e)}$. In doing so, I compare changes in reported crimes relative to the average within that sector-watch around the time that an officer receives a new supervisor within that assignment. I plot the π_1^k terms in **??**. I do not observe evidence of differential trends in crime prior to a switch. Following the switch, there is some evidence that crime falls within the sector-watch, however the estimates are imprecise and small, all under 1%. I investigate potential effects on crime in further detail in Section 6.

To further test for endogenous crime trends, I perform a placebo test by evaluating changes in the behavior of officers who already work with the new supervisor of a switching officer. For these incumbent officers, arrests should not be trending differentially based on the change in supervisor fixed effect for the new officer. That is, if high-arrest supervisors are simply those whose new officers were added at the same time that demand for arrests was increasing in their assigned location, then we would expect arrests made by incumbent officers to be increasing as well. Moreover, this placebo test also serves as a check on the assumption of no contemporaneous shocks taking place at the time an officer changes supervisors. If changes in officer behavior after switching to a high-arrest supervisor were driven by other contemporaneous changes in department policy or peer effects that increase officer arrests, then we would expect incumbents to be equally affected by these changes.

I employ the following empirical specification for officers $l \neq i$ who are managed by officer i's new supervisor 4 months before the switch and 4 months after:

$$Arrests_{let} = \alpha_{le} + \sum_{k \neq -1} [\pi_0^k D_{et}^k + \pi_1^k D_{et}^k (\Delta \hat{\psi}_e)] + x'_{let} \beta + \epsilon_{let}. \tag{8}$$

Once again, coefficients of interest are the π_1^k terms, which describe how arrests made by incumbent officers in month k differ for every one-unit increase in the change in the supervisor fixed effect of the new officer. In Figure 5, I present the estimates and 95% confidence intervals for the event-study coefficients. The estimates are close to 0 and insignificant across all months relative to the new officer's switch. The lack of pre-trends indicate that there is no evidence that supervisors who are estimated to have high arrest effects are more likely to receive a new officer when crime is trending upwards. Moreover, the lack of effects following the new officer's switch into the unit suggests that there are no contemporaneous policies occurring at the time of the switch that would affect all officers within the unit but be attributed to the supervisor in my model. The lack of post-switch effects also indicates that it is unlikely that a change in officer peer groups is driving behavioral changes at the time of a supervisor switch, since we would expect to see these peer effects reflected in the incumbent officers who experience a change in their peer group upon the entry of a new officer.

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Another concern involves the presence of endogenous sorting based on supervisory match quality. In addition to the nonparametric tests in Section 4, I conduct two additional tests of this assumption. First, I examine the average residuals of equation 1 separately by groups of officer and supervisor fixed effects. Specifically, I divide each officer-month observation into the quintile of the officer fixed effect and the quintile of the supervisor fixed effect. In the presence of pair-specific effects, I would expect average residuals to be systematically large (in absolute value) for certain pairings of officers and supervisors. If, for example, a supervisor that exhibits little oversight encourages active officers to make more arrests but cannot properly motivate less active officers, then we would expect large positive (negative) average residuals for top (bottom) quintile officers matched with bottom quintile supervisors. Appendix Figure 1 demonstrates that the mean residuals do not exhibit any clear pattern that would indicate a violation of the additive separability assumption. Across all officer-supervisor pairs, the residuals are relatively small - ranging from -0.1 to 0.18 - suggesting that the threat of misspecification is minimal in my setting.

I also assess the importance of matching effects by comparing the explanatory power of the baseline specification with additively separable fixed effects to a fully saturated model that contains a fixed effect for each officer-supervisor pair. I report the R^2 and Adjusted R^2 for these models in columns 3 and 5 of Appendix Table 9. The fully saturated model fits better than the baseline, though the increase in Adjusted R^2 of 0.054 suggests that match components play a limited role in this setting. To the extent that match effects are present, the evidence presented up to this point supports the idea that they are uncorrelated random effects and that the additive separability assumption is unlikely to result in model misspecification.

5.2 Heterogeneity in supervisor effects across crime types

The previous section established that supervisors matter for officer arrest behavior. However, arrests may be reflective of enhanced officer effort to respond to serious crimes or greater discretionary use of enforcement powers toward low-level victimless crimes (Huff, 2021). While the former indicates that high arrest supervisors motivate their officers to be active and potentially more responsive to the public, the latter would suggest that these supervisors instead encourage officers to aggressively enforce in situations that may provide low returns to public safety (Cho et al., 2023).

To distinguish between these cases, I follow Rivera (2022) and partition arrests into two types: serious and low-level. Serious arrests include index crimes (i.e. most violent and property crimes) as well as domestic violence, simple assault, fraud, and DUI. Low-level arrests include arrests for all other crimes, including drug possession, warrants, traffic offenses, and disorderly conduct. As discussed in Section 3, arrest types are mutually exclusive by construction. I then re-estimate the event study from equation 6 separately using the number of serious and low-level arrests as the dependent variable. Since the baseline arrest rates for each arrest type differ substantially (low-level arrests make up 75% of arrests), I normalize

the estimates so that the effect sizes are interpreted relative to the reference period mean.

In Figure 2, I plot the event study coefficients for serious and low-level arrests. Once again, there is a reassuring lack of pre-trends in both types of arrests prior to a switch. Following a move to a high arrest supervisor, both low-level and serious arrests increase. The effects on low-level arrests are slightly larger compared to serious arrests: just over a 25% increase relative to the final pre-period compared to a 16% increase. However, both of the estimated effect sizes are meaningful and, given that serious crimes are likely more costly to society overall, it is possible that the benefits of such arrests may outweigh the costs of arrests for low-level crimes. An important auxiliary question is how preferences for arrests of different severity levels are correlated among supervisors.

To understand the relationship between a supervisor's low-level and serious arrest preferences, I disaggregate the estimated supervisor *effects* by estimating equation 1 separately for low-level and serious arrests and applying Bayesian shrinkage to each of the estimates. The composite arrest effect that was estimated using the baseline two-way fixed effects model represents the sum of a supervisor's effects on low-level (ψ_j^L)) and serious arrests (ψ_j^L)). In order to compare magnitudes of changes in the serious and low-level effects, I focus on standardized versions of both estimates (i.e. z-scores calculated by dividing each estimate by the standard deviation of the associated effects distribution).

In Figure 7, I plot supervisors' low-level effects against their arrest effects, along with a linear fit and nonparametric 95% confidence band (Cattaneo et al., 2024).The linear fit implies a positive relationship between low-level and serious effect and the estimated correlation is a small but statistically significant 0.11. However the confidence band does not allow me to rule out a highly non-linear relationship. Specifically, visual inspection of the plot suggests that supervisors who induce far fewer low-level arrests than the average tend to induce fewer serious arrests as well, however, throughout the rest of the distribution the relationship appears to be flat. Indeed, if I remove just the bottom 5% of supervisors in the low-level distribution, the estimated correlation falls to 0.07 and is statistically insignificant. Thus, while there is a concentration of supervisors who seem to reduce officer effort across the board, preferences for most supervisors appear to be uncorrelated across crime severity. This point is made clearer by looking at how supervisors are distributed across different terciles of low-level and serious effects (Figure **??**), which reveals substantial heterogeneity in supervisor preferences. While a plurality of supervisors — 14.4% — are in the lowest tercile of both effects, nearly are extreme along both dimensions. Around 10% of supervisors are in the highest tercile of low-level effects and the lowest tercile of serious effects, 11% are in the highest tercile of serious effects and the lowest tercile of low-level effects, and 11% are in the highest terciles of both dimensions. Supervisors in the latter group are able to increase effort along multiple dimensions, while the 21% of supervisors who are low on one dimension and high on the other explicitly trade off one type of enforcement for the other.

These results suggest that the behaviors induced by different types of supervisors are independent

from one another. I now investigate the specific behaviors that are affected by supervisors of each type. First, I estimate how changes in serious and low-level supervisor effects change the specific crimes that officers arrest for. To do so, I leverage regressions of the following form:

$$y_{it}^c = \alpha_L^c \hat{\psi}_{J(i,t)}^L + \alpha_S^c \hat{\psi}_{J(i,t)}^S + \theta_i^c + + x_{it}'\beta^c + v_{it}^c, \tag{9}$$

where y_{it}^c is the number of arrests made by officer i in year-month t that result in a particular charge. As in the baseline specification, I control for officer, sector-watch, and day-off group fixed effects as well as officer tenure. The α coefficients measure the change in arrests for crime c that results when the low-level (serious) effect of an officer's supervisor increases by one standard deviation.

