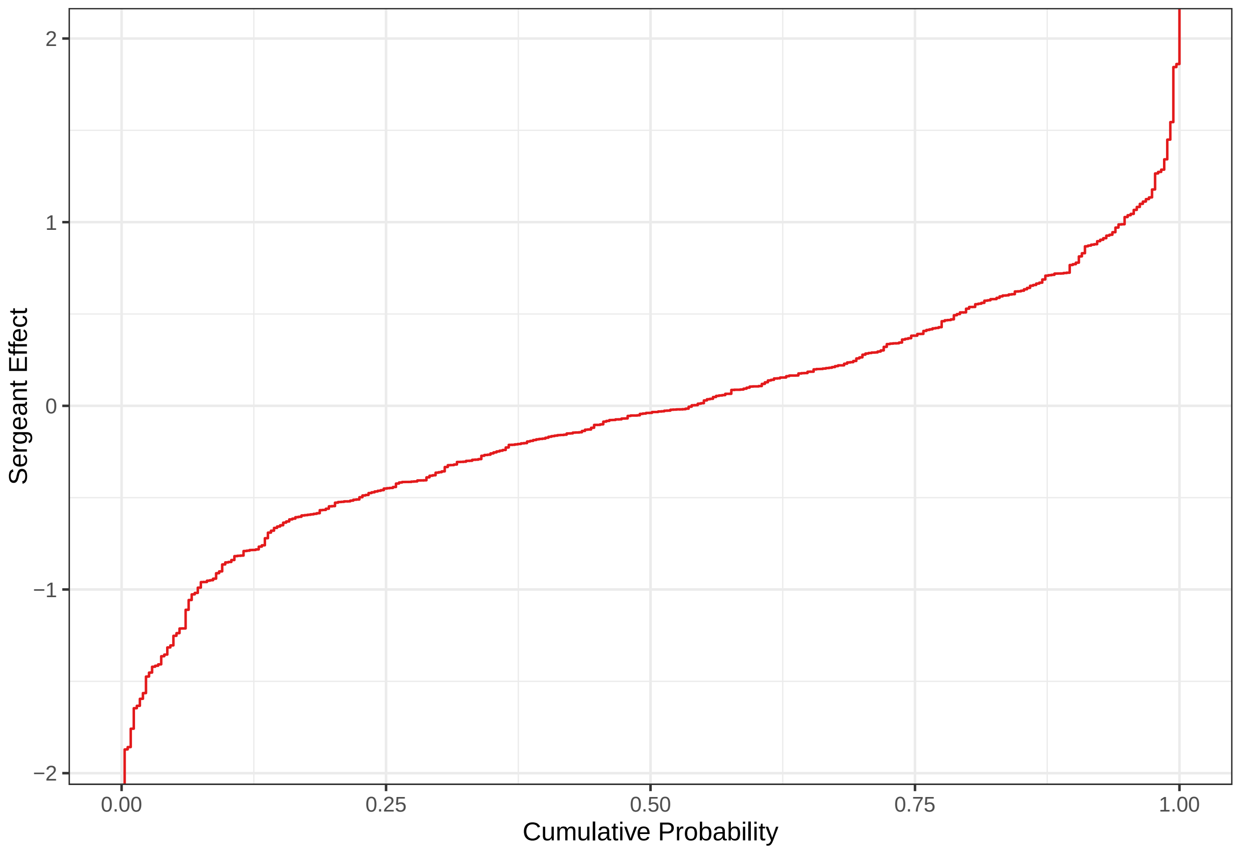
Supplementary Appendix for The Boss in Blue: Supervisors and Police Behavior

A Additional Figures

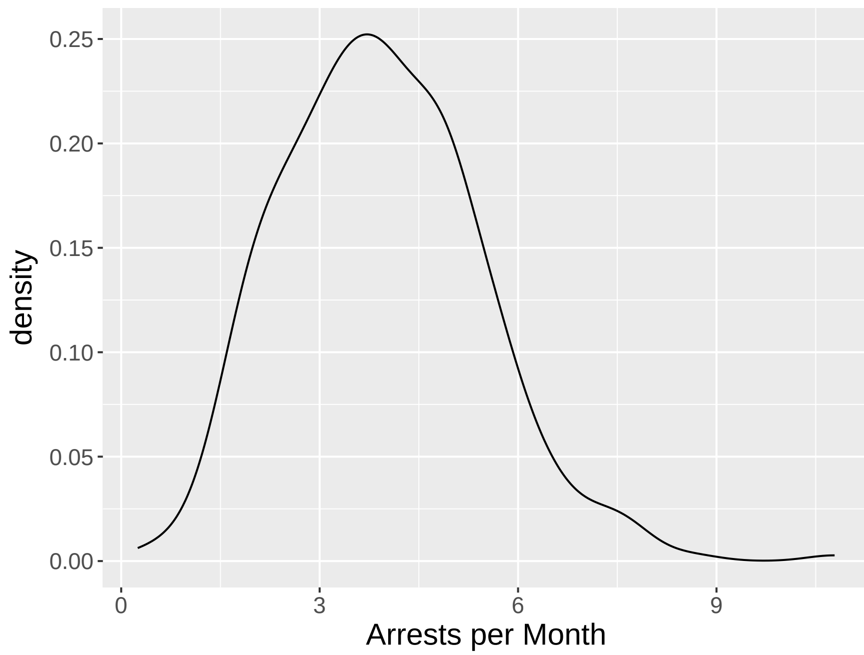
Figure A.1: CDF of Supervisor Fixed Effects



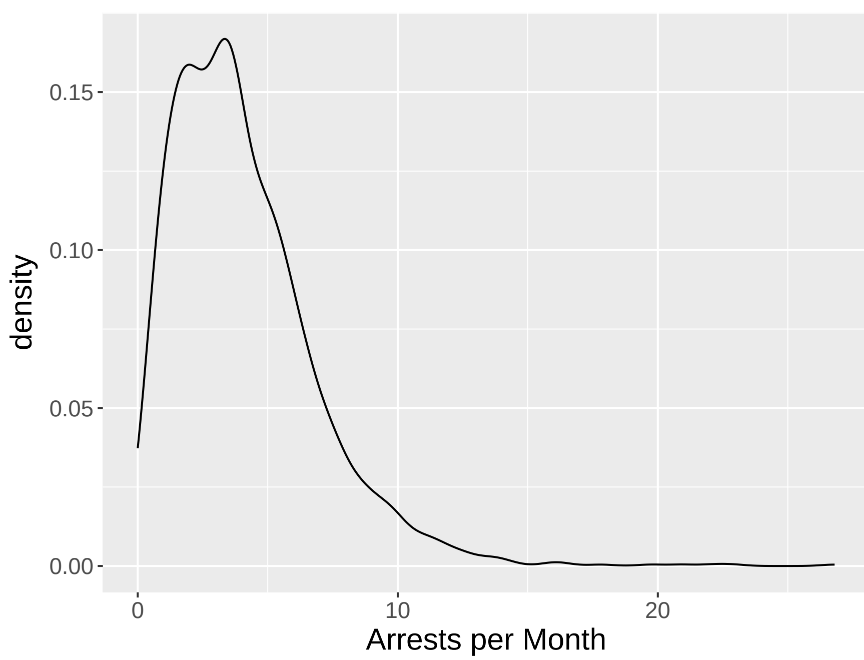
Notes: This figure displays the CDF of the sergeant effects, estimated using the sergeant fixed effects in equation 1 that are multipled by the Bayesian shrinkage factor described in Section 4. Sergeant fixed effects are estimated controlling for a second-degree polynomial of officer tenure, as well as officer, sector-watch, and day-off group fixed effects.

Figure A.2: Distribution of Arrests

(a) Across Sergeants

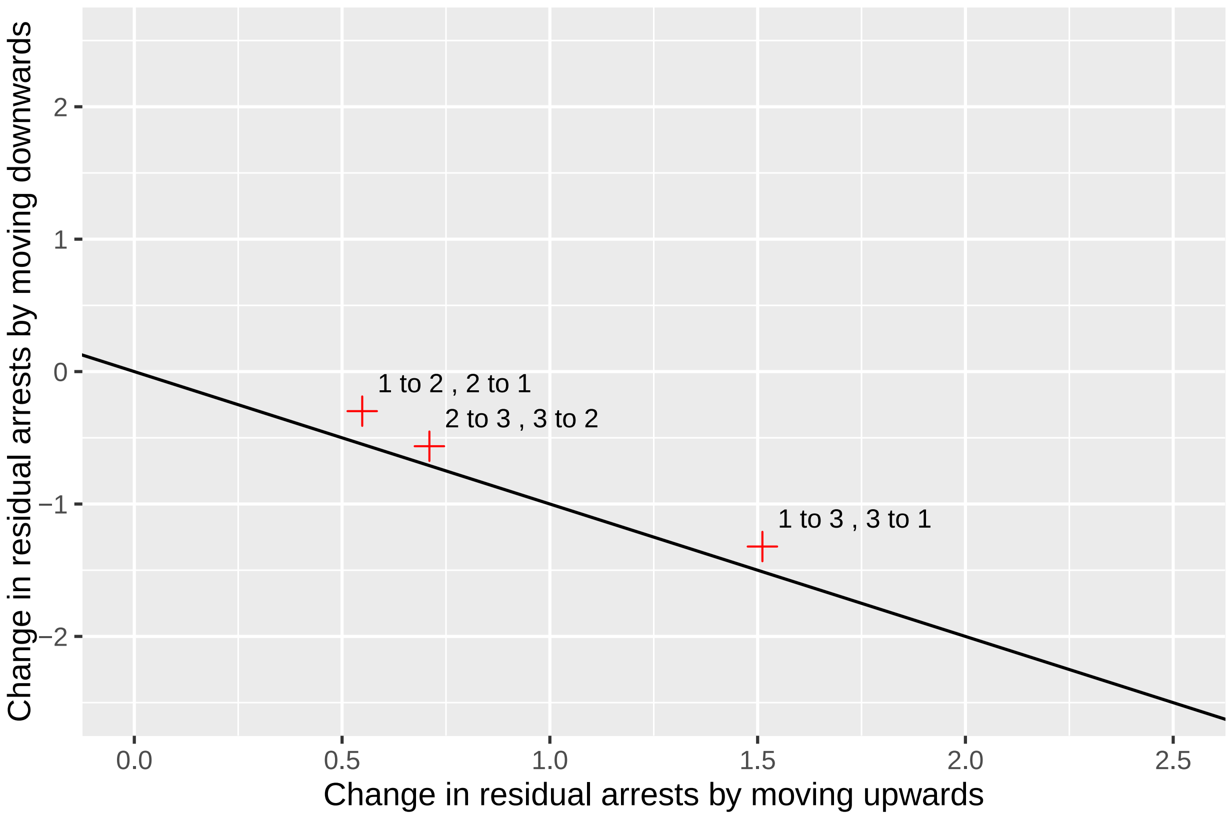


(b) Across Officers



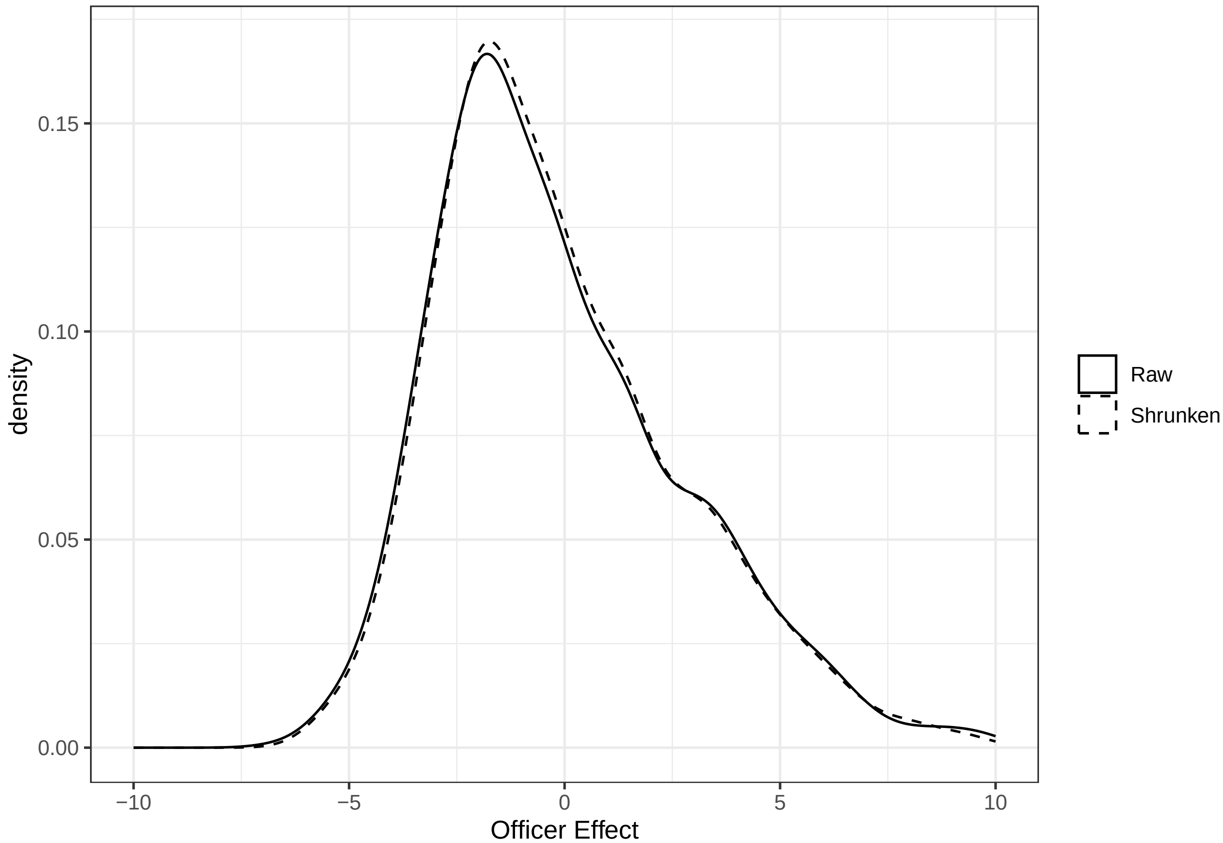
Notes: These figures present empirical distributions for monthly arrests. For a given sergeant, I calculate the average number of monthly arrests made by officers who work for them. A.2a then plots the distribution of this average. Then, for each officer, I calculate the average number of arrests they make in a month across all months they are in the sample. I plot the distribution of this average in A.2b.

Figure A.3: Symmetry in moves



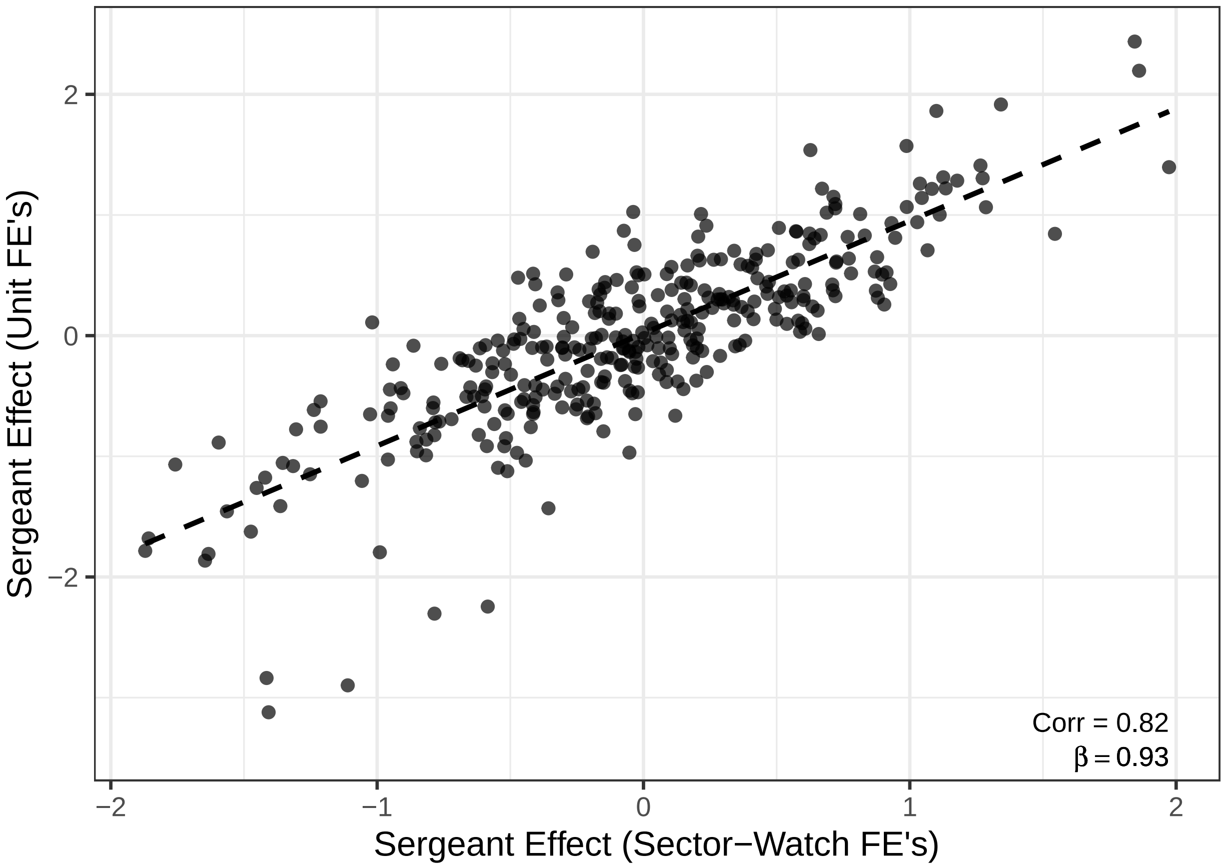
Notes: Each crosshair represents a pair of symmetric moves between sergeants in different terciles of average residualized arrests. The solid black line is a negative 45-degree line. Changes in residual arrests are calculated as the average difference between the average number of arrests 2 months after a move and 2 months before a move. Arrests are residualized by officer, sector-watch, and day-off group fixed effects and a second degree polynomial of tenure, as described in Section 4.

Figure A.4: Distribution of Officer Effects



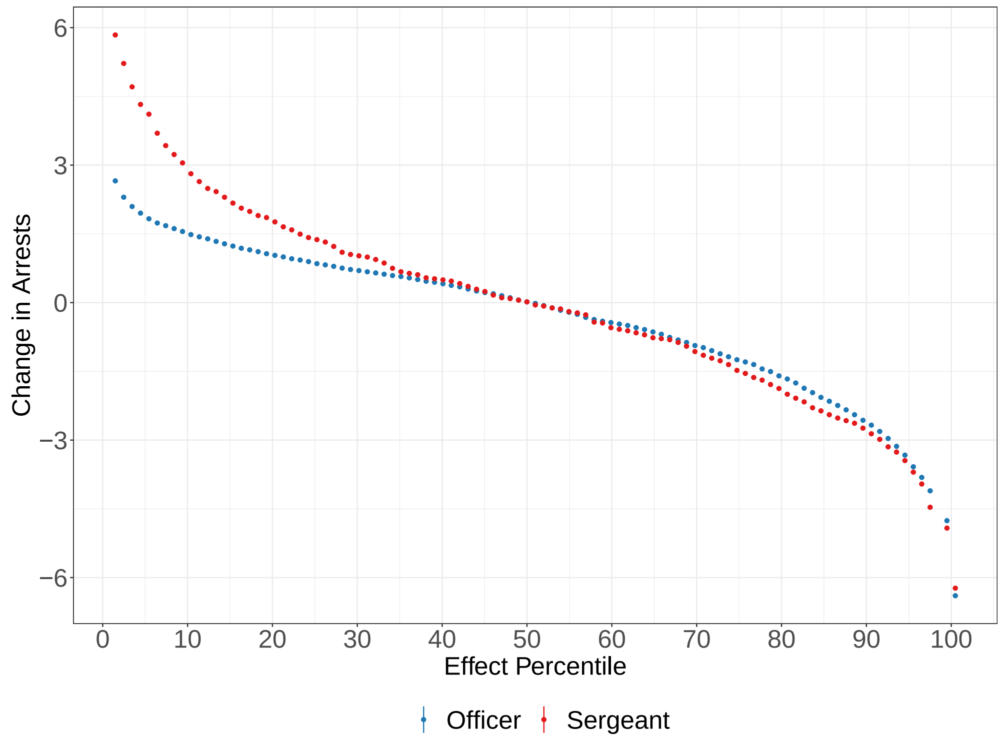
Notes: This figure plots the officer fixed effects estimated using equation 1. The solid line presents the raw fixed effects, while the dotted line presents the raw effects multiplied by the Bayesian shrinkage factor as described in Section 4. Officer fixed effects are estimated controlling for a second-degree polynomial of officer tenure, as well as sergeant, sector-watch, and day-off group fixed effects.

Figure A.5: Sergeant Fixed Effects using Unit FE's



Notes: This figure plots the baseline sergeant effects estimated using equation 1 on the x-axis, against sergeant effects estimated by replacing the sector-watch fixed effects in equation 1 with fixed effects for the unit. There may be up to two units within a sector-watch. Both effects are shrunken using the Empirical Bayes procedure described in Section 4. Correlation and a linear slope estimate (β) are given in the bottom right corner. All regressions include a second-degree polynomial of officer tenure, and officer and day-off group fixed effects.

Figure A.6: Impact of a Median Replacement



Notes: This figure plots the calculated effect of one-month replacements of sergeants (in red) and officers (in blue) with a median employee from the relevant effects distribution. Each sergeant is placed into their percentile in the effects distribution and the change in arrests that would be produced from replacing them with a median sergeant is calculated by subtracting each sergeant's effect from the median sergeant effect, and multiplying by the average number of officers managed in a month (6.33). I then plot the change in arrests against each percentile by averaging over all sergeants within that percentile. The change in arrests for officers is calculated identically, except I do not multiply by 6.33. For this exercise, sergeant and officer fixed effects are re-estimated using equation 1, where the outcome is an adjusted arrest measure that only credits officers for half an arrest when there is another officer listed on the arrest report. The regression includes controls for a second-degree polynomial of officer tenure, as well as sector-watch and day-off group fixed effects.

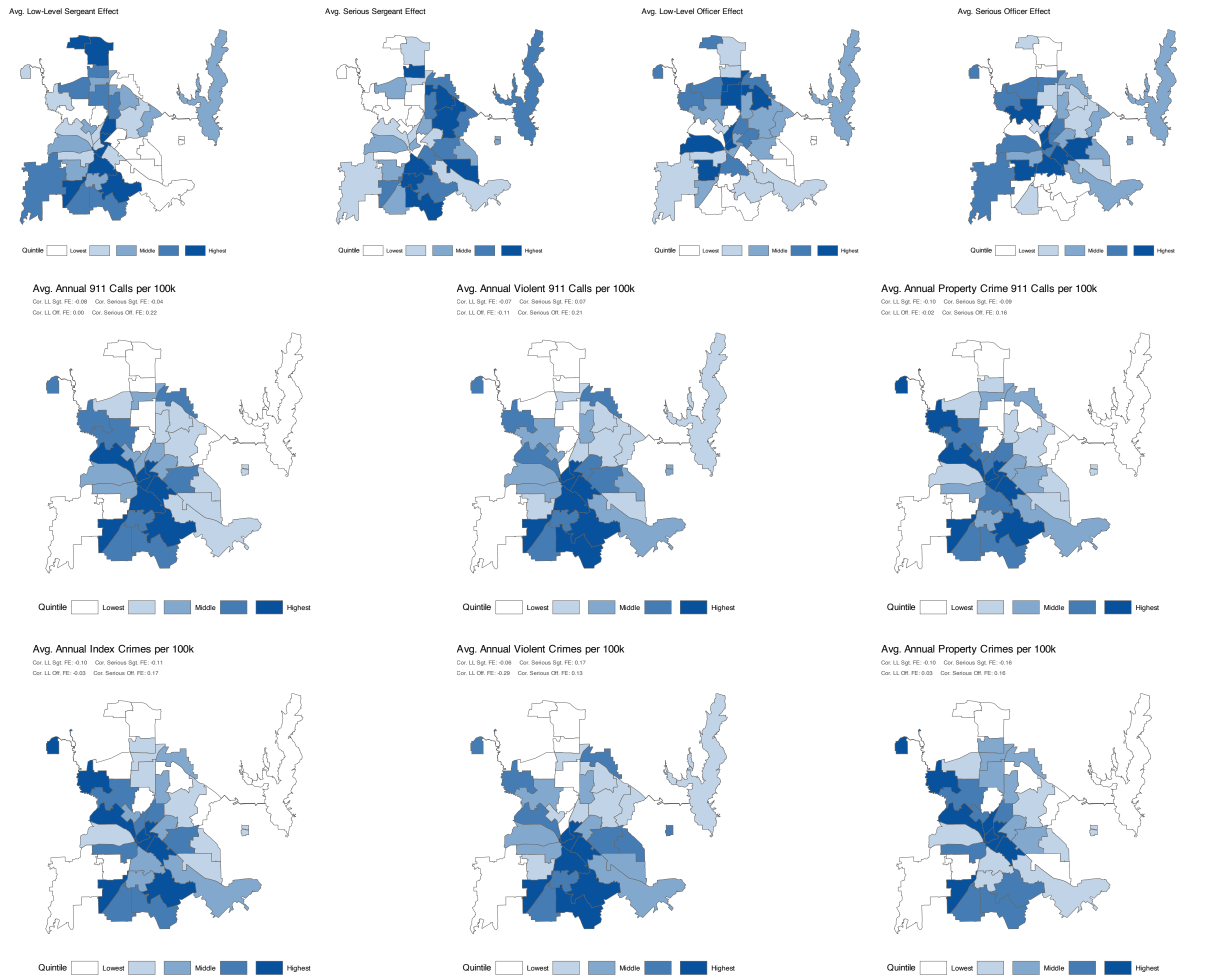
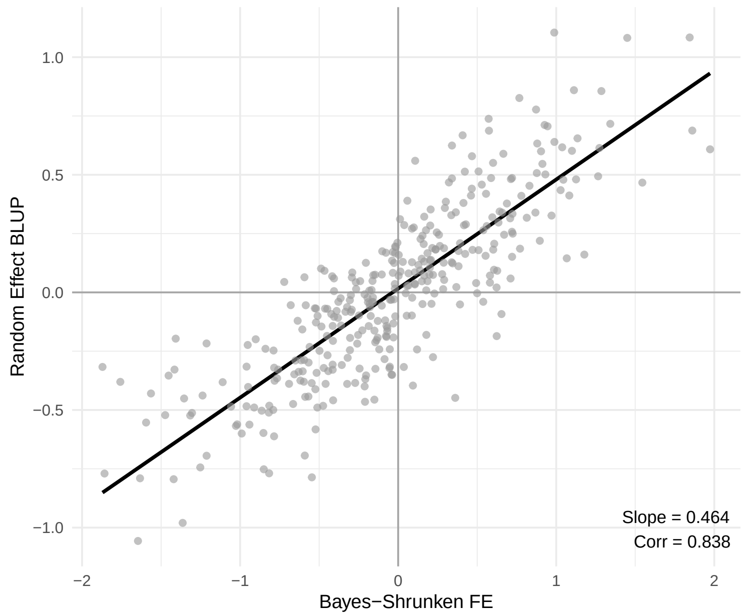


Figure A.7: Spatial Distribution of Sergeant and Officer Fixed Effects and Crime Rates

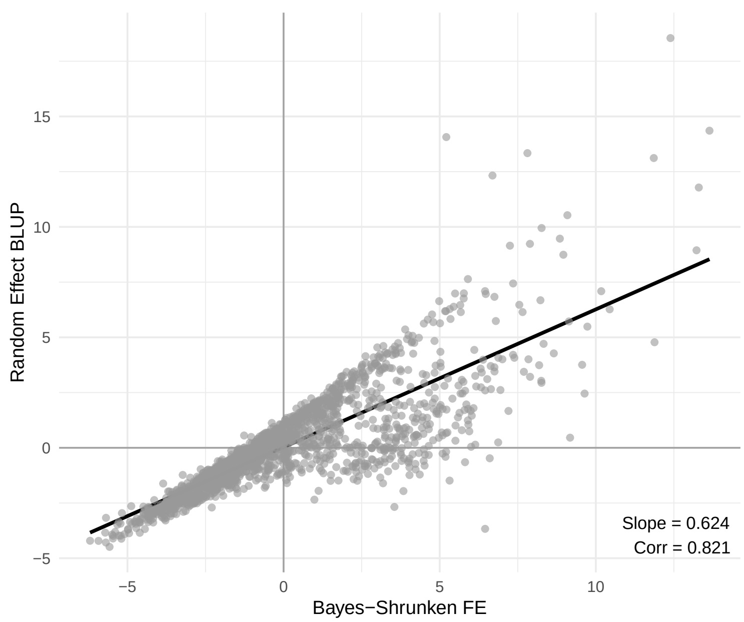
Notes: Each figure is a map of Dallas overlayed with Dallas Police sectors. Maps are built using a sector-level dataset. For the variable given in bold above each figure, the corresponding map shades sectors by quintile. For officer and sergeant effects, sector averages are calculated over the sample period using officer-by-month and unit-by-month panels, respectively. For crime variables, sector averages are calculated over the sample period using a sector-by-year panel. Correlations between average officer/sergeant effects and sector crime averages (given in unbolded text below figure titles) are calculated at the sector level. 911 call data is obtained from CAD. Violent 911 calls include shootings, assaults, major disturbances, and robberies; property crime calls include burglary, larceny, and auto theft. Crime data is obtained via Dallas' NIBRS database. Violent crimes include homicide, aggravated assault, and robbery (note, sex crimes are not reported in publicly available data); property crimes include burglary, theft, arson, and auto theft. Index crimes are the sum of violent and property crimes. Officer and sergeant effects are estimated using equation 1, controlling for a second-degree polynomial of officer tenure and sector-watch and day-off group fixed effects. Officer and sergeant effects are shrunken using Empirical Bayes, as described in Section 4.

Figure A.8: Fixed Effects vs. Random Effect BLUPs

(a) Sergeant Effects



(b) Officer Effects



Notes: Each figure plots the Bayes-shrunken fixed effects on the x-axis, calculated using equation 1 and the Empirical Bayes shrinkage procedure described in Section 4.1, and Best Linear Unbiased Predictors (BLUPs) from the random effects model described in Section 4.2 on the y-axis. Each point is a sergeant (top panel) or officer (bottom panel). The black line represents a linear fit, with its slope and the estimated correlation given in the bottom right corner. The random effects model controls for a second-degree polynomial of officer tenure and includes sector-watch and day-off group as fixed effects. The random effects model is estimated using REML.

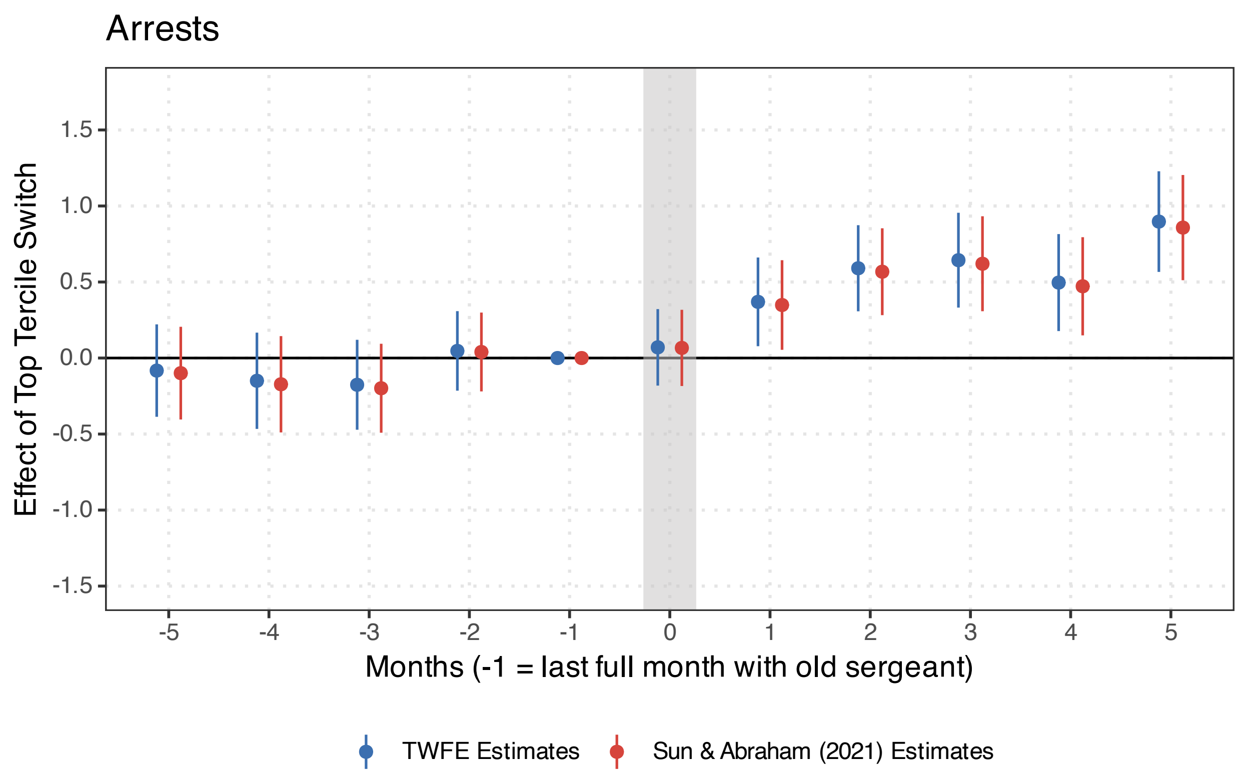
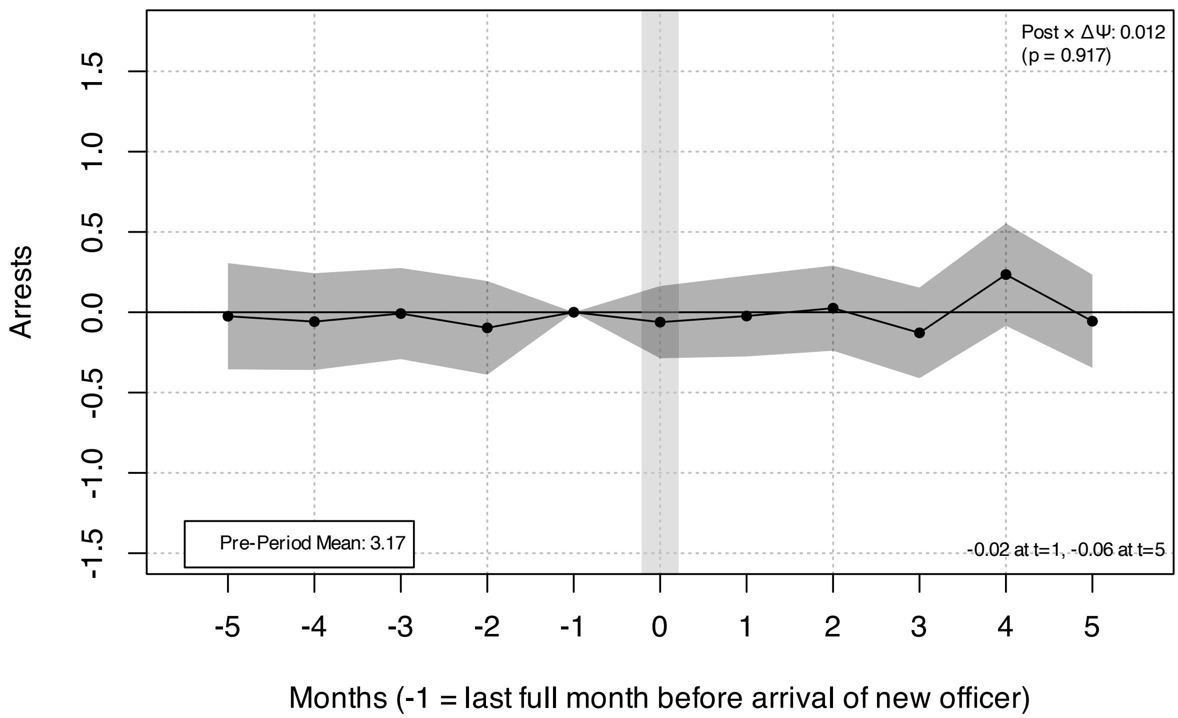


Figure A.9: Robustness to staggered timing and treatment effect heterogeneity

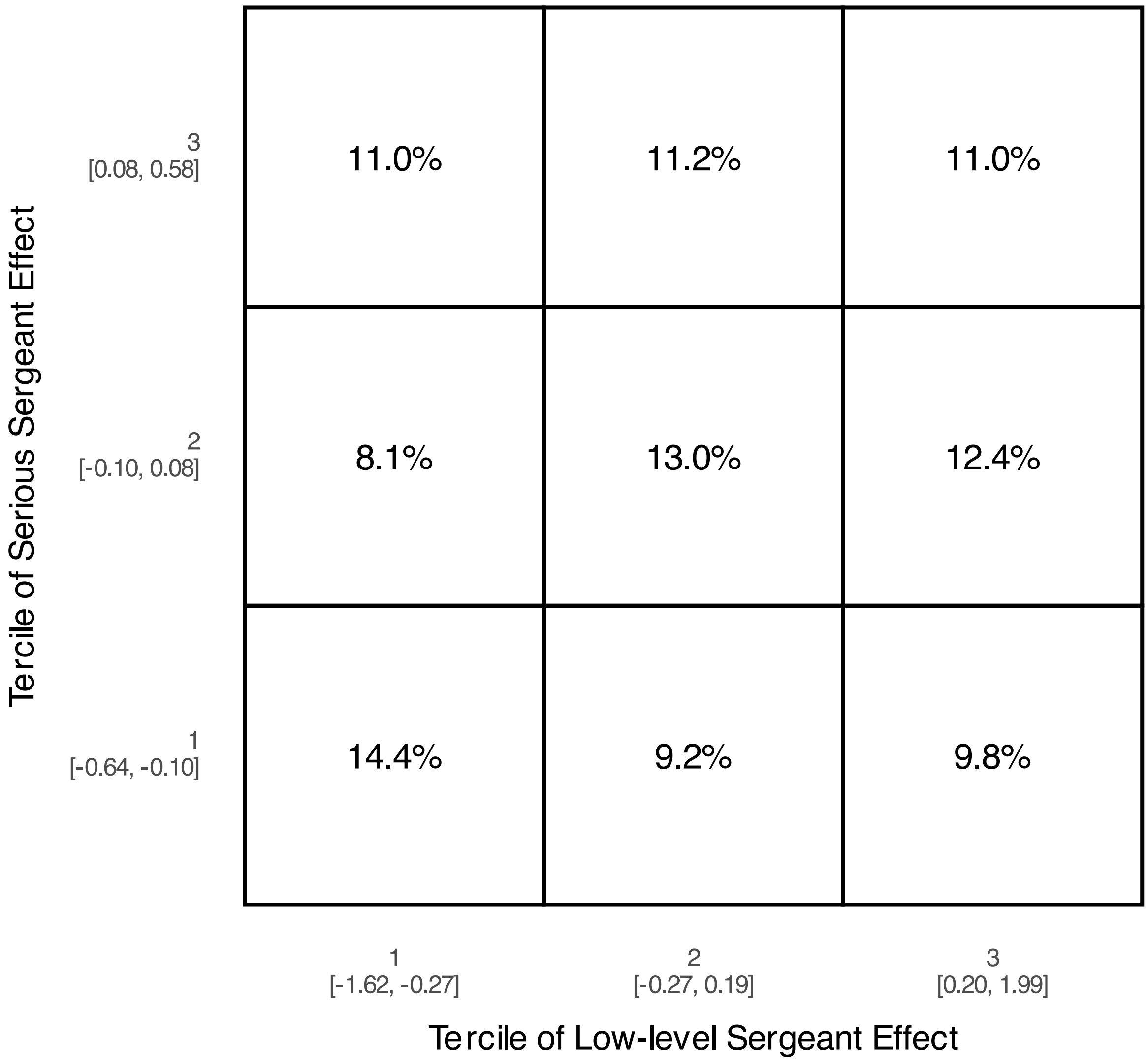
Notes: Each coefficient (with 95% confidence interval) is estimated from a modified version of equation 4, where $\Delta\hat{\psi}_e$ is replaced with an indicator that takes the value 1 if $\Delta\hat{\psi}_e$ is in the top tercile of switches in the sample. The outcome is number of monthly arrests made by the switching officer. The blue lines report event study results using a TWFE specification, as in equation 4. The red lines report event study results using the interaction-weighted estimator developed by Sun and Abraham (2021). Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. Both esetimates use the event study sample described in Table B.1.

