Peer Effects in Police Use of Force

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Abstract

We study the link between officer injuries-on-duty and the force-use of their peers using a network of officers who attended the police academy together through a random lottery. We find that peer injuries-on-duty increase the probability of using force by 7%. The effect is concentrated in a narrow time window near the event and is not associated with significantly lower injury risk to the officer. Complaints of improper searches and failure to provide service also increase after peer injuries, suggesting that the increase in force might be driven by increased caution by the officers.

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1. INTRODUCTION

Market economies depend on the enforcement of property rights and the protection of public safety (Besley and Ghatak, 2010; Atkinson and Stiglitz, 2015). One of the state's primary means of ensuring these protections is the use of law enforcement officers (Becker, 1968). An extensive empirical literature establishes that policing can be an effective way of reducing the quantity of crime (Levitt, 2002; Chalfin and McCrary, 2018; Morales, 2020). However, policing often requires the use of physical force as many suspects would prefer to evade the arresting officer rather than face potential punishments in the court system. As a result of these uses of force, police officers kill roughly one-thousand people a year in the U.S. (Guardian, 2016; WashingtonPost, 2019) and injure far more, with growing evidence of negative spillovers on civilians. As such, evaluating the costs and benefits of policing policies requires understanding how police officers decide whether or not to use force. In this paper, we provide novel evidence that peer-injuries are an important determinant of this decision.

The estimation of causal peer effects is typically hindered by a lack of microdata on social networks, the reflection problem, unobserved correlated effects, and endogenous group membership (Manski, 1993). Identifying the effect of a peer's choices, characteristics, or outcomes requires the exogeneity of those factors with an assignment probability that is independently distributed across groups (Angrist, 2014; Crépon et al., 2013). In this paper, we overcome these hurdles using detailed administrative panel data on Chicago Police Officers, the random assignment of officers to police academy cohorts, and the quasi-random exposure to peer injuries.

The peer groups we study are officers who were randomly drawn into the police academy during the same month.[1] These officers likely form social connections which remain intact after graduating the academy and working in different parts of the city, providing an exogenously formed social channel through which we can identify how officers respond to a peer being injured. Crucially, we exclude peers working together in the same police district to avoid contamination from correlated shocks to civilian noncompliance. Our results are significant due to their causal nature as the research design exploits variation in peer outcomes that is uncorrelated with individual characteristics, avoiding common issues in the estimation of endogenous peer effects or contextual effects (Manski, 1993; Angrist, 2014).

We first document a strong correlation between officer injuries and the propensity of other officers to use force (Figure 1). Then, using our identification strategy, we show that peer injuries-on-duty increase the use of force by seven percent in the subsequent week, leading to more civilian injuries. Officers are twice as responsive when the injured peer is of the same race. These effects are driven primarily by increases in lower levels of force. These effects are likely a lower bound on peer responsiveness as connections formed during the academy may fade over time and injuries to current coworkers likely provoke more extreme responses.

To better understand the mechanisms driving this effect, we leverage the multiple policing outcome metrics available in our data, including arrests and complaints. While arrests, a traditional metric for police productivity, do not change in response to a peer injury, we find that officers are fifteen percent more likely to have a citizen issue a complaint against them for failing to provide a requested service after a peer injury. At the same time, officers are not significantly less likely to suffer an injury themselves in the week

[1]This definition follows Shue (2013), along with Ager, Bursztyn and Voth (2017).

following a peer injury. These results suggest that officers are not merely updating their beliefs about the injury risk but are acting more cautiously. Furthermore, officers do not increase their propensity to use force after a peer uses force when the peer is not injured. This suggests that, unlike traditional models of peer effects, officers are responding to the *outcomes* that their peers experience rather than the *choices* their peers make.

By identifying evidence for a differential effect between peers' *outcomes* and peers' *choices*, this paper also expands on the literature on peer effects in the workplace (Mas and Moretti, 2009; Cornelissen, Dustmann and Schönberg, 2017). This result suggests that researchers must account for the effects of events that may coincide with peer actions when interpreting the channel through which peers respond. Furthermore, our paper suggests that direct responses to peer outcomes may be partially driving the results in other studies that find negative spillovers. For example, using domestic violence at home as an instrument, Carrell and Hoekstra (2010) find negative spillovers from children in troubled families and argue that these effects operate through the reduced achievement or increased disruption of the affected child. Similarly, Murphy (2019) attributes contemporaneous misconduct in the military to peers responding to the poor behavior of other members of the military. Our finding provides new mechanisms for exploring such results. Lastly, the paper contributes to the body of evidence showing that one's peer group can affect the choices and outcomes of individuals long after the group dissipates (Bayer, Hjalmarsson and Pozen, 2009; Shue, 2013).