In Table 5, I report estimates using the 3 most frequent serious and low-level offenses. Increasing a supervisor's serious effect results in statistically significant increases across the three largest crimes: domestic violence (column 1), theft (column 2), and DWI (column 3). However, the effects are largest, both nominally and relative to the mean, for domestic violence arrests, which account for 0.45 arrests each month (12% of the average total) but increase by 37% when officers are assigned to a supervisor who has a one standard deviation larger serious effect. Additionally, supervisors with larger low-level effects actually *reduce* domestic violence arrests by 0.016 (3.5% relative to the mean) per month. The estimated effects of low-level supervisors are negative but statistically insignificant across the other three serious crime types.

Among low-level crimes, the largest discrepancy in behavior is for drug arrests. A one standard deviation increase in the low-level effect of a supervisor increases an officer's drug arrests by 0.17 per month, over 50% on the mean. However, the same size increase for a supervisor's serious effect reduces drug arrests by .036 per month (10% on the mean). On the other hand, both dimensions of supervisor effects are positively associated with warrant and disorderly conduct arrests, though the estimated effects are larger in both cases for the low-level supervisor effect.

Together, these results indicate that, even though the two dimensions of supervisor effects are uncorrelated in the aggregate, there are still significant crime-specific tradeoffs being made by supervisors of different types. In particular, supervisors with serious arrest preferences differ the most from those who induce low-level arrests with respect to domestic violence and drug arrests. Both of these types of arrests may be highly discretionary in this context. Numerous papers relating to police search decisions have demonstrated that officers vary in their propensity to search for contraband (e.g. Feigenberg and Miller, 2021). Moreover, Texas is one of 27 states without a mandatory arrest law in the case of domestic violence calls. Rather than just affecting officer effort, supervisors are able to substantially change how officers use their discretion, and they do so heterogeneously across categories of criminal behavior.

How do supervisor preferences change the types of interactions that officers have with the public? I answer this question by estimating equation 9 using officer-initiated arrests and call-initiated arrests as

the outcome variables. The results (Table 4) show that supervisor-induced serious arrests are entirely initiated from calls (column 2), low-level arrests originate from a mixture of calls and officer-initiated interactions, however the latter accounts for over 60% of the total increase in arrests, a stark result given that officer-initiated arrests account for fewer arrests than 911 calls on average.

Table 6 further clarifies supervisor effects on call-level arrests. Serious arrests that are induced by a supervisor result from both more officer interactions and a higher probability of making arrests conditional on an interaction. A one standard deviation increase in the serious arrest effect is associated with 2 more calls answered per month relative to a base of 61. I show in a later section that it is unlikely that the overall number of calls is increasing, meaning that officers are electing to answer more calls overall. In Dallas, patrol officers can choose to volunteer for calls if the unit assigned to that area is not available, and the vast majority of calls that officers answer are outside of their assigned beat. Thus, the evidence suggests that supervisors who induce more serious arrests cause their officers to be more active in volunteering for calls. In addition, officers are .001pp more likely to make arrests at the calls they answer (column 2), indicating that supervisor-induced serious effects are not purely a result of greater exposure to crime.

As the low-level effect of supervisors increase, their officers also answer more calls, however the estimated increase is over half the size of the increase for the same magnitude of change in the serious effect. The increase in arrest probability at calls is, however, nearly twice the size for an increase in a supervisor's low-level effect. All of the arrests that officers working for supervisors with low-level preferences are for low-level charges (column 3), while higher serious effects are associated with substantially more serious arrests at 911 calls (column 4), and a small but statistically significant increase in low-level arrests made at calls.

The arrests induced by supervisors may vary in quality. To assess whether this quality differs across the dimensions of supervisory preference, I estimate the impact of larger serious and low-level arrest propensities on conviction rates. Officer-months in which there are no arrests made are not infrequent. As a result, I am unable to study conviction rates directly. Instead, I follow an approach similar to Gudgeon et al. (2023) and estimate the impact of increasing a supervisor's serious (low-level) type on the number of convicted arrests and total arrests separately. The ratio of the coefficients in these regressions provides an estimate of the conviction rate for the marginal serious (low-level) supervisor-induced arrest. I then compare this ratio to the ratio of means for convicted and total arrests. I report results from each of the regressions, along with the difference in the estimated ratios and bootstrapped standard errors, in Table 8.

Both supervisors that induce serious arrests and those that induce low-level arrests increase conviction rates on the margin. This increase is larger when the supervisor's serious effect increases compared to their low-level effect — the marginal arrest induced by a one standard deviation increase in a supervisor's serious effect has a 33% chance of resulting in a conviction, compared to an average conviction rate of 20%. However, arrests induced by supervisors with a high low-level effect are 3pp more likely to result

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in conviction than the average arrest, which is surprising considering low-level arrests are significantly less likely to be convicted compared to serious ones. Although supervisors may prefer different types of enforcement activities, these results suggest that those who desire more arrests do not value low-quality apprehensions which are later dismissed.

Finally, in Table 7, I consider how supervisor effects on arrests interact with two measures of costly behavior: use of force and complaints. In column 1, find that increases in both a supervisor's low-level effect and their serious effect increase officer use of force incidents. However, the increase is significantly larger for supervisor's with a large low-level effect. A one standard deviation increase in the low-level effect of a supervisor leads to .02 more uses of force per month, a 14% increase relative to the mean. In column 2, I find that a supervisor's low-level effect is positively correlated with complaints and their serious effect is negatively correlated, however both estimates are imprecise. Both the serious and low-level arrest propensities of supervisors result in more officer activity and more formal interactions with civilians, which likely contributes to increased use of force. However, the stark difference in the effect sizes for low-level and serious arrest types suggests that the interactions induced by supervisors who prefer low-level arrests are for more likely to escalate to an officer use of force. This result may be especially concerning to departments and the public writ large, since the offenses that supervisors who are high in this dimension target are generally the least costly to society.

(Race results, put in later)

Overall, the results in this section reveal that, even though supervisors have a substantial ability to shape their officers' arrest decisions, they do so in heterogeneous ways. In particular, supervisors may induce more serious or more low-level arrests, and I do not find evidence that these effects are mutually exclusive nor mutually reinforcing. However, whether supervisors to motivate their officers to make more low-level arrests or more serious arrests results in very different downstream policing behaviors. Inducing more serious arrests means significantly more officer call activity, an increased likelihood of arresting for domestic violence, and substantially larger conviction rates. On the other hand, to induce more low-level arrests, supervisors incentivize their officers to be more proactive in detecting low-level crimes on patrol, particularly involving drug possession. A consequence of this behavior is a large increase in the amount of force that officers use.

5.3 Mechanisms

I now consider *how* supervisors are able to change the actions of their officers. Considering that police not only have discretion but operate largely outside the direct view of their supervisor, there has been a long-standing debate within the research community concerned with whether supervisors can shape officer behavior, much less if they actually do Brown (1988). As described in Section 2, supervisors have several levers they can pull to incentivize their officers toward particular types of policing. In this section, I

evaluate a few of these levers in order to provide evidence regarding how supervisors are able to influence their officers' decisions despite the highly independent nature of police work. Sergeants are distinct from higher levels of police supervision due to their ability to interact with officers in the field. Specifically, I leverage measures of supervisor activity to estimate the degree to which supervisors who are high along the low-level or serious arrest dimension "lead by example", by modeling their ideal behaviors to officers in the field. Supervisors can assign themselves or be dispatched to their officers' calls and some may be more willing to do so than others. However, they also maintain the ability to engage in self-initiated patrol duties. Anecdotal and field survey evidence suggests that supervisors vary in their preferences for field activity (Engel, 2001). Those who choose to spend time in the field may have more access to their officers and thus provide a greater incentive for them to appear active in the way that their supervisor prefers..

I consider two dimensions of supervisor activity: arrests and calls. At a given point in time, a supervisor's activity may be influenced by their assignment or the composition of their subordinates. I thus estimate the difference in activity between supervisors of different types using within-assignment variation in supervisors. Specifically, for unit u (i.e. a sector-watch) managed by sergeant j in month t, I estimate the following model of supervisor activity:

$$y_{jut} = \alpha_L \hat{\psi}_j^L + \alpha_S \hat{\psi}_j^S + \alpha_1 \bar{\hat{\theta}}_{ut}^L + \alpha_2 \bar{\hat{\theta}}_{ut}^S + x_u + \epsilon_{jut}, \tag{10}$$

where y is an action of supervisor j in unit u during year-month t. I include sector-watch (x_u) fixed effects in order to control for variation in sergeant behaviors that are driven by the time and location of their assignment. Sergeants may respond to more calls when they have active officers, so I control for the average estimated low-level and serious arrest propensity of the unit's officers in order to isolate variation in sergeant behavior that is driven purely by their own preferences for field enforcement.

