# The Relationship between Officer Misconduct and Conviction-less Arrests*

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Given the use of an individual's arrest history for many economic and social outcomes, policymakers have enacted criminal justice reform measures. This paper examines which officers are making conviction-less arrests (arrests that result in no charges or where the defendant is found not guilty), and whether these arrests can be reduced with increased oversight. Using the Chicago Police Department's rotational duty calendar to obtain plausibly exogenous variation in the set of officers assigned to work on a particular day, we find that high-misconduct officers are 12% more likely than no-misconduct officers to make arrests that result in no charges and 31% more likely to make arrests with "not guilty" outcomes, with no difference in arrests yielding "guilty" outcomes. We also analyze two events that increased the transparency of police misconduct through public disclosure of complaint records and find that increased oversight reduces conviction-less arrests, but with nuances across misconduct profiles–low-misconduct officers show stronger responses and high-misconduct officers show weaker responses.

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# 1 Introduction

In the United States, around 35% of all charges result in no convictions (Chien, 2020, Appendix Table A-2). Yet, these individuals still face adverse consequences as arrest records remain accessible and appear in background checks despite the lack of conviction.[1] As a potential solution, policy-makers across states have implemented criminal remediation laws, ranging from criminal-record expungement (Jackson et al., 2017; Selbin et al., 2018; Prescott and Starr, 2019; Agan et al., 2024b) to downgrading felonies or not prosecuting misdemeanors (Agan et al., 2024a, 2023) to the removal of criminal history questions from job applications (Agan and Starr, 2018; Craigie, 2020; Doleac and Hansen, 2020; Rose, 2021). However, evaluations of these policies find mixed employment outcomes, collectively underscoring a scarring effect from having an arrest record (see also McWilliams and Hunter, 2021; Cullen et al., 2023)[2] and pointing to the need for an earlier intervention in the criminal justice process. Building upon this insight, this paper examines a critical upstream factor: the role of the law enforcement officer.

This paper asks which officers are making conviction-less arrests (defined as arrests that result in no charges or where the defendant is found not guilty), and whether these arrests can be reduced with increased oversight. Specifically, we examine whether officers with greater alleged misconduct are more likely to make conviction-less arrests.[3] The focus on officer misconduct stems from prior research showing that high-misconduct officers face more civil rights lawsuits and settlements (Rozema and Schanzenbach, 2019). Similarly, Chalfin and Kaplan (2021) find that just 2% of officers account for 26% of use of force complaints. While these studies establish patterns of concentrated misconduct among certain officers, they do not fully explore the downstream effects on civilians.

[1] For example, until 2016, people with criminal records were ineligible for public housing and housing choice vouchers (Carey, 2005). In 2016, the U.S. Department of Housing and Urban Development (HUD) provided guidance clarifying that arrest records alone cannot be used as evidence of criminal activity that could disqualify a person from housing assistance (U.S. Department of Housing and Urban Development, 2015). However, the discretion to use blanket exclusions based on arrest records alone (without further evidence that actual criminal activity occurred) takes place in other sectors (Jain, 2015). In the employment sector, 86% of employers conducted criminal history checks for at least some jobs and 69% conducted checks on all job applicants (Society for Human Resource Management, 2012). A specific example of this practice can be seen in New York, where arrest data are automatically transmitted to the NYC and NYS Departments of Education and the NYC Taxi and Limousine Commission (Jain, 2015, p. 839).

[2] The aforementioned research articles find improved criminal justice outcomes and mixed employment outcomes. McWilliams and Hunter (2021) finds that criminal record-related stigma negatively impacts the individual's quality of life, pointing to the importance of reducing criminal record stigma and discrimination. Relatedly, Cullen et al. (2023) implements a field experiment to test whether at what "price" businesses would be willing to hire workers with a criminal record, thereby mitigating the scarring effect.

[3] We use citizen allegations of misconduct (or complaints) as our measure of alleged misconduct but throughout the paper, we will use the term "misconduct" instead of "alleged misconduct" for simplicity.

This paper expands this literature by looking at conviction-less arrests, providing policymakers with an additional lens through which to evaluate the consequences of officer misconduct.

In the first part of the paper, we establish that officers with greater misconduct make different types of arrests than those with no misconduct. Specifically, high-misconduct officers are 12% more likely to make arrests that result in no charges and 31% more likely to make arrests with "not guilty" outcomes compared to no-misconduct officers on the same shift, while showing no increase in arrests yielding "guilty" outcomes.

To obtain plausibly causal estimates, we compare Chicago Police Department (CPD) officers working on the same shift but with differing levels of misconduct.[4] We measure misconduct by the total number of complaints made against the officer in the previous calendar year. This approach aligns with a growing body of evidence that civilian allegations of misconduct are meaningful indicators of actual misconduct (Rozema and Schanzenbach, 2019; Stoddard et al., 2024). We argue that we estimate causal estimates because the set of officers working on the same shift is as-good-as-randomly determined due to the rotational duty calendar. Specifically, because officers are assigned to their work duties based on a rotational calendar, we argue that observed differences in arrest-making across officers cannot be explained by differences in the offenses these officers are observing.

After establishing these facts on the relationship between officer misconduct and arrest patterns, we turn to analyzing a policy change in the second part of the paper. Given the observed differences in arrest behavior between officers with varying levels of misconduct, a natural question is: can increased oversight influence officer behavior and potentially reduce these conviction-less arrests? To answer this question, we analyze two events that increased the transparency of police misconduct and, consequently, oversight.

On March 10, 2014, the Illinois Supreme Court ruled in *Kalven v. City of Chicago* that police-misconduct records are public information under the Illinois Freedom of Information Act (FOIA). Prior to this ruling, officer misconduct records were not public information. The court decision altered how much information about complaints against individual police officers could be seen by the public. This change was expected to impact policing practices. The Chicago police union's

[4] A shift is defined as a unique combination of the day of the week + month-year (e.g., January 2014) + district + sector (which identifies the geographic area where the officer is working) + watch (which identifies the time of day that the officer is working).

reaction showed this, with their president writing a front-page letter in the August 2014 newsletter discussing the ruling and its effects on CPD officers.

We conduct an event study to examine how officers responded to these two events. After the court ruling, officers with misconduct make fewer arrests with "not guilty" outcomes without a corresponding increase in arrests with guilty verdicts. The subsequent FOP newsletter prompted additional changes, particularly reduced non-index crime arrests among officers with misconduct. For both events, we see heterogeneous responses, with effects varying inversely by misconduct level—low-misconduct officers showing stronger responses and high-misconduct officers showing weaker responses. Meanwhile, no-misconduct officers exhibited a different pattern: initially withdrawing from policing activities after the court ruling, but returning to pre-ruling arresting behavior patterns following the newsletter.

Our findings reveal that increased transparency of officer misconduct may reduce conviction-less arrests, but with nuances across officer misconduct profiles. These results are largely in line with prior research indicating that police oversight can effectively influence officer behavior (Mummolo, 2018; Cheng and Long, 2018; Rivera and Ba, 2023; Rozema and Schanzenbach, 2023; Campbell, 2024).[5] However, a contribution of this paper over existing studies is its differentiation between high-, medium-, low-, and no-misconduct officers. This enables us to offer more targeted insights for developing nuanced oversight policies that can more precisely address variations in officer behavior and potentially reduce conviction-less arrests. For example, we find more pronounced effects among officers with low misconduct, suggesting that a blanket transparency intervention may have limited effectiveness for officers with high-misconduct and perhaps an unintended effect for no-misconduct officers. This nuanced finding aligns with research suggesting that civilian complaints correlate positively with officer productivity (Lersch, 2002) while also supporting studies showing that at high levels, complaints cannot be explained merely as a byproduct of officer activity (Stoddard et al., 2024).

[5] For example, Mummolo (2018) finds that requiring NYPD officers to provide detailed justifications for stops increased "hit rates" (the rate of stops yielding evidence of a crime). Cheng and Long (2018) finds that tracking patrol officers with a Global Positioning System and enforcing the dismissal of underperforming officers reduced robberies and aggravated assaults in New Orleans. Similarly, Rozema and Schanzenbach (2023) show that CPD officers reduce misconduct following sustained civilian complaint investigations, while Rivera and Ba (2023) document that increased complaint transparency reduces future misconduct without decreasing policing activity (e.g., stops, arrests, or uses of force). Campbell (2024) finds that a 2011 federal investigation into the Seattle Police Department reduced stops with no detectable effect on reported serious crime.

This paper expands the current literature on alternatives to traditional arrest methods. Several counties and states have implemented reforms aimed at reducing unnecessary arrests and their negative impact on individuals. For example, an examination of Florida's Juvenile Civil Citation Program, which recommends police officers issue a civil citation instead of making an arrest for first-time misdemeanor offenses, finds that juvenile recidivism declined (Nadel et al., 2018; Krishna, 2024). Mueller-Smith and T. Schnepel (2021) examines two policy changes in Harris County, Texas that changed diversion rates and finds that diversion from a felony conviction for first-time offenders reduces future convictions and increases employment rates and earnings.[6] These findings also point to the promise of alternative response models, which are slowly gaining traction (Bell, 2021; Dee and Pyne, 2022; Ba et al., 2024).[7] We examine two policy changes that increased the transparency of misconduct allegations and find that increased oversight reduces conviction-less arrests.

More broadly, this paper also relates to work that examines different dimensions of law enforcement practices and their impact on society (Heaton et al., 2017; Ouss and Rappaport, 2020; Ang et al., 2024; Cunningham et al., 2024). For example, Cunningham et al. (2024) finds that collective bargaining rights appear to create protective mechanisms that may shield problematic officers from meaningful accountability. As Ouss and Rappaport (2020) documents, there is an increasing societal intolerance of policing harms, highlighting the need for reform. Developing practices that strengthen community-police relations is important, especially as an erosion of trust in the police can have important ramifications for public safety (Ang et al., 2024; Sánchez De La Sierra et al., 2024). This paper examines the possibility of using officer misconduct as a policy lever to reduce conviction-less arrests for officers with misconduct.

Worden et al. (2013) evaluates an early intervention (EI) program using a matched control group and finds that the EI program reduces arrests and proactive arrests with no statistically significant change in complaints, uses of force, and secondary arrests. The authors point to this as evidence that EI is not effective as it discourages officers from being proactive. But they do not fully explore the downstream effects of these arrests. Do the arrests result in charges? Are the defendants found

[6] Specifically, the diversion program in Texas is a deferred adjudication agreement wherein the defendant admits guilt without receiving a criminal conviction record and completes a probationary period of community supervision.

[7] Bell (2021) argues that future research on policing should evaluate policing alternatives recognizing that policing and public safety are not necessarily the same. Dee and Pyne (2022) evaluates a community response pilot experiment in Denver that directed targeted emergency calls to health care responders instead of the police and finds reduced reports of less serious crimes and no reported changes in serious crimes. Ba et al. (2024) runs a series of experiments and finds evidence of bipartisan public demand for police alternatives.

guilty? This paper documents a causal relationship between officer alleged misconduct measured in the previous calendar year and conviction-less arrests, painting a fuller picture and speaking to its potential as a policy instrument to reduce conviction-less arrests.

Further, the literature is mixed on the welfare effects of the marginal arrest.[8] This paper sheds light on the marginal arrest made by *high-misconduct* officers. This is distinct from prior research that focused on high-arresting officers as arrests are positively correlated with productivity. For example, Weisburst (2024) finds that high-arrest officers make more low-level arrests but have a higher share of low-level arrests that end in conviction. This is different from our finding that high-misconduct officers make more arrests that do *not* end in conviction. Therefore, the use of officer misconduct may provide a more precise approach in identifying who is making conviction-less arrests.

The rest of the paper is organized as follows: Section 2 describes the data and provides background on CPD patrol assignments. Section 3 describes our identification strategy. Section 4 discusses our results, and Section 5 discusses additional analyses to explore heteroengeous effects. Section 6 presents the policy analysis on the court ruling and the Chicago police union president's letter. Section 7 concludes.

## 2 Data and Background

### 2.1 Data and Summary Statistics

Officer personnel data was acquired from the Chicago Police Department through Freedom of Information Act requests. We construct a panel dataset that contains a continuous record of officer shift assignments, complaints, and arrests between 2010 and 2016. We link officer arrests to court records, which was acquired through the Chicago Data Collaborative. The analysis dataset has 3.2 million observations on 8,843 patrol officers between 2010 and 2016.