Second, we expand the vast literature on the economics of policing. This literature primarily focuses on the police's effect on crime, remaining agnostic about the police officers' production function. [2] More recent studies have documented how aggressive policing can reduce the educational performance of minority groups (Ang, 2018; Legewie and Fagan, 2019), negatively affect attitudes toward the state (Skolnick and Fyfe, 1993; Weitzer and Tuch, 2004; Brunson and Miller, 2006) and undermine police legitimacy (Tyler, 2004; Lum and Nagin, 2017; Manski and Nagin, 2017). By identifying a causal determinant in the decision to use force, this paper not only contributes to the literature on the impact of the use of force, but it also builds upon the burgeoning literature attempting to unpack the black box of police productivity by providing evidence that police officers respond to the their peers' outcomes.[3]

Additionally, our study contributes to the behavioral literature on the effect of transitory emotion shocks and negative experiences on individual preferences. We show how such events can impact productivity and performance through a behavioral channel in a high stakes setting. Lab and artefactual field experiments have uncovered evidence that traumatic events and violence can increase preferences for certainty and impatience (Cameron and Shah, 2015; Callen et al., 2014; Imas, Kuhn and Mironova, 2018; Moya, 2018; Brown et al., 2019) while decreasing emotional regulation (Osofsky, 1997). Outside of the lab, researchers have also documented that "near-miss" accidents increase risk-aversion (Shum and Xin, 2019) and that negative emotional shocks can increase domestic violence (Card and Dahl, 2011; Munyo and Rossi, 2015). We find that peer injuries induce police officers to increase their propensity to use force in a manner that is consistent with increased

[2]See Levitt (2002), Di Tella and Schargrodsky (2004), Evans and Owens (2007), Lin (2009), Draca, Machin and Witt (2011), Chalfin and McCrary (2018), and Morales (2020) for papers documenting the effect of policing on reducing crime.

[3]See Harris et al. (2017), Bove and Gavrilova (2017), Fryer Jr (2018), Owens et al. (2018), Ba and Rivera (2019), Ba et al. (2020), Annan-Phan and Ba (2019), and Zimring (2019) for other work attempting to uncover the determinants of police force.

caution due to heightened risk aversion, providing novel empirical evidence for behavioral phenomena in policing.

Lastly, this paper introduces a new dimension for policymakers to consider. Policies that increase the risk of injury to officers will have a muted effect on force-use when officers respond to risk by increasing force. For example, Ariel et al. (2017) find that assaults against officers increased after the introduction of body cameras. Body cameras might have a direct reduction in the propensity to use force. Yet, if cameras increase the injury risk then the resulting change in force-use may be smaller or even positive when implemented at scale. Alternatively, policies that reduce the risk to officers may have positive externalities on force use. Ba and Grogger (2018) find that the introduction of tasers reduced injuries to police officers. Similarly, Harris et al. (2017) along with Bove and Gavrilova (2017) find that military-grade equipment reduce the risk to an officer.[4] If technology decreases the propensity of an officer to be injured, our findings suggest there will be positive spillovers due to a reduction in peer use of force.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background, providing information on the network formation and the policy governing use-of-force decisions. Section 3 describes the relevant datasets, sample definitions, and summary statistics. Section 4 explains the research design used to generate the estimates provided in section 5. Section 6 sheds light on the mechanisms suggested by auxiliary analysis of the data. Section 7 concludes.

[4]These studies find that complaints fall after the introduction of the technology, but do not find a decrease in offender deaths.

2. BACKGROUND

2.1 FORMATION OF POLICE NETWORKS

The Chicago Police Department's recruitment process creates an ideal setting to study spillover effects. The recruitment process generally follows five steps: (1) a recruitment call[5], (2) an entrance exam, (3) a referral lottery, (4) a battery of physical and mental tests, and finally (5) attending a police academy.

The Chicago Police Department regularly issues recruitment calls. Table 1 displays the nine recruitment calls made between 2002 and 2013. After applying, prospective officers take an exam meant to evaluate the officer's cognitive and non-cognitive abilities. All applicants who pass the exam move on to Step 3, where the CPD adds them to an eligibility list and provides a lottery number. These applicants are referred to the CPD academy in lottery order as vacancies become available.[6] Applicants remain on the lottery list until it is either exhausted or retired (Chicago Police Department, 2016), with veterans receiving priority in the randomization. This application process ensures that individuals did not select into specific cohorts based on the propensity to use force, be injured, or respond to peer injuries with violence.