I report the results in Table 2. In columns 1-3, I report specifications that use arrests (total, serious, and low-level) as outcomes. I find that, as a supervisor's low-level effect increases, they are involved in substantially more arrests (column 1). A one-standard deviation increase in a the low-level effect of a unit's supervisor increases the number of supervisory arrests by 24% on the mean.[17] This effect is driven exclusively by low-level arrests, which increase by 30% on the mean with a one standard deviation increase in low-level supervisor effect (column 3), whereas I find no evidence of a change in serious arrests (column 2). Thus supervisors who tend to induce low-level arrests appear to have a preference for making these arrests themselves. On the other hand, I do not find evidence that increases in a supervisor's serious effect are correlated with their behavior in the field. The point estimates in each of the regressions that use arrests as a dependent variable are negative, but small in magnitude (not exceeding 5% compared to the mean) and statistically insignificant.

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[17]Note that it is unlikely that the estimated supervisor effects can be explained by arrests made with the supervisor's involvement, as 99.4% of arrests in my sample are made without the officer's supervisor on the arrest report.

In columns 4-6, I report estimates from models that use total calls responded to (column 4), calls responded to as the sole first responder (column 5), and calls responded to in which one of the supervisor's subordinates is present (column 6) as dependent variables. Once again, I only find statistically significant effects for a supervisor's low-level arrest preferences. High low-level effects are correlated with supervisors being involved in more calls-for-service (2.5 more calls per month for a one standard deviation increase), and these calls come by way of assisting officers as well as "taking the hot call" for themselves, as indicated by the positive effect on solo calls in column 5. Notably, supervisors with a strong preference for low-level arrests are more involved at *their own subordinates'* calls (column 6), suggesting that they may be more able to change behavior through direct advice to their officers.

I interpret these estimates as evidence in support of the "lead by example" model of supervisory influence, at least for supervisors who prefer low-level arrests. While I do not find evidence that more or less field activity is related to a supervisor's effect on serious arrests, it is likely that these supervisors are engaging in other forms of behavior that cannot be captured in data. These supervisors may provide better transfer recommendations for their officers contingent on activity around serious crimes. Alternatively, supervisors with a high serious effect may be more willing to communicate their preferences for arrests to officers directly through radio assistance. Given that I find a large increase on the number of arrests made for domestic violence under supervisors with a high serious arrest effect, it is possible that supervisory influence arises in potential domestic abuse situations where officers call their supervisors, uncertain of how to proceed, and supervisors with a high serious effect often make the suggestion to arrest. Such an interaction would be consistent with anecdotal evidence (e.g. Brooks, 2021).

5.4   Crime effects

Do supervisor-induced arrests either low-level or serious dimensions improve public safety? In order to answer this question, I leverage the rotation of supervisors between sector-watches in an event study design similar to the one used in Section 5.1, by estimating how the effect of a sector-watch changing supervisors on 911 calls varies by the magnitude of the change in a supervisor's low-level and serious arrest effect:

$$Log(911Calls)_{et} = \alpha_e + \sum_{k \neq 0}[\pi_0^k D_{et}^k + \pi_L^k D_{et}^k(\Delta\hat{\psi}_e^L)] + \pi_S^k D_{et}^k(\Delta\hat{\psi}_e^S)] + x_{et}'\beta + \epsilon_{et}. \tag{11}$$

I control for event fixed effects and, as in Section 5.3, the average low-level serious arrest propensities of officers and effect for the officers working within a unit each month.

I plot the estimates for π_L^k in Figure 10b and the estimates for π_S^k in Figure 10a. Once again, there is no evidence that supervisor switches along either dimension are driven by trends in crime within an area. Moreover, I find no evidence of that supervisor variation along either dimension of arrests leads to reductions in crime crime. The point estimates are below 1% in magnitude for each arrest type in all

months following the switch and are statistically insignificant. I can rule out crime reductions larger than 2% related to changes along each dimension.

6   Predictors of Supervisor Effects

My results demonstrate that supervisors can be a powerful target for police reform policy. To what extent are supervisor effects mediated by observable characteristics that are determined *before* officers are promoted?

I evaluate the differences in supervisor effect distributions by four observable pre-promotion characteristics: race, gender, age at the time of the promotional exam, and score on the promotional exam. Since I only observe exam scores beginning with the 2012 round of tests, I limit my sample to the 202 supervisors who were promoted from these exams. I split supervisors into two categories of age at the time of exam and promotional score. I call supervisors "older" if they were above the average across all exams and I call supervisors "high-scorers" if they were above the average score for their particular exam.

In the appendix, I present the empirical density of low-level and serious effects, separately by race, gender, and age groups. I do not find evidence of significant differences in the distribution of either supervisor effect along each of these three observable dimensions. Kolmogorov-Smirnov tests for the equality of the distributions generate p-values that are well-above standard significance thresholds.

In Figure **??**, I present densities separately by exam score. I find a striking difference between high and low scorers in the distributions of both serious and low-level effects. For low-level effects, the distribution of high scorers is shifted to the left to he low score distribution, and a Kolmogorov-Smirnov suggests that the this difference is statistically significant (p-value = 0.034). Thus, on average, those who score below average on the promotional exams induce *more* low-level arrests than those who score above the average. These differences are meaningful considering that exams are the primary mechanism to determine who gets promoted and when. The exams test for knowledge of department procedures and aptitude within relevant supervisory situations. To the extent that marginal promotees who are barely promoted by virtue of their exam score are more likely to value low-level arrests, my results indicate that the knowledge required to perform well on promotional exams may be inversely correlated with aggressive policing of mostly victimless crimes.

In Figure **??**, I also find evidence of a statistically significant difference in the distribution of serious effects between high and low scorers (Kolmogorv-Smirnov p-value = 0.013). In contrast to the low-level effects, it does not appear that the differences are driven by a monotonic shift in one direction. Instead, high-scorers exhibit significantly more spread in their propensities to serious arrests compared to low-scorers. The distribution for low-scorers is concentrated around 0. Whereas marginally promoted supervisors tend to focus on low-level arrests and not at the expense of serious ones, the top candidates

for promotion disagree over how much involvement they ought to have in the decisions of their officers to make serious arrests. Thus, many high scorers appear to be split between motivating their officers to be active and arrest more when confronted with accusations of serious criminal behavior, however a meaningful portion seem to be uninvolved with their officers' decisions altogether.

7   Conclusion

This paper shows that supervision matters for police enforcement decisions. Critically, supervisors' preferences are heterogeneous and operate through distinct forms of officer behavior. My findings have several important implications for police reform policy.

First, training or personnel realignments that target first-line supervisors may be cost-effective interventions to change the way that police use discretion. Other police reforms, such as training programs, have shown to be effective but can be degrade in impact over time (Owens et al., 2018). My findings that supervisors provide persistent incentives for their officers to police in a particular way suggests that policies that can change the preferences of supervisors could be effective in a long-lasting way. Moreover, because supervisors represent a smaller portion of police agencies, even policies that would require supervisors to be regularly re-trained would be less costly than effective programs that would require the entire stock of frontline officers to be re-trained at regular intervals.

Second, policing activities that aggressively target low-level crimes are often disconnected from activities targeted at abating more serious crimes. That supervisory effects along these dimensions appear to be largely independent supports a burgeoning body of literature that reducing violent and property crimes does not necessarily require harsh enforcement for low-level crimes that may be more connected to public health and civilians' overall quality of life (Cho et al., 2023). Moreover, I provide evidence that actions that specifically target low-level crimes produce more collateral damage via use of force than actions that increase arrests for serious crimes.

Third, this paper highlights the importance of future research regarding promotion mechanisms in the public sector broadly and policing in particular. By showing that the preferences of managers vary significantly by their performance in the standardized promotion process, my findings indicate that even "objective" promotion tools can produce unexpected tradeoffs for public organizations when selecting staff to occupy positions with supervisory powers.