Figure A.10: Event Study Arrests Made by Incumbent Officers



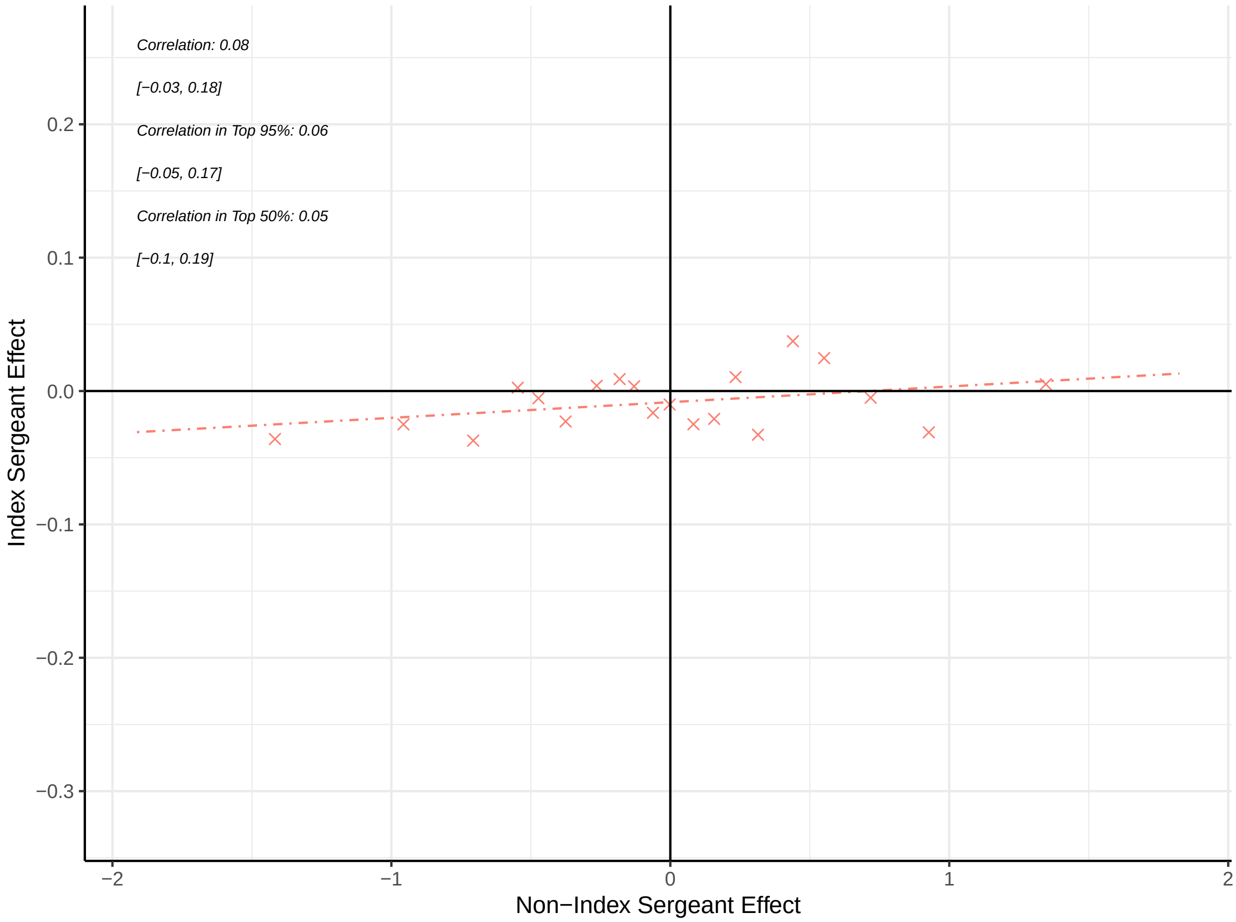
Notes: This figure plots the event study coefficients for incumbent officers around an officer-driven switch, as described in Section 5.2. For an officer-driven switching event e in which officer i switches from sergeant j to sergeant j' by moving to a different unit, incumbent officers are those who work with sergeant j in the same pre- and post-period months that officer i is included in the baseline imbalanced event study sample described in Section 3. The x-axis indicates months relative to officer i's switch. The specification is identical to equation 4, however is estimated at the incumbent officer-by-switching officer-by-event-by-month level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, incumbent officer- by - officer-event, and day-off group fixed effects; Standard errors are clustered at the level of the switching officer.

Figure A.11: Distribution of Sergeants between Serious and Low-level Effects



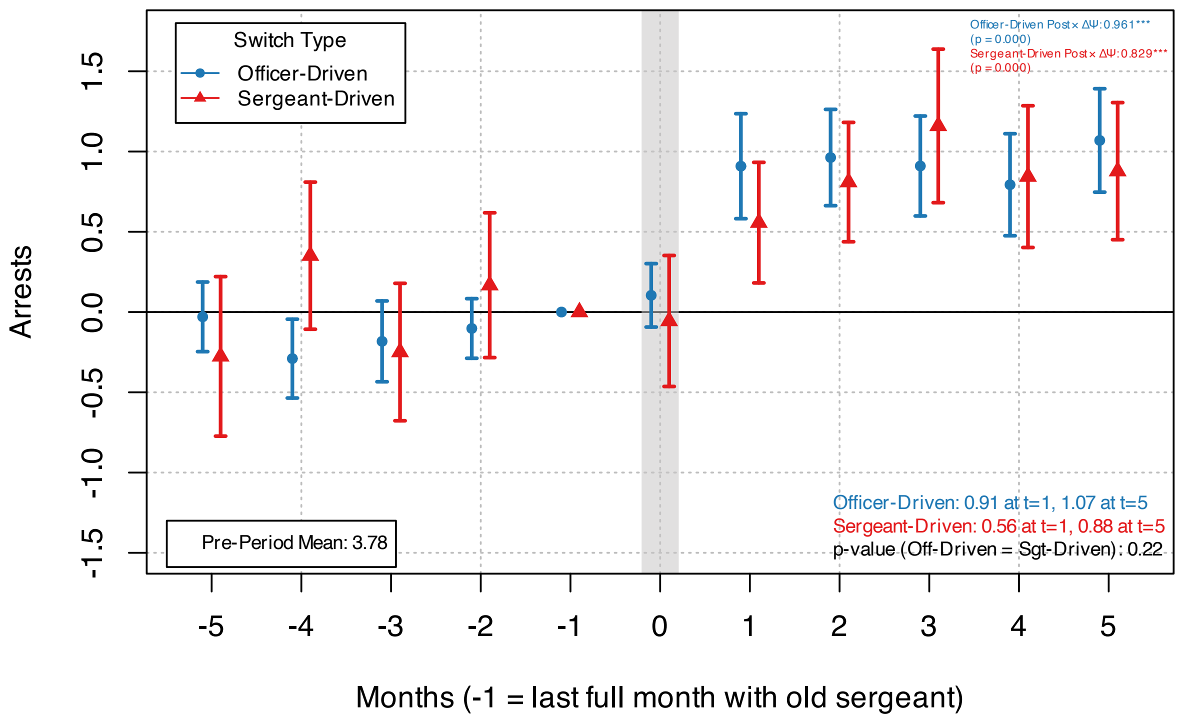
Notes: This figure displays the percentage of sergeants within each tercile grouping of (Bayes-shrunken) low-level and serious sergeant effects. The brackets below each tercile label represent the range of sergeant effect values in the associated tercile. Low-level sergeant effects are estimated using equation 1, with low-level arrests as the outcome. Serious sergeant effects are estimated using equation 1, with serious arrests as the outcome. Both regressions include a second-degree polynomial of officer tenure, as well as officer, sector-watch, and day-off group fixed effects.

Figure A.12: Relationship between non-index and index sergeant effects



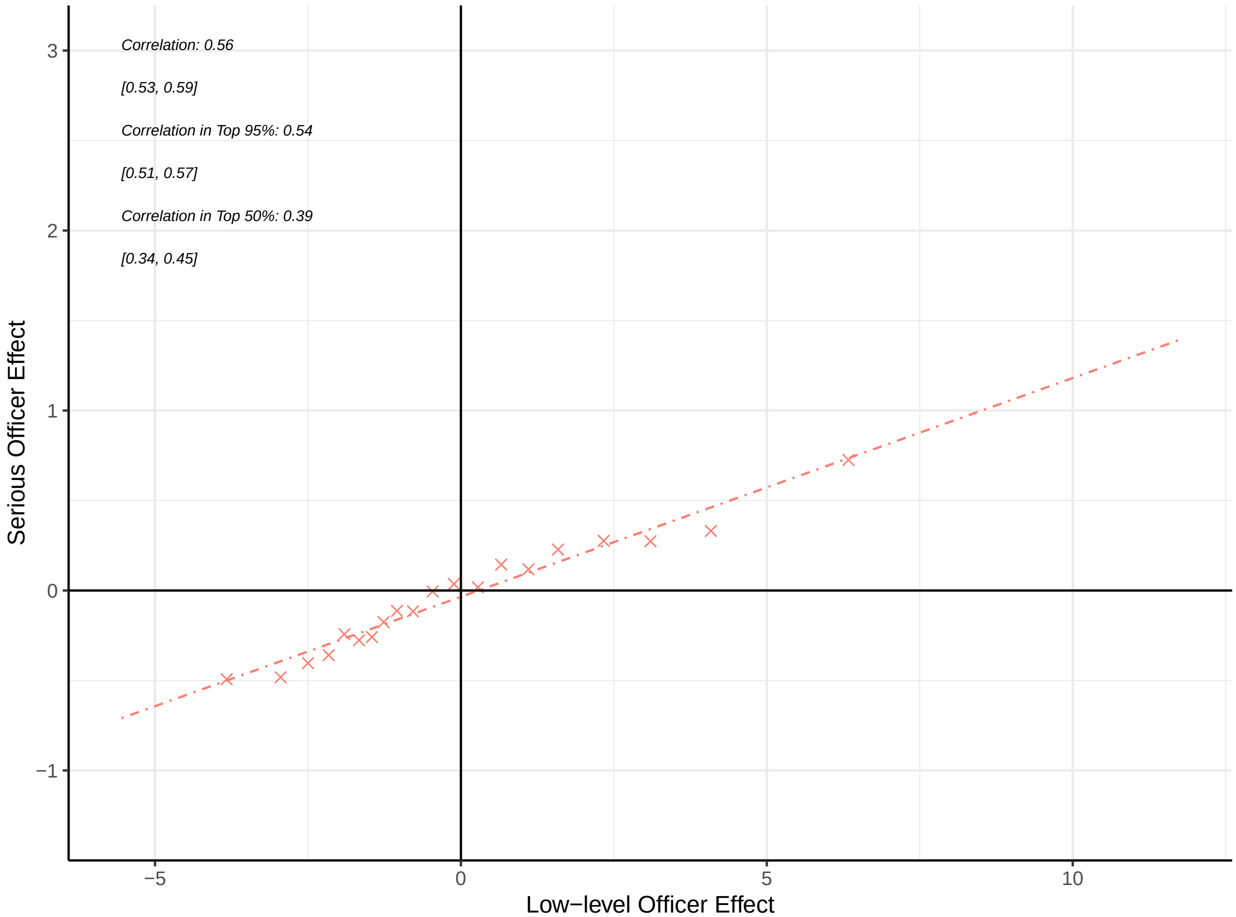
Notes: This figure plots sergeants' index effects against their non-index effects. The effects are estimated using the two-way fixed effects model in equation 1, which includes controls for a second-degree polynomial of officer tenure, as well as officer, sector-watch, and day-off group fixed effects. Index crimes encompass murder, rape, aggravated assault, robbery, burglary, theft, and arson. Both types of sergeant effect are shrunken using the Empirical Bayes procedure described in Section 4. Pearson correlations are provided in black text, with 95% confidence intervals for the correlations, calculated using the Fisher z-transformation, displayed below each correlation. Correlation in the Top 95% is calculated by dropping sergeants below the 5th percentile of non-index effects. Correlation in the Top 50% is calculated by dropping sergeants below the 50th percentile of non-index effects. Each point represents an equal-sized bin of sergeants. The dashed line is a linear fit across all sergeants.

Figure A.13: Event Study by Switch Type



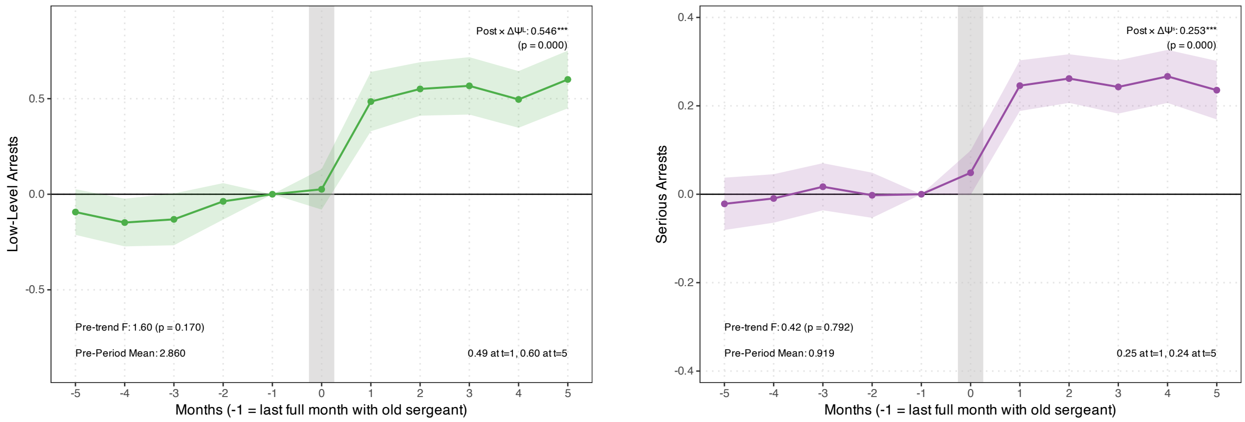
Notes: Each point estimate (with 95% confidence interval) represents $\hat{\pi}_1^k$ from equation 4 — the effect on officer arrests at period k relative to the switch of moving to a sergeant who induces one more arrest per month. Blue points represent estimates from a regression using only officer-driven switches; red points represent estimates from a regression using only sergeant-driven switches. Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. The top right reports the $Post \times \Delta\psi$ coefficient and p-values from the DD specification in equation 5 for each switch type, summarizing the average post-switch effect across periods 1 through 5. The bottom right lists point estimates at selected months, and the p-value from an F-test of equality between point estimates from officer-driven and sergeant-driven regressions interaction coefficients across all post-switch periods jointly, using a Seemingly Unrelated Regression model. The models are estimated using the event study sample described in Table B.1, with standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

Figure A.14: Relationship Between Serious and Low-Level Officer Effects



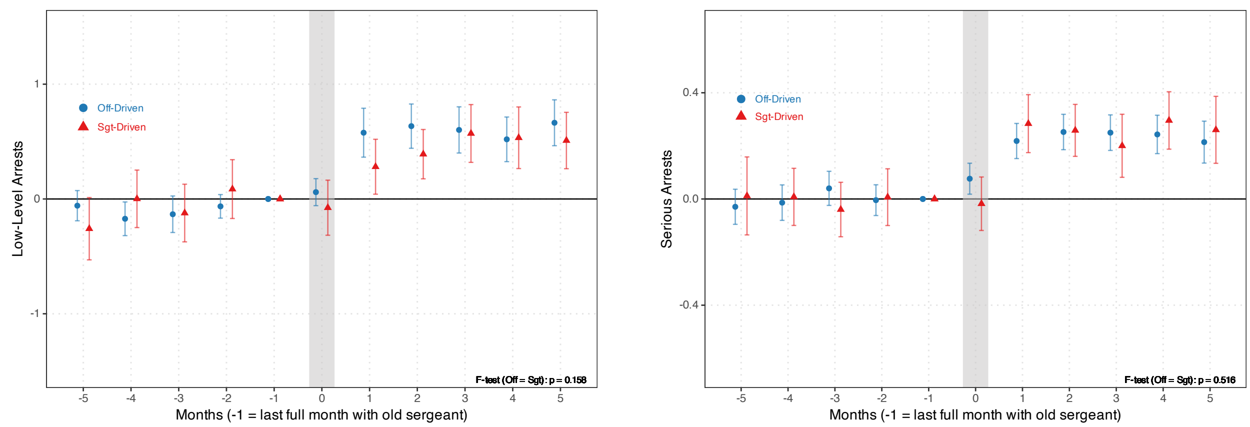
Notes: This figure plots officers' serious effects against their low-level effects. Low-level (serious) effects describe officer effect on arrests for low-level (serious) crimes, defined as described in Section 3. Both types of officer effect are estimated using officer fixed effects in models that include a second-degree polynomial of officer tenure and sergeant, sector-watch, and day-off group fixed effects. Both types of officer effect are shrunken using the Empirical Bayes procedure described in Section 4. Pearson correlations are provided in black text, with 95% confidence intervals for the correlations, calculated using the Fisher z-transformation, displayed below each correlation. Correlation in the Top 95% is calculated by dropping officers below the 5th percentile of low-level effects. Correlation in the Top 50% is calculated by dropping officers below the 50th percentile of low-level effects. Each point represents an equal-sized bin of officers. The dashed line is a linear fit across all officers.

Figure A.15: Event Study For Serious and Low-Level Sergeant Effects



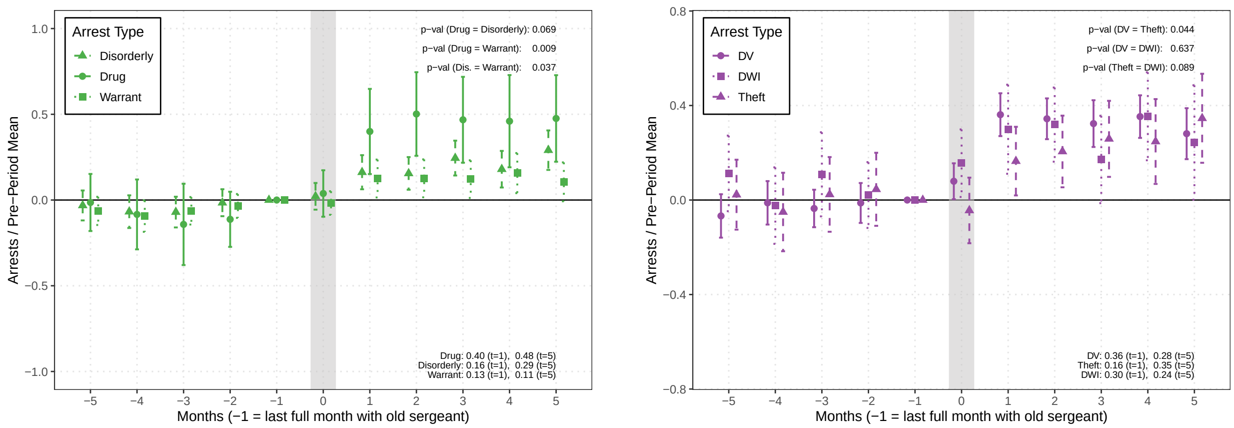
Notes: This figure reports results from two officer-level event studies around sergeant switches for serious and low-level arrests, each estimated using equation 6. Each plot is a separate regression. The left column plots point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level sergeant effects for a regression using low-level arrests as the outcome; the right column plots the analogous interaction for serious sergeant effects in a regression using serious arrests as the outcome. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors are clustered at the officer level. All regressions include controls for officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects, and are estimated on the event study sample described in Table B.1.

Figure A.16: Serious and Low-Level Sergeant Effects by Switch Type



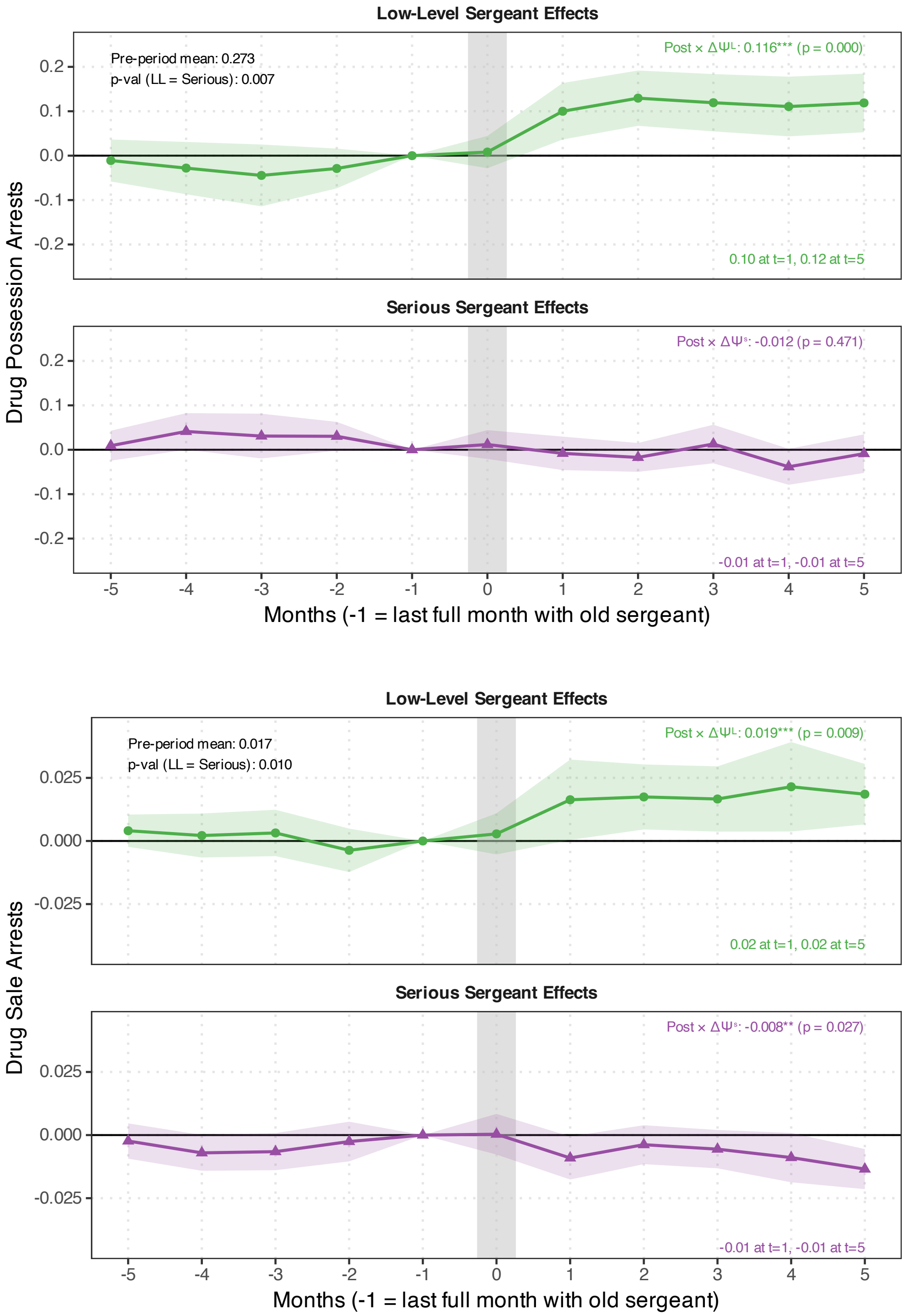
Notes: Each panel reports results from two different regressions. The left panel reports the low-level sergeant effect interaction coefficients from equation 6, using low-level arrests as the outcome; the right panel reports the serious effect interaction coefficients from equation 6, using serious arrests as the outcome. Blue points represent estimates from a regression using only officer-driven switches; red points represent estimates from a regression using only sergeant-driven switches. Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. The bottom right in each panel lists p-values from an F-test of equality between point estimates from officer-driven and sergeant-driven regressions across all post-switch periods jointly, using a Seemingly Unrelated Regression model. The models are estimated using the event study sample described in Table B.1, with standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

Figure A.17: Sergeant Effects by Arrest Sub-Type: Mean-Normalized Coefficients



Notes: The left figure plots low-level sergeant effect interaction coefficients for models given by equation 6, using the three most frequent low-level arrests as outcomes. The right figure plots serious sergeant effect interaction coefficients for models given by equation 6, using the three most frequent serious arrests as outcomes. Coefficients are divided by their respective outcome means. The bottom right in each panel reports selected point estimates. The top right in each panel The reports the p-value from an F-test of equality between the mean-normalized coefficients across all post-switch periods jointly, using a Seemingly Unrelated Regression model. For all regressions: standard errors clustered at the officer level; Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; and estimation is performed using the main event study sample described in Table B.1.

Figure A.18: Event Studies: Disaggregated Drug Categories



Notes: This figure reports results from officer-level event studies around sergeant switches for drug arrest sub-types, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; estimated on the event study sample described in Table B.1.

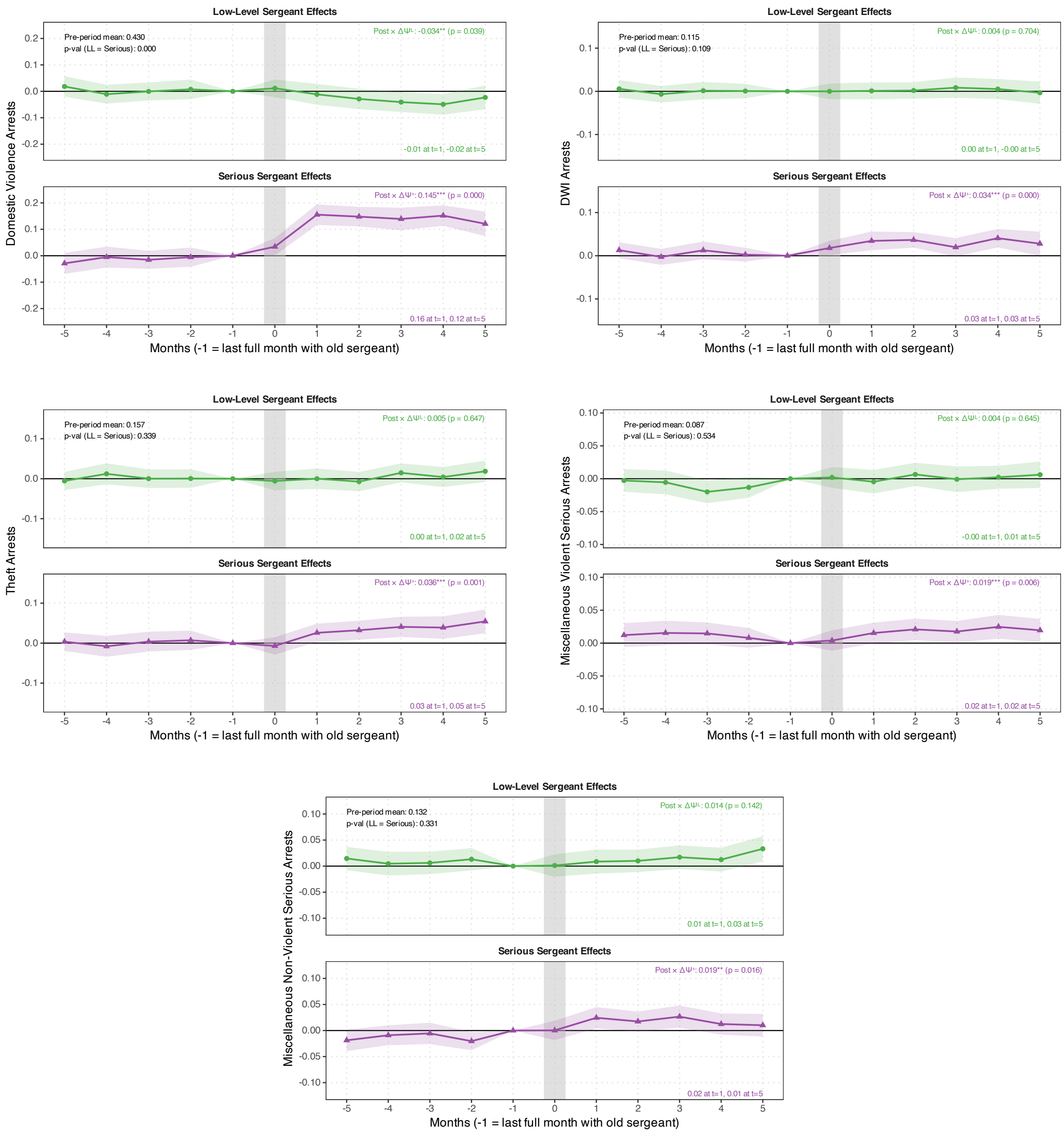


Figure A.19: Sergeant Effects on Serious Arrest Subtypes

Notes: This figure reports results from officer-level event studies around sergeant switches for serious arrest sub-types, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; estimated on the event study sample described in Table B.1.

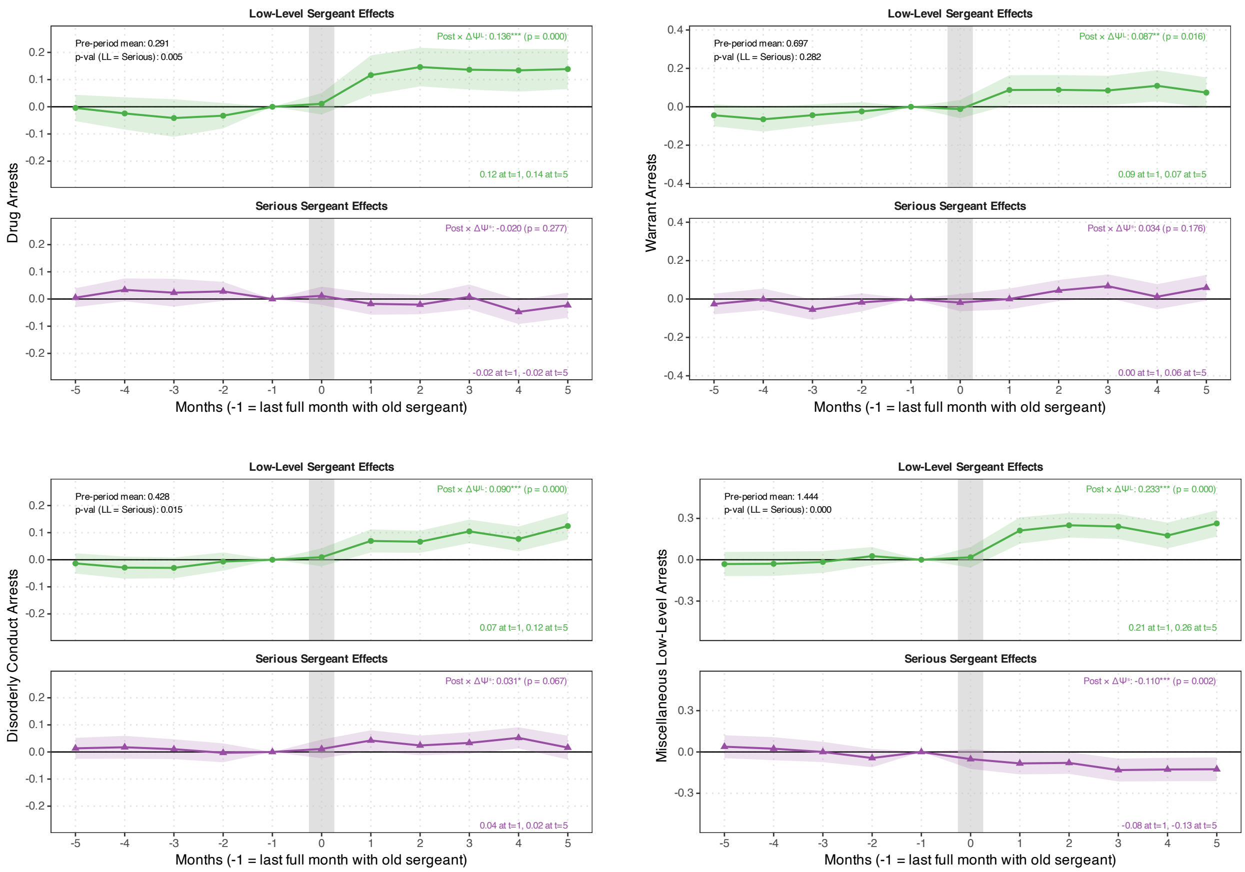


Figure A.20: Sergeant Effects on Low-Level Arrest Subtypes

Notes: This figure reports results from officer-level event studies around sergeant switches for low-level arrest sub-types, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; estimated on the event study sample described in Table B.1.