Table 1 presents descriptive statistics on the analysis sample. The first row lists the number of unique officers in each misconduct category. These are not mutually exclusive categories as an officer may have 0 complaints one year and 3 complaints the next year. In terms of percentages, on average, 66 percent of officers between 2010-2016 have 0 complaints in a year, 22 percent have 1

[8] For example, Cho et al. (2024) suggests that marginal arrest may not be productive, while Chalfin et al. (2022) suggests that increases in low-level arrests and homicide reduction may be complementary.

complaint, 7 percent have 2 complaints, and 4 percent have 3 or more complaints. To reiterate what the data are showing, complaints are not common; an officer at the 90th percentile of complaints has 2 complaints a year.

Table 1: Descriptive Statistics

| | | Number of complaints last year: | | | |
|---|---|---|---|---|---|
| | Overall | 0 | 1 | 2 | 3+ |
| Unique number of officers | 8,843 | 8,217 | 4,711 | 2,045 | 1,072 |
| Average share of officers | 1.000 | 0.664 | 0.222 | 0.073 | 0.041 |
| Share with any arrest | 0.053 | 0.046 | 0.055 | 0.071 | 0.095 |
| Mean number of arrests | 1.104 | 1.093 | 1.106 | 1.125 | 1.151 |
| | (0.382) | (0.364) | (0.377) | (0.411) | (0.472) |
| Mean number of verdicts | 1.051 | 1.046 | 1.044 | 1.059 | 1.103 |
| | (0.799) | (0.786) | (0.784) | (0.844) | (0.870) |

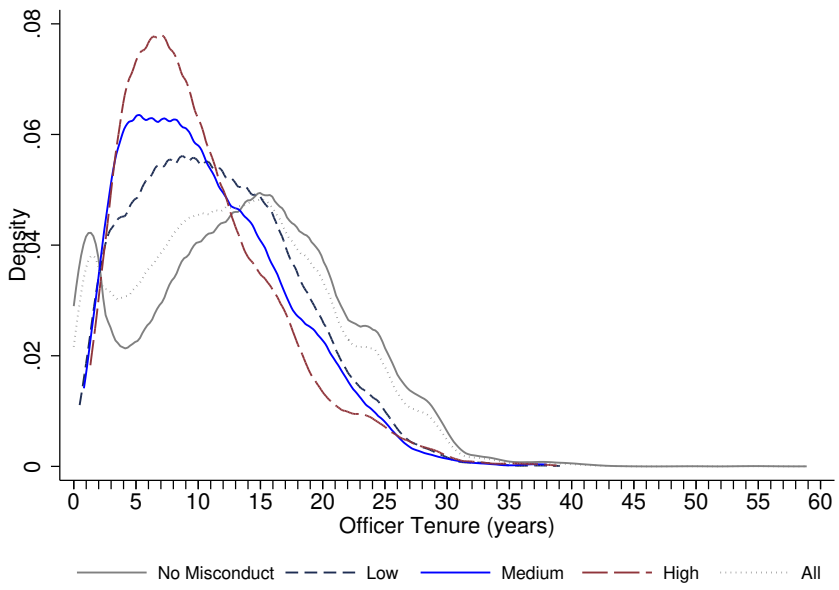
Notes: This table depicts summary statistics on the analysis sample of all Chicago Police Department patrol officers between 2010 and 2016. "Unique number of officers" is the unique number of officers between 2010-2016 in the relevant misconduct category. "Average share of officers" is the average over years. "Share with any arrest" is the share of all officer-shifts between 2010-2016 with any primary arrests. The mean number of arrests and verdicts are conditional on making an arrest. Standard deviations are in parentheses.

About 5 percent of officer-shifts between 2010-2016 have at least one arrest a shift. This translates to an average of 68 arrests a shift across all of Chicago. On any given shift, 5 percent of patrol officers with no complaints last year make at least one arrest as the primary arresting officer, while 9.5 percent of officers with 3 or more complaints last year on shift make at least one arrest as the primary arresting officer. These numbers suggest that officers with more complaints may be more active when they are on duty, which makes sense as complaints and arrests are positively correlated.

Conditional on making an arrest, the average officer makes 1.1 arrests per shift. This is not too different across the different misconduct levels. An officer with 3 or more complaints last year makes 1.15 arrests compared to the 1.09 arrests made by an officer with no complaints. This suggests that the heterogeneity is coming from the extensive margin, the likelihood of making an arrest, rather than the intensive margin.

The last two rows in Table 1 list the mean number and standard deviation of verdicts. Because an arrest can have multiple charges, the number is greater than one. Since we do not have charge data, we use verdicts as a proxy for charges. Most arrests have about about one verdict, which is slightly higher for officers with more misconduct.

Figure 1: Distribution of Officer Tenure by Misconduct Level




Notes: This figure depicts the distribution of officer tenure by misconduct level.

Figure 1 depicts the distribution of officer tenure by misconduct level. The dotted line, which is for all officers, indicates that a plurality of officers are between 8-20 years of experience in our data. If we look separately by misconduct level, most of the officers who had high misconduct are relatively junior–between 5-10 years of experience (maroon, dashed line). Last, the no-misconduct officers tend to have more experience, with a hump around 16 years of experience (navy, dashed line).

## 2.2 Background on CPD patrol assignments

There are three components to an officer's assigned work environment: the geographic district or where they work; the watch or the time of day they work; and the calendar day that they work.

Chicago is geographically divided into 22 districts (see Appendix Figure A1). Officers are initially assigned to one of these districts based on available vacancies and departmental needs.[9] Districts are subdivided into beats. Our analysis works with sectors, which are aggregated beats. Sectors are

[9]After the initial assignment, an officer may transfer to another district if there are vacancies in that district. The transfer process relies on a bidding process wherein the winning bid goes to the most senior officer. In reality, there are few transfers. For example, between 2010-2016, 67% of officers did not change districts.

determined by the second digit in a beat's ID number. There is an average of 3-4 beats per sector, and 3 sectors per district.

Officers are assigned to one of three watch periods, which are 8-hour long periods. Watch 1 is from midnight to 8 am, Watch 2 is from 8 am to 4 pm, and Watch 3 is from 4 pm to midnight. Officers bid every fall for their preferred watch period for the next calendar year. The bidding process for watches is based on seniority. Most officers are on Watch 1 and 3, while more senior officers are most likely to be on Watch 2 (Appendix Figure A2).

Officers are also assigned to "day-off groups", which determine who works on rotating calendar dates. This schedule is set late in each calendar year for the following year. A notable feature of this system is that cycles do not occur on a weekly basis; a typical duty cycle is four days "on" and two days "off". This means that the set of officers working on, say, Mondays in November will not be the same. Further, because operations schedules are made far in advance, officers effectively have no ability to anticipate fluctuations in crime or civilian behavior while on duty, conditional on assigned district and watch. In other words, officers are unable to pre-emptively choose to take days off when they expect crime to spike.

# 3 Identification Strategy

## 3.1 Summary

We use the CPD's rotational duty calendar to obtain plausibly exogenous variation in the set of officers assigned to work on a particular day. Specifically, we compare all officers working on the same shift but with different levels of misconduct last year. A shift is defined as a unique combination of the day of the week + month-year (e.g., January 2010) + sector (which identifies the geographic area where the officer is working) + watch (which identifies the time of day that the officer is working). Misconduct is measured by the total number of complaints made against the officer in the previous calendar year. Comparing officers on the same shift ensures that we compare the behavior of officers in the same, very narrowly defined working conditions. This design also ensures that officers have the same opportunity to take enforcement action; failure to account for officers' inactivity may lead to biased conclusions (Knox et al., 2020).

The claim is that the level of misconduct is as-good-as-randomly assigned across officers working the same shift because rotations are out of an officer's control. Therefore, the *set of officers* working on the same shift is not determined by the officer's level of misconduct.

## 3.2 Exogeneity checks

To check whether our identification strategy is valid, we conduct three tests.[10] First, we examine whether officers are able to choose which day of the week they work. We calculate the total number of complaints an officer received in one week and see if it can predict which day of the week the officer works the following week. Table 2 shows that officers with more misconduct are not able to choose which days they work. All of the coefficient estimates are trivial in magnitude and not statistically significant.[11] In addition, the R-squared is very low, ranging from 0.02% to 0.4%. This is consistent with our argument that, since these officers work 4 days on / 2 days off, they are not consistently working the same days each week.

Table 2: Does misconduct predict which day of the week an officer works?

| Outcome Variable: | Monday (1) | Tuesday (2) | Wednesday (3) | Thursday (4) | Friday (5) | Saturday (6) | Sunday (7) |
|---|---|---|---|---|---|---|---|
| Total Complaints Last Week | 2.80e-05 | 0.000312 | 0.00124 | 0.000174 | -0.00117* | 6.28e-05 | -0.000647 |
| | (0.000620) | (0.000479) | (0.000789) | (0.000476) | (0.000584) | (0.000855) | (0.000636) |
| Constant | 0.139*** | 0.142*** | 0.144*** | 0.144*** | 0.145*** | 0.144*** | 0.141*** |
| | (2.21e-05) | (1.74e-05) | (3.17e-05) | (1.54e-05) | (2.19e-05) | (3.54e-05) | (2.22e-05) |
| Observations | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 |
| R-squared | 0.004 | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.003 |

Notes: This table reports estimates for the total number of complaints last week on the probability of working on a specific day of the following week. All estimates also include fixed effects for officer, district, and year. Standard errors clustered by officer and district are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Second, we examine whether officers are able to choose which days to work based on expected crime. Although the rotational calendar is out of the officer's control, officers are able to request certain holidays off. Therefore, one potential concern to our analysis is that officers with more misconduct may be choosing to work on days where the crime rate is expected to be higher. This would lead to more potential opportunities for arrests, which leads to more opportunities for complaints made against the officer.

[10] We conduct these tests on patrol officers, which is our analysis sample, rather than on all officers. We replicate these tests on all police officers in Appendix Tables A2, A3, and A4. The results are the same.

[11] The coefficient estimate for Friday is weakly significant at the 10% level, but the magnitude is trivial (0.1 percentage point).

To check this, we calculate the officer's total number of complaints last year and see if it can predict whether they work on holidays the subsequent year that tend to experience the largest number of violent crimes.[12] Table 3 shows that officer misconduct is orthogonal to working on these holidays, suggesting that officers with greater misconduct are neither choosing to nor not choosing to work on these days.[13]

Table 3: Are higher misconduct officers able to choose which days to work based on expected crime?

| Outcome Variable: | Memorial Day (1) | Fourth of July (2) | Labor Day (3) | St. Patty's Day (4) | New Year's (5) |
|---|---|---|---|---|---|
| Total Complaints Last Year | -0.000800 | -0.00189 | -0.00401 | 0.00255 | 0.00388 |
| | (0.00410) | (0.00315) | (0.00401) | (0.00398) | (0.00287) |
| Observations | 26,207 | 26,207 | 26,207 | 26,207 | 26,207 |
| Mean Outcome | 0.248 | 0.237 | 0.250 | 0.244 | 0.411 |

Notes: This table reports estimates for the total number of complaints last year on the probability of working on a specific holiday the following year. New Year's includes working on New Year's Eve and New Year Day. The sample is at the officer-year level. All estimates also control for officer tenure at the start of the year and include fixed effects for officer, lagged modal district, and year. Standard errors clustered by officer and district are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Last, we examine whether misconduct is correlated with an officer's assigned watch. Similar to holiday requests, watch assignments are determined in the previous calendar year. Therefore, we check whether the total number of complaints last year can predict which watch an officer is on this year. Table 4 shows that the estimates are all trivially small in magnitude and not statistically significant, which suggests that officer misconduct is orthogonal to the assigned watch. Taken together, these three tables support our claim that the set of officers who are working on a given day, time, and location—or "shift"—is as good as randomly assigned with respect to misconduct level.

[12] The reason for the asynchronous timing is because officers' work schedules for a given calendar year are set in the fall of the previous year.

[13] The first three holidays are listed in decreasing order of gun violence. St. Patrick's Day and New Year's were included because they typically involve lots of alcohol and, thus, arguably more opportunities for police involvement.

Table 4: Does misconduct predict watch?

| Outcome Variable: | Watch 1 (1) | Watch 2 (2) | Watch 3 (3) |
|---|---|---|---|
| Total Complaints Last Year | -0.00118 | 0.00157 | -0.000998 |
| | (0.00274) | (0.00304) | (0.00303) |
| Observations | 18,725 | 18,725 | 18,725 |
| Mean Outcome | 0.347 | 0.334 | 0.442 |

Notes: This table reports estimates for the total number of complaints last year on the probability of working a given watch the following year. The sample is at the officer-year level. All estimates also control for officer watch last year, officer tenure at the start of the year, officer total arrests two years ago, and include fixed effects for officer, modal district, and year. Standard errors clustered by officer and district are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In Appendix Tables A2, A3, and A4, we conduct these checks using a categorical measure of misconduct instead of as a linear measure. In Panel A, we compare officers with medium/high misconduct (two or more complaints) to those with low- and no-misconduct (zero or one complaint). In Panel B, we compare officers with high misconduct (three or more complaints) to the rest. Our findings remain unchanged when using these alternative specifications.