Ultimately, my paper indicates that policy interventions that target first-line police management would be a fruitful direction for future research.

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8   Tables and Figures

Figure 1: Event Study Coefficients
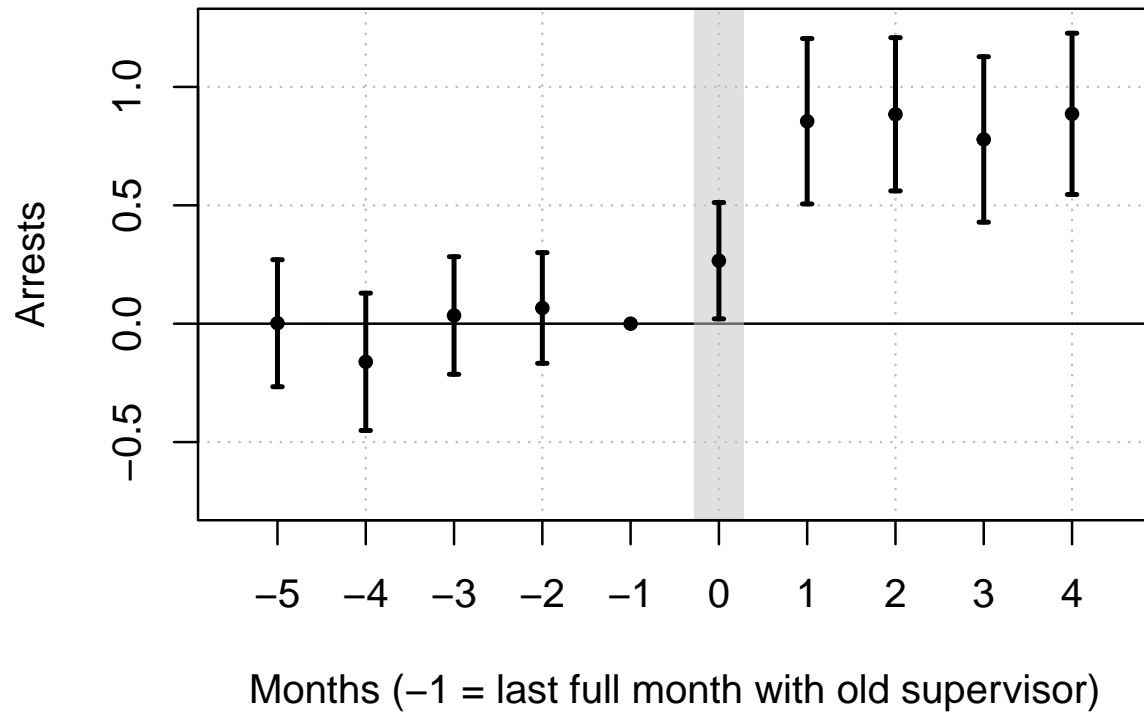
Notes: This presents the event study coefficients for equation 6. Standard errors are clustered at the officer level. Month -1, the last full month an officer spends with their old supervisor, is used as the reference month. The model is estimated using the event study data that are balanced on [-5, 4].

Figure 2: Event Study by Arrest Type

Months (−1 = last full month with old supervisor)
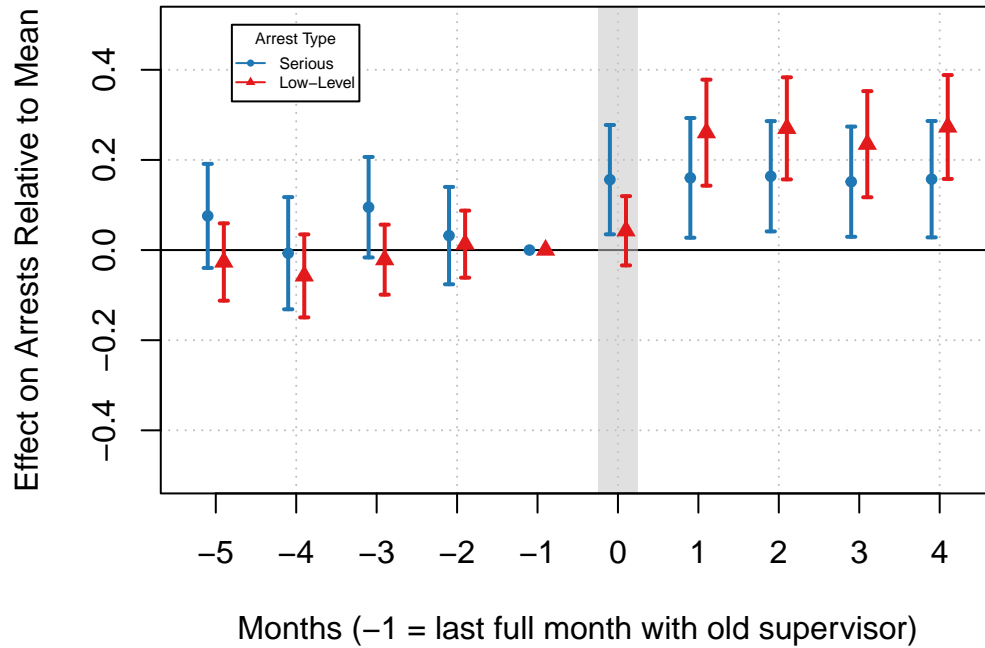
Notes: This figure plots event study coefficients for equation 6, separately for models that use serious arrests (defined as index arrests as well as domestic violence, fraud, simple assault, and DUI) and low-level arrests as the dependent variable. Serious arrest results are given in blue and low-level arrest results are given in red. Month -1, the last full month that the officer spends with the old supervisor, is the reference month in all specifications. For each severity level, the effects are normalized to the average number of arrests in month -1. The model is estimated using the event study data that are balanced on [-5, 4].

Figure 3: Arrests Made by Incumbent Officers
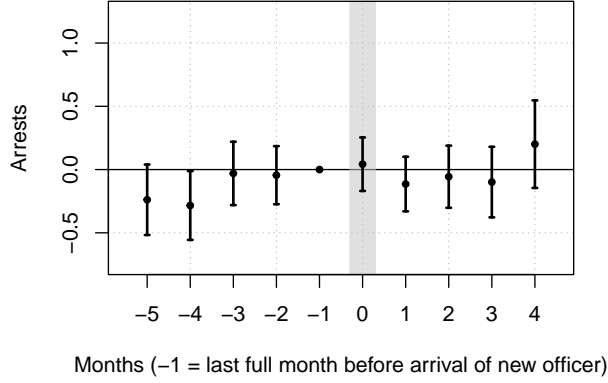
Months (−1 = last full month before arrival of new officer)

Notes: This figure plots the event study coefficients from equation 8. For an officer switching event e in which officer i switches from supervisor $\underline{j}$ to supervisor $\bar{j}$, incumbent officers are those who work with supervisor $\bar{j}$ for 5 months before the event and and 4 months after the event. The x-axis indicates months relative to officer i's switch. Standard errors are clustered at the level of the switching officer.

Appendices

.1 Empirical Bayes Shrinkage

The raw supervisor fixed effects are estimated with error. Suppose that the estimates are given by:

$$\hat{\psi}_j = \psi_j + \epsilon_j, \tag{12}$$

where $\psi_j \sim \mathcal{N}(0, \sigma_\psi^2)$, $\epsilon_j \sim \mathcal{N}(0, \sigma_{\epsilon_j}^2)$, and ψ_j and ϵ_j are independently distributed across the population of 347 supervisors. The mean of the supervisor fixed effects is 0 by construction, since the true mean is unidentified in the model. Under these distributional assumptions, we have that

$$\hat{\psi}_j | \psi_j \sim \mathcal{N}(\psi_j, \sigma_\epsilon^2). \tag{13}$$

Hence, it is implied that each of the fixed effects are unbiased estimates of supervisor j's effect, as is the case under the identifying assumptions laid out in Section 4. As shown by Morris (1983), one can construct a more efficient estimator of ψ_j using the posterior mean of ψ_j conditional on the estimate $\hat{\psi}_j$:

$$E[\psi_j | \hat{\psi}_j] = \lambda_j \hat{\psi}_j, \tag{14}$$

where $\lambda_j = \frac{\sigma_\psi^2}{\sigma_\psi^2 + \sigma_{\epsilon_j}^2}$. As described in the text, I estimate the shrinkage factor $\hat{\lambda}_j$ by bootstrapping the estimation of equation 1. For each supervisor j, I obtain bootstrap estimates of the fixed effect $\hat{\psi}_j^k$, where k= 1,...,1000. I estimate the error variance of each $\hat{\psi}_j$ using the sample variance of the bootstrap distribution: $\hat{\sigma}_{\epsilon_j}^2 = \frac{1}{k-1} \sum_{k=1}^{1000} (\hat{\psi}_j^k - \bar{\hat{\psi}}_j^k)^2$. I then estimate $\hat{\sigma}_\psi^2$ using the variance estimator proposed by Morris (1983):

$$\hat{\sigma}_\psi^2 = \frac{\sum W_j (\hat{\psi}_j^2 - \hat{\sigma}_j^2)}{\sum W_j}. \tag{15}$$