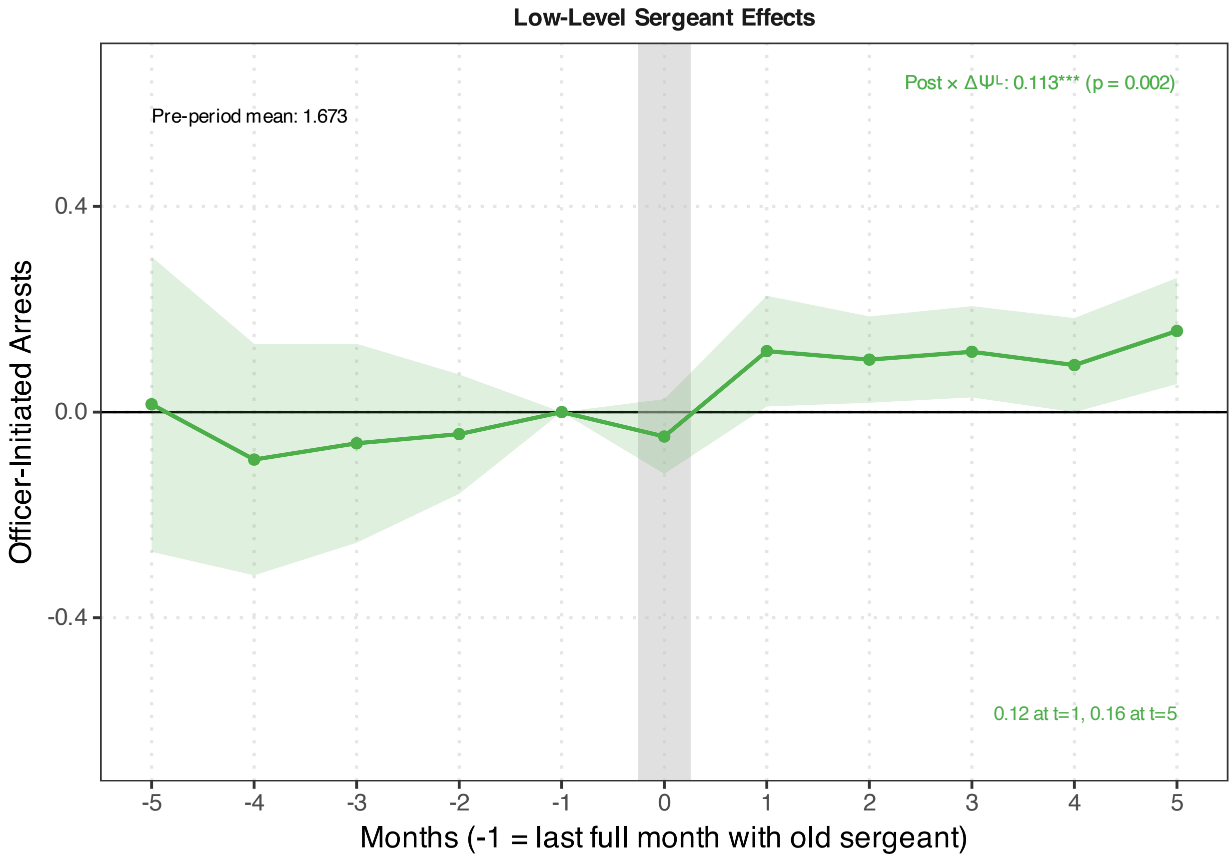
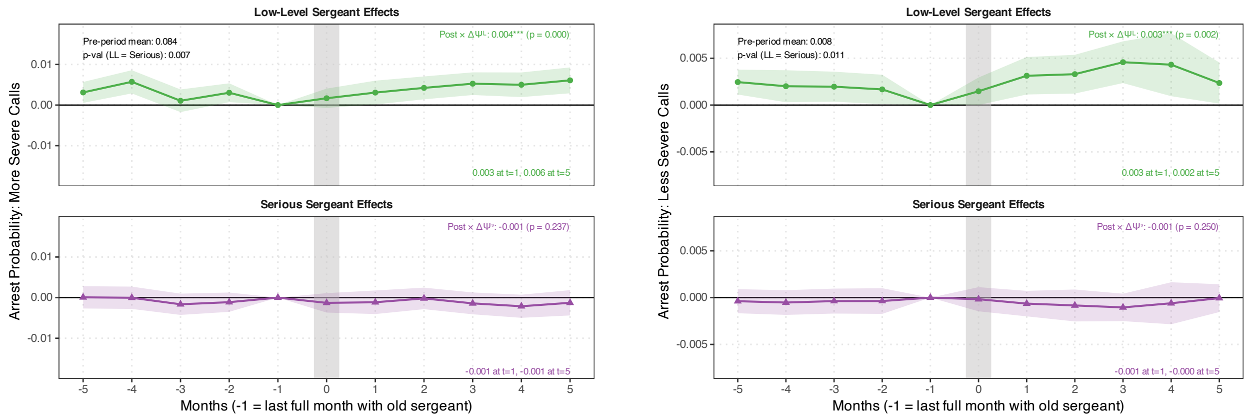


Figure A.21: Officer-Initiated Arrests Event Study with Linear Time Trend

Notes: This figure replicates the officer-initiated arrests panel from Figure 6 using a modified specification that includes an event-specific linear time trend. The plotted coefficients represent the interaction between period indicators and the move-induced change in low-level sergeant effects after absorbing this event-specific trend. The top right reports the associated DD estimate; the bottom left reports the pre-period outcome mean. Standard errors are clustered at the officer level. The regression include a second-degree polynomial and sector-watch and day-off group fixed effects. The regression is estimated on the event study sample described in Table B.1.

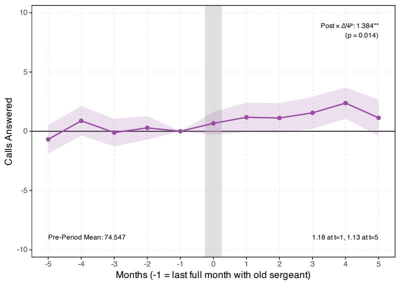
Figure A.22: Sergeant Effects on Call Arrest Probability by Call Severity



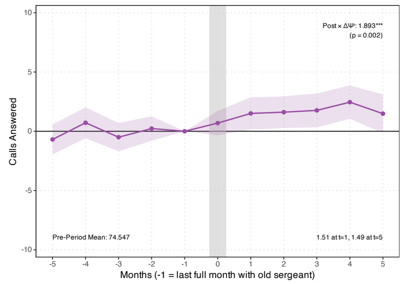
Notes: This figure reports results from officer-level event studies around sergeant switches for arrest probabilities at 911 calls, estimated using equation 6. Arrest probability is the share of calls answered within a month that result in arrest. More severe calls are those whose dispatch code has an above-median arrest rate across all dispatch codes in the CAD system. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; the regression is estimated on the event study sample described in Table B.1.

Figure A.23: Serious Sergeant Effects on Officer Calls Answered: Robustness to Crime Condition Controls

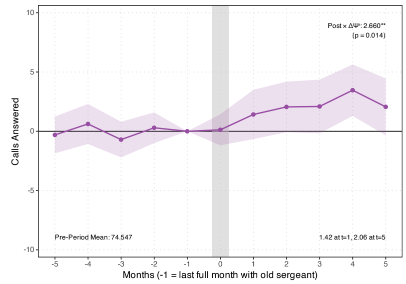
(a) Year-Month FE



(b) Div.-Watch-Year-Month FE

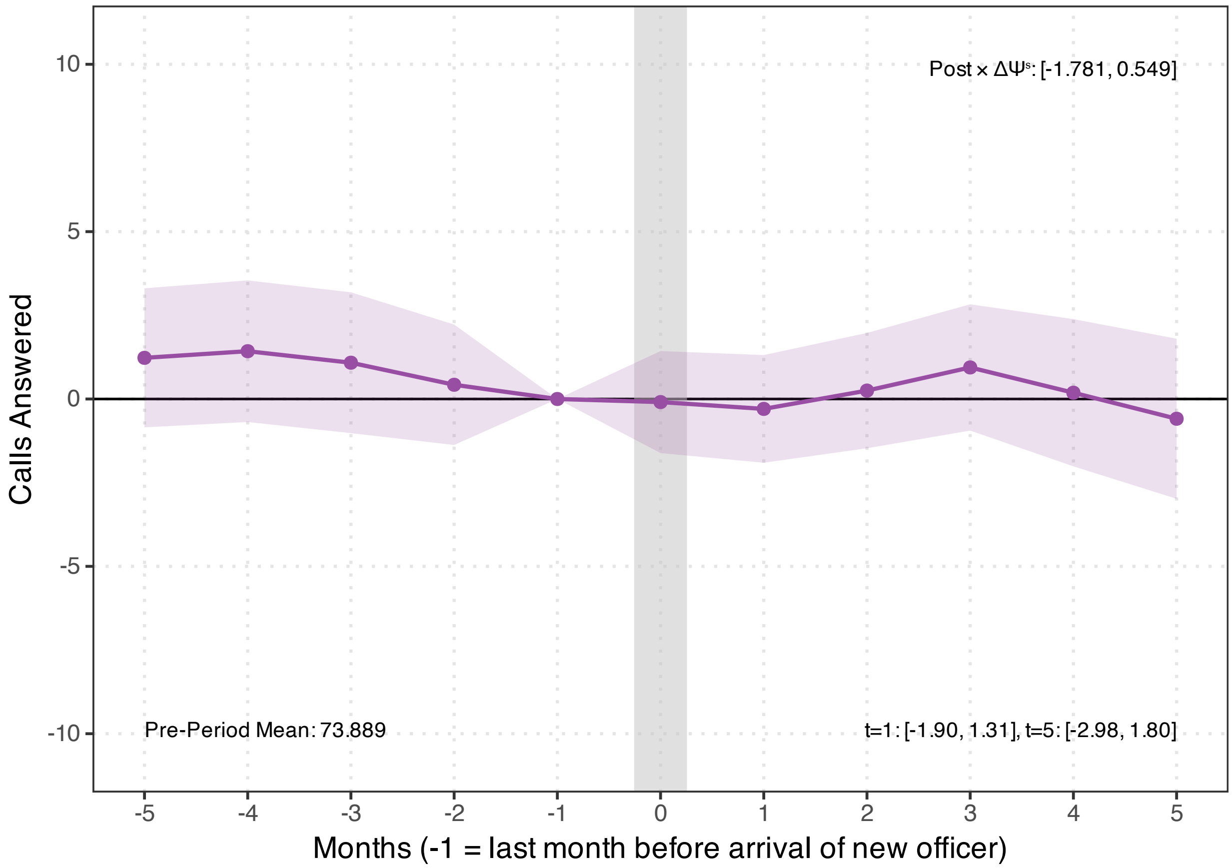


(c) Sec.-Watch-Year-Month FE



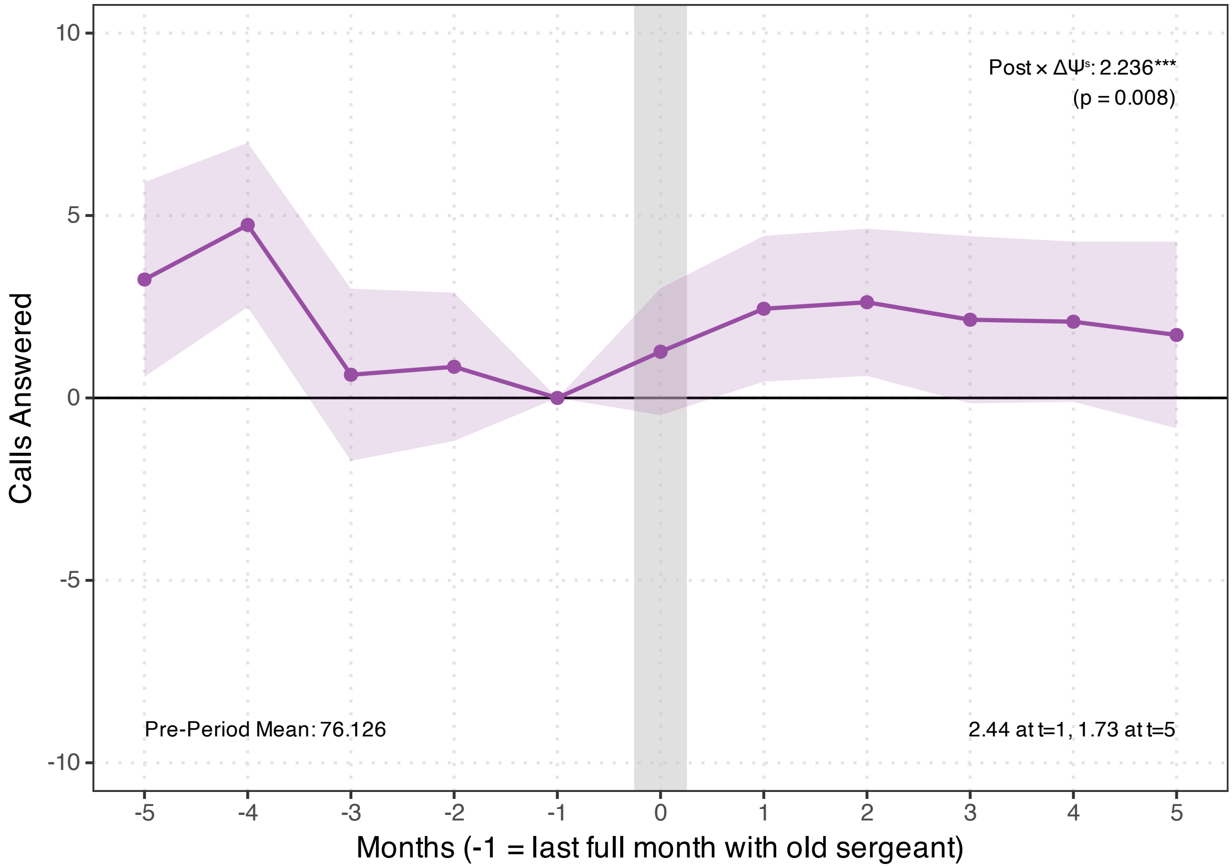
Notes: Each figure reports results from a separate officer-level event study around sergeant switches, estimated using equation 6. Each figure plots point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in serious sergeant effects in regressions with an officer's total 911 calls answered within a month as the outcome. In each figure, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom left reports the pre-period outcome mean; and the bottom right reports coefficients at selected months. For each figure, standard errors are clustered at the officer level and controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and officer-event and day-off group fixed effects; each regression is estimated on the event study sample described in Table B.1. Each figure includes different time, assignment, and assignment-by-time fixed effects, which absorb average average officer call volume at the corresponding level and thereby isolate variation in individual officer behavior from changes in crime conditions. Subfigure (a) includes sector-watch and year-by-month fixed effects. Subfigure (b) includes sector-watch and division-by-watch-by-year-by-month fixed effects. Subfigure (c) includes sector-by-watch-by-year-by-month fixed effects, the most granular specification.

Figure A.24: Serious Sergeant Effects on Incumbent Officer Call Volume: Ruling Out Crime Condition Changes



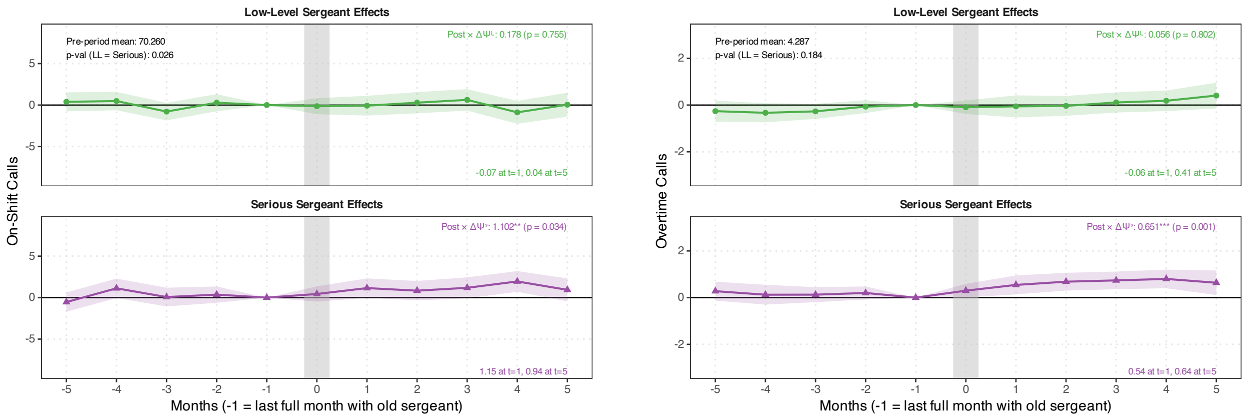
Notes: This figure plots point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in serious sergeant effects in regressions that use total 911 calls answered within a month by incumbent officers as the outcome. For an officer who changes sergeants by switching units, an incumbent officer is an officer who is in the switching officer's new unit during the switching office's full event study sample period, as described in Section 5.2. The x-axis indicates months relative to the switching officer's move. The specification is identical to equation 6, however is estimated at the incumbent officer-by-switching officer-by-event-by-month level. The serious sergeant effect interaction term scales period indicators by the change in the *switching officer's* serious sergeant effects, so the coefficients capture how incumbent officer call volume co-moves with the switching officer's change in sergeant. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, incumbent officer- by - officer-event, and day-off group fixed effects. Standard errors are clustered at the level of the switching officer. The top right reports the 95% confidence interval of the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom left reports the pre-period outcome mean; and the bottom right reports 95% confidence intervals of coefficients at selected months.

Figure A.25: Serious Sergeant Effects on Call Volume: Sergeant-Driven Switches



Notes: This figure plots serious sergeant effect interaction coefficients from equation 6, using monthly 911 call volume as the outcome. The bottom right reports selected point estimates. The top right reports the p-value from an F-test of equality between the coefficients across all post-switch periods jointly, using a Seemingly Unrelated Regression model. For all regressions: standard errors clustered at the officer level; controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; estimation is performed using only sergeant-driven switches from the event study sample.

Figure A.26: Sergeant Effects on Calls: Overtime and On-Shift



Notes: This figure reports results from officer-level event studies around sergeant switches, estimated using equation 6. Overtime calls are calls answered outside the officer's regular shift on days in which they have recorded overtime payments. All other calls are considered on-shift. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; the regression is estimated on the event study sample described in Table B.1.

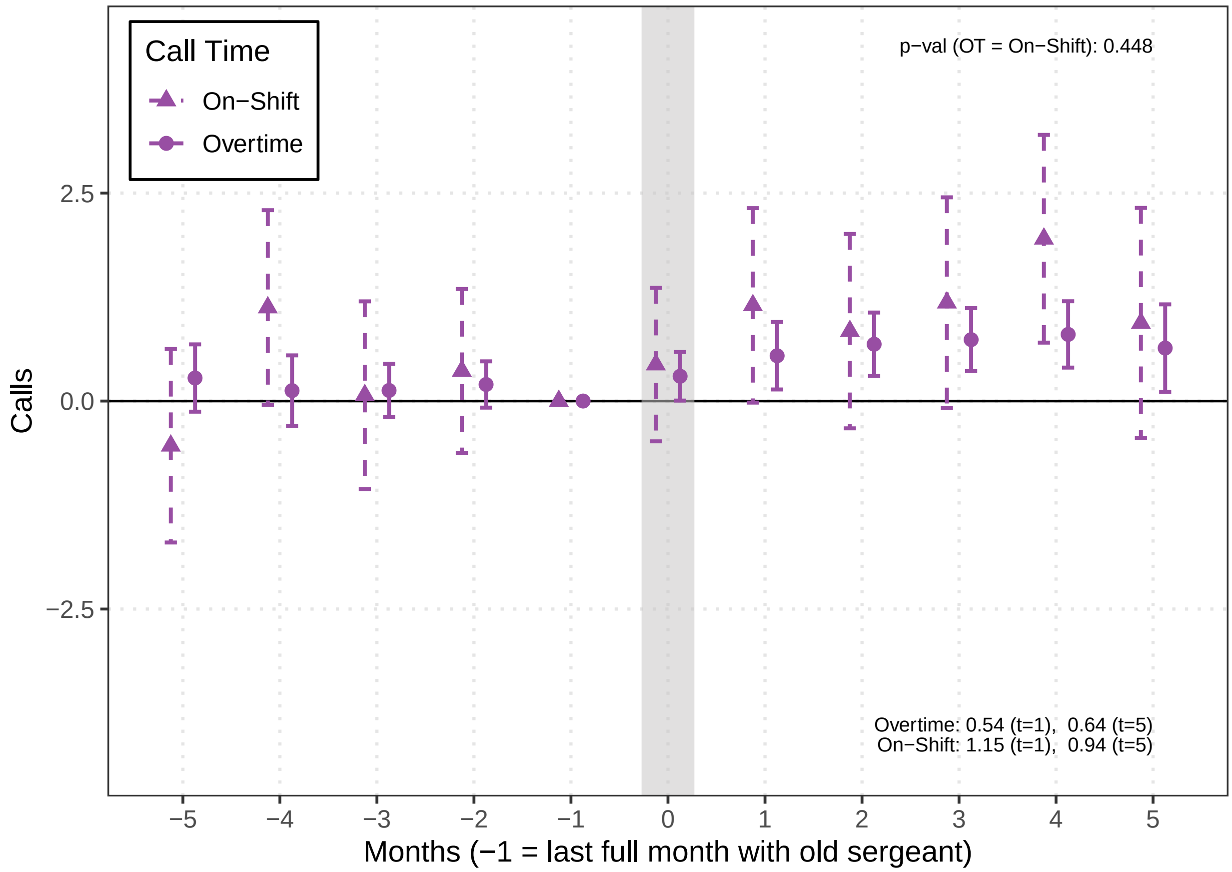


Figure A.27: Comparing Overtime and On-Shift Call Estimates

Notes: This figure plots serious sergeant effect interaction coefficients from equation 6, using overtime calls and on-shift calls as outcomes. Overtime calls are calls answered outside the officer's regular shift on days in which they have recorded overtime payments. All other calls are considered on-shift. The bottom right reports selected point estimates. The top right reports the p-value from an F-test of equality between the coefficients across all post-switch periods jointly, using a Seemingly Unrelated Regression model. For all regressions: standard errors clustered at the officer level; controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; estimation is performed using the main event study sample described in Table B.1.

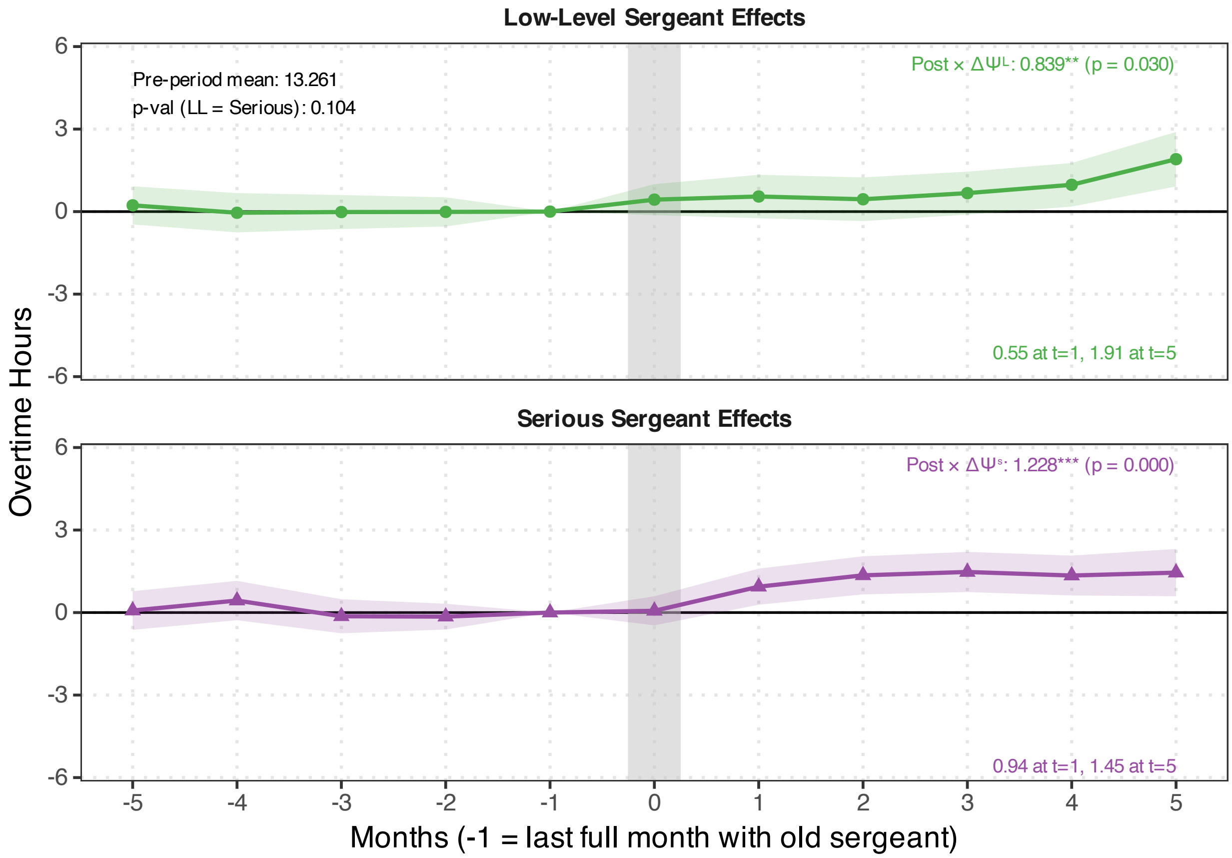


Figure A.28: Serious Sergeant Effects on Overtime Hours

Notes: This figure reports results from officer-level event studies around sergeant switches, estimated using equation 6. The outcome is the number of overtime hours logged in a month. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; the regression is estimated on the event study sample described in Table B.1.

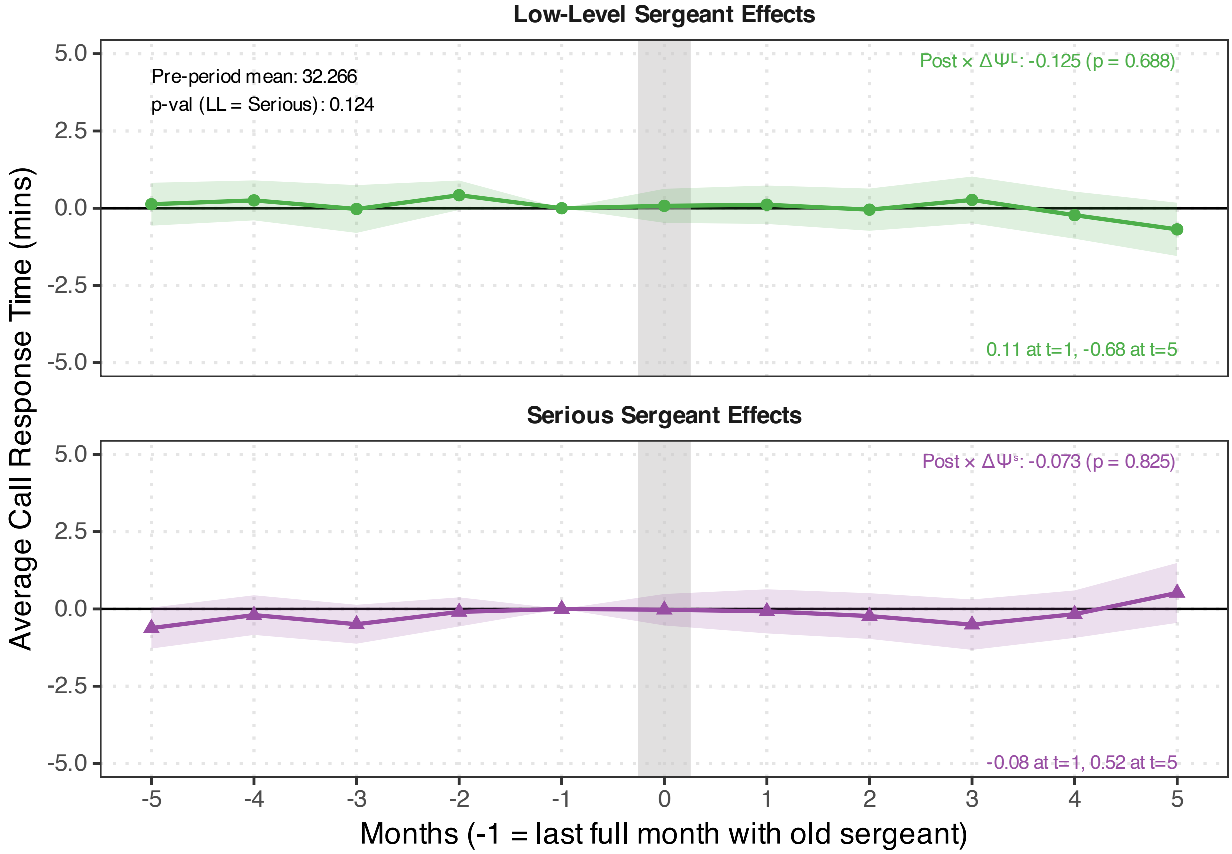


Figure A.29: Sergeant Effects on Response Times

Notes: This figure reports results from officer-level event studies around sergeant switches for call response times, estimated using equation 6. The outcome is the average response time among calls the officers answered in a month. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, day-off group, and year-month fixed effects; the regression is estimated on the event study sample described in Table 1.

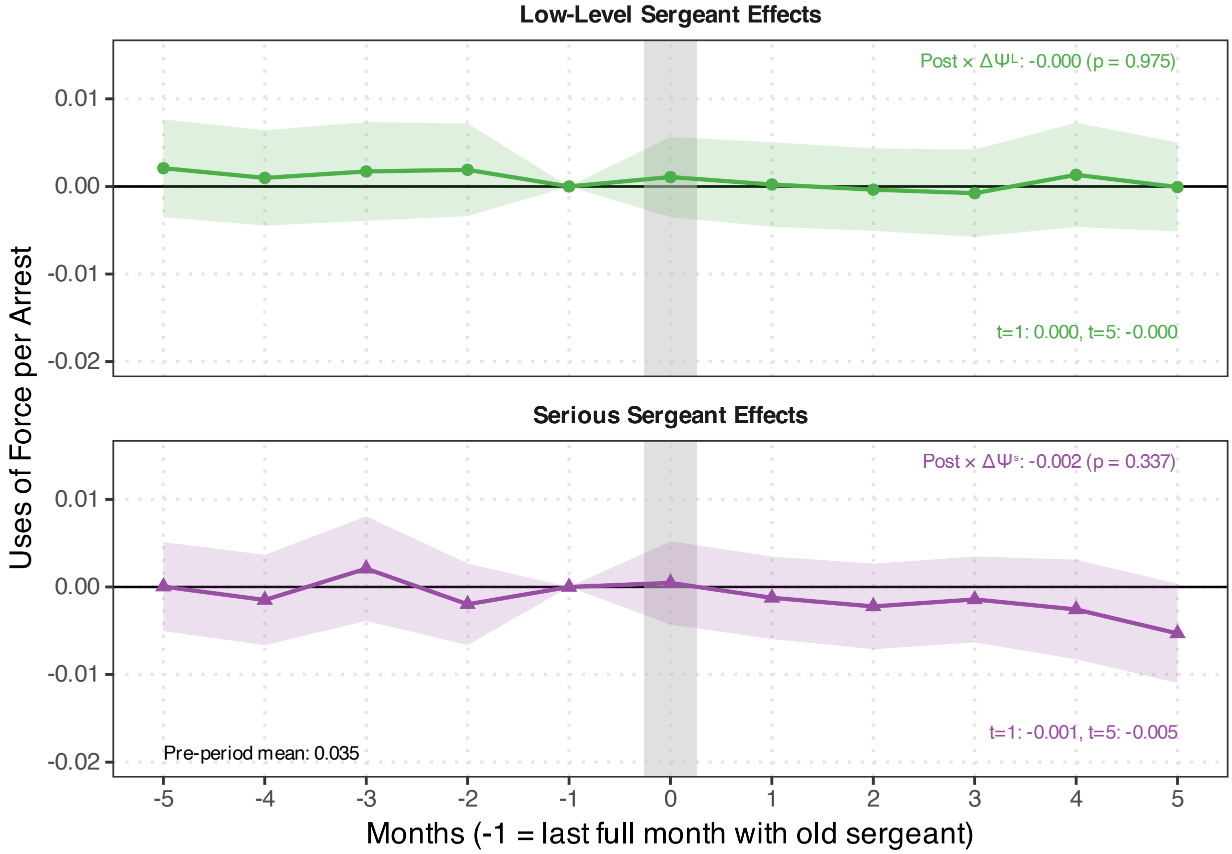
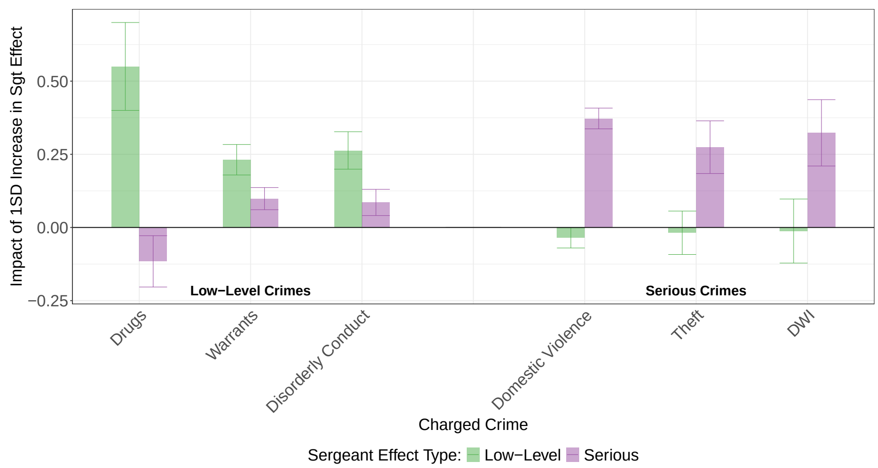


Figure A.30: Sergeant Effects on Use of Force Per Arrest

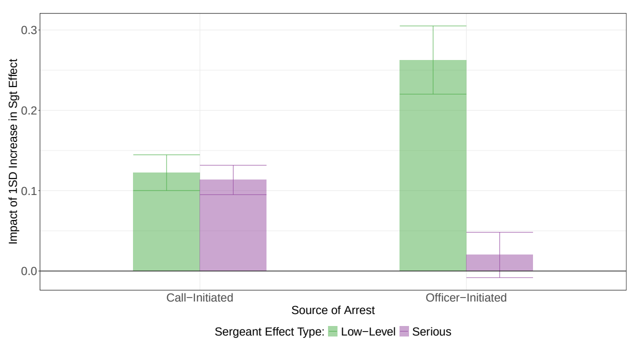
Notes: I estimate use of force per arrest using a ratio estimator based on two separate regressions following equation 6: one with monthly use of force incidents as the outcome and one with monthly arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting use of force value by the resulting arrest value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for use of force and arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the use of force and arrest equation residuals, clustering at the officer level. The top right reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, day-off group, and year-month fixed effects.

Figure A.31: TWFE Estimates of Sergeant Effect Mechanisms

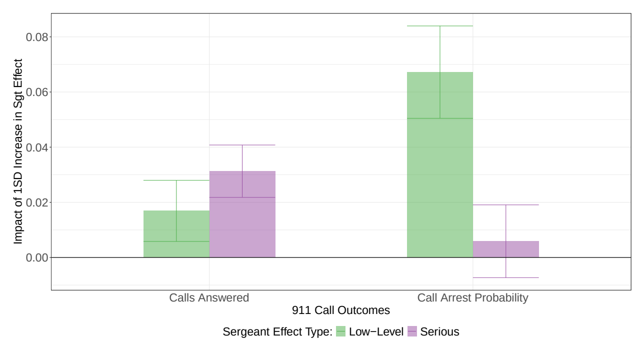
(a) Arrests by Crime Type



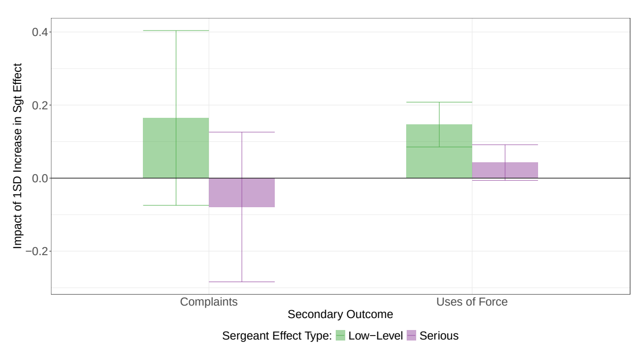
(b) Arrests by Interaction Source



(c) 911 Call Outcomes



(d) Secondary Police Outcomes



Notes: This figure depicts estimates of coefficients α_L^c and α_S^c from the following equation, estimated using the analysis sample: $y_{it}^c = \alpha_L^c \hat{\psi}_{J(i,t)}^L + \alpha_S^c \hat{\psi}_{J(i,t)}^S + \theta_i^c + x_{it}'\beta^c + \nu_{it}^c$. The superscript c represents a police outcome. Estimates are divided by the appropriate outcome mean. The estimated serious and low-level sergeant effects, $\hat{\psi}^{L]}$ and $\hat{\psi}^S$ are divided by their standard deviations, so that the plotted coefficients are interpreted as the proportion change in the outcome relative to the mean that results from increasing the low-level (serious) sergeant effect by 1 standard deviation. Green lines represent α_L^c estimates and purple lines represent α_S^c estimates. Police outcomes are given on the x-axis of each figure. 95% confidence intervals are depicted for each of the estimates, calculated using standard errors clustered at the officer level. All regressions include officer fixed effects, sector-watch fixed effects, day-off group fixed effects, and a second-degree polynomial of officer tenure.

B Additional Tables

Table B.1: Summary Statistics

	Full Sample (1)	Analysis Sample (2)	Event Study Sample (3)
1. Number of officers	2,067	1,805	1,459
2. Number of sergeants	387	347	341
3. Number of officers with >1 sup.	1,856	1,623	1,459
4. Number of sergeants with >1 off.	384	344	330
5. Mean number of sergeants per off.	5.21	3.97	3.18
6. Mean number of officers per sgt.	27.7	20.6	13.6
7. Total officer-sergeant spells	15,355	8,432	4,727
8. Total switching events	13,288	5,798	2,877
Officer-Driven Switches	6,238	3,635	1,970
Sergeant-Driven Switches	7,050	2,163	907
9. Number of sector-watches	105	102	102
10. Mean number of sergeants per sector-watch	8.48	6.95	6.56
11. Number of units	193	183	183
12. Mean number of sergeants per unit	4.89	4.05	3.82
13. Arrests mean	3.81	3.80	3.78
SD	3.65	3.64	3.62
14. Low-level arrests mean	2.88	2.87	2.85
SD	3.03	3.02	3.00
15. Serious arrests mean	0.925	0.923	0.927
SD	1.29	1.29	1.29
Number of observations	61,166	49,923	26,357

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 consists of the 5 months before and after the switching events in the Analysis Sample that have no other switching events occurring in the 5 months pre- or post-switch, as described in Section 3. I allow the sample to be imbalanced in order to accommodate moves near the edges of the officer's sample period and that contain any time spent in a temporary or non-patrol assignment in pre- or post-switch periods. Officer-driven switches are those where an officer changes units; sergeant-driven switches are those where an officer stays in the same unit and receives a new sergeant. Serious arrests are defined as arrests for index crimes as well as domestic violence, fraud, simple assault, and DUI. All other arrests are considered low-level.