# 4 Results

## 4.1 Officers with more misconduct are more likely to make an arrest than those with no misconduct

To examine whether officers with more misconduct are more likely to make an arrest relative to officers with no misconduct, we estimate this regression model:

$$y_{itr} = \beta_0 + \beta_1 Low_{i,r-1} + \beta_2 Medium_{i,r-1} + \beta_3 High_{i,r-1} + X'_{itr}\delta + \varepsilon_{itr} \quad (1)$$

where $y_{itr}$ is the probability that officer $i$ made an arrest on day $t$ in year $r$. We also estimate the probabilities of making an arrest for an index crime and for a non-index crime. We classify the total number of complaints officer $i$ had the previous year, in year $r-1$, into four buckets: No complaints ("No misconduct"), 1 complaint ("Low misconduct"), 2 complaints ("Medium misconduct"), and 3 or more complaints ("High misconduct"). The reference group is 0 complaints. The term $X_{itr}$ contains

controls for officer tenure (included as a quadratic) and a fixed effect for shift. Standard errors are clustered by shift.

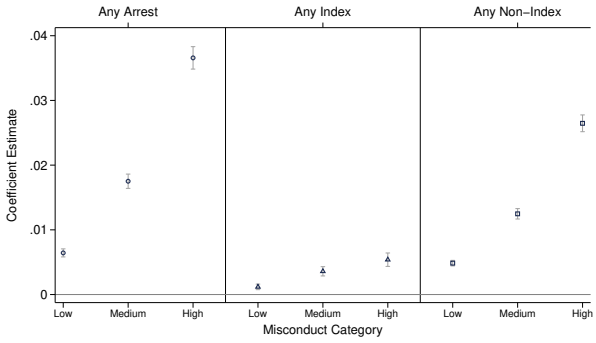
Figure 2a depicts the coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$. An officer with no misconduct has an 4.6% chance of making at least one arrest on a shift. Those with one complaint, or a low level of misconduct, are 0.64 percentage points more likely to make an arrest and this is statistically significant at the 1% level. Those with two complaints, or a medium level of misconduct, are 1.8 percentage points more likely to make an arrest ($p < 0.01$). Those with three or more complaints, or a high level of misconduct, are 3.7 percentage points more likely to make an arrest ($p < 0.01$). This is an effect size of about 80% of the reference group mean.

We also conduct $t$-tests to see if these estimates are statistically significantly different from each other, and they are (see Appendix Table A5). This indicates that officers with high misconduct are more likely to make arrests than those with low- or medium-misconduct.

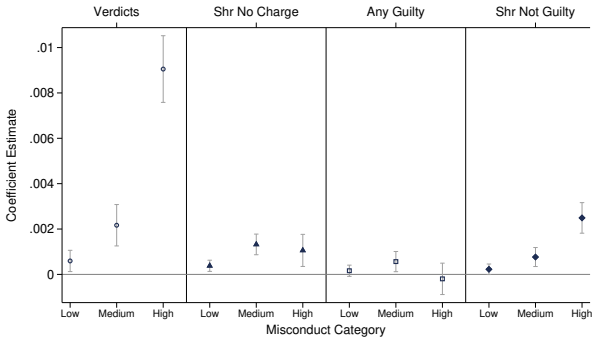
We also look at the likelihood of making an arrest for an index crime, which are more serious crimes. We see similar results here though on a smaller scale. We also report estimates for the probability of making an arrest for a non-index crime, which are less serious crimes. Again, we see similar results. Further, the magnitudes suggest that the overall arrest results are being driven by arrests for non-index crimes.

Figure 2: Impact of Officer Misconduct on Conviction-less Arrests

(a) Arrest Outcomes




(b) Court Outcomes




Notes: This figure depicts coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for various outcomes. Figure (a) reports arrest outcomes: probability of an arrest, probability of an arrest for an index-crime, and probability of an arrest for a non-index crime. Figure (b) reports court outcomes: number of verdicts, share of arrests with no charge, probability of an arrest having a guilty outcome, share of an arrest's verdicts with a not guilty outcome. All estimates control for officer tenure and shift. Figure (b) also includes the number of index arrests and non-index arrests and the number of verdicts as controls. Wings are 95% confidence intervals with clustered standard errors.

Appendix Table A5 reports these estimates in table format. Appendix Table A6 reports estimates for the number of arrests rather than the probability. The results are the same; officers with more misconduct make more arrests than those with no misconduct. These results are in line with the summary statistics, which suggested that greater misconduct officers are being more active while on patrol. In the next section, we will assess the "quality" of these arrests, as proxied by court outcomes.

### 4.2 Officers with more misconduct make more conviction-less arrests than those with no misconduct

To examine whether officers with more misconduct make more conviction-less arrests, we estimate equation (1) on four court outcomes: the number of verdicts an arrest has (used as a proxy for charges); the share of arrests that have no charges (since officers may make more than one arrest on a shift); the probability of any of the arrests having a guilty outcome; and the share of arrests that are not guilty. Because an officer may make more than one arrest per shift, we estimate the share of arrests that are not guilty rather than the likelihood of any "not guilty" outcome in order to be more conservative.[14] We consider these outcomes in totality to see if conviction-less arrests are being made. For example, we would need to see an increase in either the share of arrests with no charges or found not guilty (or both) accompanied by a decrease or no change in the probability of an arrest being found guilty.

The regression model is the same as (1), except the term $X$ also includes the total number of index arrests and non-index arrests officer $i$ made on day $t$ in year $r$. We included these controls because we found in Section 4.1 that high-misconduct officers make more non-index arrests. The regression model for the outcomes "any guilty" and "share not guilty" also include the total number of verdicts from officer $i$'s arrests made on day $t$ in year $r$.

Figure 2b displays the estimates. Appendix Table A5 reports these estimates in table format. First, we look at the share of arrests that had no charge. Relative to officers with no misconduct, on any given shift a larger share of arrests made by officers with any misconduct are more likely

[14] If a single arrest has multiple charges, we have recoded the court outcome to reflect a single outcome. If any of the charges related to a single arrest were found guilty, then the court outcome is coded as "Guilty" in our dataset. Otherwise, if all of the charges are found not guilty, then the court outcome is coded as "Not Guilty" in our dataset.

to result in no charges. The effect sizes are about 4.2% for low-misconduct officers, 14.7% for medium-misconduct officers, and 11.8% for high-misconduct officers (all $p < 0.01$). The estimate for high-misconduct officers is statistically significantly different from the estimate for low-misconduct officers at the 10% level, but the estimates for medium- and high-misconduct officers are not (see Appendix Table A5). Similarly, if we look at the share of arrests that were found "not guilty" in court, then we do see differences by misconduct level. High-misconduct officers are 0.25 percentage points more likely to make an arrest with a "not guilty" outcome than no-misconduct officers. This is an effect size of 31%.

If we examine the probability of any of these charges having a "guilty" court outcome, we find that it does not differ drastically across misconduct level. Medium-misconduct officers are 0.06 percentage points more likely to have a guilty outcome, relative to no-misconduct officers, and this estimate is significant at 5%. However, high-misconduct officers are not statistically significantly different from no-misconduct officers and their coefficient is negative–suggesting that the probability of making an arrest with any guilty outcome is *less* than that for non-misconduct officers. Put together, these results are consistent with the claim that high-misconduct officers make more conviction-less arrests. Even though we found in Section 4.1 that these officers are more proactive, when we compare the court outcomes of these arrests, we find that they are also more likely to result in no charges and be found "not guilty" with no change in "guilty" outcomes.

In Appendix Table A7, we probe these results to see which arrest-types are driving them. In this analysis, we subset the sample to officers who made at least one non-index (or index) arrest while on their shift. As an example, we take all the officers who are assigned to the same watch period and geographic sector on the Mondays in January and compare the court outcomes of their non-index arrests. This provides a more even comparison between two officers who are more likely to make an arrest while patrolling the same narrow geographic area during the same day and time but with differing misconduct levels. Consistent with our main results, we find that the increase in "not guilty" outcomes is driven by arrests for non-index crimes.[15]

As a robustness check, we examine whether our results hold when we redefine misconduct as complaints about more serious violations of appropriate officer conduct (Appendix Table A8).

[15] Interestingly, we find that the share of no charges is negative and statistically significant, but our general conclusion stands as the probability of any guilty outcome is not statistically significant and negative while the share not guilty is positive and statistically significant.

Because the severity of a complaint can vary, this approach will categorize officers by the same severity level.[16] We classify complaints into three categories: Serious, Failure to Provide Service (FPS), and Personnel Conduct.

"Serious" complaints include allegations of use of force violations, arrest/lockup violations, and search violations. These complaints generally involve potential misconduct by officers during their interactions with civilians. "FPS" complaints, on the other hand, allege a neglect of duty or inadequate service/failure to provide service. These complaints are typically filed by individuals who are seeking police assistance or reporting themselves as victims of a crime. The "Personnel Conduct" category encompasses a broader range of complaints related to the officer's general professional conduct and behavior, such as being absent without permission, being intoxicated, extortion, or the possession/sale of drugs. These complaints signal concerning behavior by the officer rather than during specific incidents involving civilians.[17]

We see similar results when we classify misconduct using the Serious and Personnel Conduct complaint categories.[18] This is not surprising as both types signal concerning behavior by the officers, either in their interactions with civilians (Serious) or in their overall professional conduct (Personnel Conduct). In contrast, the estimates using FPS complaints are largely not statistically significant.[19] This difference likely stems from the different nature of FPS complaints, which are typically filed by complainants who are potentially victims of a crime or who seek police help, rather than by complainants who may viewed as criminal by the officers.

Last, we expand the analysis to include arrests made by primary and secondary arresting officers in Appendix Table A9. The results remain the same. In summary, the results across all of these analyses suggest that officers with more misconduct, as measured by complaints, are making a higher number of conviction-less arrests compared to their no-misconduct colleagues.

[16] For example, our main method would lump together an officer with two brutality complaints and an officer with two complaints about failure to provide service. This robustness check will instead separately categorize these two officers due to the severity of the complaint.

[17] See Appendix Table A.5 in Ba (2018) for a complete list of allegation categories.

[18] Specifically, we see that high serious-misconduct officers have a higher share of not guilty outcomes while high personnel-misconduct officers have a higher share of no charges compared to no-misconduct officers. Both types do not see any change in guilty outcomes.

[19] Interestingly, the magnitude of the coefficient estimates for the share of no charges and share not guilty are similar for high-misconduct officers identified using FPS complaints compared to those identified using Serious complaints.

# 5 Additional Analyses

## 5.1 Officer tenure

In this section, we examine whether the results are driven by officer tenure. This is a plausible hypothesis because more junior officers (those with 5-12 years of experience) are overrepresented in terms of misconduct level (see Figure 1). We control for this relationship in our main regression (equation (1)) by including a quadratic term for officer tenure, but we examine this relationship further in this section. Specifically, we estimate equation (1) separately for each tenure category: $< 8$ years (25th percentile or below); $[8, 13)$ years (25th-50th percentile); $[13, 18)$ years (50th-75th percentile); and $\geq 18$ years (75th percentile or above).

The results are in Appendix Table A10. Although we see that the most junior officers are making conviction-less arrests, we also see similar evidence for mid-level officers—those with 8-18 years of experience. Among these tenure levels, high-misconduct officers have a higher share of no charges and not guilty outcomes with no change in guilty outcomes. We do not see similar patterns among the most senior officers (those with 18 or more years of experience). This suggests that tenure alone cannot explain our results.

## 5.2 Bad apples

In this section, we examine whether these results are driven by "bad apples"–individual officers whose misconduct may disproportionately account for the patterns we observe. Recent empirical work has documented substantial officer-level heterogeneity in misconduct and its impacts on civilian outcomes (Goncalves and Mello, 2021; Rozema and Schanzenbach, 2019; Chalfin and Kaplan, 2021). For example, Chalfin and Kaplan (2021) find that the top 2% of officers (ranked by total complaints) account for 26% of use of force complaints.