Referred applicants proceed to Step 4, where they take further examinations. These include a physical test, a background check, a psychological evaluation, and a drug test. After the officer passes these examinations, they start

[5]Officers who apply to be part of the Chicago Police Department (CPD) must fulfill age and citizenship requirements. Applicants must also have a combination of post-secondary and army training. The CPD accepts individuals with at least sixty semester hours from an accredited university, three years of active duty in the armed forces or thirty semester hours and one continuous year of active duty.

[6]Although, military veterans are given some priority in the lottery process.

at the police academy. If potential officers fail any examination, they do not proceed. Successful applicants attend the police academy, which we classify as the peer group. Figure 2 presents a histogram of the cohort sizes during the sample period. The cohort size, on average, is 48.60 individuals, although cohort sizes have considerable range. Figure 6 shows that these cohorts are starting throughout the sample period. On average, police academy cohorts are 77% male, 49% white, 17% black, and 34% Hispanic. The median age of new officers in our sample is 28.

Once applicants enter the academy, the Education and Training Division provides over 900 hours of basic training over six months. Training includes instruction on the use of force tactics, including firearms and control techniques. There is also physical and scenario-based training in the classroom. CPD recruits receive extra training on gangs, drugs, law, ethics, report writing, vehicle stops, use of force, and driving. The academy also includes diversity training for their officers (Chicago Police Department, 2017).

After completing the academy, officers complete roughly twelve months of probationary field training (Chicago Police Union Agreement, 2017).[7] During the initial twelve months of active duty, the CPD assigns probationary officers to districts at their discretion. Duty assignments can change day-to-day during this period, and we have no data on officer assignments during this time. After the probationary period is over, officers move to a more permanent police unit. These assignments prioritize the needs of the CPD rather than the preferences of the police officer.

[7] Nearly all officers who begin training graduate from the police academy with fewer than 3% of officers failing (Chicago Tribune, 2017). The probationary period consists of eighteen months of active duty. The first six months are spent in the academy and the final twelve months are spent in probationary assignments. Time absent from duty does not apply toward completion of the probationary period.

Police districts operate under the Bureau of Patrol which segments these districts into three geographic areas (North, Central, and South) and a Special Functions Division. The Special Functions Division contains units such as Canine, Marine/Helicopter, SWAT, the Bomb Squad, and other similar units. Since these units operate across geographic districts, we omit them from the analysis. The remaining three areas are each segmented further into districts. Each district is then broken up further into beats that officers patrol.

The geographic unit of analysis considered in this paper is the police district. In an average week, we observe 90.24 officers in each of the 22 units. However, there is some dispersion. The distribution of unit sizes is shown in Figure 3. Each unit consists of many different cohorts (see Figure 5).

2.2 USE OF FORCE POLICY

The Chicago Police Department defines the use of force as physical contact by a Department member used to compel a subject's compliance. It is the Department's policy to attempt to gain the voluntary compliance of subjects when possible. However, members are not required to take actions or fail to take actions that endanger themselves or third parties (Chicago Police Department, General Order G03-02).[8]

In general, the CPD requires officers to use force that is "objectively reasonable, necessary, and proportional" to the actions of the subject (Chicago Po-

[8]When attempting to gain the compliance of subjects, officers have several options available to them. Officers can use mitigation efforts such as verbal directions to gain compliance without using force. They may also use control tactics such as handcuffing or applying pressure to sensitive areas. Officers are also permitted to use higher-level responses with or without weapons; these include open hand strikes, punches, kicks, and other forms of physical violence. Lastly, the CPD permits officers to use Tasers, pepper spray, batons, and firearms under some circumstances.

lice Department General Order G03-02, 2017). However, there is no formal definition of "objectively reasonable." Instead, officers are instructed to consider whether there is an imminent threat to themselves or third parties, how much harm the threat poses, and whether the subject has immediate access to weapons. When assessing the validity of force use, the CPD explicitly accounts for the imperfect information regarding the compliance status of the suspect and that officer decisions are made quickly under tense circumstances.

The proportionality requirement relies on the officer's contemporaneous beliefs about the threat he or she faces. These beliefs may differ from the threat determined by an objective observer. The guidelines only permit officers to use force that may cause great bodily harm or kill subjects when the subject poses an imminent threat of death or great bodily harm or when a suspect who committed a forcible felony that involved threatening the infliction of great bodily harm was trying to avoid arrest or run from the police. Under the guidelines, the department only permits officers to use this type of force as a last resort when all other de-escalation methods have failed.