Table B.2: Comparison of Variance Estimates: Fixed Effects vs Random Effects

Statistic	Fixed Effects	Random Effects
Panel A: Officers		
Variance	8.067	5.250
Standard Deviation	2.840	2.291
90 pctl - 10 pctl	7.193	4.937
Panel B: Sergeants		
Variance	0.379	0.224
Standard Deviation	0.616	0.473
90 pctl - 10 pctl	1.618	0.938

Notes: The table reports summary statistics for estimated officer effects (Panel A) and estimated sergeant effects (Panel B) under different model specifications. "90 pctl - 10 pctl" is the difference between the 90th and 10th percentile of the relevant effects distribution. The middle column reports statistics from fixed effects specifications of equation 1; the rightmost column reports the same statistics from random effects specifications that are estimated using REML. For random effects specifications, officer and sergeant effects are modeled as random effects, and the models include controls for a second-degree polynomial of officer tenure as well sector-watch and day-off group effects, which are modeled as fixed effects. For fixed effects specifications, the variance and standard deviations are estimated using the Kline et al. (2020) heteroskedasticity-robust method; the 90th/10th percentile difference is calculated using the distribution of Bayes-shrunken fixed effects. For random effects, 90th/10th percentile differences are calculated on the distribution of Best Linear Unbiased Predictors of the sergeant/officer effects.

Table B.3: Serious & Low-Level Random Effects Estimates

Statistic	Estimate
Panel A: Officers	
$Var(\theta^L)$	3.930
$Var(\theta^S)$	0.296
$\mathrm{Cor}(\theta^L, \theta^S)$	0.603
Panel B: Sergeants	
$Var(\psi^L)$	0.287
$Var(\psi^S)$	0.024
$\mathrm{Cor}(\psi^L, \psi^S)$	-0.017

Notes: The table reports estimated parameters from a random effects model that specifies officers' and sergeants' effects as joint normal random variables with parameterized correlations between officers' serious and low-level effects, as well as between sergeants' serious and low-level effects. The model follows that of Beuermann et al. (2023), with 0 correlation assumed between officer and sergeant effects, and 0 correlation assumed between the errors for different outcomes and the officer/sergeant effects. The model is estimated using REML. The model includes controls for a second-degree polynomial of officer tenure as well sector-watch and day-off group effects, which are modeled as fixed effects.

Table B.4: Variance Decomposition: Low-Level Arrests

	Raw		Shrinkage		Homosk. Bias-Correction		Heterosk. Bias-Correction	
	Component	% Share	Component	% Share	Component	% Share	Component	% Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Var(y^*)$	8.383	100.00%	8.383	100.00%	8.383	100.00%	8.360	100.00%
$Var(\psi^L)$	0.445	5.31%	0.310	3.70%	0.301	3.59%	0.299	3.58%
$Var(\theta^L)$	6.566	78.32%	5.868	70.00%	5.999	71.56%	5.958	71.27%
$Cov(\psi^L, \theta^L)$	-0.0975	-1.16%	-0.0952	-1.14%	-0.011	-0.13%	-0.00723	-0.087%
$Var(\psi^L + \theta^L)$	6.816	81.31%	5.988	71.43%	6.278	74.89%	6.24	74.67%
N supervisors	347		347		347		344	
N officers	1805		1805		1805		1622	

Notes: This table presents the variance decompositions described in equation 2, using low-level arrests as the outcome. y^* is the number of monthly low-level arrests, residualized on sector-watch, day-off group, and a second-degree polynomial of tenure; ψ is the sergeant 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 sergeants and officers decrease. This implementation adapts the Julia package provided by Kline et al. (2020) and developed by Paul Courcera, which can be found at https://github.com/HighDimensionalEconLab/VarianceComponentsHDFE.jl. Sergeant and officer fixed effects are estimated using equation 1, which includes controls for a second-degree polynomial of officer tenure, and sector-watch and day-off group fixed effects.

Table B.5: Variance Decomposition: Serious Arrests

	Raw		Shrinkage		Homosk. Bias-Correction		Heterosk. Bias-Correction	
	Component	% Share	Component	% Share	Component	% Share	Component	% Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Var(y^*)$	0.813	100.00%	0.813	100.00%	0.813	100.00%	0.817	100.00%
$Var(\psi^S)$	0.0731	8.99%	0.0385	4.74%	0.0488	6.00%	0.0483	5.91%
$Var(\theta^S)$	0.455	55.90%	0.306	37.61%	0.357	43.89%	0.361	44.12%
$Cov(\psi^S, \theta^S)$	-0.00217	-0.27%	-0.00319	-0.39%	0.0052	0.64%	0.00495	0.61%
$Var(\psi^S + \theta^S)$	0.523	64.36%	0.338	41.56%	0.416	51.17%	0.42	51.24%
N supervisors	347		347		347		344	
N officers	1805		1805		1805		1622	

Notes: This table presents the variance decompositions described in equation 2, using serious arrests as the outcome variable. y^* is the number of monthly serious arrests, residualized on sector-watch, day-off group, and a second-degree polynomial of tenure; ψ is the sergeant 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 sergeants and officers decrease. This implementation adapts the Julia package provided by Kline et al. (2020) and developed by Paul Courcera, which can be found at https://github.com/HighDimensionalEconLab/VarianceComponentsHDFE.jl. Sergeant and officer fixed effects are estimated using equation 1, which includes controls for a second-degree polynomial of officer tenure, and sector-watch and day-off group fixed effects.

Table B.6: Trends in crime do not predict changes in sergeant effects

	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)
$Log(911Calls)_{-1}$	-0.0162	0.0014	0.0713	0.1357
	(0.0467)	(0.0483)	(0.1654)	(0.1482)
$Log(911Calls)_{-2}$	-0.0043	0.0173	-0.0571	0.0140
	(0.0459)	(0.0570)	(0.1871)	(0.1764)
$Log(911Calls)_{-3}$	0.0063	-0.0594	-0.0722	-0.3354*
	(0.0552)	(0.0479)	(0.1709)	(0.1770)
$Log(911Calls)_{-4}$	-0.0398	0.0148	-0.0915	0.0855
	(0.0515)	(0.0543)	(0.1877)	(0.1545)
$Log(911Calls)_{-5}$	0.0372	-0.0460	0.2426	-0.0737
	(0.0391)	(0.0473)	(0.1723)	(0.1704)
Observations	5,525	5,525	1,387	1,424
Mean $\|Y\|$	0.13460	0.13891	0.53615	0.53896
Joint F p-value	0.91938	0.62787	0.79073	0.38557
Sector-Watch fixed effects	✓	✓	✓	✓

Notes: This table examines the correlation of crime trends with sergeant switches. Regressions are performed at the unit by month level. For each unit and month, I calculate the average change in sergeant effects separately for officers who move in and out of the unit in that month. The table reports results for a regression of the sergeant effect changes on the natural logarithm of 911 calls originating in the unit's sector-watch in each of the 5 months prior to the focal month. Regressions include fixed effects for the sector-watch. Dependent variables are the average change in the sergeant effects for out-movers (columns 1 and 3) and in-movers (columns 2 and 4). Out-movers are officers who leave the unit and in-movers are officers who join the unit in a given month. Columns 1 and 2 use all monthly observations for each unit. Columns 3 and 4 only use the monthly observations in which at least one out-move (column 3) or one in-move (column 4) occurs. Standard errors are clustered at the sector-watch level.

Table B.7: Trends in violent 911 calls do not predict changes in sergeant effects

	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)
$Log(Violent911Calls)_{-1}$	0.0147	0.0133	0.0075	0.0573
	(0.0296)	(0.0318)	(0.1082)	(0.0981)
$Log(Violent911Calls)_{-2}$	-0.0230	0.0088	-0.0133	0.0688
	(0.0346)	(0.0321)	(0.1055)	(0.1170)
$Log(Violent911Calls)_{-3}$	-0.0221	-0.0687**	-0.0902	-0.2999***
	(0.0378)	(0.0288)	(0.1202)	(0.1128)
$Log(Violent911Calls)_{-4}$	0.0128	0.0042	-0.0552	0.0064
	(0.0331)	(0.0335)	(0.1212)	(0.1097)
$Log(Violent911Calls)_{-5}$	0.0111	-0.0100	0.0746	-0.0015
	(0.0279)	(0.0310)	(0.1110)	(0.1113)
Observations	5,525	5,525	1,387	1,424
Mean $\|Y\|$	0.13460	0.13891	0.53615	0.53896
Joint F p-value	0.90432	0.26836	0.93117	0.17286
Sector-Watch fixed effects	✓	✓	✓	✓

Notes: This table examines the correlation of crime trends with sergeant switches. Regressions are performed at the unit by month level. For each unit and month, I calculate the average change in sergeant effects separately for officers who move in and out of the unit in that month. The table reports results for a regression of the sergeant effect changes on the natural logarithm of violent 911 calls originating in the unit's sector-watch in each of the 5 months prior to the focal month. Violent 911 calls include shootings, assaults, major disturbances, and robberies. Regressions include fixed effects for the sector-watch. Dependent variables are the average change in the sergeant effects for out-movers (columns 1 and 3) and in-movers (columns 2 and 4). Out-movers are officers who leave the unit and in-movers are officers who join the unit in a given month. Columns 1 and 2 use all monthly observations for each unit. Columns 3 and 4 only use the monthly observations in which at least one out-move (column 3) or one in-move (column 4) occurs. Standard errors are clustered at the sector-watch level.

Table B.8: Trends in property crime 911 calls do not predict changes in sergeant effects

	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)
$Log(PropertyCrime911Calls)_{-1}$	-0.0108	-0.0100	0.0137	0.0619
	(0.0221)	(0.0176)	(0.0666)	(0.0604)
$Log(PropertyCrime911Calls)_{-2}$	0.0322	0.0072	0.0699	0.0072
	(0.0197)	(0.0254)	(0.0672)	(0.0823)
$Log(PropertyCrime911Calls)_{-3}$	3.96×10^{-5}	0.0081	-0.0134	-0.0480
	(0.0253)	(0.0223)	(0.0829)	(0.0754)
$Log(PropertyCrime911Calls)_{-4}$	-0.0371*	-0.0199	-0.0708	-0.0101
	(0.0210)	(0.0191)	(0.0777)	(0.0723)
$Log(PropertyCrime911Calls)_{-5}$	-0.0152	-0.0002	-0.0491	0.0497
	(0.0187)	(0.0220)	(0.0686)	(0.0786)
Observations	5,525	5,525	1,387	1,424
Mean $\lvert Y \rvert$	0.13460	0.13891	0.53615	0.53896
Joint F p-value	0.24419	0.87368	0.76811	0.91392
Sector-Watch fixed effects	✓	✓	✓	✓

Notes: This table examines the correlation of crime trends with sergeant switches. Regressions are performed at the unit by month level. For each unit and month, I calculate the average change in sergeant effects separately for officers who move in and out of the unit in that month. The table reports results for a regression of the sergeant effect changes on the natural logarithm of property crime 911 calls originating in the unit's sector-watch in each of the 5 months prior to the focal month. Property crime calls include those for burglary, larceny, and auto theft. Regressions include fixed effects for the sector-watch. Dependent variables are the average change in the sergeant effects for out-movers (columns 1 and 3) and in-movers (columns 2 and 4). Out-movers are officers who leave the unit and in-movers are officers who join the unit in a given month. Columns 1 and 2 use all monthly observations for each unit. Columns 3 and 4 only use the monthly observations in which at least one out-move (column 3) or one in-move (column 4) occurs. Standard errors are clustered at the sector-watch level.

Table B.9: Trends in index crime report do not predict changes in sergeant effects

	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)
$Log(IndexCrimeReports)_{-1}$	-0.0170	-0.0220	-0.0096	-0.0368
	(0.0197)	(0.0167)	(0.0684)	(0.0502)
$Log(IndexCrimeReports)_{-2}$	0.0261	0.0241	0.0378	0.0813
	(0.0196)	(0.0261)	(0.0687)	(0.0881)
$Log(IndexCrimeReports)_{-3}$	-0.0186	-0.0159	-0.0241	-0.0971
	(0.0243)	(0.0218)	(0.0796)	(0.0805)
$Log(IndexCrimeReports)_{-4}$	-0.0448**	-0.0023	-0.1838**	0.0004
	(0.0189)	(0.0173)	(0.0702)	(0.0565)
$Log(IndexCrimeReports)_{-5}$	-0.0037	0.0015	0.0141	0.0178
	(0.0167)	(0.0210)	(0.0641)	(0.0769)
Observations	5,525	5,525	1,387	1,424
Mean $\|Y\|$	0.13460	0.13891	0.53615	0.53896
Joint F p-value	0.11036	0.74749	0.17207	0.80693
Sector-Watch fixed effects	✓	✓	✓	✓

Notes: This table examines the correlation of crime trends with sergeant switches. Regressions are performed at the unit by month level. For each unit and month, I calculate the average change in sergeant effects separately for officers who move in and out of the unit in that month. The table reports results for a regression of the sergeant effect changes on the natural logarithm of index crime reports originating in the unit's sector-watch in each of the 5 months prior to the focal month. Index crime reports are calculated using Dallas PD' National Incident Based Reporting System (NIBRS) database. Index crimes include murder, aggravated assault, rape, robbery, burglary, theft, auto theft, and arson. Dependent variables are the average change in the sergeant effects for out-movers (columns 1 and 3) and in-movers (columns 2 and 4). Out-movers are officers who leave the unit and in-movers are officers who join the unit in a given month. Columns 1 and 2 use all monthly observations for each unit. Columns 3 and 4 only use the monthly observations in which at least one out-move (column 3) or one in-move (column 4) occurs. Standard errors are clustered at the sector-watch level.

C Data Appendix

C.1 Sergeant Assignments

The Computer Aided Dispatch (CAD) system used by the Dallas Police Department stores assignment indicators for every sworn employee assigned to a call. These indicators are known internally as "element numbers." Element numbers contain information about the employee's sector and watch, as well as an identifier that distinguishes between two units operating within a single sector-watch, when applicable. Element numbers are assigned daily to each patrol car. An element number takes the form of a letter followed by a three-digit numeric. The letter denotes the watch and identifies the specific unit within a sector-watch. The numeric denotes the assigned beat, a smaller geographic area within a sector.

Watches are denoted by letters A–F. A, B, and C denote overnight, day, and evening watches, and E, D, and F denote a second unit within each of those watches respectively: E is the second overnight unit, D the second day unit, and F the second evening unit. Each unit has a sergeant whose element number matches that unit. An officer with element number A135 is working the overnight shift patrolling beat 135, which is part of sector 130.

Sergeants receive element numbers denoting their sector, watch, and unit. The sergeant for an officer with element number A135 has element number A130. Where multiple units exist within a sector-watch, each unit is associated with a sergeant whose element number matches that unit. An officer in variant overnight unit E135 has a sergeant with element number E130. Sergeants A130 and E130 work in the same sector-watch, each managing a distinct set of officers. I use this pattern in element numbers to construct sergeant and officer unit assignments, as described in Section 3.

The assignments that I construct exclude officer spells in specialty patrol units whose element number does not match a geographic sector. Based on conversations with DPD, these units perform distinct duties from regular patrol officers, as evidenced by the fact that they are not assigned to specific geographic beats. DPD did not provide me with the specific details of the job duties related to these assignments for reasons related to officer safety. But, consistent with a separate and distinct role, officers in these specialty units exhibit significant more variation in arrests than regular patrol officers and sergeants cannot be reliably identified for these units.

C.2 Sample Construction

In the main text, I describe assigning each officer to the unit in which they appear on the most days within a month, and assigning each unit the sergeant most frequently observed with that assignment. This forms the initial sample construction, which yields a panel of 2,067 officers, 388 sergeants, 15,355 officer-sergeant spells, and 61,166 officer-month observations.

I am interested in the effects of an officer's regularly assigned sergeant, who evaluates officer performance and works with the officer on most of their workdays. In practice, officers may not be assigned to the same sergeant every day. During a sergeant's off days, their duties are filled by a rotational substitute. Officers may also be temporarily reallocated to a different sector-watch based on manpower needs. In both cases, the regularly assigned sergeant still retains administrative responsibility for the officer. To the extent that officers receive advice and instruction from multiple sergeants within a month, my assignment method captures the influence of the sergeant to whom they are most frequently exposed.

To ensure that my estimates reflect the influence of regularly assigned sergeants, I impose two sample restrictions. First, I require that officer-sergeant spells last at least two consecutive sample months. This reduces the risk of assignment errors stemming from officers working temporary assignments with more activity than their permanent one. This restriction eliminates 5,747 spells, 19% of which are single-month spells with no assigned sergeant. Second, I exclude the remaining 866 spells in which a sergeant cannot be identified. In Figure C.1, I show that these sample restrictions do not meaningfully affect the ranking of sergeants in terms of their arrest effects. However, removing these restrictions attenuates the magnitude of sergeant effects by including reassignments which may be mismeasured or otherwise uncorrelated with changes in officer behavior.

To facilitate identification of sergeant and officer fixed effects, I remove any officers and sergeants who appear only together, any officer/sector-watch and sergeant/sector-watch pairs that appear only together, and any officers, sergeants, sector-watches, or day-off groups that appear only once in the data. I also require that officers appear 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 officer-month observations.

C.3 Linking Arrests to Court Cases

I obtained data on felony and misdemeanor court convictions from the Dallas County District Attorney's Office. Dallas County includes the city of Dallas as well as smaller surrounding jurisdictions that are primarily policed by their own municipal police forces or the Dallas County Sheriff.

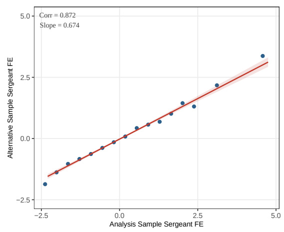
I link each arrest that is charged with either a felony or misdemeanor to court outcomes using records obtained from the Dallas County District Attorney's Office and classify its conviction status. This means that I do not observe convictions for warrant arrests, as well as other miscellaneous categories such as municipal ordinance violations and mental health detentions.

I link Dallas PD arrest files to court cases using a fuzzy match on name and offense date within the universe of cases disposed in Dallas County from 2014 to 2020. I first match arrests to all court cases with the same offense date, then 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 both names perfectly matching (i.e., Jaro-Winkler score equal to 1), I keep only that case. For all other arrests, I keep a match if the Jaro-Winkler score is 0.9 or higher. This matching technique, similar to that used by Adger et al. (2025), allows for minor spelling errors in arrest reports while remaining conservative in name similarity requirements. In the case that a felony or misdemeanor arrest does not match to court data, I consider it to be unfiled, meaning that it was either not advanced for prosecution by either the District Attorney or submitted for charges by the police department. On average, roughly half of charged arrests are unfiled, which is similar to the proportion of 911 call arrests that are unfiled in Adger et al. (2025), who use data from the same jurisdiction during an overlapping sample period.

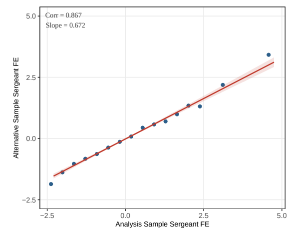
The court data include information on case outcomes, such as whether the case was dismissed, convicted by jury, by judge, or by plea bargain. I classify an arrest as convicted if it was convicted on the highest associated charge by any means, including plea bargain. Using this classification, 38% of charged arrests receive a guilty conviction on average in an officer-month. This figure is similar to the average proportion of officer arrests that result in a conviction within an officer-year found by Chalfin and Gonçalves (2026), 34%, who study the same setting during a sample period that overlaps with my own.

Figure C.1: Robustness to Alternative Sampling Decisions

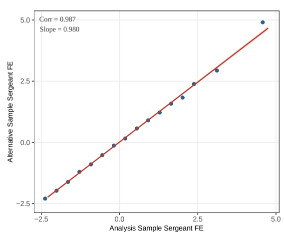
(a) Unrestricted



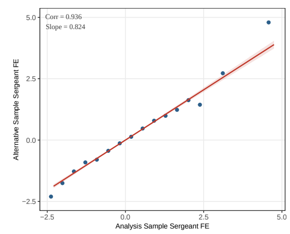
(b) Impute Missing Within Spell



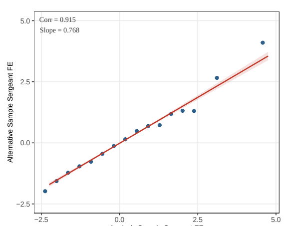
(c) Impute Missing Within Spell, remove everything else



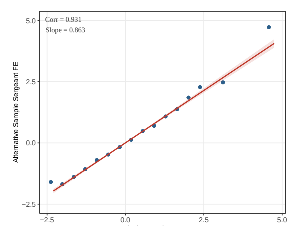
(d) Impute all temporary assignments



(e) Keep temporary, remove missing



(f) Keep missing, remove temporary



Notes: This figure presents the correlation between sergeant fixed effects under different sampling restrictions. (a) makes no sample restrictions, (b) imputes missing months within a sergeant spell and keeps any other observations where the sergeant is unknown, (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 (single month) assignments but removes all months with an unknown sergeant, and (f) keeps months with an unknown sergeant but removes the temporary (single month) assignments. In the specifications that include months with an unknown sergeant, the model includes an 'Unknown Sergeant' fixed effect. All models used to estimate the plotted fixed effects include a second-degree polynomial of officer tenure and officer, sector-watch, and day-off group fixed effects.

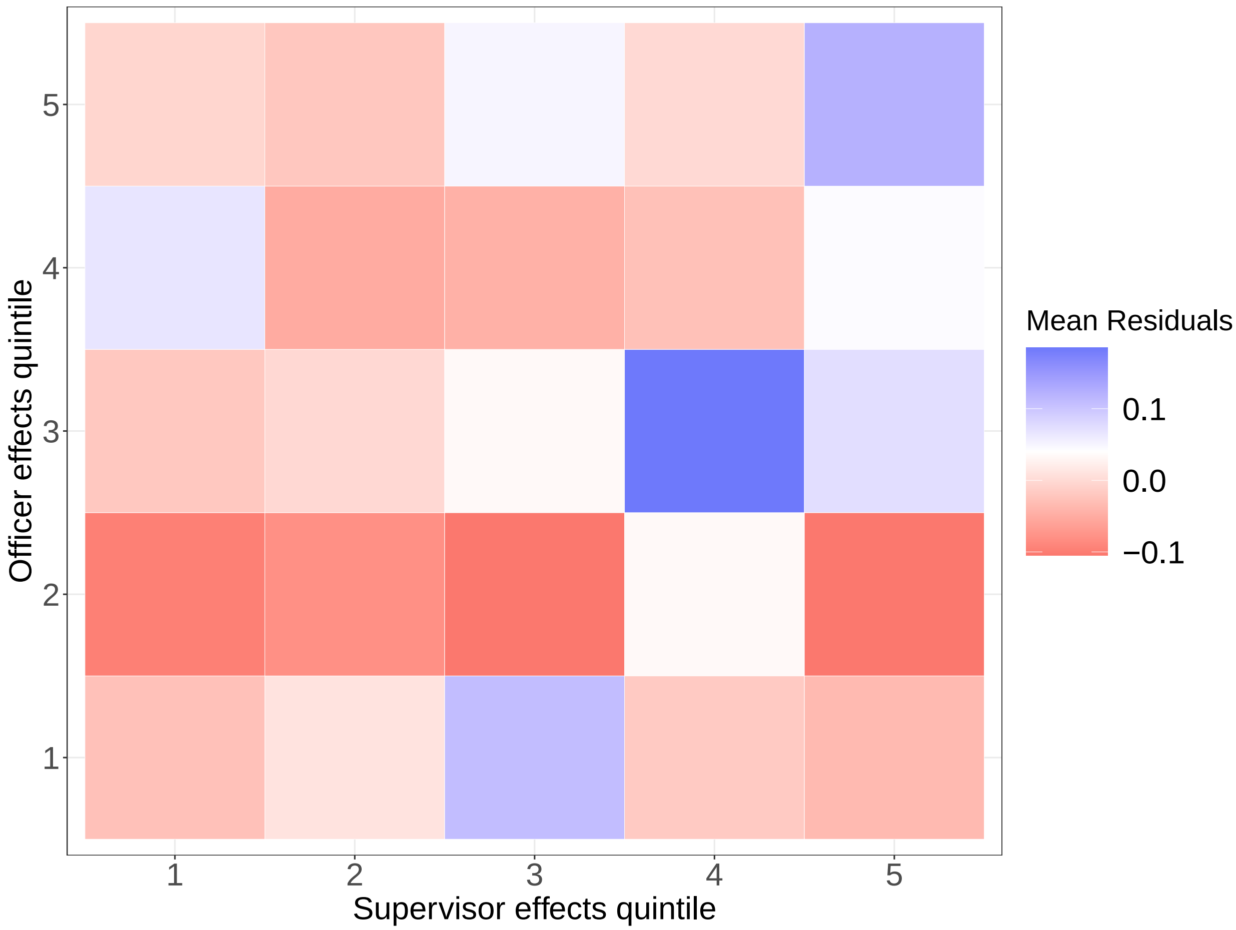
D Additional Diagnostic Checks

D.1 Additive Separability Assumption

The AKM model in equation 1 assumes that officer and sergeant effects are additively separable. If sergeant effects were officer-specific, then the separate officer and sergeant fixed effects would not be informative and may be a product of statistical noise. I conduct two tests to assess this assumption. First, following Card et al. (2013), I examine the average residuals from equation 1 separately by groups of officer and sergeant effects. Specifically, I divide each officer-month observation into quintiles of officer and sergeant effects. If the additive separability assumption did not hold, then I would expect the model to systematically under- or over-estimate arrests for certain officer-sergeant groups. For example, if aggressive officers perform especially well under disengaged sergeants, we would expect large positive residuals in the cell corresponding to top-quintile officers paired with bottom-quintile sergeants. Figure D.1 demonstrates that the mean residuals do not exhibit any clear patterns that would indicate a violation of the additive separability assumption. Across all officer-sergeant groups, the residuals are relatively small — none are larger, in magnitude, than 5% of average monthly arrests — suggesting that the threat of misspecification is minimal in this setting.

The second test of additive separability compares the explanatory power of the baseline specification to a fully saturated model that contains a fixed effect for each officer-sergeant pair. I report the R^2 and Adjusted R^2 for these models in Table D.1. The fully saturated model fits better than the baseline, with match effects increasing Adjusted R^2 by 0.054 relative to the two-way fixed effects model — modest relative to the 0.341 increase from including officer and sergeant fixed effects additively. This suggests that match effects play a relatively minor role in explaining variation in officer arrests, making the additively separable model a useful approximation.

Figure D.1: Residuals by quintile of officer and sergeant arrest effects



Notes: This figure reports the average residuals by quintiles of officer and sergeant (supervisor) arrest effects, estimated using equation 1, which includes controls for a second-degree polynomial of officer tenure as well as sector-watch and day-off group fixed effects. Darker blue indicates more positive residuals and darker red indicates more negative residuals.

Table D.1: 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		✓	✓		
Sergeant FE			✓	✓	
Sergeant-by-Officer FE					✓
Observations	49,923	49,923	49,923	49,923	49,923

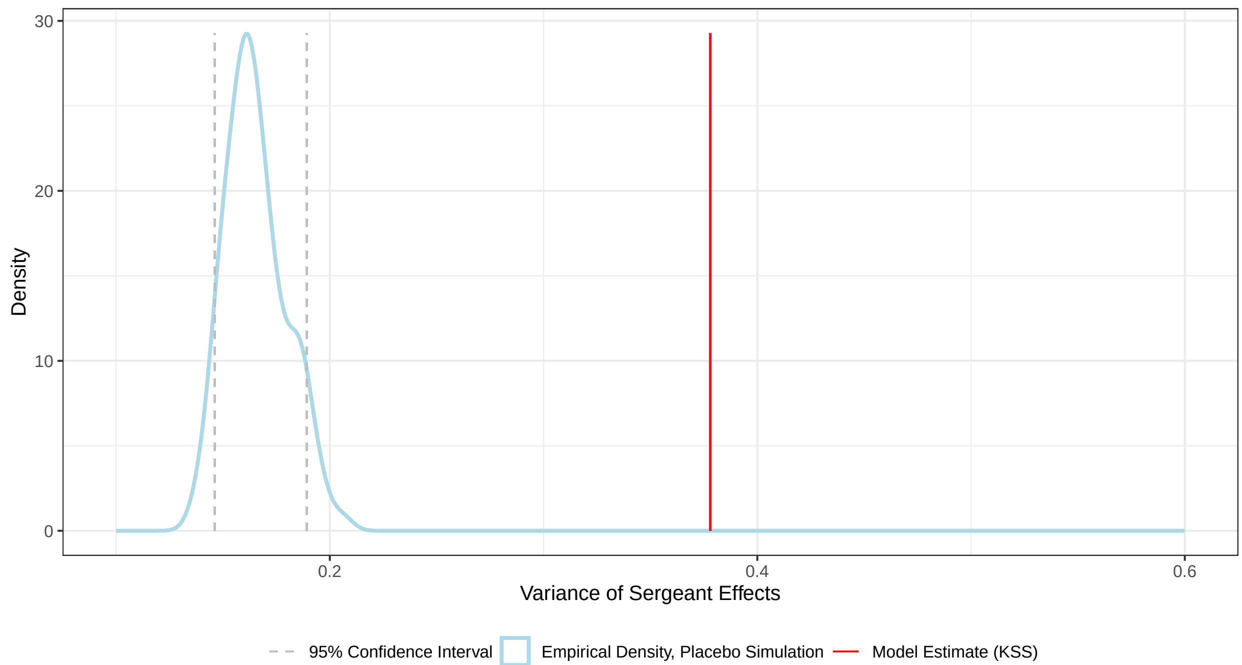
Notes: This table reports R^2 and adjusted R^2 for models that vary the included fixed effects. Controls include a second degree polynomial of officer tenure, and sector-watch and day-off group fixed effects.

D.2 Estimation Noise

In practice, each sergeant effect is identified from a small number of officer switches — 33.4, on average — relative to the number of officers, which is over 1,800. This raises concerns that estimated variance may reflect idiosyncratic noise rather than genuine managerial influence. To test this, I estimate placebo sergeant effects by randomly reallocating sergeants to officers while preserving the number of unique officers per sergeant — a permutation test that yields a distribution of the variance estimate under the null of no sergeant effects. Weisburst (2024) applies this same technique to officer fixed effects.

For each random assignment, I estimate the variance in arrests attributable to these placebo sergeants. I repeat this procedure 100 times and plot the resulting distribution of placebo variance estimates in Figure D.2, along with the KSS variance estimate from Table 1. To be conservative, I do not apply bias correction to the placebo estimates, which biases them upward and makes the test more stringent. Reassuringly, the placebo estimates are close to 0 and my model variance estimate lies well outside a 95% confidence interval of the sergeant effect variance that would be obtained by chance.

Figure D.2: Placebo Test, Sergeants randomly reassigned to officers



Notes: This figure depicts the results of placebo tests that randomly reallocate sergeants to officers, preserving the number of unique officers managed for each sergeant. For every reallocation, I estimate equation 1 and report the resulting (unadjusted) variance of sergeant effects. The empirical density, in light blue, plots the density of variance estimates for 100 reallocations. The dashed lines denote the 95% confidence interval of the placebo variance estimates. The red vertical line denotes my main estimate of the variance in sergeant effects, adjusted for bias using the KSS method described in Section 4.

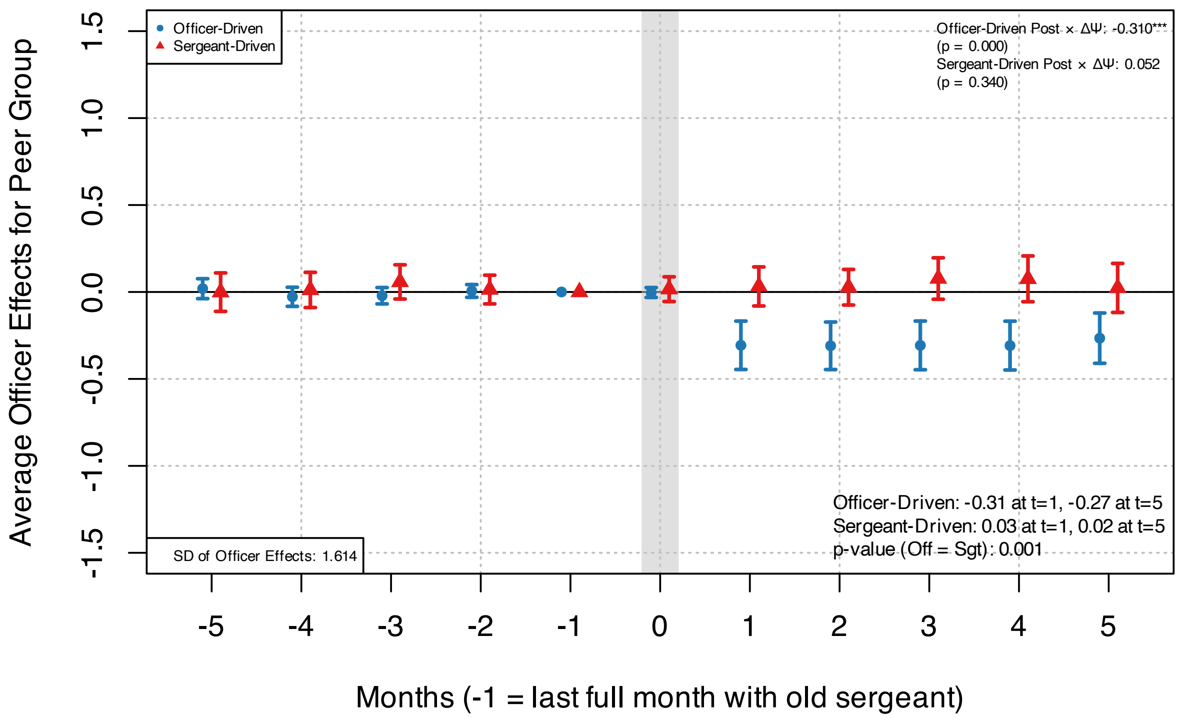
D.3 Peer Effects

There may be concerns that sergeant effects are driven by peer effects. In the case of officer-driven switches, officers' peer groups may change in ways that affects their arrests. I first test for systematic changes in peers associated with sergeant changes using an event study. For each switching event in the event study sample, I calculate the average Bayes-shrunken arrest effect of the focal officer's peer officers — those who work in the same unit as them in each month. I then estimate event studies as in equation 4, splitting by officer-driven and sergeant-driven switches. I plot results in Figure D.3. The results suggest that officer-driven moves are associated with changes to the officer's peer group: moving to a unit with a sergeant who induces one more arrest than your previous sergeant is associated with a reduction in peer officers' average arrest effects by 18% of a standard deviation in the officer arrest distribution. Moving to a sergeant who induces more arrests than your previous sergeant is thus associated with moving to a peer group that consists of officers who make *fewer* arrests on average than those in your previous peer group. If peer effects on arrests operate through officers' time-invariant arrest effects, which is a strong assumption, these results would imply that the presence of peer effects attenuates the estimated sergeant effects. Reassuringly, peer attributes do not change with a sergeant-driven switch, since the officer remains in the same unit.

Note that even if the composition of peer groups change when officers switch sergeants, this does not necessarily imply that differences in peers' arrest effects will systematically change officer behavior. It is possible that peer effects within patrol units are limited, since officers often patrol alone. To assess the extent to which peer groups contaminate the sergeant effects, I re-run the AKM specification including controls for peer demographic composition in each officer-month; this includes proportion of peers who are black, white, Hispanic, and female; as well as average tenure of peers. I plot the relationship between the (unshrunken) fixed effects from this specification against the (unshrunken) fixed effects from the main specification in Figure D.4. Sergeant effects are nearly identical in this alternative specification that includes peer attributes.

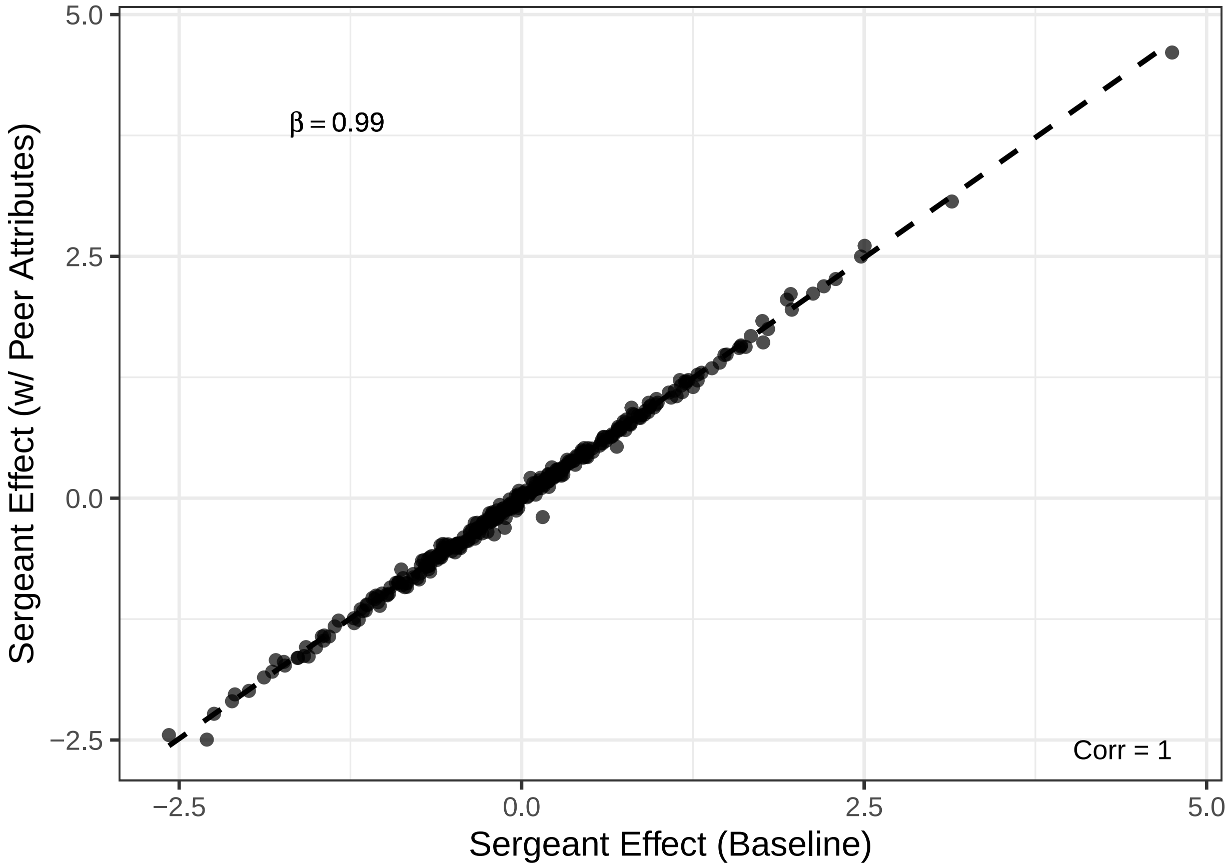
Finally, I note that sergeant-driven switches produce similar changes to officer behavior, as shown in Figure A.13. Figure D.3 shows that peer groups do not systematically change around sergeant-driven switches, thus it is unlikely that the sergeant effects are explained by peer effects.