To examine this, we first identify officers who are in the 95th percentile of the rate of complaints over our entire sample. The rate of complaints is defined as the total number of complaints divided by the total number of shifts. Because some officers join the CPD in the middle of our sample period, we make this adjustment to make the number of complaints comparable across officers. We then create quintiles of the total number of shifts and classify officers in the 95th percentile in each

quintile. These officers make up about 9% of all officers. About 13% of all officers are ever in the high-misconduct category, and 58% of the officers who are ever in the high-misconduct category are not "bad apples".

The regression model is a modified version of equation 1:

$$\begin{aligned} y_{itr} = \beta_0 &+ \beta_1 Low_{i,r-1} + \beta_2 Medium_{i,r-1} + \beta_3 High_{i,r-1} + \beta_4 95th_i \\ &+ \beta_5 \left(Low_{i,r-1} \times 95th\right) + \beta_6 \left(Medium_{i,r-1} \times 95th\right) \\ &+ \beta_7 \left(High_{i,r-1} \times 95th\right) + X'\delta + \varepsilon_{itr} \end{aligned} \tag{2}$$

Appendix Table A11 report results. High-misconduct officers who are also bad apples have a higher share of arrests with "not guilty" outcomes compared to no-misconduct officers ($p < 0.05$), while high-misconduct officers who are not bad apples have a higher share of arrests with no charges compared to no-misconduct officers ($p < 0.05$). For both types of high-misconduct officers, the probability of any guilty outcome is not statistically significantly different from that for no-misconduct officers. These results suggest that using a time-varying measure of alleged misconduct, as opposed to a fixed person-specific measure, may also be an informative method of dealing with conviction-less arrests.

### 5.3 Partners

In this section, we examine the extent to which high-misconduct officers may influence their partners. The research on peer effects in education documents how individual behavior can be shaped through social learning and behavioral changes (Sacerdote, 2001; Carrell et al., 2018; Lavy et al., 2012; Burke and Sass, 2013). Similarly, high-misconduct officers may influence their partners' behavior through direct interaction and observation, or they may strategically ask their partners to serve as the primary arresting officer on arrest forms to deflect potential scrutiny.

To examine this, we exploit changes in partner assignment due to the CPD rotational duty calendar. We focus on officers who have only one partner in their assigned beat, which make up the majority of shifts[20], and compare an officer's policing behavior when they are assigned to a high-misconduct partner vs. a no-misconduct partner, conditional on working on the same geographic

[20] Specifically, 69% of shifts have 2 officers, 24% have 1 officer, and 4.6% have 3 officers.

beat, watch, day of the week, and month-year. The regression model is as follows:

$$\begin{aligned} y_{itr} = \beta_0 &+ \beta_1 Low_{i,r-1} + \beta_2 Medium_{i,r-1} + \beta_3 High_{i,r-1} + \beta_4 LowP_{it,r-1} \\ &+ \beta_5 MediumP_{it,r-1} + \beta_6 HighP_{it,r-1} + \beta_7 \left(Low_{i,r-1} \times LowP_{it,r-1}\right) \\ &+ \beta_8 \left(Medium_{i,r-1} \times MediumP_{it,r-1}\right) + \beta_9 \left(High_{i,r-1} \times HighP_{it,r-1}\right) \\ &+ X'\delta + \varepsilon_{itr} \end{aligned} \tag{3}$$

where $y_{itr}$, $Low_{i,r-1}$, $Medium_{i,r-1}$, and $High_{i,r-1}$ are defined as in equation (1). $LowP_{it,r-1}$ is a binary variable equal to 1 if officer $i$'s partner on day $t$ had 1 complaint last year (in year $r-1$) and equal to 0 otherwise. $MediumP_{it,r-1}$ is equal to 1 if officer $i$'s partner on day $t$ had 2 complaints last year. $HighP_{it,r-1}$ is equal to 1 if officer $i$'s partner on day $t$ had 3 or more complaints last year. The term $X$ contains fixed effects for officer and shift. Standard errors are clustered at the officer level.

Appendix Table A12 reports results. In general, most of the coefficient estimates are small in magnitude and/or not statistically significant. Some estimates that are of note are: when a high-misconduct officer is paired with a high-misconduct partner, they are 0.78 percentage points ($p < 0.05$) more likely to make a non-index arrest (72% of the reference group mean). The estimates on "share not guilty" are not statistically significant but the magnitudes are non-trivial. For example, two high-misconduct officers partnered together have 0.15 percentage points (22% of the mean) more "not guilty" outcomes compared to a no-misconduct officer pair.

## 6 Which officers respond most to increased oversight?

In this section, we examine whether increased oversight can affect officer behavior. The policy change is the *Kalven v. City of Chicago* ruling on March 10, 2014 that made police-misconduct records public information under the Illinois Freedom of Information Act (FOIA). Prior to *Kalven v. City of Chicago*, officer misconduct records were not public information. On November 26, 2009, journalist Jamie Kalven submitted FOIA requests to the Chicago Police Department (CPD) asking for records on officer complaints.[21] On December 8, 2009, CPD denied the requests stating that

[21] https://casetext.com/case/kalven-v-city-of-chi/

they were exempt from FOIA disclosure. On December 22, 2009, Kalven filed a lawsuit challenging the protective order.

On March 10, 2014, the Illinois Appellate Court ruled that the police misconduct records were not exempt from FOIA disclosure. However, even after this decision, the Chicago police union (Fraternal Order of Police "FOP") fought to keep these records from being released. In 2014, prompted by FOIA requests from the Chicago Sun-Times and the Chicago Tribune, the city of Chicago attempted to release all officer complaint information going back to 1967.[22] This release was held up by a temporary injunction brought by the Fraternal Order of Police.[23] The injunction allowed only the four most recent years of information to be released, due to a provision in FOP's collective bargaining agreement with the city of Chicago requiring the destruction of records of alleged police misconduct once the record is more than four years old.

FOP then sued CPD for failing to destroy records of police misconduct older than five years.[24] The appellate court vacated the injunction as being against public policy (meaning CPD was able to release misconduct records more than four years old) and also found that destroying these records violated an explicit, well-defined, and dominant public policy. The Illinois Supreme Court agreed.

We theorize that this ruling changed the visibility of officer-specific information on the number and nature of complaints filed against the officer. It was also expected to change policing practices, as evidenced by the Chicago police union's response. The FOP president published a letter on the front page of the union's August 2014 newsletter about the court ruling and how it would affect officers:

> An additional concern of ours is the impact this type of disclosure might have on the reputation of the individual Police Officers and quite frankly, the Chicago Police Department as well. A Police Officer might suffer undue scrutiny from family members, friends, neighbors and even co-workers, when this information is released.

We conduct an event study analysis to examine how an officer's policing behavior changed after these two events. We restrict the analysis sample to be between September 2013 (6 months before the March 2014 court ruling) and January 2015 (5 months after the August 2014 newsletter). This

[22] https://caselaw.findlaw.com/court/il-supreme-court/1913070.html
[23] https://www.abajournal.com/news/article/largest_public_database_of_chicago_police_misconduct_debuts
[24] https://casetext.com/case/fraternal-order-of-police-v-the-city-of-chicago

is to follow the strategy in Rivera and Ba (2023), which also examined this court ruling and its impact on crime rates.

As before, we classify officers into four misconduct levels: no misconduct (0 complaints last year), low misconduct (1 complaint last year), medium misconduct (2 complaints last year), and high misconduct (3 or more complaints last year). Because the event study spans from 2013 to 2015, we use the number of complaints from 2013 to classify officer misconduct levels. This approach ensures that each officer's misconduct level remains constant throughout the entire analysis period.

We estimate this regression model separately for each misconduct group $g$:

$$r_{it}^{g} = \beta_0^{g} + \beta_1^{g} Event1_t + \beta_2^{g} Event2_t + a_i^{g} + \varepsilon_{it}^{g} \tag{4}$$

where $r_{it}$ is the outcome examined in Section 4 but adjusted for seasonal trends: the probability of making an arrest; the probability of making an index-crime arrest; the probability of making a non-index crime arrest; the number of verdicts; the share of arrests with no charges; the probability of having any guilty verdict; and the share of verdicts that are not guilty. Because there is no comparison group in an event study, we want to make sure that we are not capturing and conflating any seasonal trends in our estimates of the policy changes.[25] $Event1_t$ is an indicator variable equal to 1 if the day $t$ is in April 2014 or later and 0 otherwise. $Event2_t$ is an indicator variable equal to 1 if the day $t$ is in September 2014 or later and 0 otherwise. We exclude the months of the two events (March 2014 and August 2014) from our analysis. The reference period is September 2013 to February 2014, the six-month period before the *Kalven* ruling. The term $a_i^{g}$ denotes officer fixed effects. Standard errors are clustered by officer.

Table 5 reports results.[26] After the court ruling, we see a statistically significant drop across all officers with misconduct in the share of arrests with "not guilty" outcomes as well as a weak drop or no change in the share of arrests with no charges and no change in the probability of an arrest resulting in any guilty verdict. These results suggest that officers with misconduct may be changing the types of arrests they are making in the months following the court ruling relative to the

[25] Specifically, we regress the outcome variable on a set of month indicator variables only for the days outside our analysis period (the analysis period is September 1, 2013 to January 31, 2015): $y_{it} = \beta_0 + \delta_m + \varepsilon_{it}$, where $\delta_m$ are a set of month indicator variables. The residuals from this auxiliary regression ($r_{it}$) are used as the dependent variable in equation (4).

[26] Appendix Figure A3 depicts month-to-month changes.

months before the ruling. Specifically, the probability of an arrest does not change (same for index and non-index arrests) while the number of verdicts decreases, suggesting more straightforward offenses, and the share of arrests with "not guilty" outcomes goes down (all $p < 0.01$) while the probability of a "guilty" outcome does not change. The results reveal a gradient by misconduct level with the larger effect among low-misconduct officers and the smaller effect among high-misconduct officers. For example, low-misconduct officers see a 23.7% reduction in their share of arrests with "not guilty" outcomes while medium-misconduct officers see a 20% reduction and high-misconduct officers see a 15.7% reduction. Moreover, for low-misconduct officers, the share of arrests with no charges also decreases ($p < 0.1$) meaning that more arrests are prosecuted now relative to before. Last, allegations of officer misconduct (complaints) fall for all officers with misconduct (all $p < 0.01$), indicating that officers are indeed changing their behavior.

After the FOP newsletter, we see that officers with misconduct are less likely to make arrests, specifically for non-index crimes, though low-misconduct officers also reduce index arrests. There is no change in the number of verdicts and the probability of a "guilty" outcome, while the share of arrests with a "not guilty" outcome falls for some officers. The results again reveal a gradient by misconduct level. Specifically, low-misconduct officers reduce their arrests for non-index crimes by 15.3% ($p < 0.01$) whereas medium-misconduct officers reduce them by 12.8% ($p < 0.01$) and high-misconduct officers reduce them by 11.3% ($p < 0.05$). Further, the share of "not guilty" outcomes falls by 10.4% for low-misconduct officers and is statistically significant at the 1% level, while it falls by 8.4% for medium-misconduct officers but is not statistically significant and is 2% and not statistically significant for high-misconduct officers.

Overall, the results indicate that officers with misconduct are also reacting to the FOP newsletter, specifically in regards to their arresting behavior. The $p$-values on the $t$-test of the equality of coefficients for the court ruling (Event 1) and the FOP newsletter (Event 2) show that they are statistically significantly different at the 1% level for non-index arrests. While these officers with misconduct saw sustained reductions in their complaints after the FOP newsletter, they are not statistically significantly different from the reduced levels after the court ruling except among high-misconduct officers. High-misconduct officers saw their complaints drop even further by 60% ($p < 0.01$) after the FOP newsletter.[27]

[27] A potential alternative explanation is that the decrease is due to mean reversion since these officers had three or

In contrast, there is evidence that no-misconduct officers may be withdrawing from policing after the court ruling. Not only does the probability of an arrest go down, but so do the share of arrests with no charges, the probability of a "guilty" outcome, and the share of arrests with "not guilty" outcomes (all $p < 0.01$). After the FOP newsletter, no-misconduct officers appear to return back to their pre-court ruling arrest levels with the share of "not guilty" outcomes falling ($p < 0.01$).