3. DATA

We use four sources of administrative data from the Chicago Police Department. Force-use and injury data come from the CPD's Tactical Response Reports (TRR) for non-juvenile suspects. The CPD requires that officers fill out Tactical Response Reports after all incidents in which a member of the police department uses more than a minor level of force.[9] While minor levels

[9]This includes firearms, impact munitions, Tasers, acoustic devices, impact weapons, mechanical actions/techniques, or chemical weapons. Minor levels of force include things like holds or handcuffing. The police department also requires TRRs for force involving canines, but we exclude canine units from the analysis.

of force do not require a TRR, officers must also fill out TRRs when a suspect alleges an injury, if the suspect resists arrest or in situations where suspects use physical violence. (General Order G03-02-02).

Our data encompass over 16,000 instances of force by the CPD between January 2005 and October 2016. These data have numerous strengths relative to other existing data sets. They cover almost every instance of police use of force in Chicago, regardless of whether the officer injures or kills the suspect.[10] Second, the data contain detailed information about the time and location of the incident along with suspect, officer, and interaction characteristics. We supplement the data with officer employment records that include unit assignments and report the start date of the officer, which is critical to our identification strategy.[11]

To help identify the mechanisms driving the increase in force use, we supplement this data with data on complaints issued against officers (See Ba (2017) for a detailed discussion of this data) and arrest data. The complaint data contains all allegations of misconduct filed by civilians or other officers during our sample period, including the data of the incident and details of the actions resulting in a complaint. We investigate three specific types of complaints: force and verbal abuse, false search or arrest, and failure to provide service. The arrest data contains all CPD arrests of adults between 2001 and 2017, including crime type, arrestee demographics, and arrest date and time.[12]

[10] The data exclude incidents involving juveniles because juvenile records are not subject to Freedom of Information Act requests.

[11] We exclude all police officers without a recorded start data from the analysis.

[12] Arrest dates are only available from 2010 to 2017. For all years in this study before 2010, the arrest date is imputed as the minimum of bond and release date. Between 2010 and 2017, the median number of days between arrest date and minimum of bond and release date is 1 day (the average is 0.71 days)

The data do have some limitations. While we observe the presence of any alleged injury to officers or civilians, we do not observe the nature, severity or cause of the injury.[13] Since we are primarily interested in instances where suspects harm a police officer, we restrict our treatment definition to injuries that occur during interactions with suspects who allegedly attacked the officer.[14] As such, there is some measurement error that, in some instances, leads us to consider control periods (no former peer was injured) as treated periods (at least one former peer injury). This misclassification will attenuate the effects.

Lastly, CPD officer unit assignment data records officers as being a part of the "academy" unit until they finish their probationary period as opposed to when they graduate from the academy. We cannot observe the officers' geographic assignments in the year between graduation and the end of the probationary period. Since local non-compliance shocks constitute a significant threat to identification, we exclude every officers' probationary year from the analysis. This limitation forces us to drop officers who do not enter one of the twenty-two geographic districts such as the canine unit or S.W.A.T. team. We also drop officers who leave the police academy before six months

[13]The CPD refused to provide this information in the FOIA request citing HIPPA privacy regulations.

[14]While the data does not include information about the injuries, there is some literature suggesting that the primary cause of injuries-on-duty is violence. The Bureau of Labor Statistics reports that of the 27,660 on-the-job injuries reported in their 2014 sample, 27% were caused by violence. The next most common category was falls, slips, and trips; this category accounted for 25.3% of injuries. Overexertion followed, accounting for 21.4% of injuries. Using data from the National Electronic Injury Surveillance System-Occupational Supplement, Tiesman et al. (2019) categorized the type of injuries officers experience nationally over a similar period. They find that the leading cause of injury was from assaults and violent acts. Most of these injuries were to the hands, legs, neck, head, or shoulders. About 40% of injuries were contusions, abrasions, lacerations, fractures, or dislocations. The other 60% were sprains, strains, or other. In their sample, assault-related injuries grew between 2003 to 2011.

or individuals who never leave the police academy in our sample.[15]

4. RESEARCH DESIGN

The goal of the empirical analysis is to identify the causal effect of an injury-on-duty in one's network. There are four primary challenges to identifying this effect, which our definition of peers helps us overcome. Foremost, we need to be able to observe the network for officers. In principle, the relevant network here is the set of officers whose injury status is observable to the officer in question. There are different ways that we could classify officers into a network using our data: all officers, officers in the same unit, or officers who attended the police academy together. Following Ager, Bursztyn and Voth (2017), the network definition we use is officers who, through the random lottery, attended the police academy together but no longer worked in the same unit. We will henceforth refer to these peers as *former* peers to differentiate them from members of the police academy who still work in the same district.[16]

This definition helps overcome the fact that officers the environments in which officers work and the peers they associate with are correlated. That is, because force-use and officer injuries are co-determined, more aggressive officers sorting into more aggressive networks could spuriously generate a

[15]A total of 3,548 officers start the academy in our sample. We drop 272 officers who do not enter into a geographic district after leaving the academy, leaving us with 3,276 officers and a total of 899,894 officer-week observations. Of these officers, 2,678 officers use force at least once in the sample, with 1,886 instances accompanying an injury or alleged injury to the suspect. In our sample, 1,192 officers experience injuries and nearly all officers (3,244) experience at least one injury to a member of their police academy cohort. See Appendix for more information.