Figure D.3: Impact of Sergeant Switches on Officer Peer Groups



Notes: Each point estimate (with 95% confidence interval) represents $\hat{\pi}_1^k$ from equation 4, using the average Bayes-shrunken officer effects for the switching officer's unit peers as the outcome. Blue points represent estimates from a regression using only officer-driven switches; red points represent estimates from a regression using only sergeant-driven switches. Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. The top right reports the $Post \times \Delta\psi$ coefficient and p-values from the DD specification in equation 5 for each switch type, summarizing the average post-switch effect across periods 1 through 5. The bottom right lists point estimates at selected months, and the p-value from an F-test of equality between point estimates from officer-driven and sergeant-driven regressions interaction coefficients across all post-switch periods jointly, using a Seemingly Unrelated Regression model. The models are estimated using the event study sample described in Table B.1, with standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

Figure D.4: Sergeant Effects with Peer Group Controls



Notes: This figure plots baseline sergeant effects from equation 1 on the x-axis against sergeant effects from a specification that augments equation 1 with controls for the composition of other officers in the focal officer's unit — specifically, the proportion Black, white, Hispanic, and female, and average tenure — on the y-axis. The bottom right reports the correlation between the two sets of estimates; the top left reports the slope coefficient (β) from a linear fit. Each sergeant effect is estimated in a model that controls for a second-degree polynomial of officer tenure, as well as officer, sector-watch, and day-off group fixed effects.

E Additional Outcomes: Conviction Rates and Race

E.1 Conviction Rates

Sergeants may induce arrests by lowering officers' evidentiary threshold — entailing clear costs, since low-quality arrests burden courts and harm civilians without contributing to public safety. I measure arrest quality using court conviction rates, available only for arrests charged with a felony or misdemeanor, excluding uncharged arrests such as warrants and city ordinance violations. Lower conviction rates following a switch would suggest sergeant-induced arrests are of lower evidentiary quality, though conviction rates may also change because officers make more arrests for crimes with different ex-ante conviction rates.

Because officers frequently make no arrests in a given month, I cannot directly estimate equation 4 using monthly conviction rates as the outcome. Instead, following Gudgeon et al. (2023), I separately estimate equation 4 using total and convicted arrests as outcomes, add event study coefficients to their respective pre-period means, and calculate the implied change in conviction rates relative to the pre-period ratio, with standard errors computed via the Delta method.[1] Results are plotted separately for all, serious, and low-level arrests in Figure E.1.

The results show no statistically significant reduction in conviction rates for any sergeant effect type across total, serious, or low-level arrests, providing no evidence that sergeant-induced arrests are of lower evidentiary quality. If anything, the evidence points in the opposite direction for low-level sergeant effects. DD estimates indicate that low-level conviction rates increase by approximately 3 percentage points (9% of the pre-period mean) in the post-period, a statistically significant result, while total conviction rates are slightly positive but insignificant. These increases are most pronounced in the second post-switch month and remain positive throughout, though with a subtle decline thereafter. To assess whether this reflects genuine quality improvements or compositional changes, I re-estimate conviction rate

[1] To recover the cross-equation covariance, I stack the two equations and compute a clustered sandwich variance estimator, clustering on officer.

effects within specific offense types. Results are noisy but suggest that drug conviction rates may increase following a switch, driven by possession rather than sale arrests (Figures E.3 and E.4). However, pre-trends are noisy and the post-switch increase in drug convictions is not clearly distinguishable from pre-switch coefficients. Given the higher baseline conviction rates for drug arrests relative to other low-level offenses, I cannot rule out that the increase in low-level conviction rates is also driven by compositional changes toward drug arrests.[2]

For serious conviction rates, low-level sergeant effects generate no statistically significant changes. Figure E.1 suggests a drop in serious conviction rates at the time of the switch followed by a steady increase through month 3, but none of the individual coefficients are statistically significant.

Serious sergeant effects do not meaningfully affect conviction rates, either overall or within offense type. There is suggestive evidence of reductions in low-level conviction rates — with negative post-switch coefficients in Figure E.1 — though neither the individual coefficients nor the DD estimate are statistically significant. Given the suggestive evidence of reductions in drug arrests for serious sergeant effects (Figure A.20), these patterns may reflect compositional changes away from high-conviction drug arrests rather than lower evidentiary quality. Within-offense-type results in Figure E.3 show no statistically significant changes in low-level conviction rates by offense type. There is suggestive evidence that drug conviction rates fall starting the second month after a switch, but none of the event study coefficients or the DD coefficient are statistically significant.

As an alternative quality measure, I estimate event studies using the rate of unfiled arrests — those not advanced for prosecution by the District Attorney or the police department — as an outcome. Results, presented in Figures E.5, E.7, E.6, and E.8, are largely consistent with the conviction rate analysis. Conviction rate effects are at least partially explained by upstream case strength decisions. One notable difference is the absence of clear changes in unfiled rates for drug arrests under low-level sergeants. Marginal drug arrests under these

[2] These findings parallel Weisburst (2024), who shows that officers who make more misdemeanor arrests also have higher conviction rates on those offenses.

sergeants are no more likely to be prosecuted, but are more likely to result in conviction once charges are filed. This supports the interpretation that these arrests are of higher evidentiary quality

In sum, there is no evidence that the marginal arrests induced by either sergeant effect type are of lower quality than officers would otherwise make.

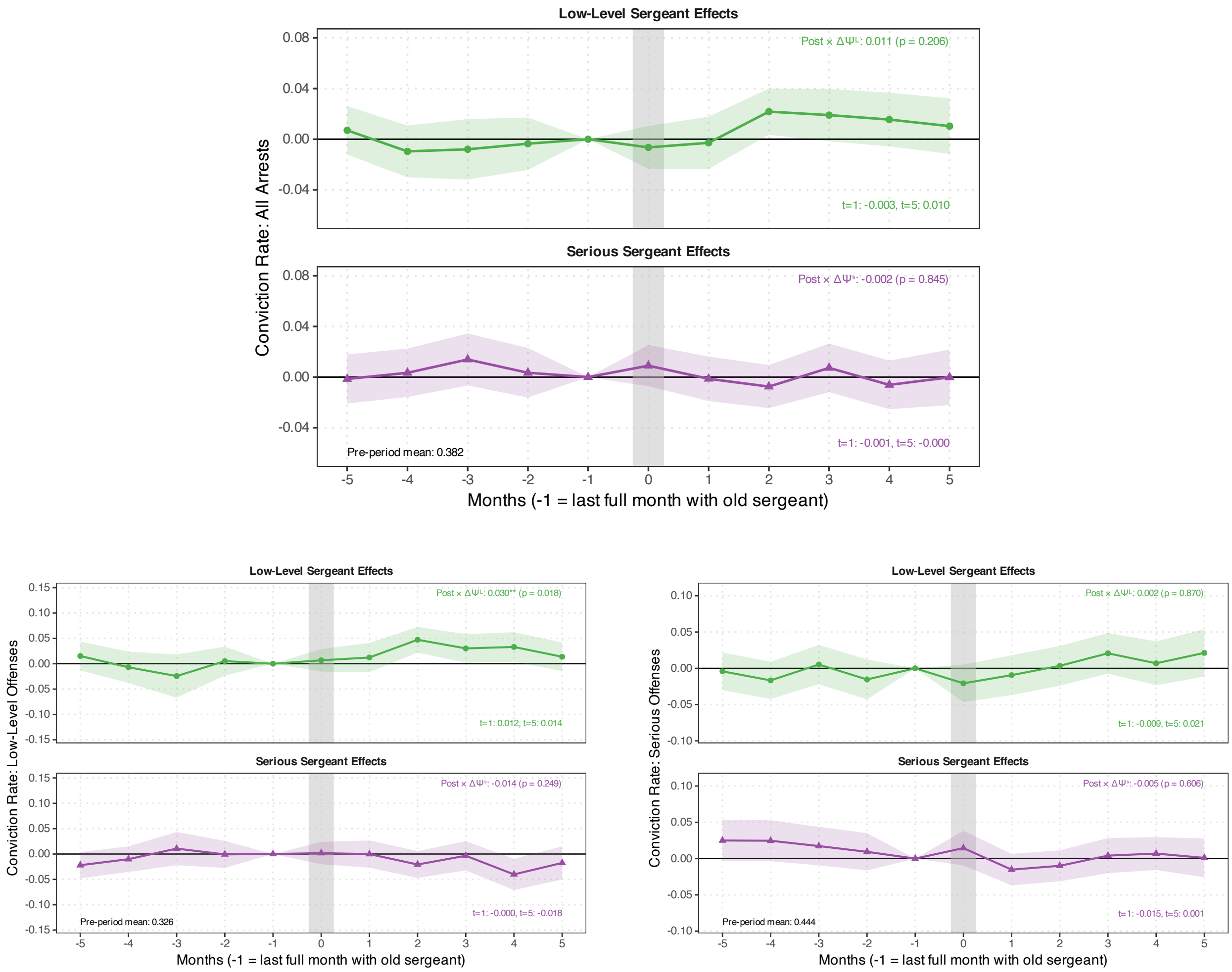


Figure E.1: Sergeant Effects on Conviction Rates

Notes: I estimate conviction rates using a ratio estimator based on two separate regressions following equation 6: one with convicted arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting convicted arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for convicted and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the convicted and total arrest equation residuals, clustering at the officer level. Results are plotted separately for all arrests, serious arrests, and low-level arrests. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

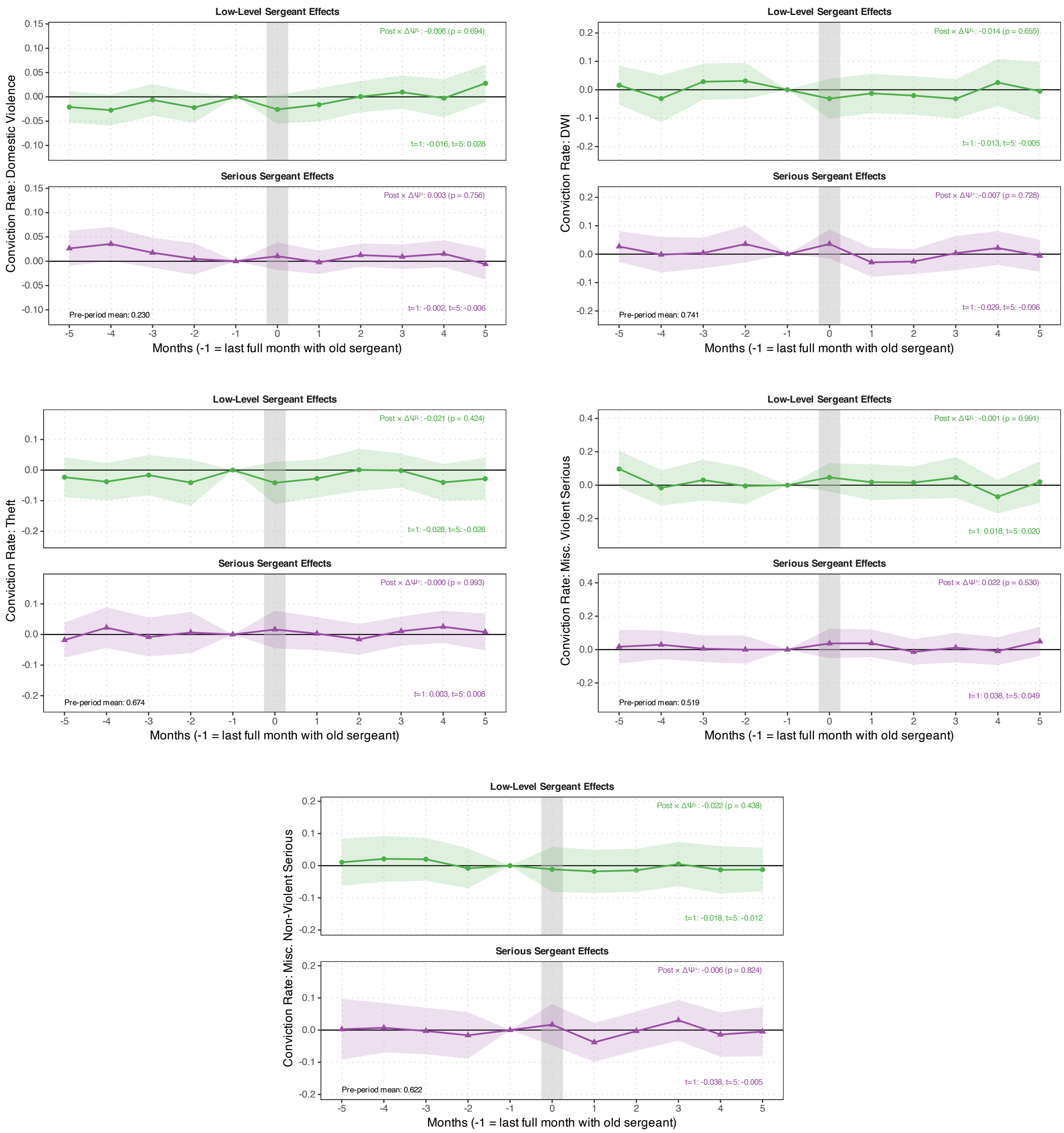


Figure E.2: Sergeant Effects on Serious Crime Conviction Rates

Notes: I estimate conviction rates using a ratio estimator based on two separate regressions following equation 6: one with convicted arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting convicted arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for convicted and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the convicted and total arrest equation residuals, clustering at the officer level. Results are plotted separately for each serious arrest sub-type. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

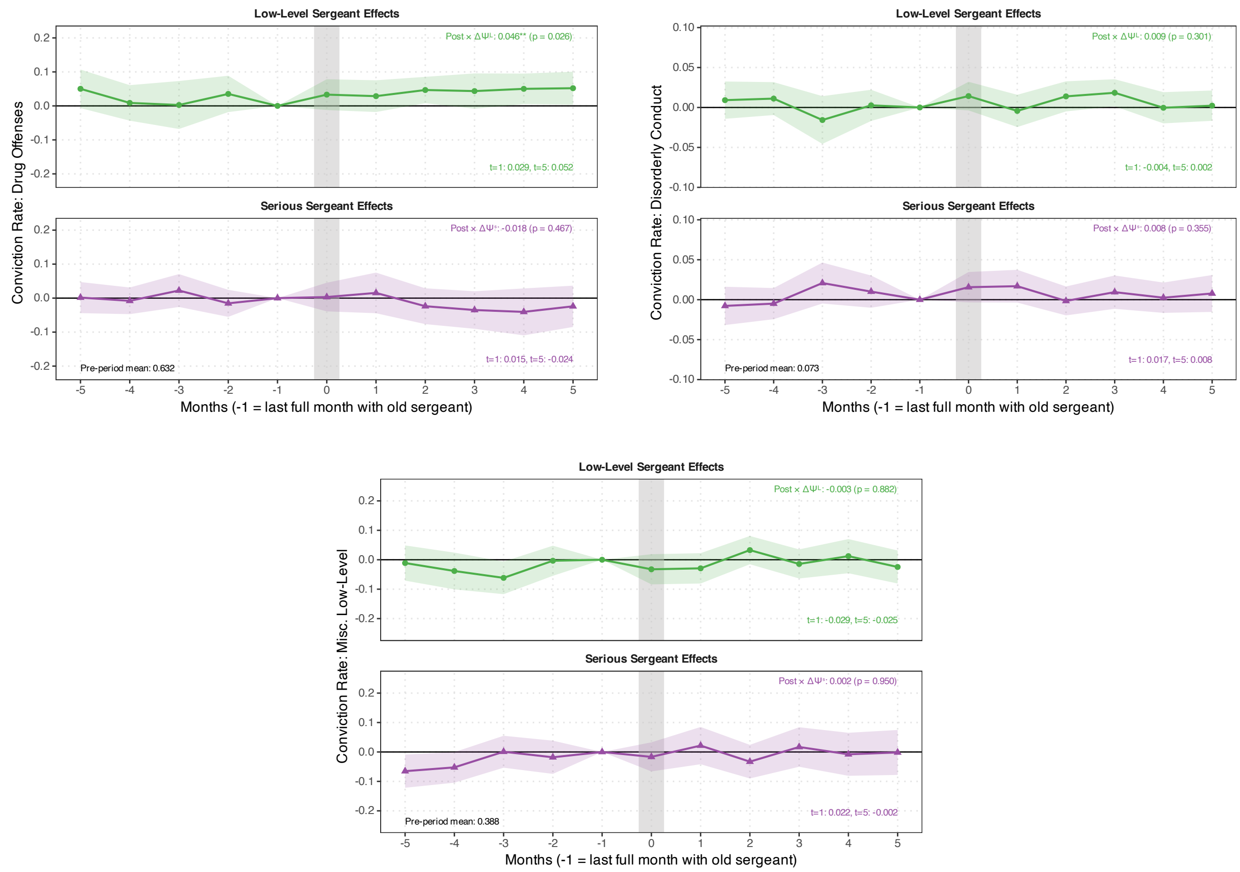


Figure E.3: Sergeant Effects on Low-Level Crime Conviction Rates

Notes: I estimate conviction rates using a ratio estimator based on two separate regressions following equation 6: one with convicted arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting convicted arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for convicted and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the convicted and total arrest equation residuals, clustering at the officer level. Results are plotted separately for each low-level arrest sub-type, excluding sub-types that do not have felony or misdemeanor charges. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

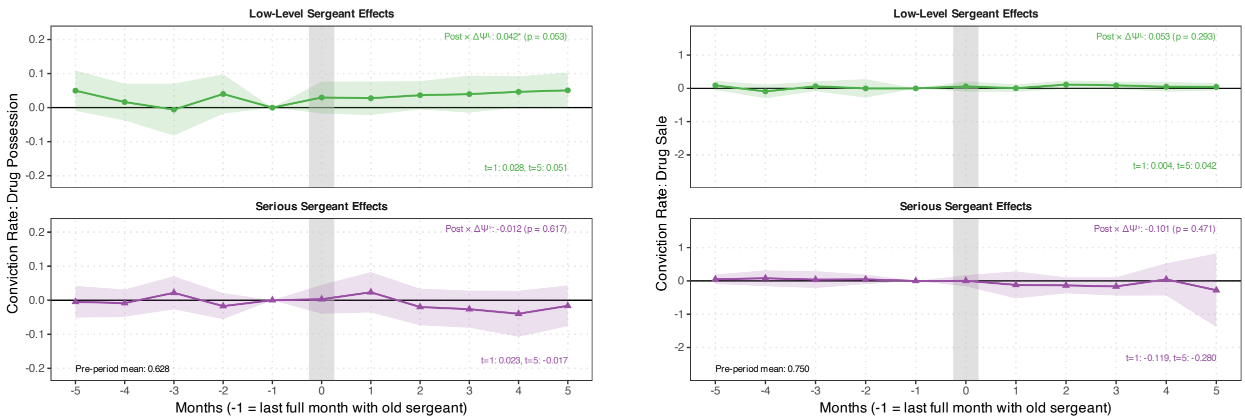


Figure E.4: Sergeant Effects on Drug Conviction Rates

Notes: I estimate conviction rates using a ratio estimator based on two separate regressions following equation 6: one with convicted arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting convicted arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for convicted and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the convicted and total arrest equation residuals, clustering at the officer level. Results are plotted separately for each low-level arrest sub-type, excluding sub-types that do not have felony or misdemeanor charges. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

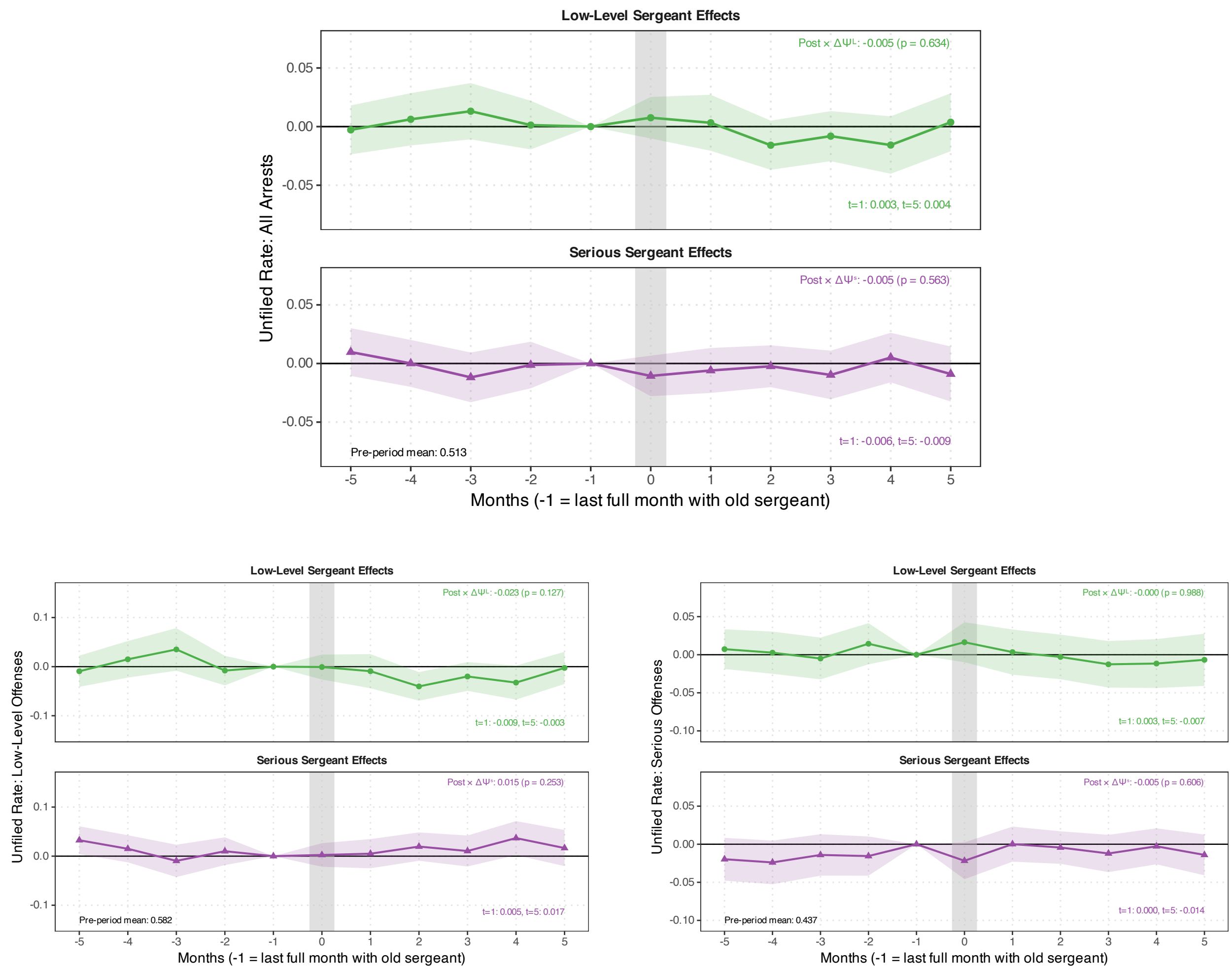


Figure E.5: Sergeant Effects on Unfiled Rates

Notes: I estimate unfiled rates using a ratio estimator based on two separate regressions following equation 6: one with unfiled arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting unfiled arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for unfiled and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the unfiled and total arrest equation residuals, clustering at the officer level. Results are plotted separately for all arrests, serious arrests, and low-level arrests. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

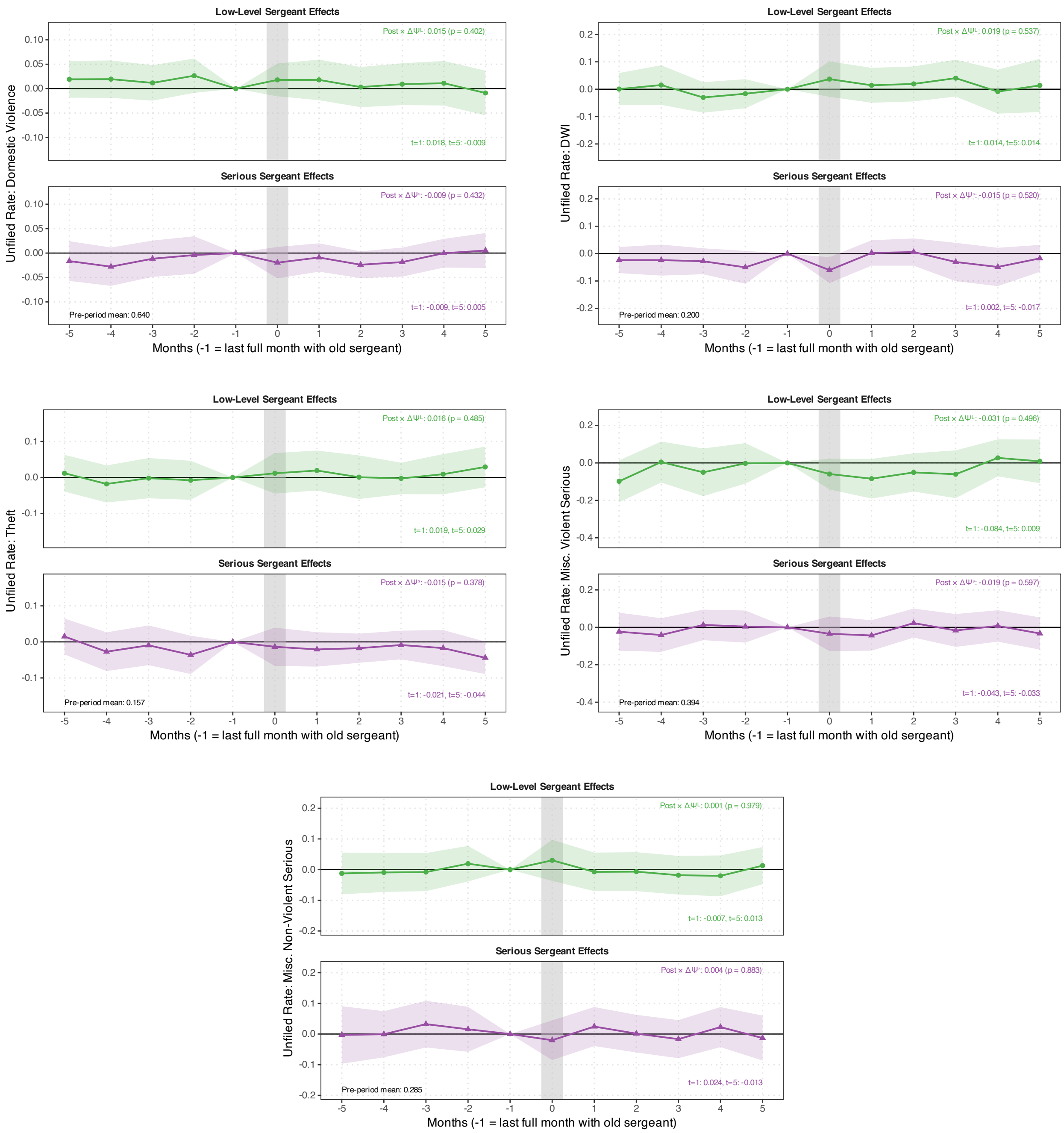


Figure E.6: Sergeant Effects on Serious Crime Unfiled Rates

Notes: I estimate unfiled rates using a ratio estimator based on two separate regressions following equation 6: one with unfiled arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting unfiled arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for unfiled and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the unfiled and total arrest equation residuals, clustering at the officer level. Results are plotted separately for each serious arrest sub-type. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

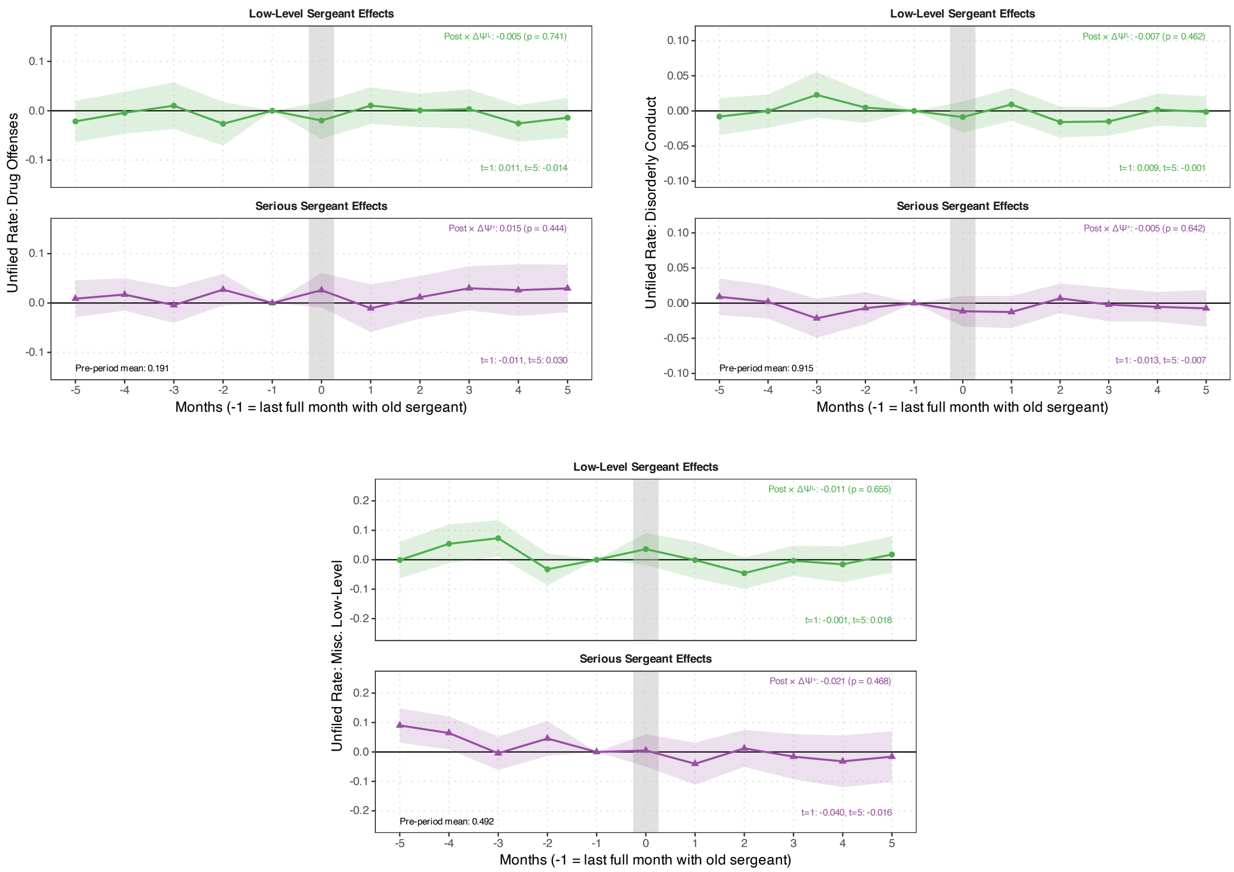


Figure E.7: Sergeant Effects on Low-Level Crime Unfiled Rates

Notes: I estimate unfiled rates using a ratio estimator based on two separate regressions following equation 6: one with unfiled arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting unfiled arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for unfiled and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the unfiled and total arrest equation residuals, clustering at the officer level. Results are plotted separately for each low-level arrest sub-type that has a felony or misdemeanor charge. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

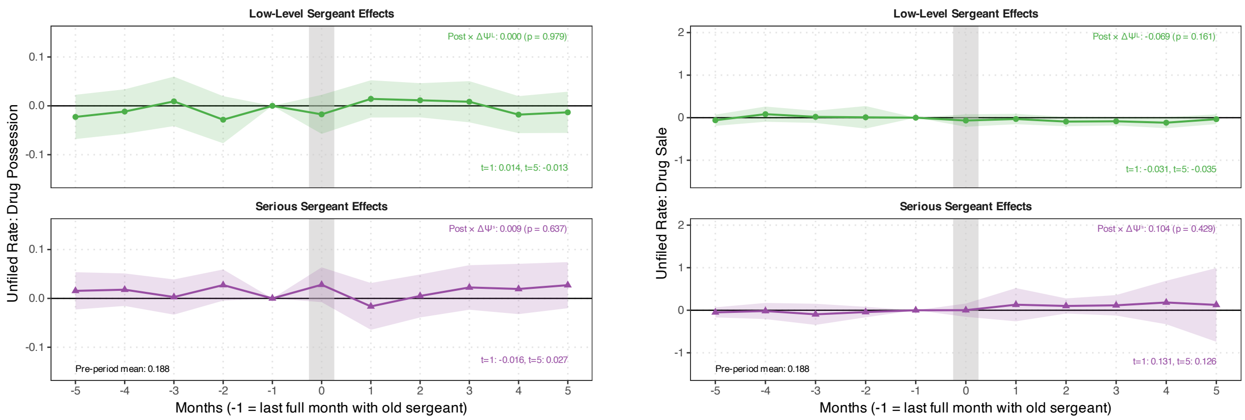


Figure E.8: Sergeant Effects on Drug Unfiled Rates

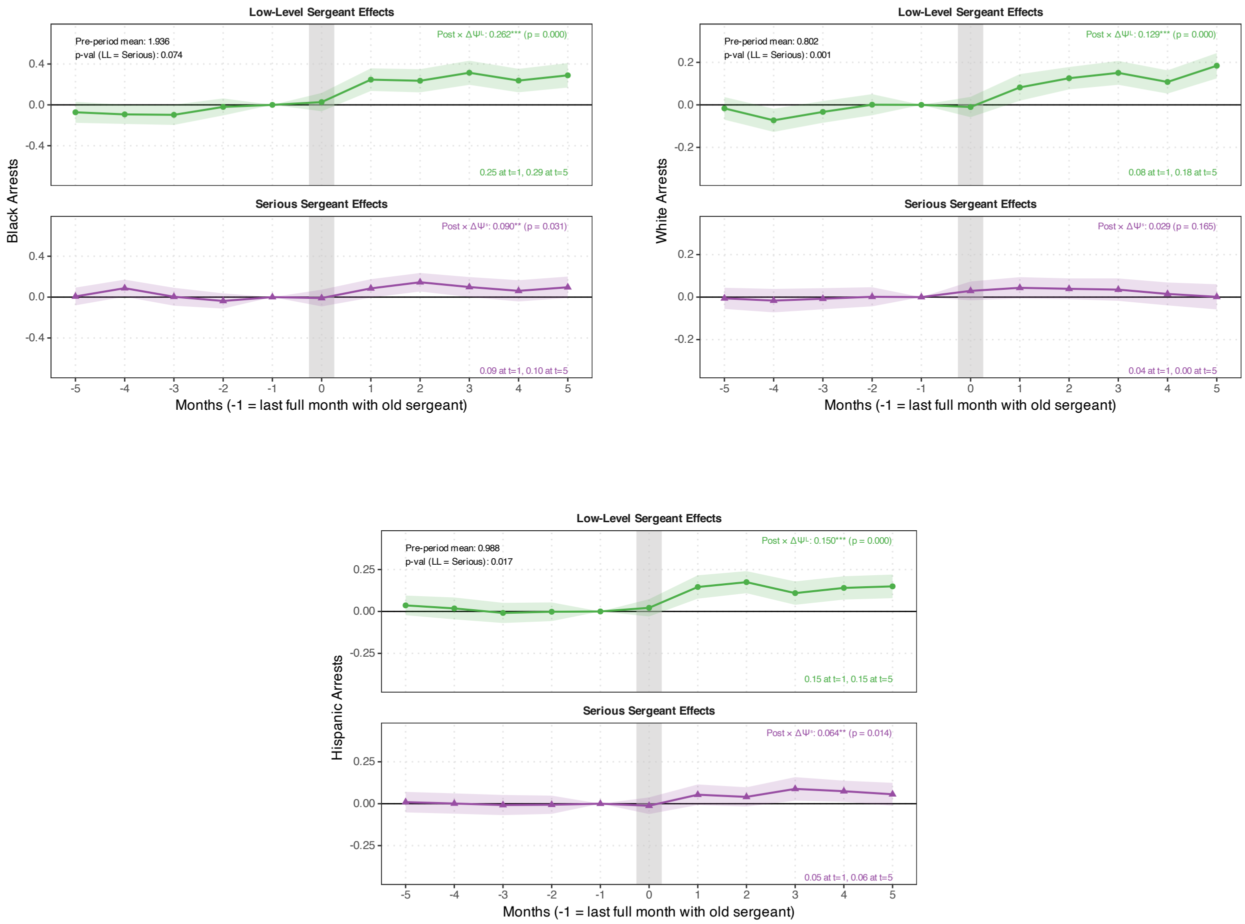
Notes: I estimate unfiled rates using a ratio estimator based on two separate regressions following equation 6: one with unfiled arrests as the outcome and one with total arrests. For each period and sergeant effect type, I add the estimated coefficient to the corresponding period -1 outcome mean, then divide the resulting unfiled arrests value by the resulting total arrests value. The plotted coefficients represent the difference between this estimated ratio and the ratio of period -1 means for unfiled and total arrests. Standard errors are computed via the Delta method, using a cross-equation clustered sandwich variance estimator to account for covariance between the unfiled and total arrest equation residuals, clustering at the officer level. Results are plotted separately for each drug arrest type. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top right of each panel reports the analogous DD estimate from equation 5, applying the same ratio estimation strategy. The bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

E.2 Racial Disparities

Given the racial disparities present in police outcomes such as use of force (e.g. Hoekstra and Sloan, 2022), sergeants may have important impacts on the racial distribution of enforcement. Figure E.9 presents event study estimates for arrests by race of arrestee. Both sergeant effect types increase arrests across white, Black, and Hispanic arrestees, with the largest point estimates for Black arrestees in absolute terms. Black arrestees account for just over 50% of total arrests. Proportional increases relative to race-specific means are similar across groups — 13–15% for low-level effects and 3.5–6.5% for serious effects — consistent with a level increase in enforcement across races rather than a meaningful shift in the racial distribution.