For my main estimates, I use weights $W_j = 1$, so that the estimate takes the form:

$$\hat{\sigma}_\psi^2 = Var(\hat{\psi}_j) - E_j(\hat{\sigma}_j^2). \tag{16}$$

One can also use the weights proposed by Morris (1983): $\frac{1}{\hat{\psi}_j^2 + \hat{\sigma}_j^2}$, which requires one to estimate $\hat{\sigma}_\psi^2$ iteratively by first plugging in a guess of the across-supervisor variance and calculating as in equation 16 until the values are sufficiently close. Using this weighted estimate provides similar shrinkage factor.

It is also possible to estimate the variance components directly from the regression residuals using a method proposed by Guarino et al. (2015) and implemented in the policing context by Weisburst (2024). I note that using this method produces nearly identical shrunken estimates to the one used in the main text.
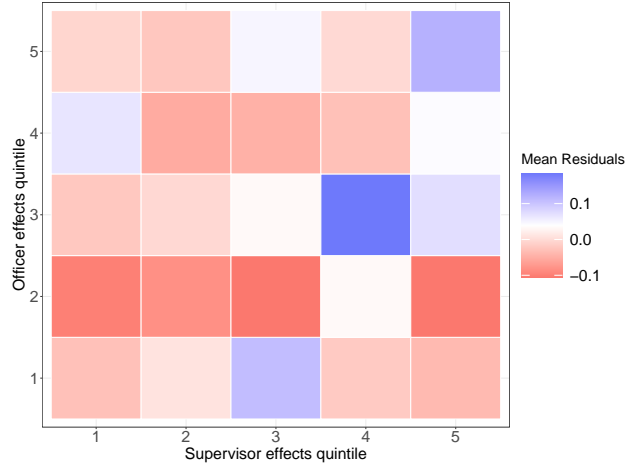
Figure 1: Residuals by quintile of officer and supervisor arrest effects

Notes: This figure reports the average residuals by quintiles of officer and supervisor arrest effects. Darker blue indicates more positive residuals and darker red indicates more negative residuals.

Figure 5: Arrests Made by Incumbent Officers
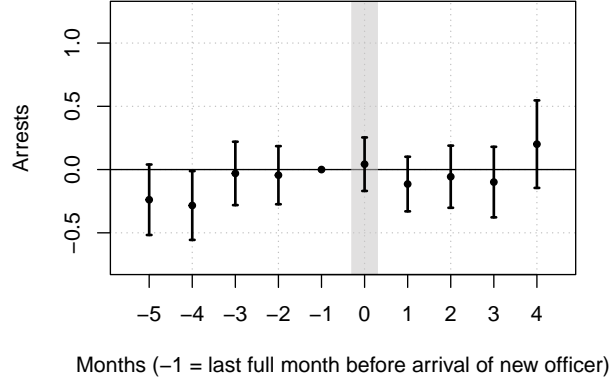
Months (−1 = last full month before arrival of new officer)

Notes: This figure plots the event study coefficients from equation 8. For an officer switching event e in which officer i switches from supervisor $\underline{j}$ to supervisor $\bar{j}$, incumbent officers are those who work with supervisor $\bar{j}$ for 4 months before the event and and 4 months after the event. The x-axis indicates months relative to officer i's switch.

Figure 10: Supervisor effects on crime

(a) $\hat{\pi}_S^k$

(b) $\hat{\pi}_L^k$

Notes: The figures report for the event study coefficients estimated in equation 11.

Figure 2: Robustness to Alternative Sampling Decisions

(a) Unrestricted

Correlation = 0.8720

(b) Impute Missing Within Spell

Correlation = 0.8666

(c) Impute Missing Within Spell, remove everything else

Correlation = 0.9873

(d) Impute all temporary assignments

Correlation = 0.9362

(e) Keep temporary, remove missing

Correlation = 0.9153

(f) Keep missing, remove temporary
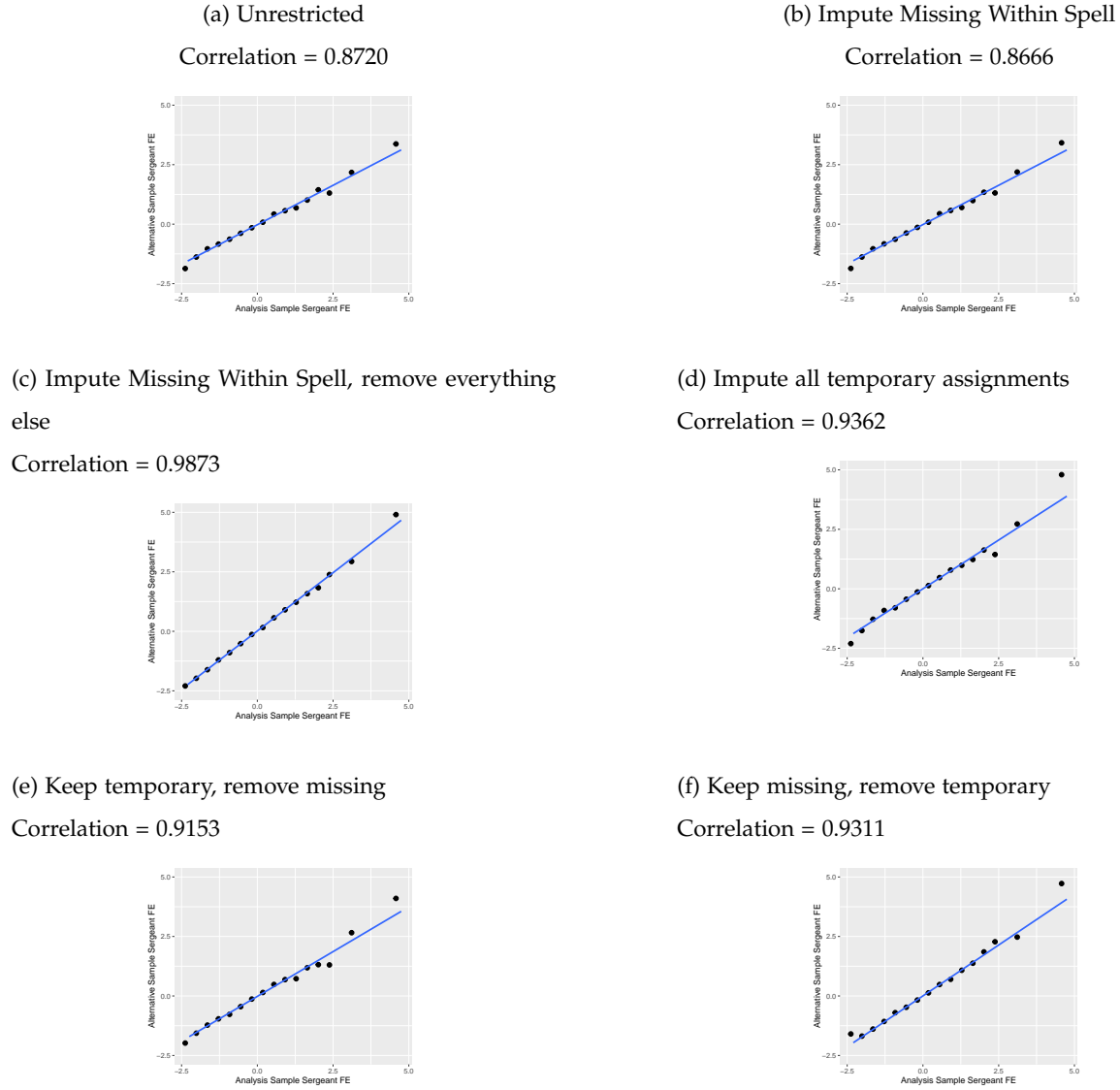
Correlation = 0.9311