[16]This means that we consider individuals to be untreated if a member of their academy cohort who currently works in the same district is injured.

correlation between injuries and force-use that could be mistaken for a peer effects in the data. The CPD assignment process rules out the possibility that officers sorted into more aggressive cohorts. Second, there may be common shocks to civilian non-compliance within a district. If some shock reduces the probability that civilians comply with the officer's requests, then both the risk to the officer and the returns to using force will increase. Using former peers allows us to rule out district level shocks to civilian non-compliance because we compare treated officers to others who face the same non-compliance rate.

Finally, we must be able to overcome the simultaneity between an individual's actions and their peer's actions, referred to as the reflection problem (Manski, 1993). Angrist (2014) shows that designs relying on random variation in cohort assignments do not overcome the reflection problem because identification relies on finite-sample fluctuations in treatment assignment. As such, the identification of spillover effects requires the random assignment of subjects to treatment with an assignment probability that is independently distributed across groups (Crépon et al., 2013; Duflo and Saez, 2003). We approximate this research design by combining the random assignment of cohorts with the quasi-random timing of injuries to officers.

In practice, we construct the counterfactual outcomes within district d and week t, using injuries to police academy classmates as the treatment. We identify the effect of an officer injury using an event study that compares individuals experiencing and not experiencing an injury to a former peer and combining them into a difference-in-differences estimator. The event of a peer injury occurs at time $t = E_i$ for individual i. We denote individual fixed effects as λ_i and district-week fixed effects as λ_{dt}. The primary equation used to recover the causal effect of peer injuries is

$$Y_{idgt} = \lambda_i + \lambda_{dt} + \beta \cdot \mathbb{1}[t = E_{g,-d} + 1] + \epsilon_{idgt} \quad (1)$$

The individual fixed effects account for time-invariant individual-level differences in the outcome. In the main specification considering the officer's decision to use force, this fixed effect will account for time-invariant differences in interpreting the suspect's actions as non-compliance. District-week fixed effects account for district-week level differences in the costs or benefits to choosing $Y_{idgt} = 1$. These fixed effects will control for district-specific shocks to civilian or officer aggression, such as the weather or pollution (Annan-Phan and Ba, 2019; Herrnstadt et al., 2016). The treatment, $\mathbb{1}[t = E_{g,-d} + 1]$, is an indicator equal to 1 if another officer who attended the police academy with the officer but is not working in the same district was injured in the previous week.

The coefficient of interest is β, which estimates the change in the outcome for affected officers relative to officers in the same district who did not experience a peer injury in the previous week. Standard errors are clustered on the academy cohort level to allow for arbitrary correlation of errors within each of the 73 cohorts (Bertrand, Duflo and Mullainathan, 2004). The main identifying assumption is that the change in the outcome in a given district-week is independent of whether the injured officer started the police academy in the same month as the officer.

To assess the plausibility of this assumption and examine the dynamic effects of a peer injury, we regress the outcomes on lags and leads of the event (injury). We denote event time in this regression as τ. We omit the dummy for the week before a former peer is injured, set period -5 to be equal to one when the event was five or more weeks before the injury and period 5 to be whether the week is five or more periods after the injury so that we can interpret the coefficients relative to the week before injury.[17]

$$Y_{idgt} = \lambda_i + \lambda_{dt} + \sum_{\tau} \beta_\tau \cdot \mathbb{1}[t = E_{g,-d} + \tau] + \epsilon_{idgt}. \tag{2}$$

where $\tau = \{-5+, -4, -3, -2, 0, 1, 3, 4, 5+\}$. In this regression, the coefficients of interest, β_τ, estimate the change in the outcome between period $t = -1$ and τ for officers who experienced a peer injury relative to members of the same district who did not. Insignificant β_τ estimates before the event alleviate concerns that the groups differ in the probability of encountering non-compliant civilians or signal interpretation; however, unobserved post-treatment shocks specific to members of a particular police academy cohort may still threaten the identification of β_τ.