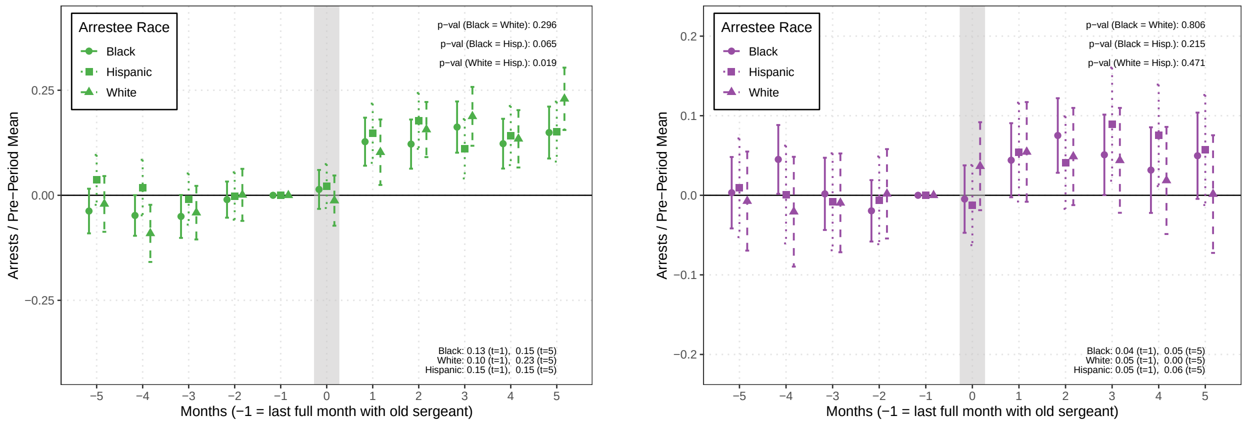
Figure E.10 plots mean-normalized coefficients by race alongside F-tests of equality of period-specific coefficients across racial groups. For serious effects, I cannot reject equality of proportional increases across races in any post-switch period. For low-level effects, I reject equality between Black and Hispanic arrestees at the 10% level and between white and Hispanic arrestees at the 5% level. These rejections appear driven by individual post-switch periods rather than a consistent pattern. No racial group is systematically above or below the others across all post-switch periods.

Figure E.9: Sergeant Effects by Race



Notes: This figure reports results from officer-level event studies around sergeant switches for arrests by race of the arrestee, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects. Regressions are estimated on the event study sample described in Table B.1.

Figure E.10: Sergeant Effects by Race: Mean-Normalized

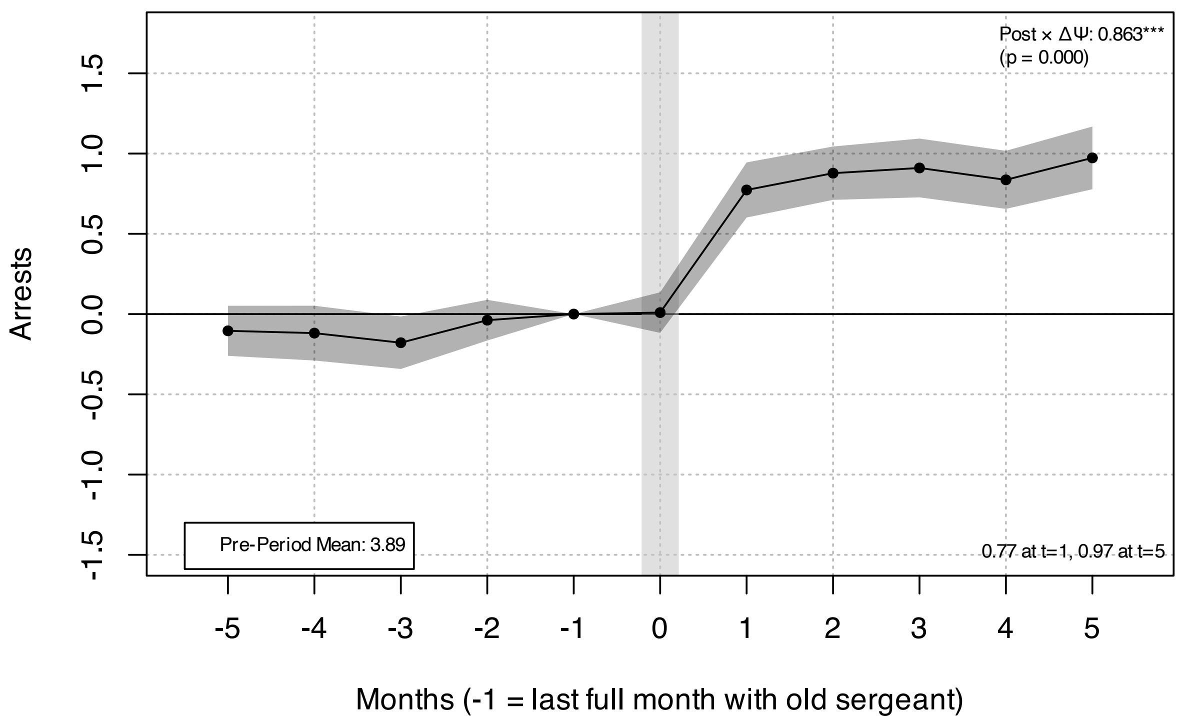


Notes: The left figure plots low-level sergeant effect interaction coefficients for models given by equation 6, using arrests by race as outcomes. The right figure plots serious sergeant effect interaction coefficients for models given by equation 6, using arrests by race as outcomes. Coefficients are divided by their respective outcome means. The bottom right in each panel reports selected point estimates. The top right in each panel The reports the p-value from an F-test of equality between the mean-normalized coefficients across all post-switch periods jointly, using a Seemingly Unrelated Regression model. For all regressions: standard errors clustered at the officer level; Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; and estimation is performed using the main event study sample described in Table B.1.

F Additional Robustness Results

F.1 All Moves

Figure F.11: Event Study Coefficients: All Move Panel



Notes: Each point estimate (with 95% confidence interval) represents $\hat{\pi}_1^k$ from equation 4 — the effect on officer arrests at period k relative to the switch of moving to a sergeant who induces one more arrest per month. Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. The pre-trend F-statistic (top left) tests joint equality to zero for periods -5 through -2. The top right reports the *Post* $\times$ $\Delta\psi$ coefficient and p-value from the DD specification in equation 5, summarizing the average post-switch effect across periods 1 through 5. The bottom right lists point estimates at selected months. The model is estimated on an imbalanced panel including all switching events, dropping any officer-months that are contaminated by switching events other than the one taking place at month 0. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects. Standard errors are clustered at the officer level.

Low-Level Crimes **Serious Crimes**

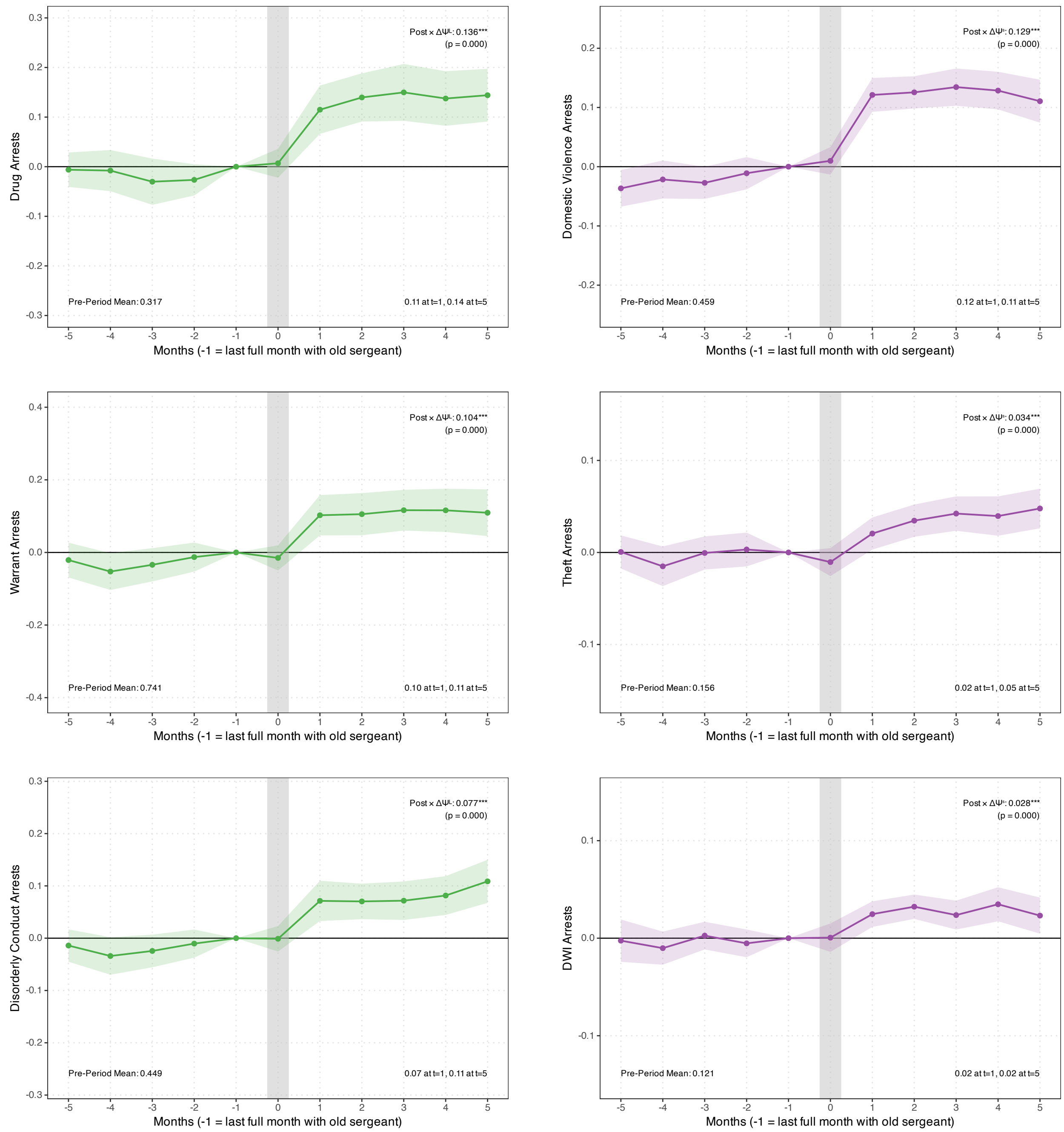


Figure F.12: Sergeant Effects by Arrest Sub-Categories

Notes: This figure reports results from officer-level event studies around sergeant switches for the three most common sub-categories of serious and low-level arrests, estimated using equation 6. Each figure is a separate regression. The left column plots point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level sergeant effects in regressions with low-level arrest sub-types as outcomes; the right column plots the analogous interaction for serious sergeant effects in regressions using serious arrest sub-types as outcomes. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors are clustered at the officer level. All regressions include controls for officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects, and are estimated on an imbalanced panel including all switching events, dropping any officer-months that are contaminated by switching events other than the one taking place at month 0. Standard errors clustered at the officer-level.

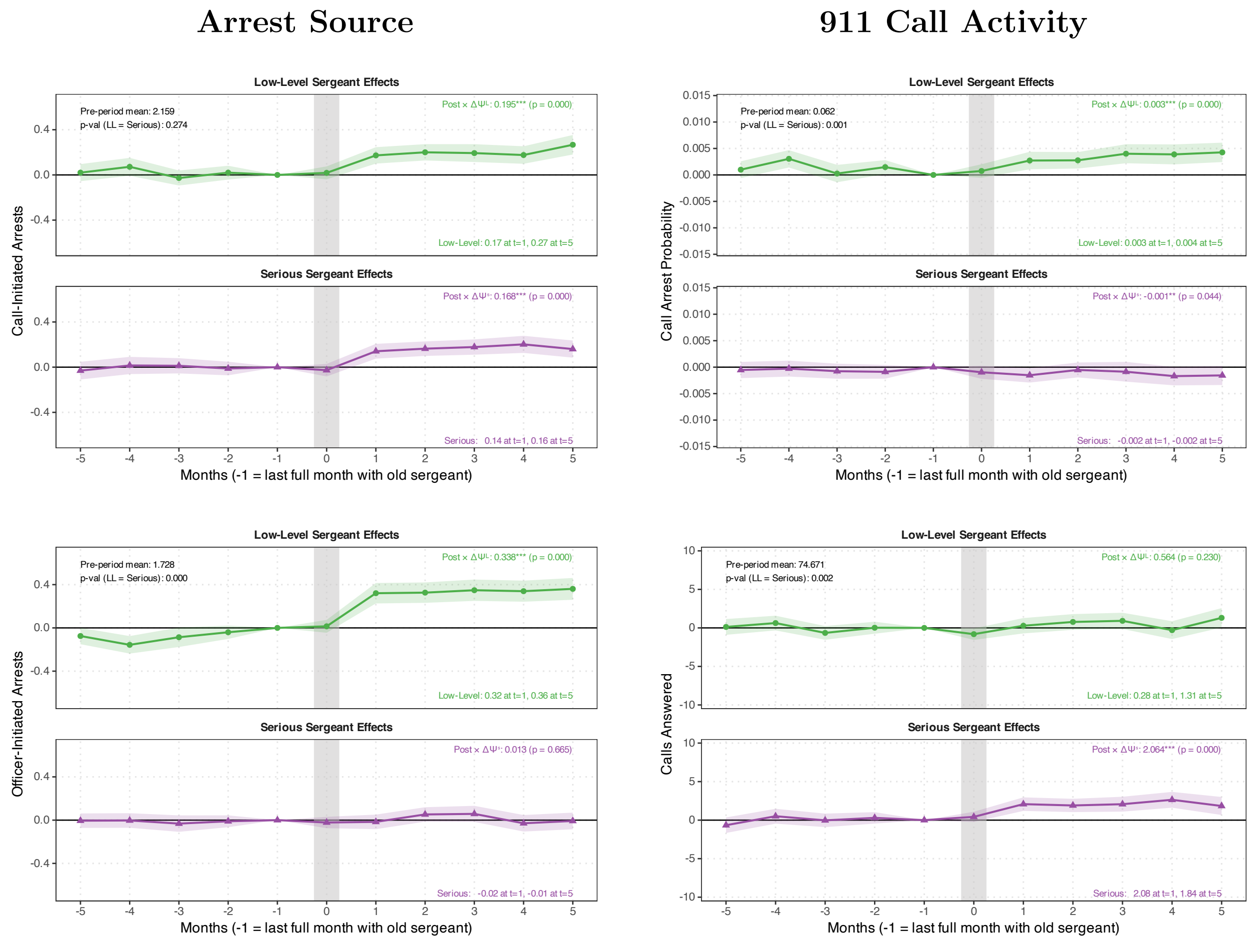


Figure F.13: Sergeant Effects on Police Interactions: All Move Panel

Notes: This figure reports results from officer-level event studies around sergeant switches for arrests by source (left column) and 911 call activity (right column), estimated using equation 6. Call arrest probability is the share of calls answered within a month that result in arrest. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects. Regressions estimated on an imbalanced panel including all switching events, dropping any officer-months that are contaminated by switching events other than the one taking place at month 0. Standard errors clustered at the officer-level.

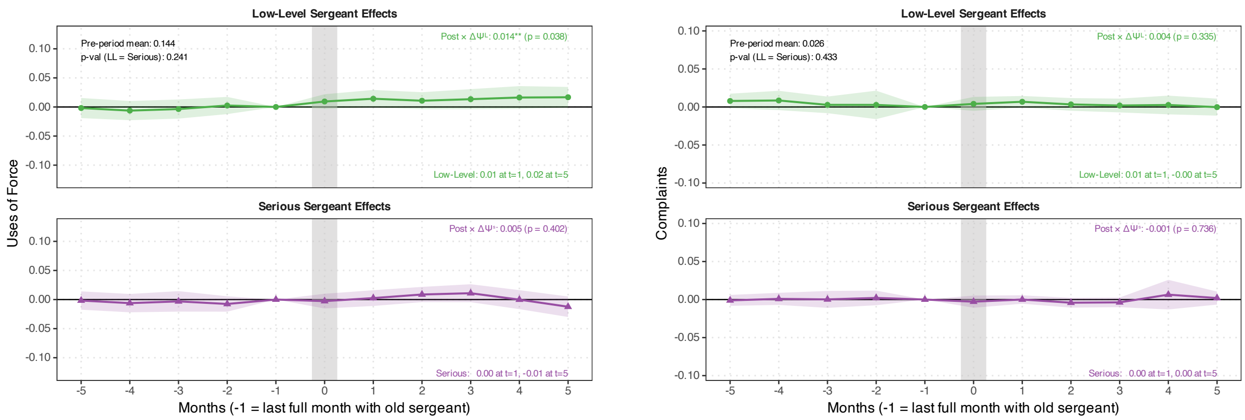
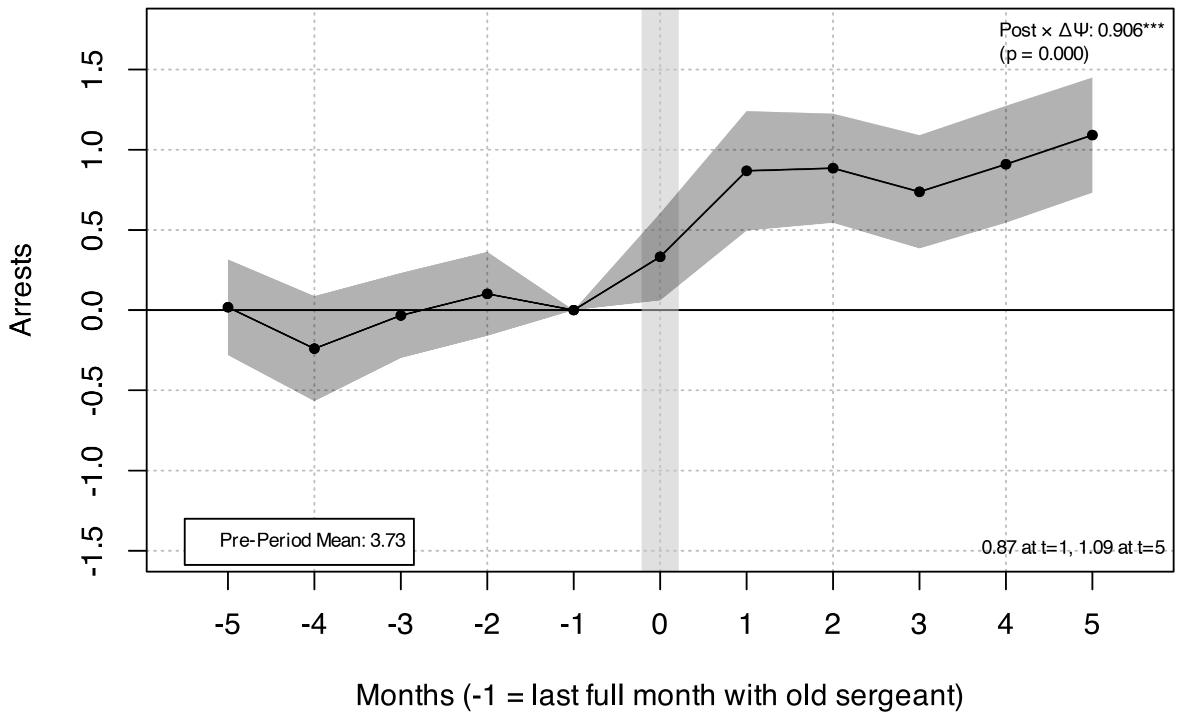


Figure F.14: Sergeant Effects on Secondary Officer Activities: All Move Panel

Notes: This figure reports results from the officer-level event study around sergeant switches for use of force incidents and complaints, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects. Regressions are estimated on an event study sample that includes all switching events in the main analysis sample, dropping any periods contaminated by another sergeant switching event.

F.2 Balanced Panel

Figure F.15: Event Study Coefficients: Balanced Panel



Notes: Each point estimate (with 95% confidence interval) represents $\hat{\pi}_1^k$ from equation 4 — the effect on officer arrests at period k relative to the switch of moving to a sergeant who induces one more arrest per month. Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. The pre-trend F-statistic (top left) tests joint equality to zero for periods -5 through -2. The top right reports the $Post \times \Delta\psi$ coefficient and p-value from the DD specification in equation 5, summarizing the average post-switch effect across periods 1 through 5. The bottom right lists point estimates at selected months. The model is estimated on a balanced panel of officer switching events, with standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects.

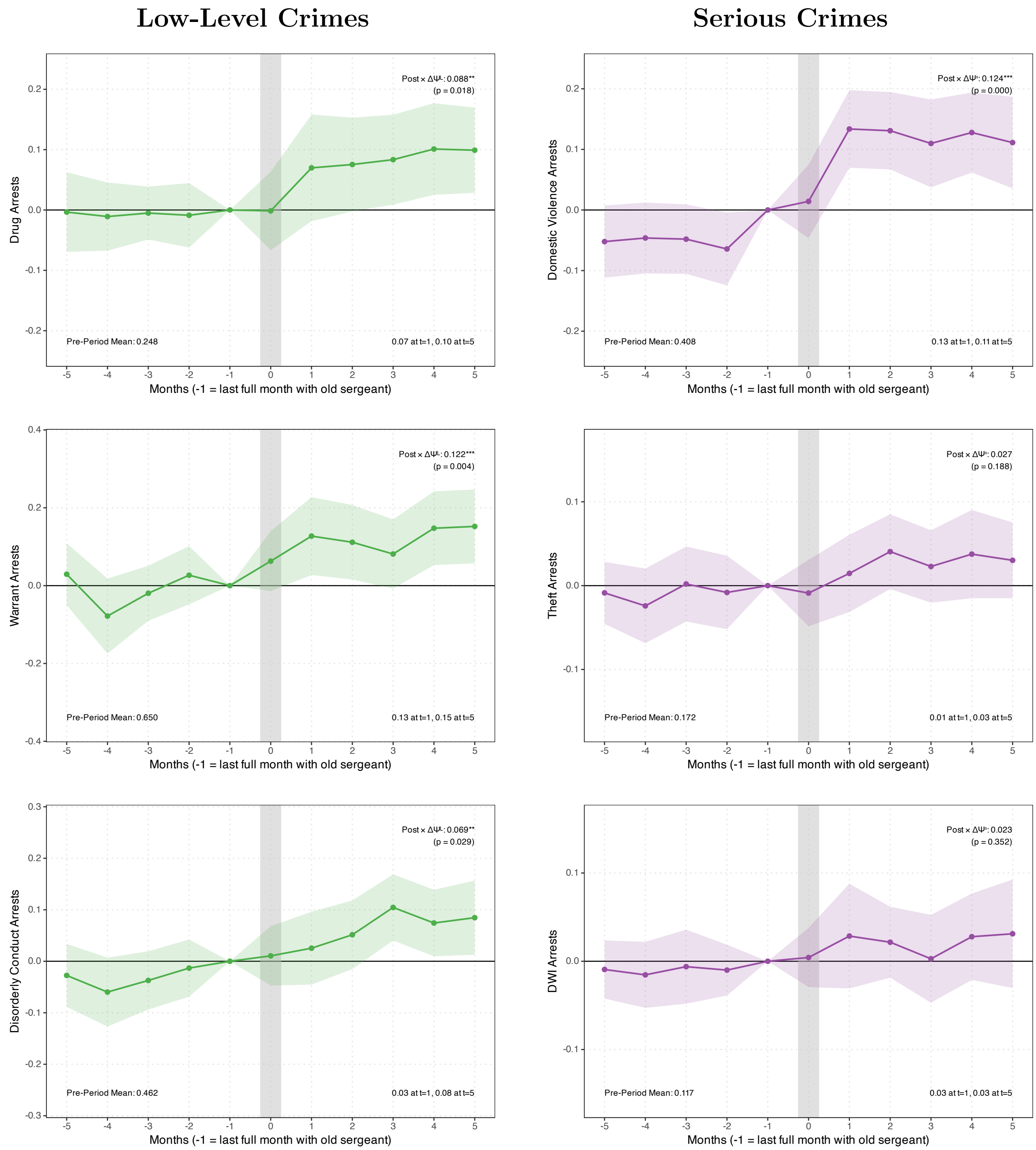


Figure F.16: Sergeant Effects by Arrest Sub-Categories

Notes: This figure reports results from officer-level event studies around sergeant switches for the three most common sub-categories of serious and low-level arrests, estimated using equation 6. Each figure is a separate regression. The left column plots point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level sergeant effects in regressions with low-level arrest sub-types as outcomes; the right column plots the analogous interaction for serious sergeant effects in regressions using serious arrest sub-types as outcomes. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors are clustered at the officer level. All regressions include controls for officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects. Regressions are estimated on a balanced panel of officer switching events.

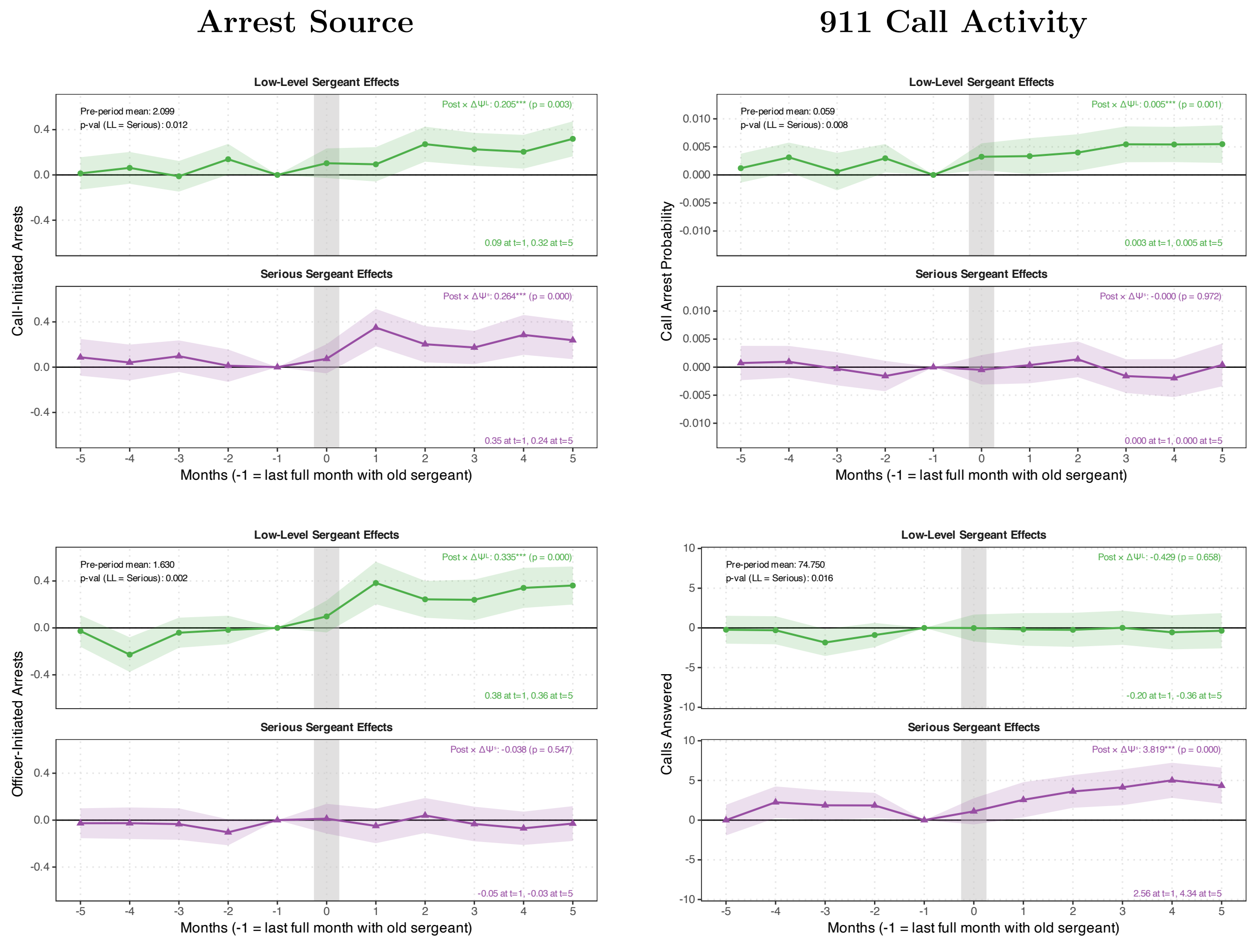


Figure F.17: Sergeant Effects on Police Interactions: Balanced Panel

Notes: This figure reports results from officer-level event studies around sergeant switches for arrests by source (left column) and 911 call activity (right column), estimated using equation 6. Call arrest probability is the share of calls answered within a month that result in arrest. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects. Regressions are estimated on a balanced panel of switching events.

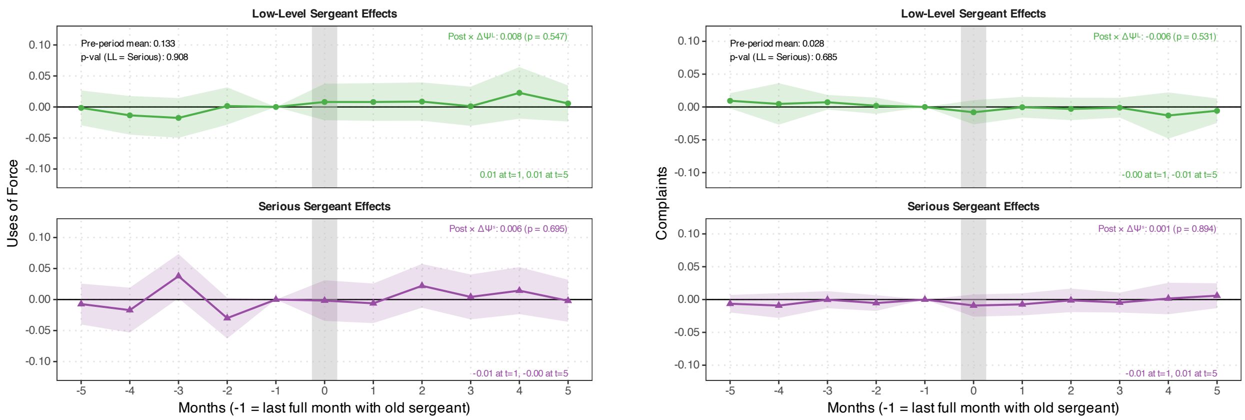
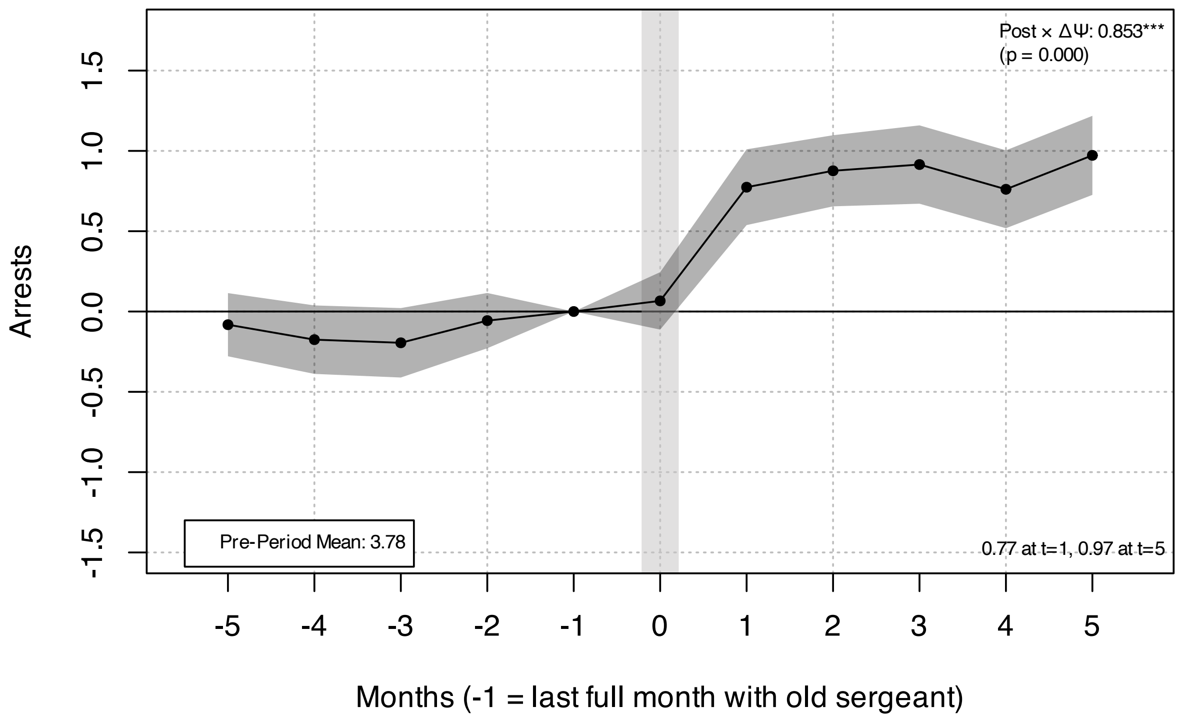


Figure F.18: Sergeant Effects on Secondary Officer Activities: Balanced Panel

Notes: This figure reports results from the officer-level event study around sergeant switches for use of force incidents and complaints, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; regressions are estimated on a balanced panel of switching events.

F.3 Including Year-Month Fixed Effects

Figure F.19: Event Study Coefficients: Including Year-Month FE's



Notes: Each point estimate (with 95% confidence interval) represents $\hat{\pi}_1^k$ from equation 4 — the effect on officer arrests at period k relative to the switch of moving to a sergeant who induces one more arrest per month. Month 0 is the switch month; month -1, the last full month with the previous sergeant, is the reference period. The pre-trend F-statistic (top left) tests joint equality to zero for periods -5 through -2. The top right reports the $Post \times \Delta\psi$ coefficient and p-value from the DD specification in equation 5, summarizing the average post-switch effect across periods 1 through 5. The bottom right lists point estimates at selected months. The model is estimated on the event study sample described in Table B.1, with standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, year-month, and day-off group fixed effects.