Table 5: Impact of Increased Oversight on Conviction-less Arrests

| Outcome Variable: | Any Arrest (1) | Any Index (2) | Any Non-Index (3) | Num Verdicts (4) | Shr No Charge (5) | Any Guilty (6) | Shr Not Guilty (7) | Complaints (8) |
|---|---|---|---|---|---|---|---|---|
| *Panel A: No-Misconduct Officers* | | | | | | | | |
| Event 1 (Court Ruling) | -0.00311*** | -0.000283 | -0.00156*** | -0.00867*** | -0.000713*** | -0.00123*** | -0.00273*** | 0.000860*** |
| | (0.000600) | (0.000326) | (0.000502) | (0.000372) | (0.000225) | (0.000208) | (0.000213) | (8.48e-05) |
| Event 2 (FOP Newsletter) | -0.000711 | -0.000300 | -0.000564 | 0.000430 | -0.000204 | -0.000215 | -0.000793*** | 0.000978*** |
| | (0.000680) | (0.000323) | (0.000574) | (0.000355) | (0.000229) | (0.000197) | (0.000206) | (7.61e-05) |
| Observations | 1,030,952 | 1,030,952 | 1,030,952 | 1,030,952 | 1,030,952 | 1,030,952 | 1,030,952 | 1,030,952 |
| Reference Group Mean | 0.035 | 0.013 | 0.023 | 0.037 | 0.007 | 0.010 | 0.007 | 0.000 |
| $p$-value for $t$-test | | | | | | | | |
| Event 1 = Event 2 | 0.000 | 0.961 | 0.057 | 0.000 | 0.020 | 0.000 | 0.000 | 0.254 |
| *Panel B: Low-Misconduct Officers* | | | | | | | | |
| Event 1 (Court Ruling) | -0.00166 | 0.000822 | -0.00115 | -0.00865*** | -0.000807* | -0.000222 | -0.00261*** | -0.00112*** |
| | (0.00129) | (0.000617) | (0.00110) | (0.000751) | (0.000444) | (0.000405) | (0.000411) | (0.000211) |
| Event 2 (FOP Newsletter) | -0.00680*** | -0.00134** | -0.00566*** | 0.000762 | -0.000484 | -0.000447 | -0.00114*** | -0.00123*** |
| | (0.00130) | (0.000596) | (0.00111) | (0.000742) | (0.000460) | (0.000400) | (0.000385) | (0.000194) |
| Observations | 353,581 | 353,581 | 353,581 | 353,581 | 353,581 | 353,581 | 353,581 | 353,581 |
| Reference Group Mean | 0.053 | 0.016 | 0.037 | 0.058 | 0.011 | 0.014 | 0.011 | 0.003 |
| $p$-value for $t$-test | | | | | | | | |
| Event 1 = Event 2 | 0.000 | 0.001 | 0.000 | 0.000 | 0.502 | 0.608 | 0.000 | 0.590 |
| *Panel C: Medium-Misconduct Officers* | | | | | | | | |
| Event 1 (Court Ruling) | -0.00173 | -0.000281 | 7.03e-05 | -0.0101*** | -0.000961 | -9.41e-05 | -0.00365*** | -0.00329*** |
| | (0.00293) | (0.00135) | (0.00262) | (0.00196) | (0.00110) | (0.000926) | (0.00100) | (0.000444) |
| Event 2 (FOP Newsletter) | -0.00977*** | -0.00152 | -0.00808*** | 0.000105 | -0.000538 | -0.000440 | -0.00152 | -0.00269*** |
| | (0.00304) | (0.00130) | (0.00286) | (0.00179) | (0.00110) | (0.000858) | (0.000984) | (0.000491) |
| Observations | 104,534 | 104,534 | 104,534 | 104,534 | 104,534 | 104,534 | 104,534 | 104,534 |
| Reference Group Mean | 0.081 | 0.019 | 0.063 | 0.093 | 0.017 | 0.023 | 0.018 | 0.006 |
| $p$-value for $t$-test | | | | | | | | |
| Event 1 = Event 2 | 0.006 | 0.331 | 0.002 | 0.000 | 0.628 | 0.708 | 0.035 | 0.207 |
| *Panel D: High-Misconduct Officers* | | | | | | | | |
| Event 1 (Court Ruling) | -0.000719 | 0.000657 | 0.000396 | -0.00915*** | 0.000835 | 0.000390 | -0.00393** | -0.00362*** |
| | (0.00444) | (0.00192) | (0.00405) | (0.00320) | (0.00159) | (0.00160) | (0.00170) | (0.000940) |
| Event 2 (FOP Newsletter) | -0.00882* | 0.00138 | -0.0105** | -0.00243 | -0.000530 | -0.00181 | -0.000510 | -0.00603*** |
| | (0.00474) | (0.00180) | (0.00462) | (0.00294) | (0.00147) | (0.00171) | (0.00167) | (0.000905) |
| Observations | 53,772 | 53,772 | 53,772 | 53,772 | 53,772 | 53,772 | 53,772 | 53,772 |
| Reference Group Mean | 0.113 | 0.021 | 0.093 | 0.147 | 0.020 | 0.035 | 0.025 | 0.010 |
| $p$-value for $t$-test | | | | | | | | |
| Event 1 = Event 2 | 0.062 | 0.663 | 0.008 | 0.026 | 0.420 | 0.192 | 0.053 | 0.008 |

Notes: This table reports coefficient estimates for $\beta_1^g$ and $\beta_2^g$ from equation (4) for various outcomes, separately for each misconduct group $g$. All outcomes have been adjusted for seasonal trends, and all estimates also include officer fixed effects. Standard errors clustered by officer are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The findings reveal distinct behavioral changes among police officers following the court ruling and the FOP President's letter in the union newsletter. After the court ruling, officers with misconduct histories maintained arrest rates but shifted their focus, evidenced by fewer "not guilty" outcomes without corresponding increases in "guilty" verdicts. This behavioral adaptation is further

more complaints the previous year, but then we do not see similar increases for no or low-misconduct officers.

supported by decreased complaint rates across all misconduct categories. The subsequent FOP newsletter prompted additional changes, particularly reduced non-index crime arrests among officers with misconduct, with effects varying inversely by misconduct level—low-misconduct officers showing the strongest response. Although high-misconduct officers showed the weakest changes to their arrest behavior, they experienced an additional 60% reduction in complaints following the newsletter. Meanwhile, no-misconduct officers exhibited a different pattern: initially withdrawing from policing activities after the court ruling (demonstrated by decreased arrests and changes in court outcomes), but returning to pre-ruling arresting behavior patterns following the newsletter. These contrasting responses highlight how different segments of the police force may adjust their enforcement activities in response to accountability mechanisms.

# 7 Conclusion

This paper examines whether police departments and policymakers can reduce conviction-less arrests by targeting officers with greater misconduct, as measured by civilian allegations of misconduct. Our focus on complaints stems from prior work that finds that civilian complaints are meaningful indicators of police misconduct (Stoddard et al., 2024). Although past papers have examined force use by the worst offenders, they do not examine arrest behavior or the court outcomes of these arrests (e.g., Rozema and Schanzenbach, 2019; Chalfin and Kaplan, 2021).

Using the Chicago Police Department's rotational duty calendar to obtain plausibly exogenous variation in the set of officers assigned to work on a particular day, we establish an arguably causal relationship between officer misconduct and conviction-less arrests. On the same shift, high-misconduct officers are 3.7 percentage points (80% of the mean) more likely than officers with no misconduct to make an arrest and 2.7 percentage points (133% of the mean) more likely to make an arrest for less serious crimes. More importantly, they are also 0.11 percentage points (11.8% of the mean) more likely to make an arrest that results in no charges and 0.25 percentage points (31% of the mean) more likely to make an arrest with a "not guilty" outcome compared no-misconduct officers.

Our event study analysis finds that increased transparency through public disclosure of complaint records can influence officer behavior—though with varying effectiveness across misconduct levels. While low-misconduct officers appear more responsive to oversight mechanisms, high-

misconduct officers are less responsive. This result has important policy implications as it demonstrates how transparency requirements may improve policing outcomes at minimal cost. Further, the potential for a nationwide impact is substantial, given that police misconduct records remain confidential in 23 states, have limited availability in 15 states, and are fully public in only 12 states—with many of these 12 still restricting access to unsubstantiated complaints or ongoing investigations.

Our findings also have policy implications for police departments. Despite the predictive power of complaints against officers, many departments do not effectively use them as monitoring tools. One reason may be departmental resource constraints that prevent comprehensive officer monitoring (Black et al., 2017, p. 9). Another issue is that being active on the job and complaints are positively correlated. Therefore, an effective policy is not one that simply seeks to reduce all complaints but rather only those related to improper police behavior. Our findings reveal that increased transparency of officer misconduct has the potential to reduce conviction-less arrests, but mainly among officers with low misconduct. If policymakers and police departments wish to influence the behavior of high-misconduct officers, a more targeted approach may be necessary.

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# Online Appendix for

# “The Relationship between Officer Misconduct and Conviction-less Arrests”

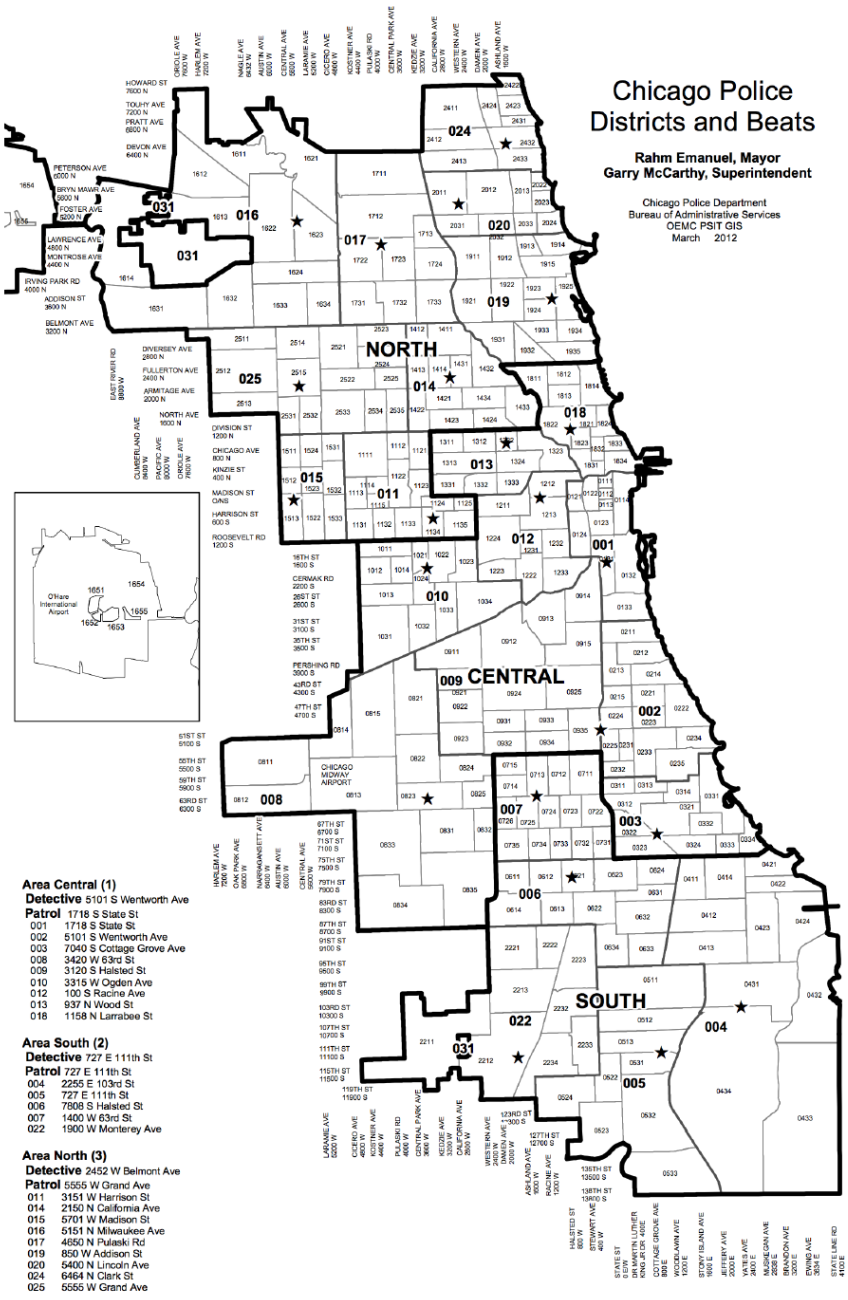
# A Additional Figures and Tables

Table A1: Descriptive Statistics: All Officers

| | | Number of complaints last year: | | | |
|---|---|---|---|---|---|
| | Overall | 0 | 1 | 2 | 3+ |
| Unique number of officers | 11,606 | 11,060 | 5,913 | 2,690 | 1,565 |
| Average share of officers | 1.000 | 0.697 | 0.193 | 0.067 | 0.043 |
| Share with any arrest | 0.093 | 0.036 | 0.065 | 0.089 | 0.126 |
| Mean number of arrests | 1.219 | 1.139 | 1.166 | 1.201 | 1.265 |
| | (0.604) | (0.484) | (0.534) | (0.586) | (0.712) |
| Mean number of verdicts | 1.193 | 1.097 | 1.129 | 1.187 | 1.284 |
| | (0.952) | (0.855) | (0.893) | (0.942) | (1.039) |

Notes: This table depicts summary statistics on all Chicago Police Department officers between 2010 and 2016. "Unique number of officers" is the unique number of officers between 2010-2016 in the relevant misconduct category. "Average share of officers" is the average over years. "Share with any arrest" is the share of all officer-shifts between 2010-2016 with any primary arrests. The mean number of arrests and verdicts are conditional on making an arrest. Standard deviations are in parentheses.