Because peer groups are large and we observe officers over several years, officers experience multiple events over the time horizon in our sample. Standard event studies usually include one event per cross-sectional unit and include mutually exclusive dummy variables representing each period from treatment. Our setting departs from this standard. While the probability an individual officer gets injured is one-quarter of a percent, over 95% of weeks in our sample contain at least one officer injury. This translates into roughly a 1 in 8 chance of experiencing a peer injury each week. Over the observed portion of an officer's career, the average officer experiences 0.89 injuries, 43.62 injuries to former peers, and 368.48 injuries to any police officer. This means that β_τ can represent the effect for a period, which is both a pre-treatment period and a post-treatment period. Assuming the response to treatment does not vary based on the number of previous events, this will

[17] The lags and leads will also alter the composition of individual-weeks that we observe. The first and last five weeks of every individual will be excluded from the regression since they are not observed with either five lags or five leads.

bias the pre-trend estimates away from zero and make it more likely for us to find significant pre-trends. However, in nearly all specifications, we do not find any evidence of significant pre-trends.

There is no accepted method of conducting event studies when there are multiple or overlapping events. However, Monte Carlo simulation results in Sandler and Sandler (2014) suggest that allowing multiple event dummies to be non-zero at one time produces unbiased results under a similar data generating process. Further, they show that restricting the estimation to consider only a single event or using only periods that have a single event per individual/event/time produces biased results. We follow their guidance in our estimation.

5. RESULTS

We first examine the effects of former peers' injuries on the propensity to use any type of force by estimating Equation 2.[18]. We estimate the base rate of force as the constant term from a regression of Equation 2 without individual or unit-week fixed effects. Figure 7 displays the coefficient estimates from Equation 2 divided by the base rate. This allows us to interpret the effects as percent changes from the baseline propensity to use force.

There are no distinguishable effects of a peer injury in the weeks before former peer injury. Baseline use of force is also small, with 1.78% of officers using any type of force in a given week. In the week of a peer injury, the use of force increases by around 3% of the baseline mean. We view this period as partially treated as some officers experience injuries toward the end of the

[18]As cohorts join throughout the sample period, the estimation equation will drop individuals who are not employed for five weeks before and after an exposure

week. It also will take time for the officers to learn about their peer's injury status.

In the week following a peer injury, the use of force increases by seven percent relative to the period before a peer is injured. The treatment effects dissipate quickly over time, immediately losing significance after the first-week after exposure. This pattern is consistent with Card and Dahl (2011) and Munyo and Rossi (2015), who both find that incidental emotional shocks have a short-lived effect on violence.

Table 3 contains the results from different specifications in the spirit of Equation 1. Column 1 displays estimates from an OLS regression of an indicator for force on the first lag of past peer injuries. We use the constant term from Column (1) as the baseline in Columns 2 through 5. Column 2 estimates Equation 1 without controlling for the number of former peers on duty in the given week or test cohort-week fixed effects. A causal interpretation of our estimates depends on the behavior of officers who did not suffer a peer injury being similar to those who did, absent the injury. Since cohorts are randomly assigned conditional on officers being a member of the same test-cohort, changes in the applicant pool may lead to cohorts that systematically differ from one another on unobservable differences that make them both more likely to experience injuries and inflict force.

As a robustness check, we include Columns (4) and (5) of Table 3 which show the coefficient estimates from Equation 1, including test-cohort week fixed effects with and without controls for the number of former peers. We find that the treatment effects do not change substantially or qualitatively with the introduction of these fixed effects. Estimates are nearly identical. Furthermore, the R^2 numbers in these regressions do not change from those without these fixed effects. This suggests that the individual level fixed-effects do a good job absorbing unobserved differences between individuals.

To better understand the consequences of increases in the use of force, we estimate Equation 1 on the probability that a suspect is injured or alleges an injury. The estimates of Equations 2 and 1 using suspect injury as an outcome are shown in Figure 8 and Table 4. The baseline rate of suspect injury is 0.55% in a given week. Injuries to former peers increase the propensity for suspects to be injured during interactions with the police by 9.78% of the baseline mean.

5.1 RESULTS USING SAME-RACE FORMER PEERS

So far, our definition of *former* peer has included everyone who attended the academy together, but no longer work together in the same police district. Our definition of the peer group relies on the assumption that these individuals were acquainted with each other and maintained their bonds after the academy ended. However, since cohorts are large, it is unlikely that all of the group members satisfy these requirements. Indeed, Carrell, Sacerdote and West (2011) find that individuals forced into exogenous groups end up endogenously forming more homogeneous groups of friends.

In this setting, such behavior will bias coefficient estimates toward zero, as we will be pooling treated and untreated officers. To further test whether peer injuries cause the observed effects, we restrict the peer definition to those who were more likely to have social interactions. Following McPherson, Smith-Lovin and Cook (2001), we assume individuals of the same race who attended the academy together are more likely to be a part of the same network. Past literature on peer effects shows that peer effects mainly operate within-race Garlick (2018).