Notes: This figure presents the correlation between supervisor fixed effects under different sampling restrictions. (a) makes no sample restrictions, (b) imputes missing observations within a continuous sergeant spell and keeps any other missing sergeant observations, (c) is the same as (b) but other observations with unknown sergeants are removed, (d) imputes temporary one-off assignments with different sergeants using an officer's permanent sergeant, (e) keeps all of the temporary assignments but removes all months with an unknown sergeant, and (f) keeps months with an unknown sergeant but removes the temporary assignments.
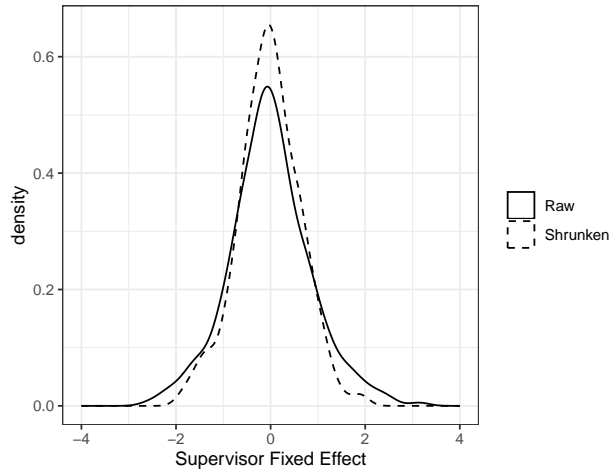
Figure 3: PDF of Supervisor Fixed Effects

Notes: This figure plots the supervisor fixed effects estimated using equation 1. The solid line presents the raw effects, while the effects multiplied by the Bayesian shrinkage factor as described in Section 4.
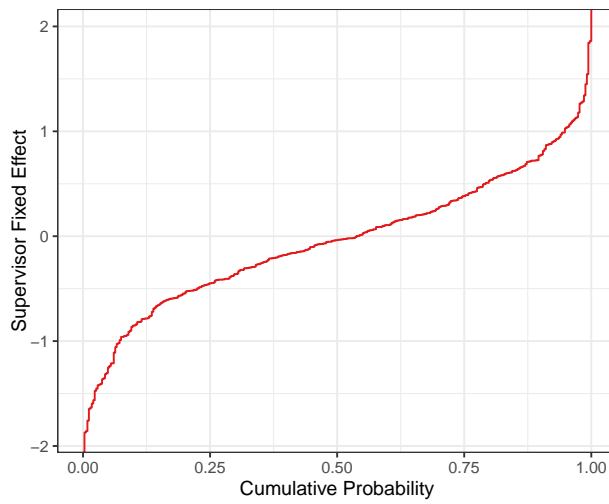
Figure 4: CDF of Supervisor Fixed Effects

Notes: This figure displays the CDF of the supervisor fixed effects estimated using equation 1 and multipled by the Bayesian shrinkage factor described in Section 4.

Figure 6: Symmetry in moves

Notes: Each crosshair represents a pair of symmetric moves between supervisors in different terciles. Changes in residual arrests are calculated as the average difference between the average number of arrests 2 months after a move and 2 months before a move.
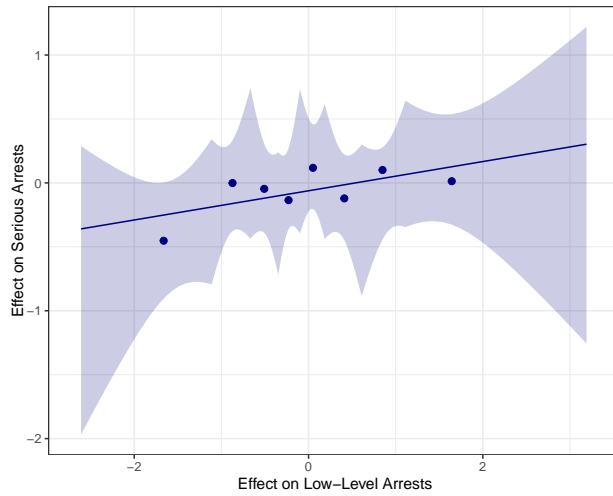
Figure 7: Comparing Low-level and serious arrest effects

Notes: This figure displays a binned scatterplot of the relationship between the standardized low-level supervisor effects and standardized serious supervisor effects. The bins are chosen according to the procedure described by (Cattaneo et al., 2024). The blue line represents a linear fit and the purple field gives a 95% nonparametric confidence band.

Figure 8: Nonparametric event study using sergeant quartiles

(a) Event Study Figure

(b) Symmetry Test

Supervisor Transition
1 to 1   1 to 3   4 to 1   4 to 3
1 to 2   1 to 4   4 to 2   4 to 4
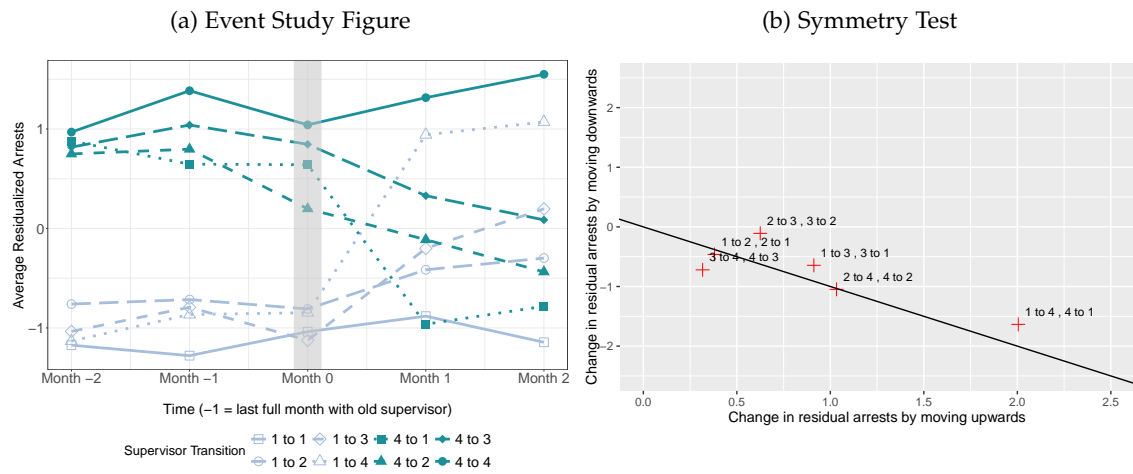
Notes: These figures present the same information as Figure 9 and **??**, instead splitting supervisors into quartiles rather than terciles. To limit the amount of lines on the figure, I only plot transitions from supervisors in the highest and lowest quartiles in (a).

Table 1: Variance Decomposition

	Raw		Shrinkage		Homosk. Bias-Correction		Heterosk. Bias-Correction	
	Component	% Share	Component	% Share	Component	% Share	Component	% Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Var(y^*)$	11.153	100.00%	11.153	100.00%	11.153	100.00%	11.159	100.00%
$Var(\psi)$	0.559	5.01%	0.382	3.43%	0.379	3.40%	0.378	3.39%
$Var(\theta)$	8.906	79.86%	8.002	71.75%	8.097	72.60%	8.068	72.31%
$Cov(\psi,\theta)$	-0.168	-1.51%	-0.157	-1.41%	-0.0592	-0.53%	-0.0599	-0.54%
$Var(\psi+\theta)$	9.129	81.86%	8.071	72.37%	8.357	74.93%	8.33	74.62%
N supervisors	347		347		347		344	
N officers	1805		1805		1805		1802	

Notes: This table presents the variance decompositions described in equation 4. As described in Section 4, y^* is the number of monthly arrests, residualized on sector-watch, day-off group, and a second-degree polynomial of tenure; ψ is the supervisor fixed effect; θ is the officer fixed effect. All statistics are calculated on data aggregated to the officer-supervisor pair. Columns (1) and (2) report results for the raw fixed effects estimates. Columns (3) and (4) use fixed effects that are multiplied by the Bayesian shrinkage factor, constructed as described in Section 4. Columns (5) and (6) use the bias correction method proposed by Andrews et al. (2008) that assumes homoskedastic error terms. This bias correction is implemented using the 'lfe' package in R Gaure (2013) and uses simulation methods to calculate the trace of large matrices, as described in Gaure (2014). As such, I report the average of 100 iterations. Columns (7) and (8) implement the Kline et al. (2020) bias correction method that allows for unrestricted heteroskedasticity in the error terms. This method can only be conducted on the leave-out connected set, which is why the number of supervisors and officers decrease. This implementation adapts Julia code that is provided publicly by the authors and developed by Paul Courcera.