Figure A1: Map of Chicago Police Department Districts and Beats




Notes: This map outlines the geographic boundaries of the CPD districts and their corresponding beats in 2012. There were 25 districts in 2012; three districts closed by 2013.

Figure A2: Distribution of Officer Tenure by Watch


Notes: This figure shows the distribution of officer tenure by watch assignments.

Table A2: Does misconduct predict which day of the week an officer works? Alternative Specifications

| Outcome Variable: | Monday (1) | Tuesday (2) | Wednesday (3) | Thursday (4) | Friday (5) | Saturday (6) | Sunday (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Patrol Officer Sample, Medium/High Misconduct vs Not* | | | | | | | |
| One or more Complaints Last Week | -6.22e-05 | 0.000357 | 0.00128 | 0.000240 | -0.00115* | -4.75e-05 | -0.000625 |
| | (0.000710) | (0.000497) | (0.000813) | (0.000521) | (0.000662) | (0.000855) | (0.000683) |
| Constant | 0.139*** | 0.142*** | 0.144*** | 0.144*** | 0.145*** | 0.144*** | 0.141*** |
| | (2.50e-05) | (1.72e-05) | (3.12e-05) | (1.65e-05) | (2.43e-05) | (3.37e-05) | (2.30e-05) |
| Observations | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 |
| R-squared | 0.004 | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.003 |
| *Panel B: Patrol Officer Sample, High Misconduct vs Not* | | | | | | | |
| Two or more Complaints Last Week | 0.00147 | -0.00190 | 0.00220 | 0.000686 | -0.00402 | 0.00324 | -0.00168 |
| | (0.00331) | (0.00276) | (0.00381) | (0.00255) | (0.00305) | (0.00401) | (0.00321) |
| Constant | 0.139*** | 0.142*** | 0.144*** | 0.144*** | 0.145*** | 0.144*** | 0.141*** |
| | (4.10e-06) | (3.60e-06) | (5.33e-06) | (2.71e-06) | (3.95e-06) | (6.32e-06) | (3.89e-06) |
| Observations | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 | 3,146,453 |
| R-squared | 0.004 | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.003 |
| *Panel C: All Officer Sample, Linear Measure of Complaints* | | | | | | | |
| Total Complaints Last Week | 0.000445 | 0.000458 | 0.000808** | 0.000249 | -0.000867*** | -0.000456 | -0.000638** |
| | (0.000326) | (0.000401) | (0.000346) | (0.000271) | (0.000270) | (0.000384) | (0.000278) |
| Constant | 0.122*** | 0.144*** | 0.154*** | 0.154*** | 0.153*** | 0.147*** | 0.126*** |
| | (7.05e-06) | (1.21e-05) | (1.16e-05) | (7.92e-06) | (8.23e-06) | (1.27e-05) | (5.05e-06) |
| Observations | 8,582,265 | 8,582,265 | 8,582,265 | 8,582,265 | 8,582,265 | 8,582,265 | 8,582,265 |
| R-squared | 0.012 | 0.004 | 0.002 | 0.002 | 0.002 | 0.003 | 0.010 |

Notes: This table reports estimates for the number of complaints last week on the probability of working on a specific day of the week. In Panels A and B, medium-misconduct is defined as 1 complaint last week and high-misconduct is defined as 2 or more complaints last week. All estimates also include fixed effects for officer, district, and year. Standard errors clustered by officer and district are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A3: Are officers with misconduct able to choose which days to work based on expected crime? Alternative Specifications

| Outcome Variable: | Memorial Day (1) | Fourth of July (2) | Labor Day (3) | St. Patty's Day (4) | New Year's (5) |
|---|---|---|---|---|---|
| *Panel A: Patrol Officer Sample, Medium/High Misconduct vs Not* | | | | | |
| Two or more Complaints Last Year | 0.000274 | -0.00491 | -0.00523 | 0.00855 | 0.0131 |
| | (0.0101) | (0.0110) | (0.00794) | (0.00847) | (0.00993) |
| Observations | 26,207 | 26,207 | 26,207 | 26,207 | 26,207 |
| Mean Outcome | 0.248 | 0.237 | 0.250 | 0.244 | 0.411 |
| *Panel B: Patrol Officer Sample, High Misconduct vs Not* | | | | | |
| Three or more Complaints Last Year | 0.0108 | -0.000354 | -0.000588 | -0.0111 | 0.0196 |
| | (0.0200) | (0.0186) | (0.0161) | (0.0212) | (0.0174) |
| Observations | 26,207 | 26,207 | 26,207 | 26,207 | 26,207 |
| Mean Outcome | 0.248 | 0.237 | 0.250 | 0.244 | 0.411 |
| *Panel C: All Officer Sample, Linear Measure of Complaints* | | | | | |
| Total Complaints Last Year | -0.00569 | -0.00674* | -0.00343 | -0.00313 | -0.00256 |
| | (0.00386) | (0.00346) | (0.00394) | (0.00349) | (0.00302) |
| Observations | 41,284 | 41,284 | 41,284 | 41,284 | 41,284 |
| Mean Outcome | 0.438 | 0.422 | 0.433 | 0.450 | 0.665 |

Notes: This table reports estimates for the number of complaints last year on the probability of working on a specific holiday the following year. New Year's includes working on New Year's Eve and New Year Day. In Panels A and B, medium-misconduct is defined as 2 complaints last year and high-misconduct is defined as 3 or more complaints last year. The sample is at the officer-year level. All estimates also control for officer tenure at the start of the year and include fixed effects for officer, modal district, and year. Standard errors clustered by officer and district are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A4: Does misconduct predict watch for officers? Alternative Specifications

| Outcome Variable: | Watch 1 (1) | Watch 2 (2) | Watch 3 (3) |
|---|---|---|---|
| *Panel A: Patrol Officer Sample, Medium/High Misconduct vs Not* | | | |
| Two or more Complaints Last Year | 0.00240 | 0.00613 | -0.00237 |
| | (0.00902) | (0.00843) | (0.00755) |
| Observations | 18,725 | 18,725 | 18,725 |
| Mean Outcome | 0.347 | 0.334 | 0.442 |
| *Panel B: Patrol Officer Sample, High Misconduct vs Not* | | | |
| Three or more Complaints Last Year | -0.00229 | 0.0153 | -0.00428 |
| | (0.0105) | (0.0144) | (0.0107) |
| Observations | 18,725 | 18,725 | 18,725 |
| Mean Outcome | 0.347 | 0.334 | 0.442 |
| *Panel C: All Officer Sample, Linear Measure of Complaints* | | | |
| Total Complaints Last Year | -0.00385* | -0.000390 | -0.00102 |
| | (0.00210) | (0.00235) | (0.00251) |
| Observations | 30,926 | 30,926 | 30,926 |
| Mean Outcome | 0.289 | 0.354 | 0.393 |

Notes: This table reports estimates for the number of complaints last year on the probability of working a given watch the following year. Watch 1 is 12AM-8AM, Watch 2 is 8AM-4PM, and Watch 3 is 4PM-12AM. In Panels A and B, medium-misconduct is defined as 2 complaints last year and high-misconduct is defined as 3 or more complaints last year. The sample is at the officer-year level. All estimates also control for officer watch last year, officer tenure at the start of the year, officer total arrests two years ago, and include fixed effects for officer, modal district, and year. Standard errors clustered by officer and district are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A5: Impact of Officer Misconduct on Conviction-less Arrests

| Outcome Variable: | Any Arrest (1) | Any Index (2) | Any Non-Index (3) | Num Verdicts (4) | Shr No Charge (5) | Any Guilty (6) | Shr Not Guilty (7) |
|---|---|---|---|---|---|---|---|
| Low Misconduct | 0.00644*** | 0.00118*** | 0.00485*** | 0.000593** | 0.000379*** | 0.000162 | 0.000226* |
| | (0.000317) | (0.000218) | (0.000219) | (0.000239) | (0.000126) | (0.000126) | (0.000118) |
| Medium Misconduct | 0.0175*** | 0.00359*** | 0.0125*** | 0.00217*** | 0.00132*** | 0.000565** | 0.000766*** |
| | (0.000562) | (0.000365) | (0.000410) | (0.000466) | (0.000232) | (0.000227) | (0.000214) |
| High Misconduct | 0.0366*** | 0.00539*** | 0.0265*** | 0.00905*** | 0.00106*** | -0.000196 | 0.00249*** |
| | (0.000884) | (0.000529) | (0.000661) | (0.000750) | (0.000361) | (0.000353) | (0.000343) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.046 | 0.024 | 0.020 | 0.048 | 0.009 | 0.012 | 0.008 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.000 | 0.000 | 0.000 | 0.067 | 0.322 | 0.000 |
| High = Medium | 0.000 | 0.003 | 0.000 | 0.000 | 0.524 | 0.061 | 0.000 |

Notes: This table reports coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for various outcomes. All estimates also control for officer tenure and shift. Columns 4-7 also include the number of index arrests and non-index arrests, and columns 6-7 include the number of verdicts. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A6: Impact of Officer Misconduct on Number of Arrests

| Outcome Variable: | Num Arrests (1) | Num Index (2) | Num Non-Index (3) |
|---|---|---|---|
| Low Misconduct | 0.00734*** | 0.00120*** | 0.00544*** |
| | (0.000371) | (0.000242) | (0.000256) |
| Medium Misconduct | 0.0206*** | 0.00419*** | 0.0143*** |
| | (0.000675) | (0.000415) | (0.000489) |
| High Misconduct | 0.0442*** | 0.00629*** | 0.0310*** |
| | (0.00110) | (0.000608) | (0.000814) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.051 | 0.025 | 0.022 |
| $p$-value of $t$-test: | | | |
| High = Low | 0.000 | 0.000 | 0.000 |
| High = Medium | 0.000 | 0.003 | 0.000 |

Notes: This table reports coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for the number of arrests (column 1), the number of index-crime arrests (column 2), and the number of non-index crime arrests (column 3). All estimates also control for officer tenure and shift. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A7: Impact of Officer Misconduct on Court Outcomes by Arrest Type

| Outcome Variable: | Num Verdicts (1) | Shr No Charge (2) | Any Guilty (3) | Shr Not Guilty (4) |
|---|---|---|---|---|
| *Panel A: Index Arrests* | | | | |
| Low Misconduct | 0.00665 | -0.00231 | -0.000670 | 0.000254 |
| | (0.00890) | (0.00359) | (0.00616) | (0.00561) |
| Medium Misconduct | -0.0255* | 0.00828 | 0.0146 | -0.00403 |
| | (0.0135) | (0.00519) | (0.00909) | (0.00827) |
| High Misconduct | 0.00386 | 0.00137 | -0.0129 | 0.0157 |
| | (0.0146) | (0.00651) | (0.0112) | (0.0106) |
| Observations | 44,987 | 44,987 | 44,987 | 44,987 |
| Reference Group Mean | 1.182 | 0.063 | 0.277 | 0.176 |
| $p$-value of $t$-test: | | | | |
| High = Low | 0.855 | 0.588 | 0.294 | 0.157 |
| High = Medium | 0.100 | 0.364 | 0.038 | 0.105 |
| *Panel B: Non-Index Arrests* | | | | |
| Low Misconduct | 0.00595 | -0.00957 | -0.00141 | 0.00981* |
| | (0.0106) | (0.00681) | (0.00547) | (0.00538) |
| Medium Misconduct | 0.00527 | -0.00607 | -0.0120* | 0.0211*** |
| | (0.0147) | (0.00885) | (0.00728) | (0.00712) |
| High Misconduct | 0.0481*** | -0.0254*** | -0.0110 | 0.0311*** |
| | (0.0168) | (0.00970) | (0.00816) | (0.00816) |
| Observations | 47,407 | 47,407 | 47,407 | 47,407 |
| Reference Group Mean | 0.904 | 0.339 | 0.241 | 0.158 |
| $p$-value of $t$-test: | | | | |
| High = Low | 0.015 | 0.115 | 0.253 | 0.012 |
| High = Medium | 0.027 | 0.084 | 0.916 | 0.297 |