Figure 9 contains analogous estimates as those estimating Equations 2 and 1 redefining treatment to be injuries of past peers of the same race. Similar to before, Figure 9 shows little evidence of differential pre-trends before the peer experiences an injury. Consistent with peer group homophily, officers respond much more strongly to injuries of peers more similar to them. Under this definition of treatment, officers are 18.13% more likely to use force in the week after a peer is injured, with a baseline probability that an officer uses force of 1.6%.

5.2 HETEROGENEITY

To better understand how officers respond to peer injuries, we investigate heterogeneity based on the type of force officers use and the characteristics of the suspects.

5.2.1 TYPES OF FORCE USED BY OFFICERS

Police officers have several types of force available. The choice of which type of force to use is governed by the CPD use-of-force model. Generally, the more resistance the officer faces, the more force they are permitted to use.

Control tactics are the lowest level of force and include actions such as escort holds, wrist locks, emergency handcuffing, or armbars. The next highest type of force is physical strikes (defined as a takedown, open hand strike, punch, kick, or elbow) that do not involve more than the officer's body. Force involving weapons is classified as non-lethal if it involves a chemical weapon, baton if the officer uses an impact weapon, or as a Taser or firearm if those weapons are involved. Other is a residual category, which includes various uncommon types of force.

We investigate heterogeneity in the types of force officers choose after a former peer is injured. To do this, we separate force into seven distinct categories and estimate Equation 1 on indicators for using each type of force; we present the results in Table 6.

Since the baseline use of any force is low, the baseline force use for each type of force-use is small and more or less decreasing in its severity. The rarest type of force is the use of firearms, followed by impact weapons such as batons. The majority of instances of force recorded in this data are from Tasers, physical attacks, or control tactics. However, there may be virtually no under-reporting for types of force that are harder to conceal (firearms and Tasers) because of the CPD regulations against it.

Similar to our main regressions, we do not find evidence of pre-trends in any specification except for non-lethal force. We find that the officers are primarily responding by increasing control tactics and force without weapons. There is a substantial increase in the percent of officers using a firearm in the week after a peer is injured; however, this represents a small percentage point increase. Assuming that officers are using force in alignment with the CPD use-of-force model, the increase in force use is primarily driven by encounters with low-resistance suspects, suggesting that officers would not have deemed these suspects to be a risk had their peer not been injured in the previous week. We investigate this claim more directly in the next section.

5.2.2 SUSPECT CHARACTERISTICS

Next, we investigate heterogeneity based on the similarity of the suspect to the individual who injured the officer's peer. In previous research, police officers have been shown to incorporate race in their decision making (Fryer Jr, 2018; Knowles, Persico and Todd, 2001; Grogger and Ridgeway, 2006). Subjects who share the same race with the suspect who injured the officer's peer may be at a higher risk of being a victim of police force.

In Table 7, we present the results from nine regressions. The outcome variable of each regression is the use of force against a member of the race listed in the column. The treatment is an indicator equal to one when a former peer is injured by an assailant of the listed race. The percent increase in force the week after treatment is presented as the coefficient with the confidence interval in brackets below.

We find that the use of force against African American subjects significantly increases the week after an African American individual injures the officer's peer. There are no significant increases in the use of force for Hispanic or White suspects in the week following a similar event. Although the confidence intervals on these estimates are very large, we find no effect of injuries from Hispanic assailants and that, surprisingly, the probability of using force against a White suspect falls dramatically after a White assailant injures an officer.

Readers should use caution when interpreting these results. Nearly 80% of officer injuries result from interactions with African American suspects. Similarly, 81% of instances of force are used against African Americans. As such, our results may be driven by the relatively small number of events observed for White and Hispanic suspects.

6. INTERPRETATION

Having established that police officers respond to peer injuries, we now attempt to understand what might be driving this behavior. We separate potential explanations into four distinct categories: social learning, peer effects in force use, worker effort, and transitory changes in risk-preferences.

6.1 SOCIAL LEARNING

The first mechanism we entertain is social learning (Banerjee, 1992; Bikhchandani, Hirshleifer and Welch, 1992). If officers maintain contact with individuals who contemporaneously attended the police academy, then they may be more likely to learn about the on-duty injuries that happen to their former peers. In this way, injuries to former peers acts as a private signal of the underlying injury risk during civilian interactions. The private signal can cause officers to update their beliefs about the probability a non-compliant civilian will injury them, thus increasing their use of force as a means of self-protection and generating a time-varying correlation between peer injuries and officer use of force.