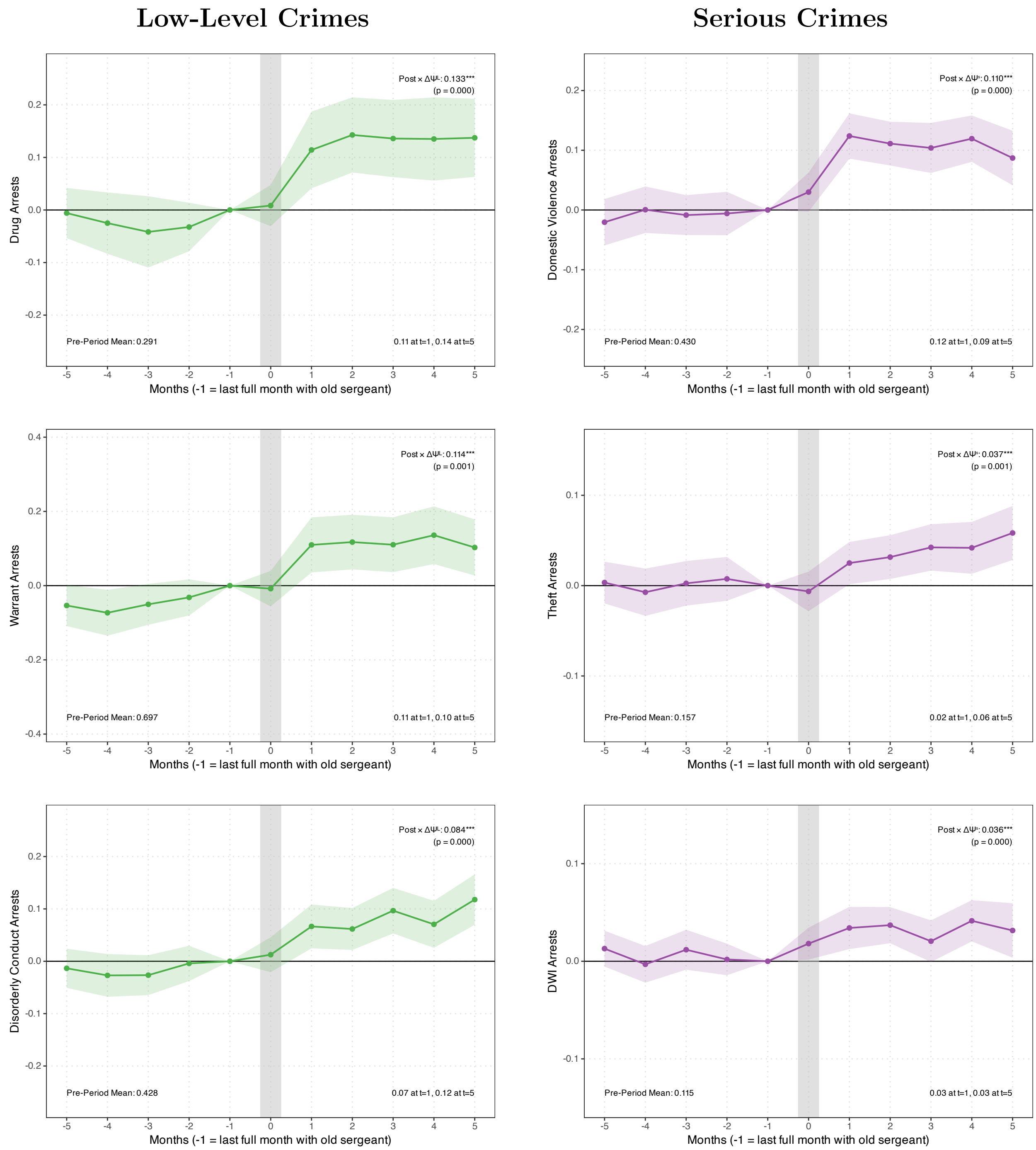


Figure F.20: Sergeant Effects by Arrest Sub-Categories: Including Year-Month FE's

Notes: This figure reports results from officer-level event studies around sergeant switches for the three most common sub-categories of serious and low-level arrests, estimated using equation 6. Each figure is a separate regression. The left column plots point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level sergeant effects in regressions with low-level arrest sub-types as outcomes; the right column plots the analogous interaction for serious sergeant effects in regressions using serious arrest sub-types as outcomes. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. The model is estimated on the event study sample described in Table B.1, with standard errors clustered at the officer level. All regressions include controls for officer tenure, non-interacted period indicators, and sector-watch, officer-event, year-month, and day-off group fixed effects.

Figure F.21: Sergeant Effects on Police Interactions: Including Year-Month FE's

Notes: This figure reports results from officer-level event studies around sergeant switches for arrests by source (left column) and 911 call activity (right column), estimated using equation 6. Call arrest probability is the share of calls answered within a month that result in arrest. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. The model is estimated on the event study sample described in Table B.1, with standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, year-month, and day-off group fixed effects.

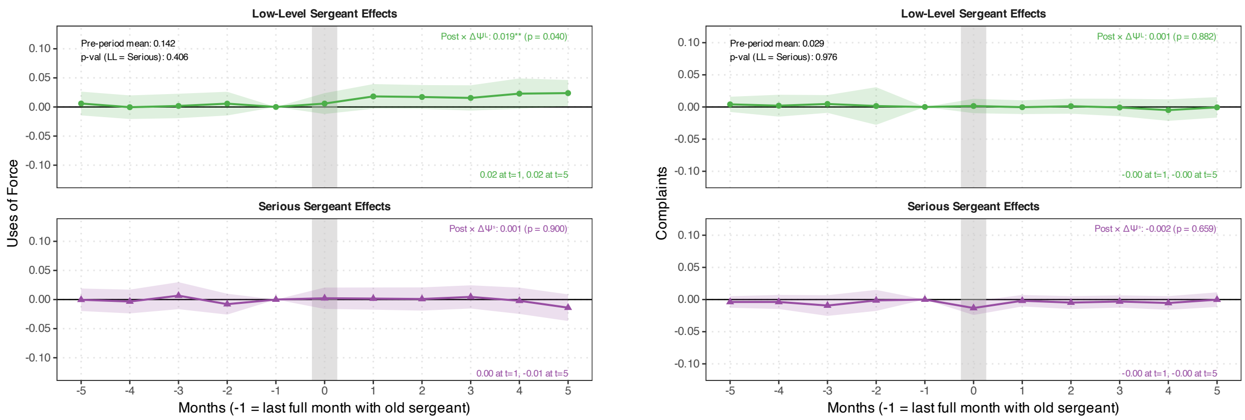


Figure F.22: Sergeant Effects on Secondary Officer Activities: Including Year-Month FE's

Notes: This figure reports results from the officer-level event study around sergeant switches for use of force incidents and complaints, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, year-month, and day-off group fixed effects; regressions are estimated on the event study sample described in Table B.1.

G Predicting Sergeant Effects

To what extent are sergeant effects mediated by observable characteristics that are determined *before* someone has been promoted to sergeant? I examine variations in sergeant effect distributions across four observable pre-promotion characteristics: race, gender, age at the time of their promotion exam, and the score achieved on the promotion exam.[3] Since I only observe exam scores beginning with the 2012 round of tests, I limit my sample to the 202 sergeants who were promoted from these exams. I call sergeants "older" if they were above the average across all exams and I call them "above-average scorers" if they were above the average score for their particular exam.

In Figure G.1a, I present densities separately by exam score. I observe a notable difference between high and low scorers in the distributions of both serious and low-level effects. For low-level effects, the distribution of above-average scorers is shifted to the left of that for below-average scorers, with a Kolmogorov-Smirnov test indicating that this difference is statistically significant (p-value = 0.034). Thus, individuals who score below average on the promotional exams tend to induce more low-level arrests than those who score above average. These differences are meaningful considering exam score is the primary determinant of promotion. The exams test for knowledge of department procedures and aptitude within relevant supervisory situations. Thus marginal promotees — who are barely promoted by virtue of their low exam score — are more likely to value low-level arrests.

In Figure G.1b, I also find evidence of a statistically significant difference in the distribution of serious effects between high and low exam scorers (Kolmogorv-Smirnov p-value = 0.013). In contrast to low-level effects, the differences in serious effects are not driven by a monotonic shift in one direction. Instead, high scorers exhibit a wider distribution of serious effects than low scorers. The distribution for low scorers is more concentrated

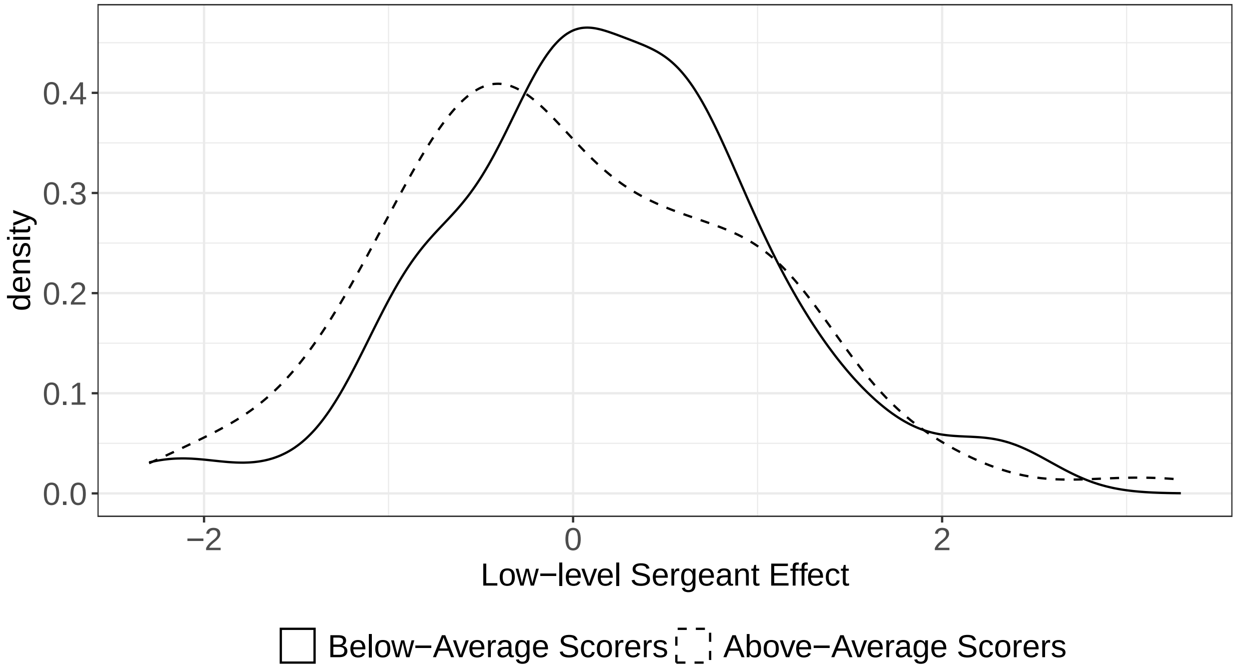
[3] The promotion exam is given once every few years to officers with at least 5 years of experience who want to take part in the promotion process. Exam-takers are ranked according to their performance and, with a few rare exceptions, promoted in order of their exam ranking when openings arise. The exam has both a written and oral component. For this study, I obtained data on the aggregate exam scores for all sergeants promoted during the sample period, accounting for 58% of sergeants in my sample.

around 0. These findings suggest that high scorers are significantly more heterogeneous than low-scorers, at least in terms of their effects on serious enforcement.

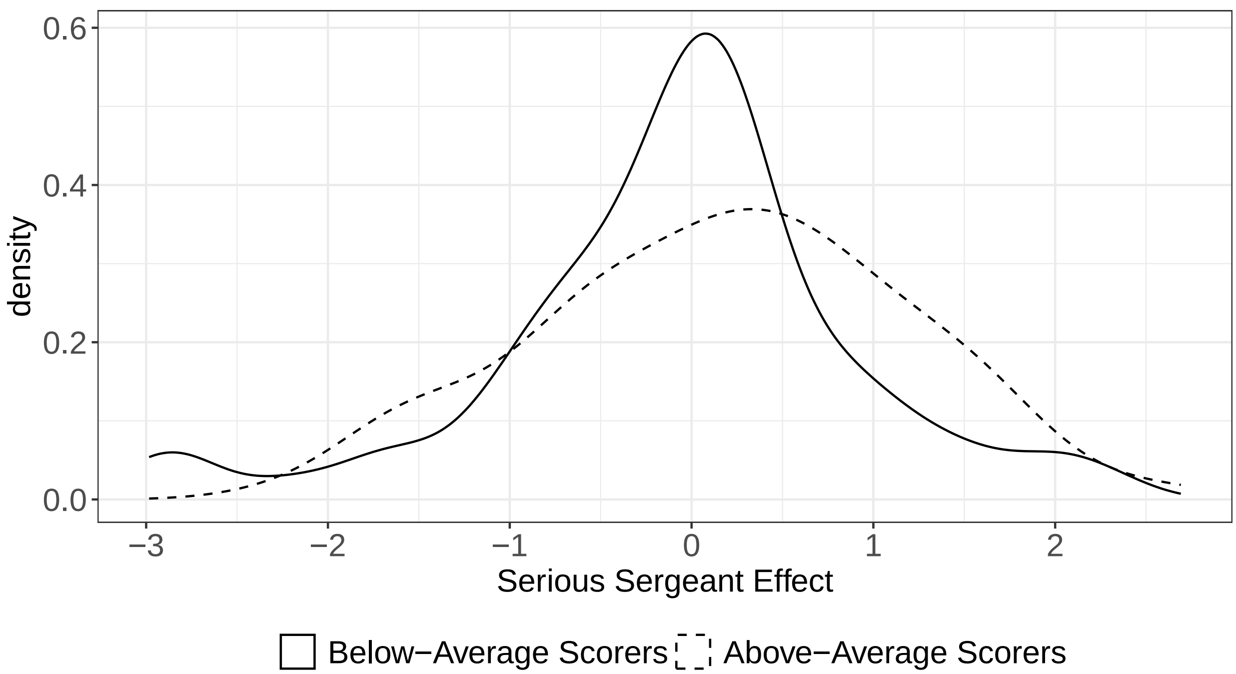
I also analyze the empirical densities of low-level and serious effects separately by race (Figures G.3b, G.3a), gender (Figures G.2b, G.2a), and age groups (Figures G.4b, G.4a). I do not find evidence of significant differences in the distribution of either sergeant effect along each of these three observable dimensions. Kolmogorov-Smirnov tests for the equality of the distributions generate p-values that are well-above standard significance thresholds. However, I find evidence that low-level officer effects differ by race (Figure G.5), consistent with the previous literature (e.g. Ba et al., 2021). Thus, it appears that variation in officer enforcement effects do not translate into similar variation in sergeant effects.

Figure G.1: Sergeant Effects Distributions by Promotional Exam Performance

(a) Low-level effect distribution



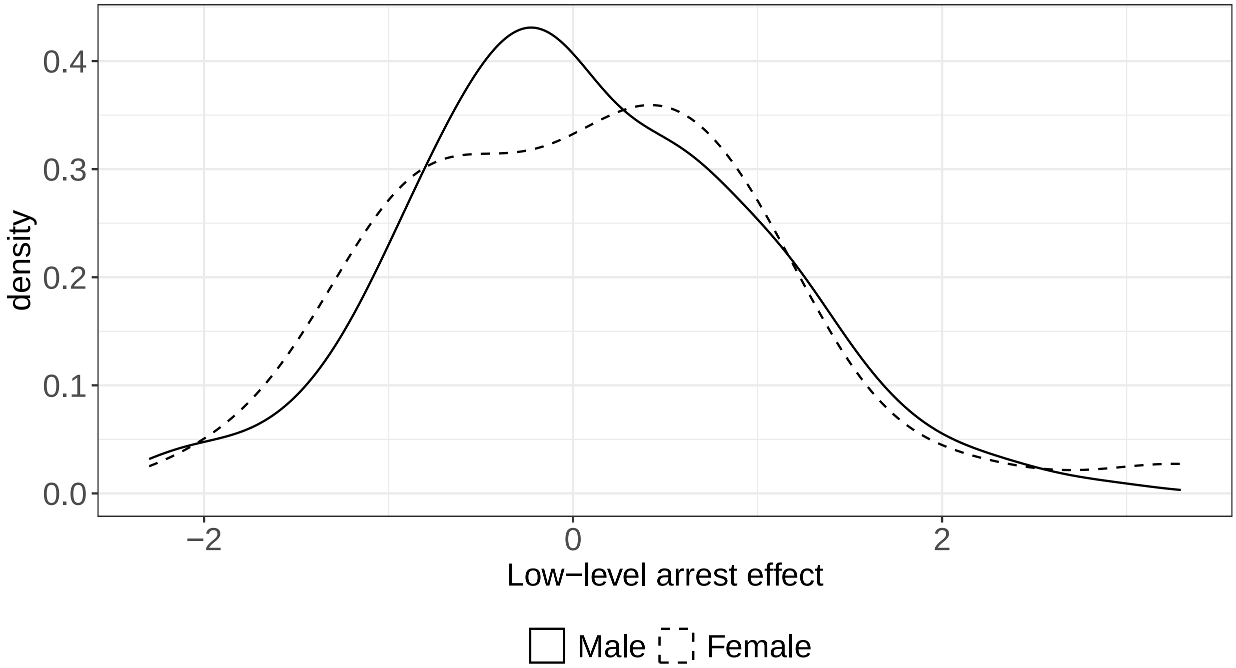
(b) Serious effect distribution



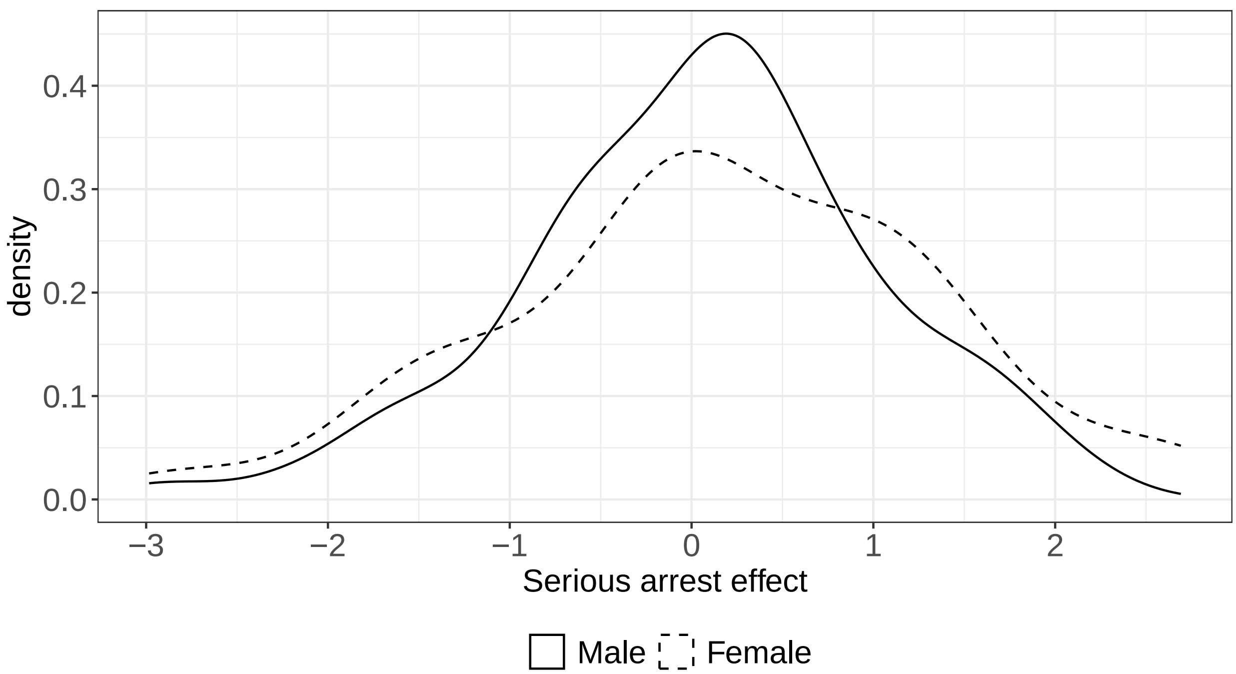
Notes: These figures plot distributions of standardized sergeant effects separately by the sergeant's performance on the promotional exam they took to attain the rank of sergeant. I define above-average scorers as sergeants who score above the average for their exam and below-average scorers as those who score below their exam-specific average. Kolmogorov-Smirnov p-value for low-level effects = 0.0343. Kolmogorov-Smirnov p-value for serious effects = 0.0128.

Figure G.2: Sergeant Effects Distributions by Gender

(a) Low-level effect distribution



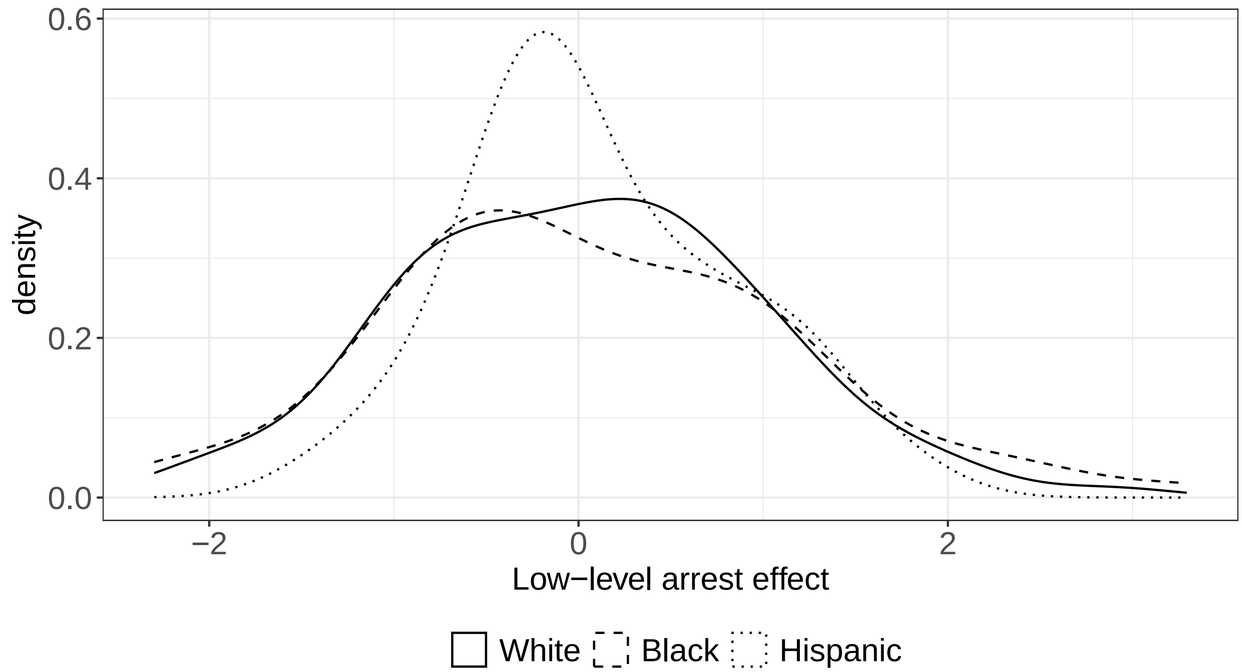
(b) Serious effect distribution



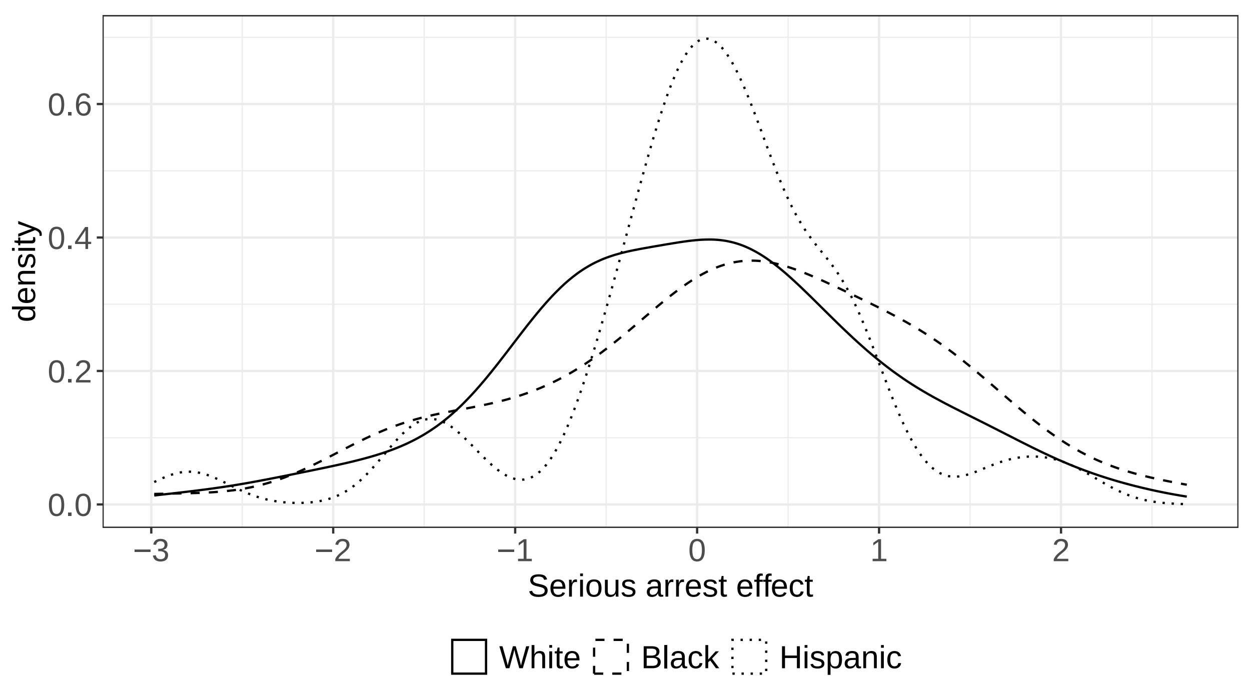
Notes: These figures plot distributions of standardized sergeant effects separately by sergeant gender. Kolmogorov-Smirnov p-value for low-level effects = 0.817. Kolmogorov-Smirnov p-value for serious effects = 0.415.

Figure G.3: Sergeant Effects Distributions by Race

(a) Low-level effect distribution



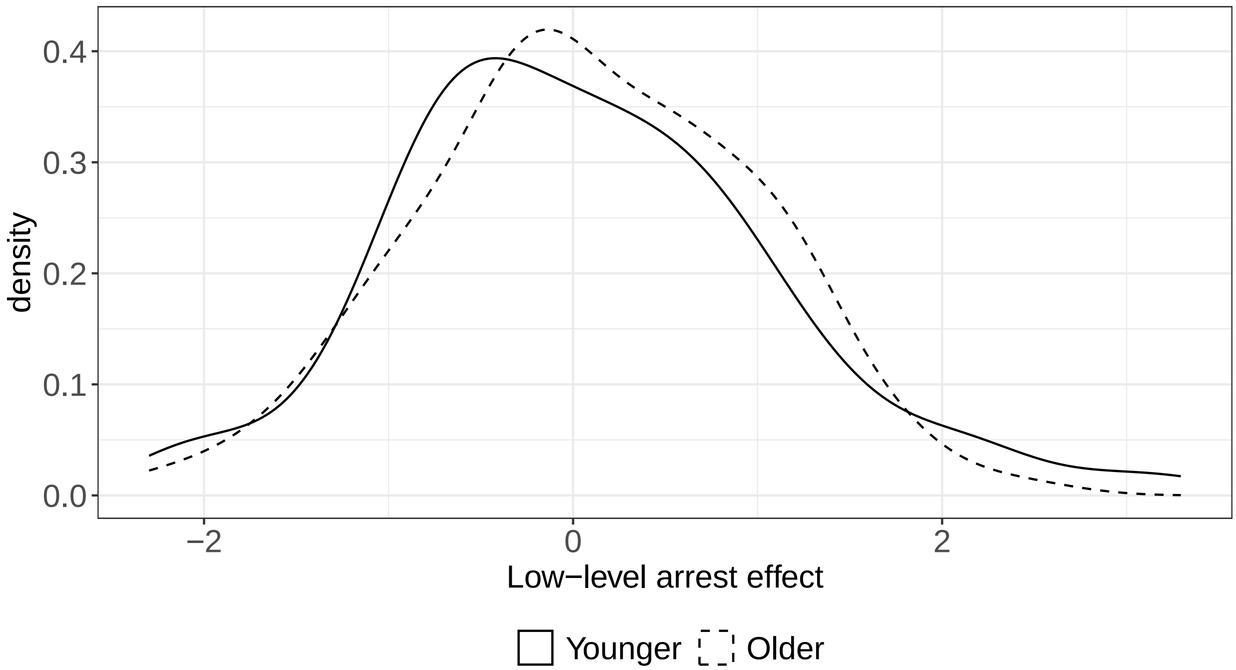
(b) Serious effect distribution



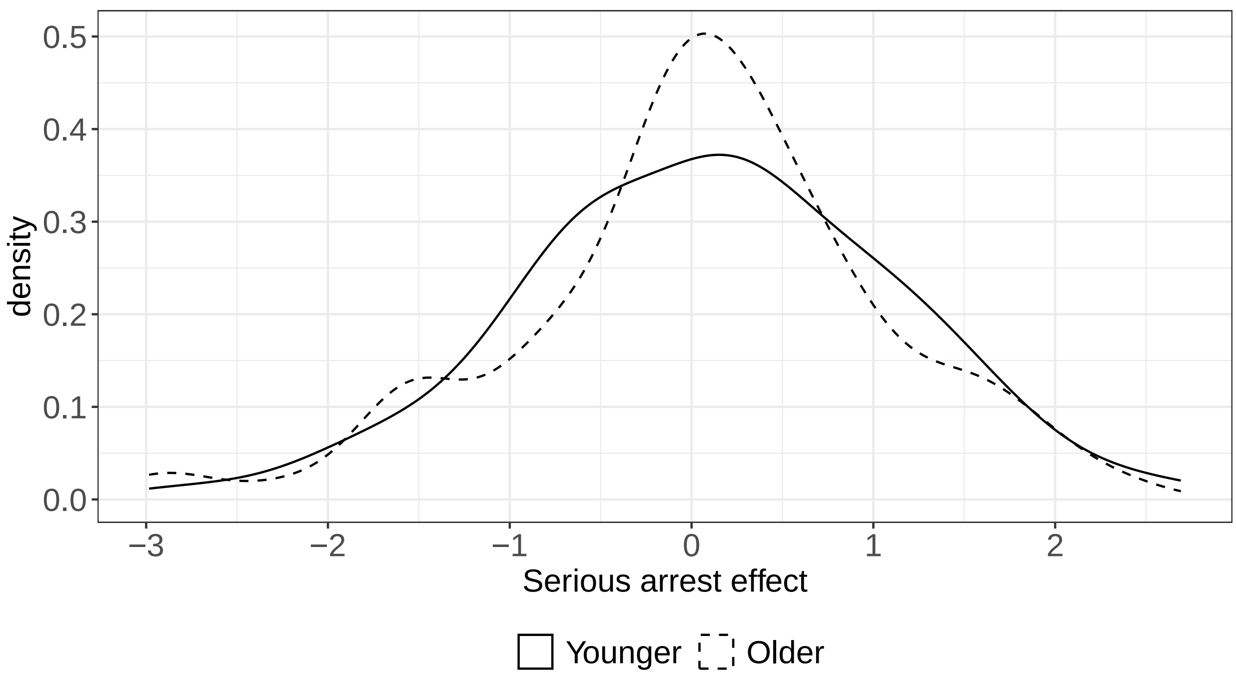
Notes: These figures plot distributions of standardized sergeant effects separately by sergeant race. For low-level effects: Black/Hispanic Kolmogorov-Smirnov p-value = 0.218. Black/White Kolmogorov-Smirnov p-value = 0.241. White/Hispanic Kolmogorov-Smirnov p-value = 0.855. For serious effects: Black/Hispanic Kolmogorov-Smirnov p-value = 0.239. Black/White Kolmogorov-Smirnov p-value = 0.219. White/Hispanic Kolmogorov-Smirnov p-value = 0.130.

Figure G.4: Sergeant Effects Distributions by Age During Promotional Exam

(a) Low-level effect distribution

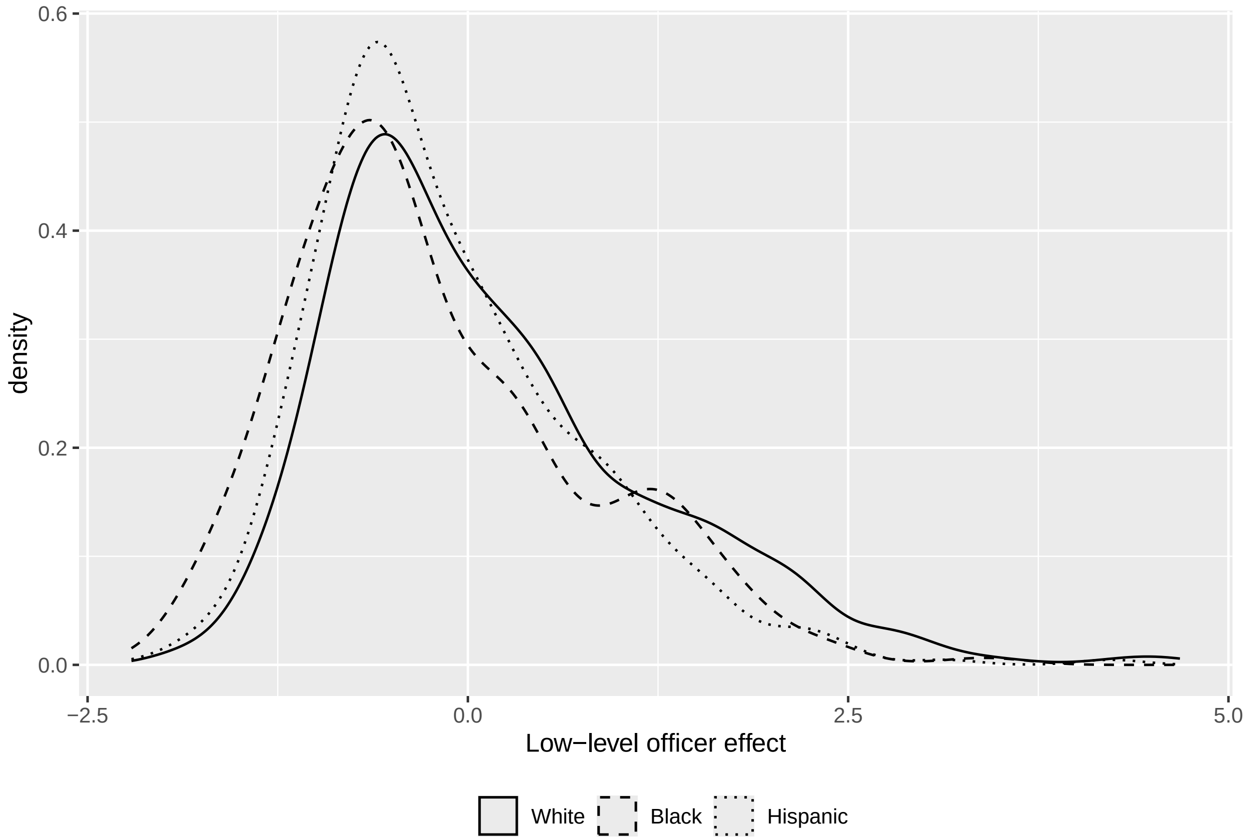


(b) Serious effect distribution



Notes: These figures plot distributions of standardized sergeant effects separately by age at the time of the promotional exam. Older sergeants are those who are above the average age across all exams in my sample, while younger sergeants are below the average. For low-level effects: Kolmogorov-Smirnov p-value = 0.587. For serious effects: Kolmogorov-Smirnov p-value = 0.587.

Figure G.5: Low-level Officer Effects by Race



Notes: This figure plots the distribution of low-level officer effects by officer race. Black/Hispanic Kolmogorov-Smirnov p-value = 0.047. Black/White Kolmogorov-Smirnov p-value = 0.000. White/Hispanic Kolmogorov-Smirnov p-value = 0.003.

H Do Sergeant Enforcement Types Affect Crime?

Given the variation in sergeant enforcement styles documented in the main text, an important question is whether these differences translate into meaningful differences in public safety. Sergeants who generate more arrests may reduce crime through deterrence — by raising the perceived cost of criminal activity (Becker, 1968) — or incapacitation — by removing repeat offenders from circulation (Rose and Shem-Tov, 2021). The crime-reducing potential of marginal arrests through either channel is theoretically ambiguous: potential offenders may not be fully informed of marginal changes in arrest risk, and the extent to which marginal arrestees would otherwise reoffend is unclear. Empirical evidence suggests that police reduce crime primarily through presence and patrol rather than through arrests (Owens, 2013).

To estimate the impact of sergeant enforcement styles on crime, I aggregate the officer-level data to the sergeant patrol unit-by-month level, averaging serious and low-level officer effects within each unit-month. I assign each unit-month the crime outcomes for its associated sector-watch-month, weighting regressions by the inverse number of units within the sector-watch-month to account for the fact that some sector-watches contain two units. I measure crime primarily using 911 calls, preferring them over crime reports because proactive police activity directly generates crime reports. I report results using total 911 calls and the most severe 911 calls — those likely to involve violence or costly property crimes. Violent calls include shootings, assaults, major disturbances, and robberies. Property crime calls include burglary, larceny, and auto theft. As a robustness check, I also report results using index crime reports, which have relatively stable reporting rates and are less susceptible to officer-generated variation.[4]

I estimate event studies around sergeant switches between units, defining switching events analogously to the officer-level analysis: two consecutive sergeant-unit spells involving the same unit but different sergeants, restricting to a 5-month window with no other switches in

[4]Results are similar when using logged 911 calls as the outcome (Figure H.5).

the pre- or post-periods. This yields 325 switching events from 623 total sergeant switches.

I estimate a model analogous to equation 6, scaling event-time indicators by unit-level differences in standardized serious and low-level sergeant effects, and including event fixed effects, sector-watch fixed effects, and controls for the average serious and low-level arrest effects of officers in the unit to capture changes in officer composition over time. Standard errors are clustered at the sector-watch level. Unbiasedness relies on the as-good-as-random mobility of sergeants between unit assignments, generated by the same vacancy and schedule realignment mechanisms that drive officer-level switches. This design captures the crime impacts of variation in sergeant arrest effects rather than the direct effect of marginal sergeant-induced arrests on crime. These two effects may differ because other sergeant behaviors correlated with arrest effects — such as patrol location decisions — may independently affect crime.[5] For each event study, I also present DD results analogous to those used in Section 6 and described in Section 4.3.

Before drawing conclusions about crime, I verify that sergeant switches between units change the amount of enforcement taking place within a sector-watch. Figure H.2 shows substantial within-unit variation in serious and low-level sergeant effects across switches: 30% of sergeant switches between units generate at least a 1SD change in serious or low-level sergeant effects. Figures H.3 and H.4 confirm that officers within the focal unit make more arrests within their assigned sector-watch following a positive change in the unit's sergeant effect. DD estimates rule out increases of less than 5% in in-sector low-level arrests and less than 12.5% in in-sector serious arrests for a one standard deviation increase in the corresponding sergeant effect.