Table 2: Supervisor Activity

	Arrest Activity			Call Activity		
	Total Arrests	Serious Arrests	Low-Level Arrests	Calls	Solo Calls	Subordinate Calls
	(1)	(2)	(3)	(4)	(5)	(6)
Low-level Arrest Effect	0.0777***	0.0062	0.0715***	2.583***	0.5387*	0.6004**
	(0.0249)	(0.0082)	(0.0192)	(0.9720)	(0.2832)	(0.2393)
Serious Arrest Effect	-0.0161	-0.0047	-0.0114	1.266	0.0937	0.2676
	(0.0210)	(0.0068)	(0.0160)	(0.8624)	(0.2776)	(0.2230)
Controls	✓	✓	✓	✓	✓	✓
Observations	7,983	7,983	7,983	7,983	7,983	7,983
R^2	0.08395	0.03647	0.07874	0.18469	0.13804	0.19850
Y mean	0.31605	0.08130	0.23475	28.105	4.2619	7.8373

Notes: This table presents results from regressing measures of supervisor behavior on the estimated low-level and serious supervisor effects, as described by equation 10. Data are at the sector-watch by month level. Controls include the average estimated low-level and serious officer arrest effects for officers within the unit and sector-watch fixed effects. The outcome variables in each column are: (1) the number of arrests that the unit's supervisor makes in the month, (2) the number of those arrests which are serious, (3) the number of those arrests which are low-level, (4) the number of calls for service that the supervisor is listed as responding to in CAD, (5) the number of calls for service that the supervisor responds to alone, and (6) the number of calls for service that a supervisor responds to in which their subordinates are also present. Standard errors are clustered at the supervisor level. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 3: Summary Statistics

	Full Sample	Analysis Sample	Event Study Sample
	(1)	(2)	(3)
1. Number of officers	2,067	1,805	833
2. Number of supervisors	387	347	287
3. Number of officers with >1 sup.	1,856	1,623	833
4. Number of supervisors with >1 off.	384	344	270
5. Mean number of supervisors per off.	5.21	3.97	2.67
6. Mean number of officers per sup.	27.7	20.6	7.74
7. Total officer-supervisor spells	15,355	8,432	2,247
8. Total switching events	13,288	5,798	1,277
9. Number of sector-watches	105	102	102
10. Mean number of supervisors per sector-watch	8.48	6.95	4.61
11. Arrests mean	3.81	3.80	3.65
SD	3.65	3.64	3.46
12. Low-level arrests mean	2.88	2.87	2.75
SD	3.03	3.02	2.88
13. Serious arrests mean	0.925	0.923	0.897
SD	1.29	1.29	1.26
14. Drug arrests mean	0.315	0.311	0.276
SD	0.931	0.928	0.885
15. Warrant arrests mean	0.771	0.766	0.739
SD	1.35	1.35	1.29
16. Disorderly conduct arrests mean	0.416	0.409	0.370
SD	0.941	0.921	0.839
17. Proactive arrests mean	1.72	1.71	1.61
SD	2.26	2.24	2.13
18. Convicted arrests mean	0.776	0.773	0.712
SD	1.30	1.30	1.21
19. Use of force mean	0.119	0.118	0.114
SD	0.324	0.322	0.318
20. Complaint mean	0.0139	0.0141	0.0151
SD	0.117	0.118	0.122
Number of observations	61,166	49,923	12,770

Notes: The table reports summary statistics for three samples. The Full Sample is the unrestricted sample of all patrol officers. The Analysis Sample contains all patrol officer months that satisfy the restrictions described in Section 3. The Event Study sample contains all switching events in which the focal officer is observed with the pre-switch sergeant at least 6 months prior to the switch and the post-switch sergeant at least 4 months after the switch. Serious arrests are defined as index arrests as well as domestic violence, fraud, simple assault, and DUI. All other arrests are considered low-level. Drug (warrant/disorderly conduct) arrests are any arrests which contain a drug (warrant/disorderly conduct) charge and do not contain any other higher-level (i.e. serious) charges. An arrest is considered to be convicted if the arrest is matched to a court disposition and not dismissed; this includes guilty findings by judge, jury, or plea. Use of force (complaint) is a binary indicator for any use of force (complaint) taking place in a month.

Table 4: Source of Civilian Interaction

	Officer Initiated Arrests	Call Initiated Arrests
	(1)	(2)
Low-level Arrest Effect	0.4495***	0.2542***
	(0.0372)	(0.0239)
Serious Arrest Effect	0.0277	0.2442***
	(0.0248)	(0.0201)
Baseline Controls	✓	✓
Observations	49,923	49,923
Y mean	1.7008	2.0967

Notes: This table reports the estimated effects of changes in the two dimensions of supervisor preferences on arrests from different sources of civilian interaction. The low-level (serious) arrest effect is given by the standardized Bayes-shrunken supervisor fixed effect on low-level (serious) arrests. The baseline controls include officer fixed effects and the full set of controls used in equation 1. Standard errors are clustered at the officer level. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table 5: Supervisor Effects by Crime Type

	Serious Crimes			Low-Level Crimes		
	Domestic Violence	Theft	DWI	Drugs	Warrants	Disorderly Conduct
	(1)	(2)	(3)	(4)	(5)	(6)
Low-level Arrest Effect	-0.0161**	-0.0025	-0.0013	0.1713***	0.1772***	0.1076***
	(0.0080)	(0.0053)	(0.0061)	(0.0238)	(0.0203)	(0.0134)
Serious Arrest Effect	0.1687***	0.0382***	0.0352***	-0.0360***	0.0755***	0.0350***
	(0.0082)	(0.0064)	(0.0063)	(0.0139)	(0.0148)	(0.0093)
Baseline Controls	✓	✓	✓	✓	✓	✓
Observations	49,923	49,923	49,923	49,923	49,923	49,923
Y mean	0.45308	0.13927	0.10885	0.31138	0.76578	0.40903

Notes: This table reports the estimated effects of changes in the two dimensions of supervisor preferences on arrests for the three most frequent serious and low-level crimes. Serious and low-level crimes are mutually exclusive categories. However, within serious and low-level crimes, an arrest may fall under multiple different criminal charges. The low-level (serious) arrest effect is given by the standardized Bayes-shrunken supervisor fixed effect on low-level (serious) arrests. The baseline controls include officer fixed effects and the full set of controls used in equation 1. Standard errors are clustered at the officer level. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table 6: Supervisor Effects on Call Activity

	Calls Answered	Arrest Probability at Calls	Low-Level Call Arrests	Serious Call Arrests
	(1)	(2)	(3)	(4)
Low-level Arrest Effect	1.069***	0.0032***	0.2637***	-0.0119
	(0.3701)	(0.0004)	(0.0192)	(0.0101)
Serious Arrest Effect	2.202***	0.0017***	0.0281*	0.2154***
	(0.3084)	(0.0003)	(0.0153)	(0.0101)
Baseline Controls	✓	✓	✓	✓
Observations	49,923	49,923	49,923	49,923
Y mean	61.415	0.02741	1.4101	0.67718

Notes: This table reports the estimated effects of changes in the two dimensions of supervisor preferences on officer activity relating to 911 call responses. Calls answered refer to calls in which the officer is among the first units dispatched to the scene. Arrest probability is measured according to arrests in which the focal officer is present on the arrest report. The low-level (serious) arrest effect is given by the standardized Bayes-shrunken supervisor fixed effect on low-level (serious) arrests. The baseline controls include officer fixed effects and the full set of controls used in equation 1. Standard errors are clustered at the officer level. $^{***}p < 0.01,^{**} p < 0.05,^* p < 0.1$.

Table 7: Supervisor Effects on Other Activity

	Use of Force Incidents	Complaints
	(1)	(2)
Low-level Arrest Effect	0.0203***	0.0040
	(0.0043)	(0.0029)
Serious Arrest Effect	0.0059*	-0.0019
	(0.0034)	(0.0025)
Baseline Controls	✓	✓
Observations	49,923	49,923
Y mean	0.13813	0.02398

Notes: This table reports the estimated effects of changes in the two dimensions of supervisor preferences on the number of use of force incidents and complaints involving the supervisor's officer. The low-level (serious) arrest effect is given by the standardized Bayes-shrunken supervisor fixed effect on low-level (serious) arrests. The baseline controls include officer fixed effects and the full set of controls used in equation 1. Standard errors are clustered at the officer level. $^{***}p < 0.01,^{**} p < 0.05,^* p < 0.1$.