Notes: This table reports coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for court outcomes, separately by arrest-type. The court outcomes are: the number of verdicts (column 1), the share of arrests with no charge (column 2), the probability of an arrest having any guilty outcome (column 3), and the share of an arrest's verdicts with a not guilty outcome (column 4). All estimates also control for officer tenure, shift, and the number of index or non-index arrests. Columns 3-4 also include the number of verdicts. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A8: Impact of Officer Misconduct on Conviction-less Arrests by Alternative Misconduct Definitions

| Outcome Variable: | Any Arrest (1) | Any Index (2) | Any Non-Index (3) | Num Verdicts (4) | Shr No Charge (5) | Any Guilty (6) | Shr Not Guilty (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Serious Complaints* | | | | | | | |
| Low Misconduct | 0.0145*** | 0.00276*** | 0.0106*** | 0.00144*** | 0.00112*** | 0.000666*** | 0.000310** |
| | (0.000406) | (0.000264) | (0.000294) | (0.000320) | (0.000165) | (0.000163) | (0.000151) |
| Medium Misconduct | 0.0368*** | 0.00638*** | 0.0272*** | 0.00616*** | 0.00203*** | 0.000350 | 0.00207*** |
| | (0.000978) | (0.000586) | (0.000751) | (0.000846) | (0.000405) | (0.000390) | (0.000377) |
| High Misconduct | 0.0628*** | 0.00937*** | 0.0445*** | 0.0186*** | 0.000914 | -0.000519 | 0.00555*** |
| | (0.00167) | (0.000936) | (0.00130) | (0.00147) | (0.000718) | (0.000681) | (0.000686) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.047 | 0.024 | 0.020 | 0.049 | 0.009 | 0.012 | 0.008 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.000 | 0.000 | 0.000 | 0.774 | 0.088 | 0.000 |
| High = Medium | 0.000 | 0.006 | 0.000 | 0.000 | 0.168 | 0.263 | 0.000 |
| *Panel B: Personnel Conduct Complaints* | | | | | | | |
| Low Misconduct | 0.00495*** | 0.000925*** | 0.00353*** | 0.00167*** | -0.000405** | 0.000308 | 3.15e-06 |
| | (0.000481) | (0.000324) | (0.000335) | (0.000379) | (0.000190) | (0.000193) | (0.000180) |
| Medium Misconduct | 0.0161*** | 0.00130 | 0.0116*** | 0.00642*** | 0.000685 | -0.000257 | 0.00107* |
| | (0.00167) | (0.00104) | (0.00121) | (0.00142) | (0.000666) | (0.000668) | (0.000651) |
| High Misconduct | 0.0264*** | 0.0106*** | 0.0178*** | -0.0130*** | 0.00527** | -0.000692 | 0.00287 |
| | (0.00515) | (0.00350) | (0.00393) | (0.00388) | (0.00223) | (0.00197) | (0.00207) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.052 | 0.025 | 0.023 | 0.054 | 0.010 | 0.014 | 0.009 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.001 | 0.006 | 0.000 | 0.000 | 0.011 | 0.613 | 0.168 |
| High = Medium | 0.056 | 0.011 | 0.133 | 0.000 | 0.049 | 0.835 | 0.411 |
| *Panel C: Failure to Provide Service Complaints* | | | | | | | |
| Low Misconduct | 0.000951** | 0.000353 | 0.000491* | -0.000860*** | 0.000523*** | -0.000377** | 3.36e-06 |
| | (0.000406) | (0.000276) | (0.000278) | (0.000316) | (0.000161) | (0.000162) | (0.000153) |
| Medium Misconduct | 0.00363*** | 0.00150** | 0.00282*** | -0.000332 | 0.000129 | 0.000202 | -6.33e-05 |
| | (0.00106) | (0.000732) | (0.000726) | (0.000863) | (0.000412) | (0.000435) | (0.000403) |
| High Misconduct | -0.00200 | -0.00521*** | 0.00154 | 0.00208 | 0.000972 | -0.00129 | 0.000989 |
| | (0.00271) | (0.00176) | (0.00188) | (0.00217) | (0.00107) | (0.00103) | (0.00104) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.052 | 0.025 | 0.024 | 0.055 | 0.010 | 0.014 | 0.009 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.280 | 0.002 | 0.578 | 0.179 | 0.677 | 0.379 | 0.348 |
| High = Medium | 0.052 | 0.000 | 0.524 | 0.303 | 0.459 | 0.181 | 0.345 |

Notes: This table reports coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for various outcomes, separately by alternative misconduct definitions. Panel A defines misconduct using "serious" complaints, which include allegations of use of force violations, arrest/lockup violations, and search violations. Panel B defines misconduct using complaints on failure to provide service. Panel C defines misconduct using complaints about personnel conduct, which relate to the officer's general professional conduct and behavior such as being absent without permission or the possession/sale of drugs. All estimates also control for officer tenure and shift. Columns 4-7 also include the number of index arrests and non-index arrests, and columns 6-7 include the number of verdicts. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A9: Impact of Officer Misconduct on Conviction-less Arrests by Primary and Secondary Arresting Officers

| Outcome Variable: | Any Arrest (1) | Any Index (2) | Any Non-Index (3) | Num Verdicts (4) | Shr No Charge (5) | Any Guilty (6) | Shr Not Guilty (7) |
|---|---|---|---|---|---|---|---|
| Low Misconduct | 0.00788*** | 0.000716** | 0.00672*** | 0.00133*** | 0.000234 | 3.92e-05 | 0.000440*** |
| | (0.000423) | (0.000298) | (0.000297) | (0.000339) | (0.000173) | (0.000175) | (0.000164) |
| Medium Misconduct | 0.0225*** | 0.00410*** | 0.0171*** | 0.00358*** | 0.00158*** | 0.000734** | 0.000996*** |
| | (0.000739) | (0.000492) | (0.000546) | (0.000650) | (0.000311) | (0.000311) | (0.000289) |
| High Misconduct | 0.0441*** | 0.00487*** | 0.0344*** | 0.0127*** | 0.00107** | -0.000187 | 0.00337*** |
| | (0.00113) | (0.000706) | (0.000854) | (0.00102) | (0.000468) | (0.000477) | (0.000453) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.090 | 0.046 | 0.039 | 0.097 | 0.017 | 0.024 | 0.015 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.000 | 0.000 | 0.000 | 0.079 | 0.642 | 0.000 |
| High = Medium | 0.000 | 0.337 | 0.000 | 0.000 | 0.334 | 0.087 | 0.000 |

Notes: This table reports coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for various outcomes on arrests made by primary and secondary arresting officers. All estimates also control for officer tenure and shift. Columns 4-7 also include the number of index arrests and non-index arrests, and columns 6-7 include the number of verdicts. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A10: Impact of Officer Misconduct on Conviction-less Arrests by Officer Tenure

| Outcome Variable: | Any Arrest (1) | Any Index (2) | Any Non-Index (3) | Num Verdicts (4) | Shr No Charge (5) | Any Guilty (6) | Shr Not Guilty (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Tenure: < 8 years* | | | | | | | |
| Low Misconduct | 0.00911*** | 0.00175*** | 0.00636*** | 0.00188*** | 0.000356 | 5.22e-05 | 0.000588** |
| | (0.000667) | (0.000418) | (0.000497) | (0.000526) | (0.000283) | (0.000265) | (0.000256) |
| Medium Misconduct | 0.0139*** | 0.00264*** | 0.0102*** | 0.00353*** | -0.000125 | 0.000191 | 0.000573 |
| | (0.00102) | (0.000628) | (0.000778) | (0.000874) | (0.000435) | (0.000421) | (0.000398) |
| High Misconduct | 0.0349*** | 0.00402*** | 0.0245*** | 0.0123*** | -0.000352 | -5.09e-05 | 0.00299*** |
| | (0.00148) | (0.000864) | (0.00113) | (0.00128) | (0.000616) | (0.000605) | (0.000592) |
| Observations | 1,125,475 | 1,125,475 | 1,125,475 | 1,125,475 | 1,125,475 | 1,125,475 | 1,125,475 |
| Mean Outcome | 0.062 | 0.027 | 0.031 | 1.014 | 0.217 | 0.254 | 0.168 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.011 | 0.000 | 0.000 | 0.264 | 0.867 | 0.000 |
| High = Medium | 0.000 | 0.166 | 0.000 | 0.000 | 0.746 | 0.727 | 0.000 |
| *Panel B: 8-13 years* | | | | | | | |
| Low Misconduct | 0.00414*** | 0.000920** | 0.00351*** | -0.000652 | 0.000398 | 0.000254 | 9.52e-05 |
| | (0.000631) | (0.000438) | (0.000423) | (0.000478) | (0.000245) | (0.000252) | (0.000233) |
| Medium Misconduct | 0.0225*** | 0.00480*** | 0.0154*** | 0.00527*** | 0.000993** | 0.000682 | 0.00200*** |
| | (0.00120) | (0.000774) | (0.000853) | (0.00101) | (0.000495) | (0.000477) | (0.000459) |
| High Misconduct | 0.0399*** | 0.00580*** | 0.0315*** | 0.00700*** | 0.00132* | 0.000847 | 0.00146** |
| | (0.00171) | (0.00104) | (0.00129) | (0.00136) | (0.000683) | (0.000673) | (0.000652) |
| Observations | 851,060 | 851,060 | 851,060 | 851,060 | 851,060 | 851,060 | 851,060 |
| Mean Outcome | 0.043 | 0.023 | 0.017 | 1.061 | 0.183 | 0.269 | 0.163 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.000 | 0.000 | 0.000 | 0.190 | 0.390 | 0.041 |
| High = Medium | 0.000 | 0.412 | 0.000 | 0.284 | 0.688 | 0.834 | 0.479 |
| *Panel C: Tenure 13-18 years* | | | | | | | |
| Low Misconduct | 0.00433*** | 0.000344 | 0.00382*** | 0.000592 | 0.000268 | 0.000197 | 8.15e-05 |
| | (0.000641) | (0.000492) | (0.000395) | (0.000455) | (0.000234) | (0.000261) | (0.000238) |
| Medium Misconduct | 0.0117*** | 0.00156* | 0.00907*** | 0.00107 | 0.00114** | 0.000539 | 0.000596 |
| | (0.00118) | (0.000848) | (0.000789) | (0.000889) | (0.000454) | (0.000477) | (0.000440) |
| High Misconduct | 0.0408*** | 0.00672*** | 0.0281*** | 0.0103*** | 0.00337*** | -0.00106 | 0.00307*** |
| | (0.00229) | (0.00149) | (0.00158) | (0.00222) | (0.000914) | (0.000902) | (0.000873) |
| Observations | 737,334 | 737,334 | 737,334 | 737,334 | 737,334 | 737,334 | 737,334 |
| Mean Outcome | 0.036 | 0.022 | 0.012 | 1.084 | 0.155 | 0.280 | 0.167 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.000 | 0.000 | 0.000 | 0.001 | 0.171 | 0.001 |
| High = Medium | 0.000 | 0.002 | 0.000 | 0.000 | 0.027 | 0.113 | 0.010 |
| *Panel D: Tenure: 18 or more yrs* | | | | | | | |
| Low Misconduct | 0.00339*** | 0.00130** | 0.00209*** | -0.00174*** | 0.000824*** | -2.68e-05 | 0.000159 |
| | (0.000793) | (0.000634) | (0.000471) | (0.000518) | (0.000279) | (0.000316) | (0.000293) |
| Medium Misconduct | 0.0160*** | 0.00474*** | 0.0103*** | -0.00327*** | 0.00348*** | 0.000737 | -0.000119 |
| | (0.00151) | (0.00114) | (0.000941) | (0.00100) | (0.000574) | (0.000587) | (0.000540) |
| High Misconduct | 0.0175*** | 0.00724*** | 0.00992*** | 0.00115 | -0.000373 | -0.000300 | 0.00184* |
| | (0.00236) | (0.00182) | (0.00146) | (0.00158) | (0.000842) | (0.000938) | (0.000967) |
| Observations | 452,310 | 452,310 | 452,310 | 452,310 | 452,310 | 452,310 | 452,310 |
| Mean Outcome | 0.031 | 0.020 | 0.010 | 1.083 | 0.155 | 0.282 | 0.163 |
| $p$-value of $t$-test: | | | | | | | |
| High = Low | 0.000 | 0.001 | 0.000 | 0.072 | 0.165 | 0.775 | 0.087 |
| High = Medium | 0.582 | 0.230 | 0.831 | 0.015 | 0.000 | 0.343 | 0.071 |