These results come with an important caveat. Figures H.3 and H.4 show that sergeants have a larger impact on arrests made *outside* their unit's assigned sector-watch than within it. Officer activity need not be confined to their assigned area: officers may be pulled outside

[5] An instrumental variables design using sergeant enforcement propensities to instrument for unit arrests would in principle isolate the crime effect of marginal arrests, but sergeant enforcement propensities are likely correlated with other sergeant behaviors that affect crime, violating the exclusion restriction.

their assigned sectors to respond to 911 calls, and officers outside a sergeant's unit may make arrests within that unit's sector-watch. Sergeant-induced changes in arrests therefore represent a small share of overall enforcement activity in any given location. The upper bound of the 95% confidence interval for the DD estimate of low-level sergeant effects on in-sector low-level arrests (Figure H.3) is only 2.3% of average monthly low-level arrests within a sector-watch. I may therefore be underpowered to detect crime reductions proportional to this share of enforcement activity.

Figure H.1 plots event study coefficients for total, violent, and property crime 911 calls. Pre-trends are flat across all outcomes, suggesting that sergeant-driven switches are not driven by crime trends. The core finding is that neither sergeant type uniquely reduces crime: joint F-tests do not reject equality of post-switch coefficients between serious and low-level sergeant effects for any call type. Event study coefficients and DD estimates are statistically insignificant across all call types. Where confidence intervals are informative, they rule out small reductions: for low-level sergeant effects, I can rule out mean reductions greater than 1.9% for total calls, 1.8% for violent calls, and 1.5% for property crime calls. For serious sergeant effects, I can rule out reductions greater than 1.6% for total calls and 1% for violent calls. There is suggestive evidence of property crime reductions for serious sergeant effects — I cannot rule out reductions of up to 4.4% of the mean — but I cannot reject that low-level sergeant effects produce similar reductions.

Results using index crime reports (Figure H.6) are consistent with this finding. For total and property index crime reports, I cannot reject equality of post-switch trends between serious and low-level effects. For low-level sergeant effects, DD estimates are close to zero, ruling out property crime reductions larger than 2.7% and total index crime reductions larger than 1.8%. There is suggestive evidence of property crime reductions for serious sergeant effects — the DD estimate is statistically significant at the 5% level ($p = 0.031$), with a 95% CI ruling out reductions larger than 7% of the mean — but I cannot reject that low-level effects are similar. The one exception is violent crime reports, where I reject equality of post-switch trends between the two effect types at the 1% level. For serious sergeant effects, violent crime DD estimates are close to zero. For low-level sergeant effects, the DD estimate cannot rule out increases up to 9% and rejects reductions greater than 1%. Given the noisiness of these estimates and the relative rarity of violent crime reports, I interpret this result with caution.

Overall, these results provide little evidence that heterogeneity in sergeants' enforcement priorities translates into meaningful differences in public safety. Crime effects are likely difficult to detect in my setting, given that sergeant-induced arrests represent a small share of overall enforcement activity within a sector-watch. The finding of limited crime reductions is nonetheless consistent with broader evidence on larger shocks to enforcement activity. Cho et al. (2023) find that substantial reductions in arrests following an officer death — a shock far larger than the sergeant-induced variation studied here — do not meaningfully increase index crimes, suggesting that marginal arrests are not substantially deterring or incapacitating those who will commit the most costly crimes. Given that low-level arrests impose significant costs on arrestees and their communities (Agan et al., 2023) while being made for crimes that are less likely to have direct victims, sergeants who prioritize low-level enforcement without generating commensurate public safety benefits may reduce net social welfare.

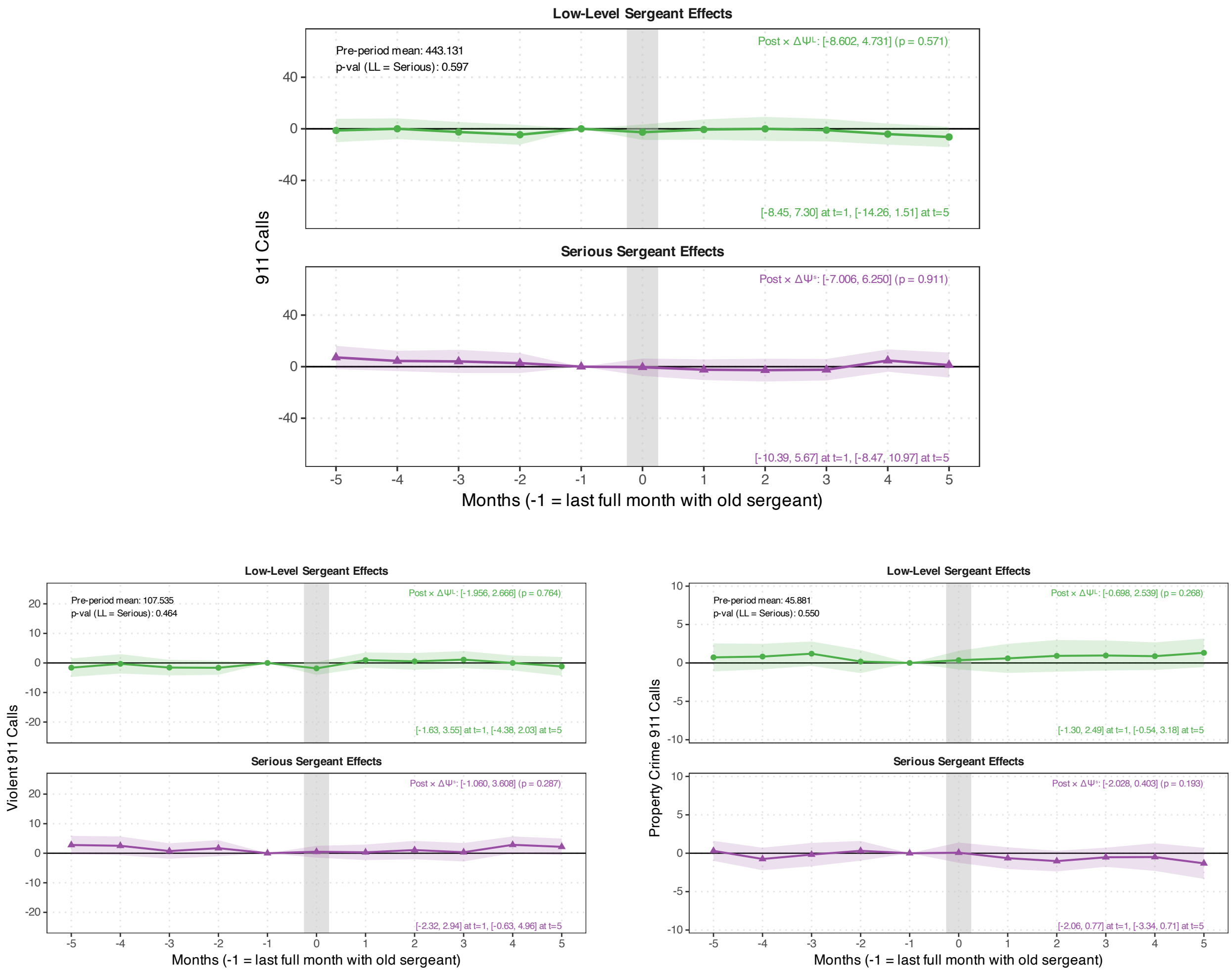


Figure H.1: Sergeant Effects on Crime

Notes: This figure reports unit-month event studies around sergeant switches described in Section H, estimated analogously to equation 6. Outcomes are assigned based on the unit's sector-watch. Violent 911 calls include shootings, assaults, major disturbances, and robberies; property crime calls include burglary, larceny, and auto theft. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. In each panel: the top left reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly; the top right reports the 95% confidence intervals of the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right reports 95% confidence intervals at selected months. Controls include average Bayes-shrunken serious and low-level officer effects, non-interacted period indicators, and sector-watch and event fixed effects. Standard errors clustered at the sector-watch level; regressions are weighted by the inverse number of units in the sector-watch.

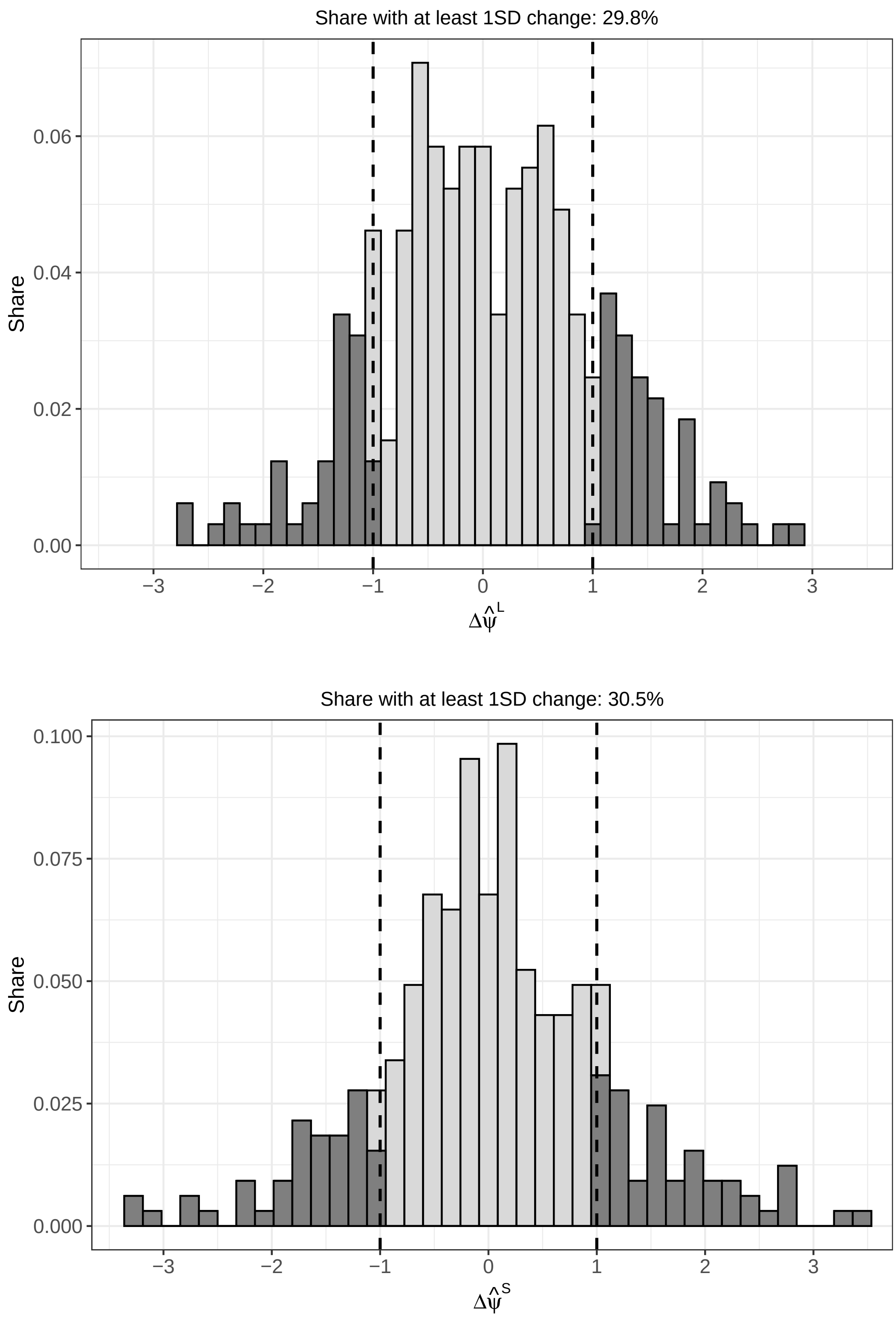


Figure H.2: Distributions of Unit Switches

Notes: The figures depict the distribution of the sergeant switching events from the unit-month event study panel described in Section H. $\Delta\hat{\psi}^L$ (top) is the difference in standardized low-level sergeant effects between a unit's new and old sergeant. $\Delta\hat{\psi}^S$ (bottom) difference in standardized serious sergeant effects between a unit's new and old sergeant.

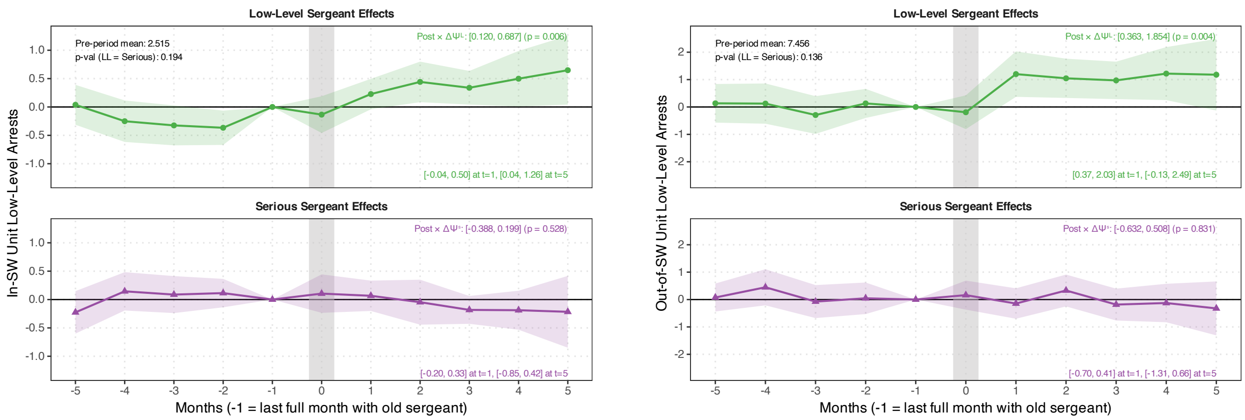


Figure H.3: Effect of Sergeant Unit Rotations on Low-Level Arrests by In-Unit Officers

Notes: This figure reports unit-month event studies around sergeant switches described in Section H, estimated analogously to equation 6. The left figure uses in-sector-watch low-level arrests — those made by the unit's assigned officers within their assigned sector-watch — as the outcome; the right figure uses out-of-sector-watch low-level arrests — those made outside the assigned sector-watch. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. In each panel: the top left reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly; the top right reports the 95% confidence intervals of the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right reports 95% confidence intervals at selected months. Controls include average Bayes-shrunken serious and low-level officer effects, non-interacted period indicators, and sector-watch and event fixed effects. Standard errors clustered at the sector-watch level; regressions are weighted by the inverse number of units in the sector-watch.

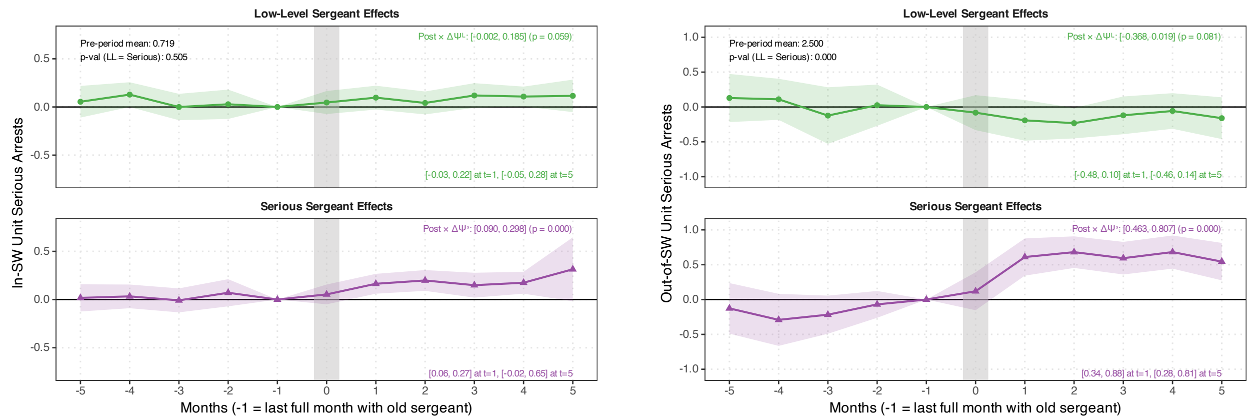


Figure H.4: Effect of Sergeant Unit Rotations on Serious Arrests by In-Unit Officers

Notes: This figure reports unit-month event studies around sergeant switches described in Section H, estimated analogously to equation 6. The left figure uses in-sector-watch serious arrests — those made by the unit's assigned officers within their assigned sector-watch — as the outcome; the right figure uses out-of-sector-watch serious arrests — those made outside the assigned sector-watch. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. In each panel: the top left reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly; the top right reports the 95% confidence intervals of the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right reports 95% confidence intervals at selected months. Controls include average Bayes-shrunken serious and low-level officer effects, non-interacted period indicators, and sector-watch and event fixed effects. Standard errors clustered at the sector-watch level; regressions are weighted by the inverse number of units in the sector-watch.

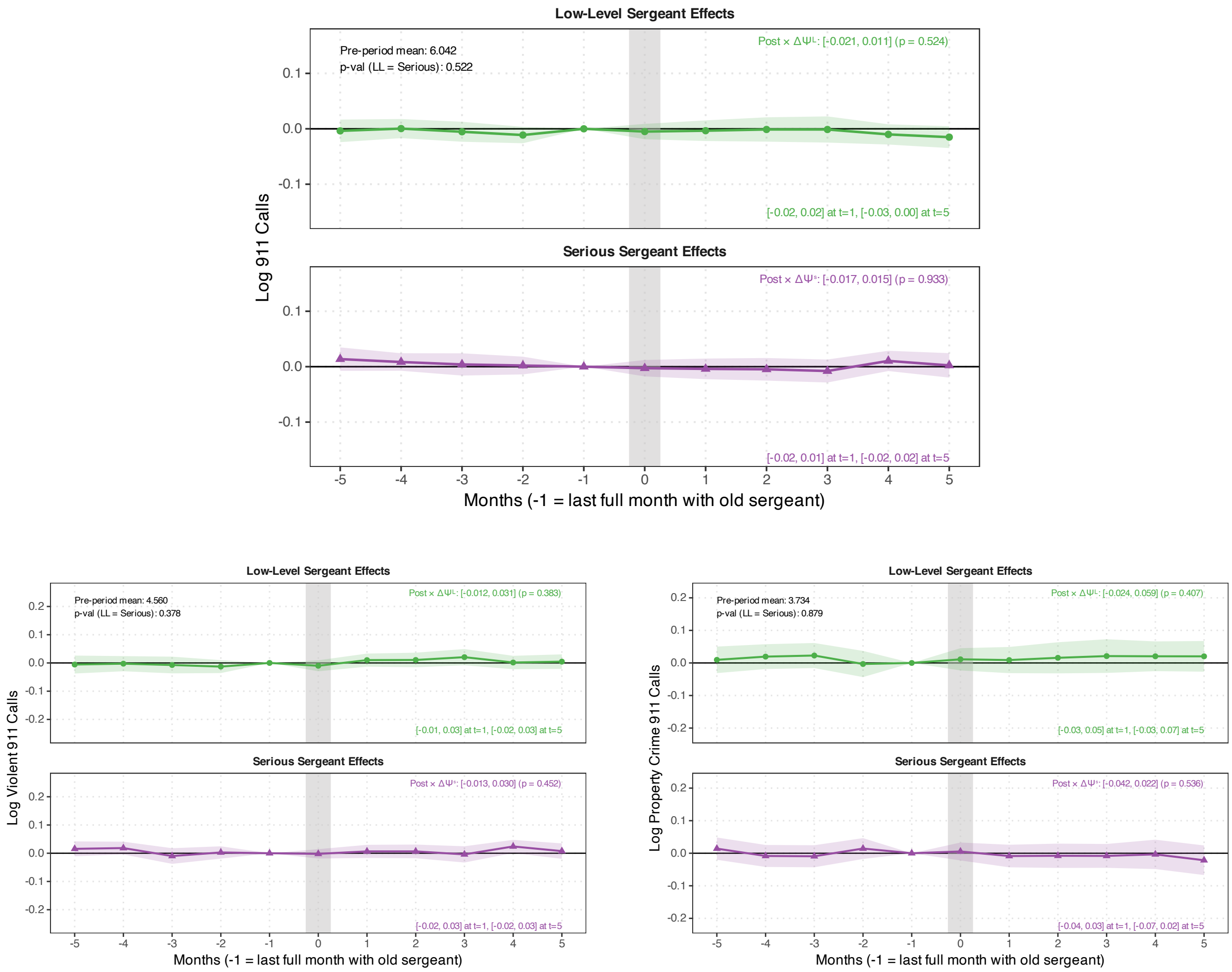


Figure H.5: Sergeant Effects on Log 911 Calls

Notes: This figure reports unit-month event studies around sergeant switches described in Section H, estimated analogously to equation 6. Outcomes are assigned based on the unit's sector-watch. Violent 911 calls include shootings, assaults, major disturbances, and robberies; property crime calls include burglary, larceny, and auto theft. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. In each panel: the top left reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly; the top right reports the 95% confidence intervals of the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right reports 95% confidence intervals at selected months. Controls include average Bayes-shrunken serious and low-level officer effects, non-interacted period indicators, and sector-watch and event fixed effects. Standard errors clustered at the sector-watch level; regressions are weighted by the inverse number of units in the sector-watch.

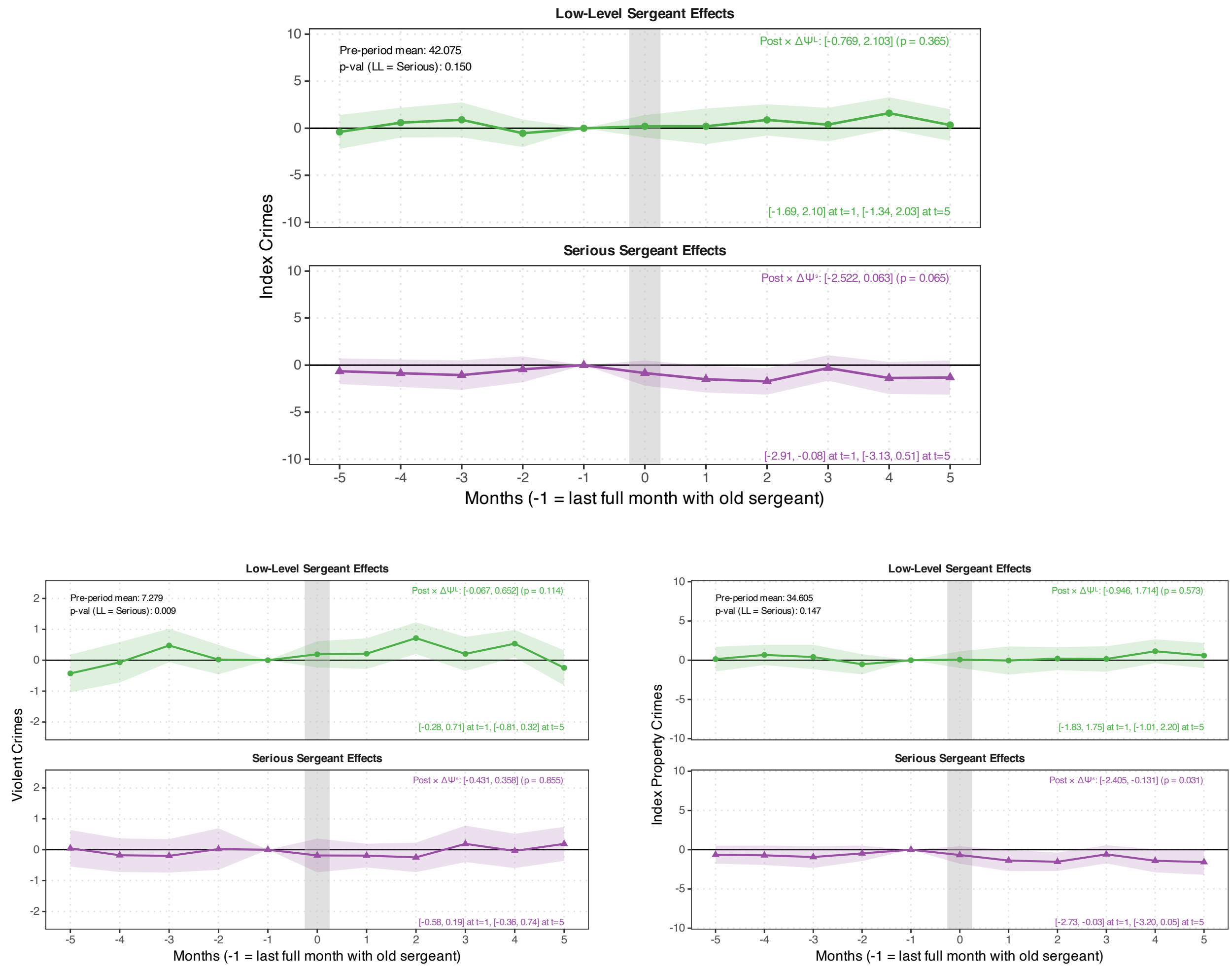


Figure H.6: Sergeant Effects on NIBRS Crime Reports

Notes: This figure reports unit-month event studies around sergeant switches described in Section H, estimated analogously to equation 6. Outcomes are assigned based on the unit's sector-watch. Violent crimes include murder, robbery, and aggravated assault. Index property crimes include burglary, theft, auto theft, and arson. Index crimes are the sum of index property and violent crimes. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. In each panel: the top left reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly; the top right reports the 95% confidence intervals of the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right reports 95% confidence intervals at selected months. Controls include average Bayes-shrunken serious and low-level officer effects, non-interacted period indicators, and sector-watch and event fixed effects. Standard errors clustered at the sector-watch level; regressions are weighted by the inverse number of units in the sector-watch.

I How do sergeants change behavior?

While the evidence in Section 6 establishes that sergeants alter distinct officer behaviors, it is not clear how they manage to do so. Like other public sector supervisors, sergeants lack access to many of the personnel tools available in the private sector. They cannot offer performance pay, terminate employees, or meaningfully influence promotions. They also face limited capacity to directly supervise their employees' actions.[6] In this section, I evaluate two measurable sergeant behaviors that may shape officer conduct: leading by example and direct monitoring. As explained in Section 2, officers may internalize their sergeants' enforcement activities as performance expectations, which can influence transfers into specialized units, commendations, or day-to-day job satisfaction due to strong cultural norms around following orders. I use two proxies for leading by example: a sergeant's own arrest activity and their frequency as a first responder to 911 calls, considered separately for serious and low-level arrests. I proxy for direct monitoring using CAD call data to measure sergeant presence at their subordinates' calls.

To estimate the relevance of each mechanism, I use the unit-month panel and event study specifications from Section H, replacing crime outcomes with the arrest and call behavior of the unit's assigned sergeant in each event period. In the pre-period, these outcomes reflect the behaviors of the outgoing sergeant; in the post-period, they reflect those of the incoming sergeant. This design exploits within-unit variation in sergeants to estimate differences in arrest and call activity between sergeants with different serious and low-level effects, controlling for the average serious and low-level effects of the unit's officers, as well as event and sector-by-watch fixed effects.

Figure I.1 reports the results. For low-level sergeant effects, the event study coefficients following a switch are imprecisely estimated but suggest a level shift in sergeant arrests,

[6] For example, Wilson (1991) highlights compliance officers in the U.S. Department of Labor's Wage and Hours Division, attorneys in the Antitrust Division of the U.S. Department of Justice, soldiers during wartime, forest rangers, teachers, and (fittingly) police officers as examples of public employees whose managers cannot closely supervise their daily activities.

driven by low-level arrests. The DD estimates are statistically significant: switching to a sergeant with larger low-level effects is associated with 0.096 additional sergeant arrests per month (28% of the pre-period mean), almost entirely for low-level offenses (0.076 additional arrests, 30% of the mean).[7] There is also suggestive evidence of more direct monitoring — the DD coefficient for calls answered with subordinates is positive and marginally significant ($p = 0.051$), indicating an increase of 7.8% relative to the mean, though the post-switch event study coefficients are positive but statistically insignificant.

For serious sergeant effects, event study and DD coefficients are generally smaller and statistically insignificant, but not distinguishable from low-level sergeant effects across any outcomes. The one exception is sergeant first-response calls, where there is a short-term post-switch increase and a positive DD coefficient exceeding 10% of the pre-period mean — potentially consistent with call-focused supervisors being more personally engaged in 911 response — though this estimate is not statistically significant at conventional levels.

One additional mechanism that can be indirectly assessed is overtime approval, studied in the context of officer call activity in Section 6. Sergeants can substantially increase officers' compensation by approving extra hours (Chalfin and Gonçalves, 2026), and Supplementary Appendix Figure A.28 shows that serious and low-level sergeant effects are associated with more overtime hours worked. Consistent with this evidence, I find that serious and low-level sergeant effects both increase overtime arrests (Figure I.2). The effect is only statistically significant for serious sergeant effects, but I cannot reject equality of effects across the two enforcement dimensions. These patterns are consistent with sergeants using overtime approval as a tool for shaping officer behavior.[8]

Overall, these results show that sergeants overcome supervisory constraints through both formal and informal mechanisms. The institutional structure of police departments — particularly around patrol roles and overtime pay — provides supervisors with tools to generate

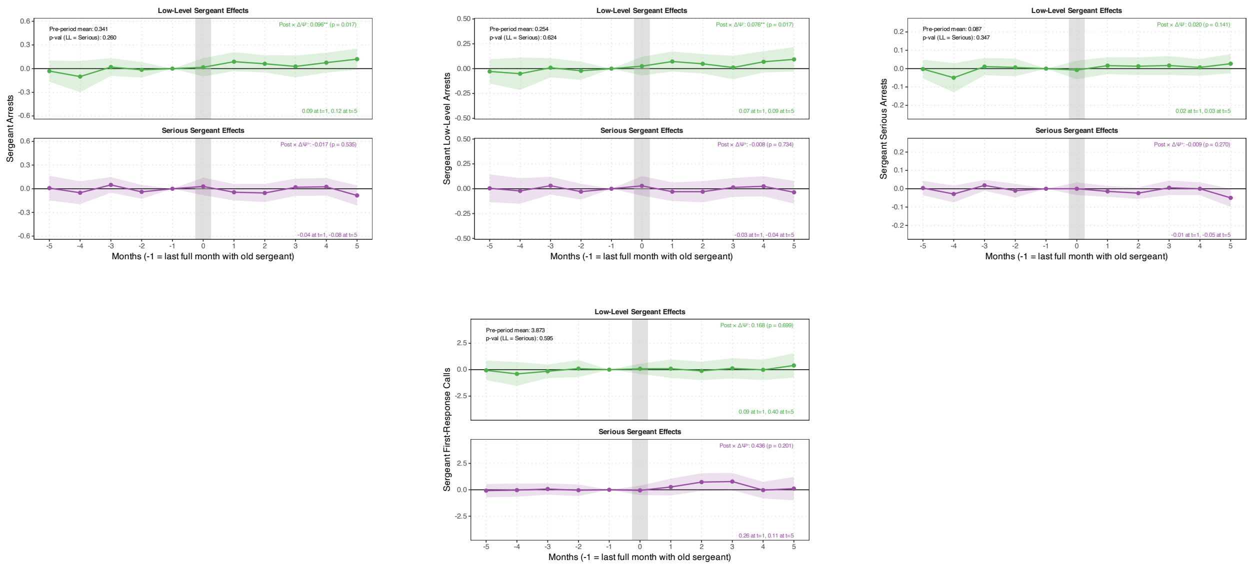
[7] Coefficients for serious arrests are also positive but smaller in both absolute and mean-relative terms, and I cannot reject equality with the low-level arrest coefficients.

[8] An additional channel through which sergeants could affect officer behavior is by influencing days missed — either by approving or denying leave requests. I lack the scheduling data needed to test this directly.

pecuniary and non-pecuniary incentives. Several other plausible mechanisms — such as commendations, transfers, or assignment preferences — remain unobservable in my data.[9] There may be longer-term professional incentives that sergeants use to influence officer behavior but which are unobservable in my data, such as recommendations for promotion or transfers. If sergeants influenced officers primarily through immediate instruction — such as task assignments at roll call — short-term supervisory relationships would generate similar effects, since the sergeant is present and giving instructions regardless of relationship length. Supplementary Appendix Figure C.1e shows that including single-month sergeant-officer spells attenuates sergeant effect estimates by around 25%. Baseline and alternative-sample effects have a correlation of 0.91, and regressing alternative-sample on baseline effects yields a slope of 0.76. The two samples produce similar rankings of sergeants, but including short-term relationships generates smaller sergeant effect estimates. This is consistent with officers anticipating longer-term professional rewards rather than responding only to immediate instruction. Overall, the evidence presented here reaffirms findings from the broader literature on public sector management: supervisors can leverage organizational features to shape employee behavior (Fenizia, 2022).

[9] Gudgeon et al. (2023) find evidence that station lieutenants affect officer arrests through their authority to approve arrests brought into the station. Sergeants in the DPD do not hold arrest approval authority over their officers, suggesting this channel is not operative in my setting.

Leading by Example



Monitoring

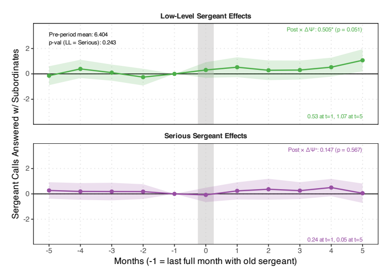
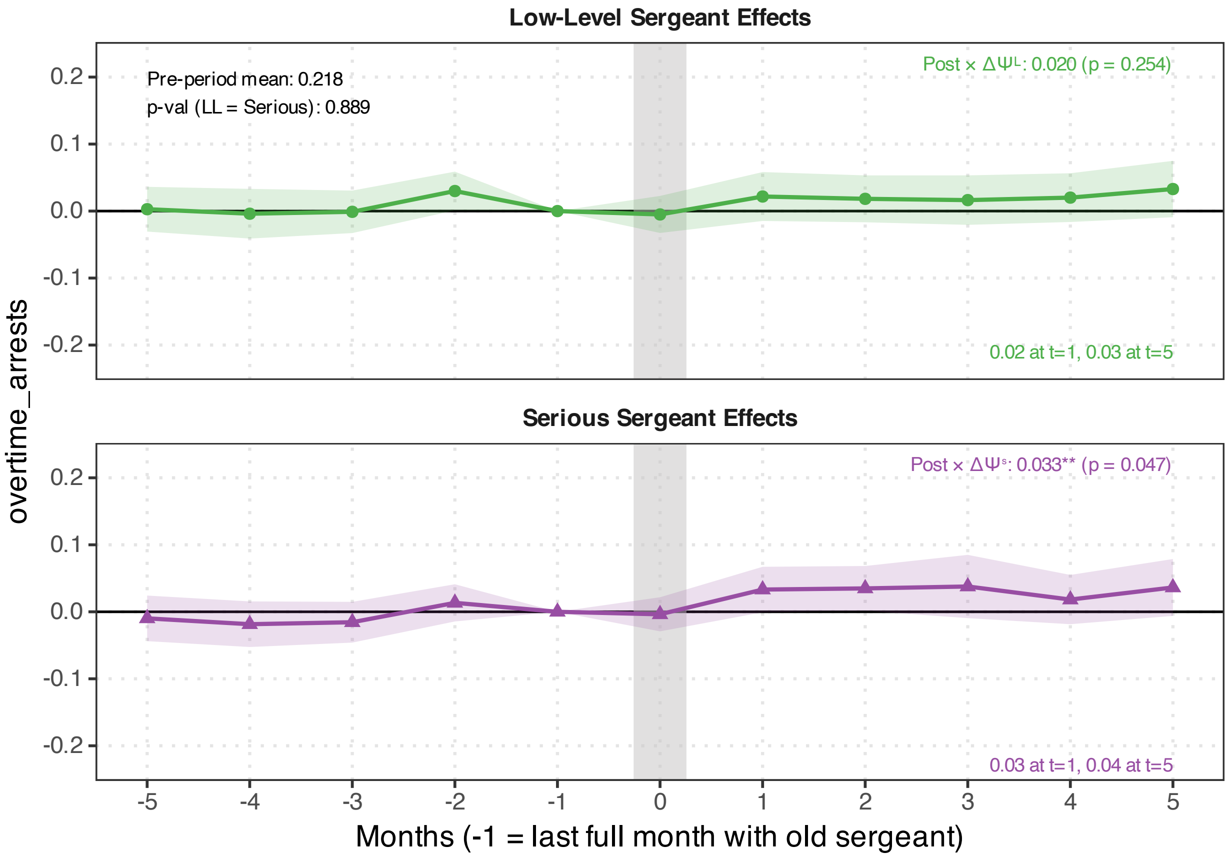


Figure I.1: Sergeant Effect Mechanisms

Notes: This figure reports unit-month event studies around sergeant switches described in Section I, estimated analogously to equation 6. Outcomes represent the behaviors of the unit's sergeant in months relative to the switch: total arrests, serious arrests, low-level arrests, calls first responded to by the sergeant, and calls responded to jointly with subordinates. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. In each panel: the top left reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly; the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right reports point estimates at selected months. Controls include average Bayes-shrunken serious and low-level officer effects, non-interacted period indicators, and sector-watch and event fixed effects. Standard errors are clustered at the sector-watch level; regressions are weighted by the inverse number of units in the sector-watch.

Figure I.2: Sergeant Effects on Overtime Arrests



Notes: This figure reports results from officer-level event studies around sergeant switches for overtime arrests, estimated using equation 6. For each outcome, the top panel (green) and bottom panel (purple) plot point estimates and 95% confidence intervals on the interaction between period indicators and the move-induced change in low-level and serious sergeant effects, respectively. The top left of the top panel reports the p-value from an F-test of equality between serious and low-level interaction coefficients across all post-switch periods jointly. In each panel, the top right reports the associated DD estimate, obtained by replacing the post-switch event-time indicators in equation 6 with a single binary indicator, *Post*, as described in Section 4.3; the bottom right lists point estimates at selected months. Standard errors clustered at the officer level. Controls include a second-degree polynomial of officer tenure, non-interacted period indicators, and sector-watch, officer-event, and day-off group fixed effects; estimated on the event study sample described in Table B.1.

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