Table 8: Supervisor Effects on Conviction Rates

	Convicted Arrests	Total Arrests	Difference in Ratio
	(1)	(2)	(3)
Low-level Arrest Effect	0.1679***	0.7037***	0.0349**
	(0.0224)	(0.0447)	(0.0138)
Serious Arrest Effect	0.0932***	0.2720***	0.1389***
	(0.0163)	(0.0345)	(0.0230)
Baseline Controls	✓	✓	
Observations	49,923	49,923	
Y mean	0.77337	3.7975	

Notes: This table reports the estimated effects of changes in the two dimensions of supervisor preferences on officer conviction rates. Column 1 reports estimates from a regression of the number of convicted arrests on each dimension of supervisor fixed effects. Column 2 reports results from the same regression, instead using total arrests as the dependent variable. In column 3, I report the difference between the ratio of the estimates in columns 1 and 2 and the ratio of the means for the dependent variables in columns 1 and 2. Standard errors in column 3 are obtained using 100 bootstrap iterations. The low-level (serious) arrest effect is given by the standardized Bayes-shrunken supervisor fixed effect on low-level (serious) arrests. The baseline controls include officer fixed effects and the full set of controls used in equation 1. Standard errors are clustered at the officer level. $^{***}p < 0.01,^{**}\,p < 0.05,^{*}\,p < 0.1$.

Table 9: Analysis of Variance

	Arrests				
	(1)	(2)	(3)	(4)	(5)
R^2	0.166379	0.516861	0.527611	0.202556	0.623947
Adjusted R^2	0.164421	0.497526	0.505139	0.195091	0.559736
Controls	✓	✓	✓	✓	✓
Officer FE		✓	✓		
Supervisor FE			✓	✓	
Supervisor-by-Officer FE					✓
Observations	49,923	49,923	49,923	49,923	49,923

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table reports R^2 and adjusted R^2 for models that vary the included fixed effects. Controls include a second degree polynomial of officer tenure, and sector-watch and day-off group fixed effects.
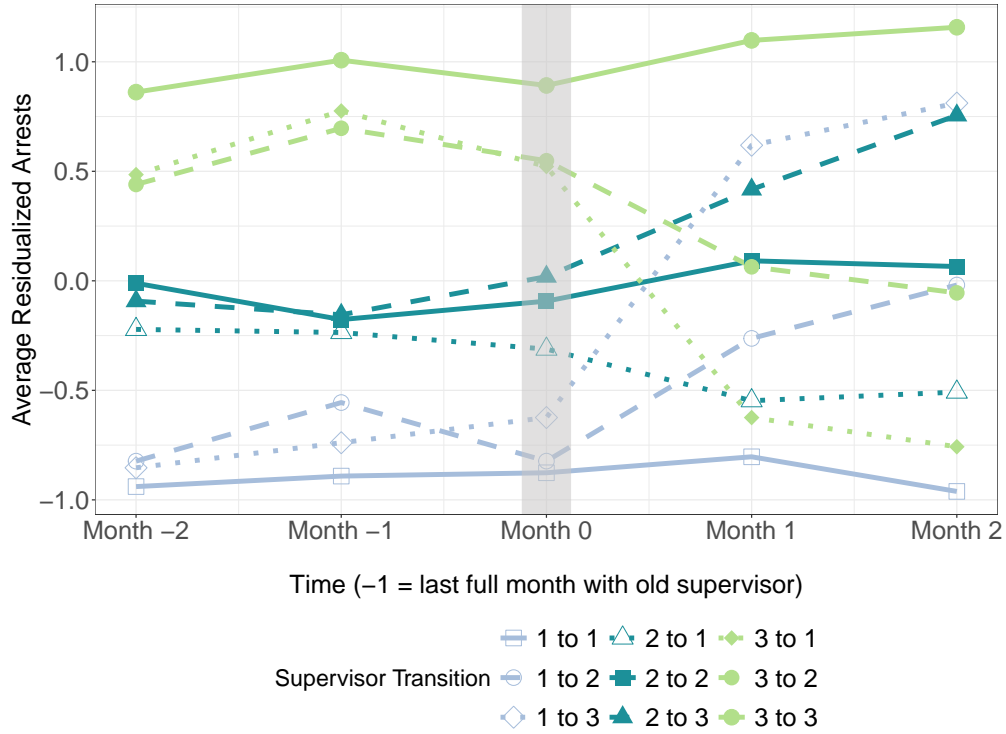
Figure 9: Event Study Around Moves

Notes: This figure plots the average number of arrests in the months around a supervisory change by the magnitude of the change. In particular, I group supervisors into terciles according to the average number of residual arrests made by their officers throughout the sample. Each line then plots the average residualized arrests made by officers who transition between terciles, where the terciles of the previous and subsequent supervisor are described by "Supervisor Transition." Arrests are residualized by a second-degree polynomial of officer tenure and officer, sector-watch, and day-off group fixed effects.

Table 10: Trends in Crime

	All Months		Months With Movers	
	$E[\Delta\hat{\psi}_{out}]$	$E[\Delta\hat{\psi}_{in}]$	$E[\Delta\hat{\psi}_{out}]$	$E[\Delta\hat{\psi}_{in}]$
	(1)	(2)	(3)	(4)
$Log(911Calls)_{-1}$	-0.0056	0.0285	-0.0052	0.1420
	(0.0401)	(0.0424)	(0.1630)	(0.1302)
$Log(911Calls)_{-2}$	-0.0022	0.0029	-0.0052	0.0053
	(0.0490)	(0.0525)	(0.1818)	(0.1700)
$Log(911Calls)_{-3}$	0.0018	-0.0799	-0.0609	-0.3495*
	(0.0535)	(0.0526)	(0.1635)	(0.1824)
$Log(911Calls)_{-4}$	-0.0647	0.0335	-0.2065	0.1106
	(0.0488)	(0.0546)	(0.1924)	(0.1488)
$Log(911Calls)_{-5}$	0.0704*	-0.0152	0.1350	0.0411
	(0.0423)	(0.0475)	(0.1884)	(0.1653)
Observations	5,525	5,525	1,387	1,424
Y mean	0.00738	0.00401	0.02938	0.01555
Joint F p-value	0.62913	0.58831	0.83440	0.39364
Sector-Watch fixed effects	✓	✓	✓	✓

Notes: This table examines the correlation of crime trends with supervisor switches. Regressions are performed at the sector-watch by month level. Dependent variables are the average change in the supervisor effects for outmovers (columns 1 and 3) and in-movers (columns 2 and 4).

Table 11: Data Sources

Data Source	Variables
(1)	(2)
Computer Aided Dispatch Entries (2014-2019)	Assignments/911 Calls
Arrest Reports (2014-2019)	Number of arrests
Charge Reports (2014-2019)	Type of arrest
Use of Force Reports (2014-2019)	Use of force incidents
Civilian Complaints (2014-2019)	Number of complaints
Disposed Cases, Dallas County DA (2014-2019)	Conviction
Various Personnel Records (2014-2019)	Watch/day-off group/promotion dates
Sergeants Exam Results (2012; 2014; 2018)	Composite Promotional Score

Table 12: Crime Trends Don't Predict Sergeant FE

	Supervisor Effect
	(1)
$Log(911 Calls)_{-1}$	0.0712
	(0.0455)
$Log(911 Calls)_{-2}$	0.0254
	(0.0319)
$Log(911 Calls)_{-3}$	-0.0352
	(0.0297)
$Log(911 Calls)_{-4}$	-0.0544
	(0.0347)
$Log(911 Calls)_{-5}$	-0.0275
	(0.0485)
Observations	5,525
Y mean	-0.03592
Joint F p-value	0.21720
Sector-Watch fixed effects	✓

Figure 11: Distribution of Supervisors between Serious and Low-level effects

		Tercile of Low-level Effect	
3	11.2%	11.2%	11.0%
2	7.8%	12.7%	12.7%
1	14.4%	9.2%	9.8%
	1	2	3

Tercile of Serious Effect

Tercile of Low-level Effect

Notes: This figure displays the percentage of sergeants within each tercile of low-level and serious arrest effects.

density

Serious arrest effect

White   Black   Hispanic

density

Low-level arrest effect

White   Black   Hispanic