Notes: This table depicts coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (1) for various outcomes, separately by officer tenure. Panel A reports estimates for officers with less than 8 years of tenure (25th percentile or below). Panel B report estimates for officers with 8-13 years of tenure (25-50th percentile). Panel C reports estimates for officers with 13-18 years of tenure (50-75th percentile). Panel D reports estimates for officers with 18 or more years of tenure (75th percentile or above). All estimates control for officer tenure and shift. Columns 4-7 also include the number of index arrests and non-index arrests, while Columns 6-7 also include the number of verdicts. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A11: Impact of Officer Misconduct on Conviction-less Arrests by "Bad Apple" status

| Outcome Variable: | Any Arrest (1) | Any Index (2) | Any Non-Index (3) | Num Verdicts (4) | Shr No Charge (5) | Any Guilty (6) | Shr Not Guilty (7) |
|---|---|---|---|---|---|---|---|
| Low Misconduct | 0.00479*** | 0.000822*** | 0.00359*** | 0.000275 | 0.000534*** | 0.000165 | -2.14e-05 |
| | (0.000320) | (0.000223) | (0.000218) | (0.000241) | (0.000126) | (0.000127) | (0.000119) |
| Medium Misconduct | 0.0118*** | 0.00288*** | 0.00837*** | 0.000228 | 0.00128*** | 0.000225 | 0.000404* |
| | (0.000590) | (0.000395) | (0.000420) | (0.000469) | (0.000241) | (0.000237) | (0.000224) |
| High Misconduct | 0.0175*** | 0.00346*** | 0.0124*** | 0.00256*** | 0.000968** | -1.24e-05 | 0.000181 |
| | (0.00105) | (0.000689) | (0.000758) | (0.000857) | (0.000416) | (0.000426) | (0.000403) |
| 95th Percentile of Complaints | 0.0320*** | 0.00556*** | 0.0231*** | 0.00727*** | 0.000909* | 0.000649 | 0.000797* |
| | (0.00118) | (0.000728) | (0.000890) | (0.000994) | (0.000498) | (0.000485) | (0.000450) |
| 95th pctile × Low | 0.00179 | 0.00125 | 0.00216* | -0.000257 | -0.00238*** | -0.000394 | 0.00252*** |
| | (0.00168) | (0.00104) | (0.00129) | (0.00136) | (0.000706) | (0.000695) | (0.000662) |
| 95th pctile × Medium | 0.00764*** | -0.000153 | 0.00542*** | 0.00520*** | -0.000511 | 0.00136* | 0.00148* |
| | (0.00194) | (0.00117) | (0.00149) | (0.00175) | (0.000835) | (0.000814) | (0.000768) |
| 95th pctile × High | 0.0125*** | -0.000689 | 0.00973*** | 0.00746*** | -0.000667 | -0.000943 | 0.00426*** |
| | (0.00206) | (0.00124) | (0.00156) | (0.00179) | (0.000870) | (0.000850) | (0.000820) |
| Observations | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 | 3,209,376 |
| Reference Group Mean | 0.045 | 0.023 | 0.019 | 0.047 | 0.008 | 0.012 | 0.007 |

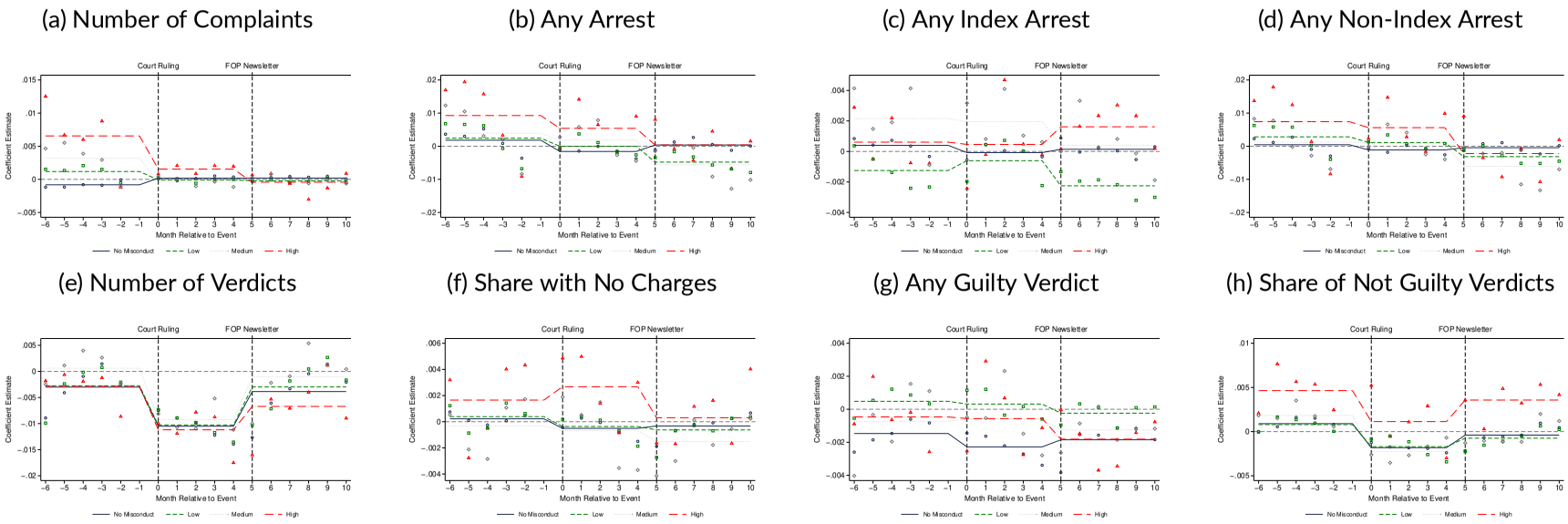
Notes: This table depicts coefficient estimates for $\beta_1$, $\beta_2$, and $\beta_3$ from equation (2) for various outcomes. "Bad apples" are defined as officers in the 95th percentile of the rate of complaints between 2010-2016. All estimates control for officer tenure and shift. Columns 4-7 also include the number of arrests, while Columns 6-7 also include the number of verdicts. Standard errors clustered by shift are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A12: Impact of Partner Misconduct on Conviction-less Arrests

| Outcome Variable: | Complaints (1) | Any Arrest (2) | Any Index (3) | Any Non-Index (4) | Num Verdicts (5) | Shr No Charge (6) | Any Guilty (7) | Shr Not Guilty (8) |
|---|---|---|---|---|---|---|---|---|
| *Own Misconduct* | | | | | | | | |
| Low Misconduct | -0.000809*** | -0.000301 | -0.000583 | 0.000734 | -3.98e-05 | -0.000238 | 9.65e-05 | -7.71e-05 |
| | (0.000112) | (0.000673) | (0.000406) | (0.000500) | (0.000523) | (0.000252) | (0.000215) | (0.000207) |
| Medium Misconduct | -0.00154*** | 0.00298** | 0.000175 | 0.00291** | -0.00113 | 0.000607 | -0.000303 | 0.000418 |
| | (0.000221) | (0.00142) | (0.000789) | (0.00115) | (0.00107) | (0.000503) | (0.000419) | (0.000415) |
| High Misconduct | -0.00190*** | -0.000753 | -0.000272 | 0.000355 | -0.000955 | -0.000340 | 0.000132 | -0.000833 |
| | (0.000490) | (0.00236) | (0.00114) | (0.00189) | (0.00162) | (0.000981) | (0.000771) | (0.000688) |
| *Partner Misconduct* | | | | | | | | |
| Low-Misconduct Partner | 0.000116 | -0.000788 | -0.00122*** | 0.000353 | 0.000476 | -0.000227 | 0.000179 | 9.22e-06 |
| | (0.000103) | (0.000601) | (0.000374) | (0.000415) | (0.000411) | (0.000217) | (0.000202) | (0.000185) |
| Medium-Misconduct Partner | 4.04e-05 | -0.000228 | 6.63e-06 | 0.000615 | -0.00145* | 0.000187 | -0.000247 | 0.000828** |
| | (0.000187) | (0.00103) | (0.000653) | (0.000759) | (0.000783) | (0.000389) | (0.000355) | (0.000351) |
| High-Misconduct Partner | 0.000485 | 0.000936 | -0.00148 | 0.00267** | -0.000746 | -0.000376 | -3.99e-05 | 0.000723 |
| | (0.000314) | (0.00171) | (0.000963) | (0.00126) | (0.00119) | (0.000630) | (0.000586) | (0.000566) |
| *Interactions (Own x Partner)* | | | | | | | | |
| Low × Low-Partner | -0.000123 | 0.00188* | 0.00101 | 0.00110 | -0.000925 | 0.000340 | -0.000256 | -0.000116 |
| | (0.000192) | (0.00110) | (0.000665) | (0.000827) | (0.000806) | (0.000419) | (0.000378) | (0.000355) |
| Low × Medium-Partner | 0.000519 | 0.00163 | 0.000120 | 0.00151 | 0.00137 | -0.000337 | 0.000532 | -0.00192*** |
| | (0.000338) | (0.00213) | (0.00109) | (0.00174) | (0.00160) | (0.000845) | (0.000604) | (0.000591) |
| Low × High-Partner | -7.84e-05 | 0.00327 | -2.50e-05 | 0.00208 | 0.00159 | 0.00150 | -0.000658 | 0.00121 |
| | (0.000540) | (0.00300) | (0.00152) | (0.00232) | (0.00237) | (0.00122) | (0.00100) | (0.000985) |
| Medium × Low-Partner | 0.000489 | -0.00140 | -0.000166 | -0.000632 | -0.000286 | -0.000201 | 0.000232 | -0.00141** |
| | (0.000350) | (0.00206) | (0.00113) | (0.00157) | (0.00149) | (0.000734) | (0.000623) | (0.000643) |
| Medium × Medium-Partner | 0.000445 | 0.000441 | -0.000921 | 0.00126 | 0.00635*** | -0.00234* | 0.000284 | -0.00133 |
| | (0.000477) | (0.00300) | (0.00160) | (0.00234) | (0.00227) | (0.00122) | (0.000936) | (0.000979) |
| Medium × High-Partner | 0.000196 | -0.00794** | 0.000109 | -0.00442 | -0.000150 | -0.00293** | 0.000979 | -0.00215* |
| | (0.000714) | (0.00397) | (0.00204) | (0.00297) | (0.00257) | (0.00141) | (0.00117) | (0.00113) |
| High × Low-Partner | -8.54e-05 | 0.00573* | 0.00108 | 0.00331 | 0.00242 | 3.81e-05 | -0.00110 | 0.00224** |
| | (0.000688) | (0.00309) | (0.00184) | (0.00246) | (0.00258) | (0.00144) | (0.00110) | (0.00110) |
| High × Medium-Partner | 0.000925 | 0.00640 | -0.000955 | 0.00565 | 0.00290 | 0.000834 | -0.00101 | -0.000802 |
| | (0.000907) | (0.00424) | (0.00199) | (0.00352) | (0.00297) | (0.00164) | (0.00131) | (0.00145) |
| High × High-Partner | -0.000349 | 0.0108** | -0.000283 | 0.00782** | 0.0106*** | -0.000653 | -0.000631 | 0.00154 |
| | (0.000987) | (0.00466) | (0.00230) | (0.00374) | (0.00336) | (0.00143) | (0.00147) | (0.00148) |
| Observations | 2,213,764 | 2,213,764 | 2,213,764 | 2,213,764 | 2,213,764 | 2,213,764 | 2,213,764 | 2,213,764 |
| Reference Group Mean | 0.002 | 0.044 | 0.022 | 0.019 | 0.045 | 0.009 | 0.012 | 0.007 |

Notes: This table reports coefficient estimates from equation (3) for various outcomes. All estimates include fixed effects for officer and shift. Columns 4-7 also include the number of index arrests and non-index arrests. Columns 6-7 include the number of verdicts. Standard errors clustered by officer are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure A3: Monthly Impact of Increased Oversight on Conviction-less Arrests




Notes: This figure depicts estimates for $\tau_m$ from the regression model: $r_{it}^g = \beta_0^g + \tau_m^g + a_i^g + \varepsilon_{it}^g$, where $\tau_m$ denotes month fixed effects and $\alpha_i$ denotes officer fixed effects. The analysis sample is restricted to 6 months before event 1 (court ruling) and 5 months after event 2 (FOP newsletter). The reference month is February 2014, the month before the *Kalven v. City of Chicago* court ruling. Each symbol denotes a coefficient estimate, while lines are the mean value of the estimates within each of the three time periods.