Under this interpretation, officers have better information about the true injury risk officers faced on duty. This implies that the officers should have a lower chance of experiencing an injury herself in the week following a peer injury. We investigate this by estimating Equations 2 and 1, with the outcome being an indicator representing the officer's own injury status. The results of this comparison are reported in Figure 8 and column (2) of Table 9. We find that injury risk falls by 0.0181 percentage points, or 7.56% in the week after a former peer is injured. However, these results are not statistically significant at conventional levels. Thus, while social learning may be a potential channel driving force-use, our results are not conclusive.

6.2 OFFICERS MIMICKING PEER FORCE USE

The second mechanism we investigate is that officers are responding to the peer's decision to use force rather than the peer's injury. There is a large body of work showing that individual choices are influenced by the actions of one's peer group (Brock and Durlauf, 2001) and consistent with our results, these effects tend to be stronger when the peer is of the same race (Garlick, 2018). Moreover, Murphy (2019) finds that misconduct by soldiers in the US Army tends to occur at similar times as the misconduct of peers, suggesting that CPD officers may be responding to the proper or improper use of force by their peers. Since injuries and force use are co-determined, the data patterns we find might result from officers choosing to use force because they learned about their peers' use-of-force decision rather than their peers' injury.

Table 8 illustrates the relationship between the decision to use force and recorded officer injuries in our data set. In 94% of instances where officers were injured, they are also recorded using force against the suspect. However, there are over 14,000 instances of force used by officers that are not accompanied by an officer injury. We use these instances to investigate whether force-use mimicry is driving these results.

Column (1) of table 9 displays the results of a variant of Equation 2 using an indicator for use-of-force as the outcome and instances of former-peer force use which is unaccompanied by an officer injury as the treatment. This effect is small in both percentage point and percent change terms and not statistically significant. Finding no evidence that officers respond to force-use by their peers when that use of force is not accompanied by an injury to their former peers, we rule out the possibility that the results are driven by traditional peer effects.

6.3 TRANSITORY INCREASES IN RISK AVERSION

Next, we investigate whether temporary changes in the officers' preferences drive these results. Previous literature has shown that negative emotional states can influence an individual's propensity to engage in violence (Card and Dahl, 2011; Munyo and Rossi, 2015; Eren and Mocan, 2018). Several laboratory experiments also show that exposure to violence can affect time and risk preferences.

Loewenstein (1996) documented that preferences can be malleable and can be temporarily affected by emotional states. For example, traumatic events and natural disasters can impact risk-preferences (Cameron and Shah, 2015; Tanaka, Camerer and Nguyen, 2010; Hanaoka, Shigeoka and Watanabe, 2018). Similarly, Hjort (2014) finds that animus discrimination can increase in response to ethnic conflict, and Rohlfs (2010) finds that exposure to violence can make individuals more violent.

This literature suggests that two different types of emotional responses to peer injuries could lead to an increase in force use: increased risk-aversion or increased frustration leading to a desire to retaliate. Since police officers principally use force as a means of self-protection from perceived threats, heightened risk-aversion would translate into force use, as Dionne and Eeckhoudt (1985) show that the demand for self-protection is increasing in risk-aversion. On the other hand, increased frustration could increase the returns to retaliating or harming civilians after a peer is injured. Both potential responses imply immediate increases that dissipate quickly after the event, as we find in the data.

However, these mechanisms offer different predictions on officers' decisions to sort in or out of interactions with civilians. Officers with heightened risk aversion should be less willing to select into dangerous situations, while officers who seek to retaliate against civilians may seek out such interactions. While we cannot directly observe sorting behavior in our data set, we can use information about civilian arrests and complaints against officers to understand their behavior.

First, we supplement the primary data set with a data set on complaints against officers used in Ba (2017). Table 10 displays the results from variants of Equation 2 using different types of complaints as outcomes. Outcomes are an indicator equal to one when the officer committed an action that week resulting in a complaint from either a civilian or a fellow officer.[19] Column (1) displays the results for any type of complaint, column (2) displays the results for complaints about improper force and verbal usage, column (3) shows results for improper arrests or improper searches and column (5) shows results for failure to provide service.

We find that the probability an officer commits an action resulting in a complaint increases by about 5.5% the week after a peer is injured. This is driven mainly by a 15% increase in the probability of receiving a complaint for failure to provide service (FPS) in the week after a peer is injured. Arrests for im-

[19]Importantly, we consider the timing of the *action* resulting in a complaint, not the time of the complaint since that might have a considerable lag.

proper search or arrest also increase after a peer is injured, suggesting that officers may be choosing to search individuals because they viewed them as more likely to be a threat. Importantly, complaints about improper force use or verbal usage (for example, racial slurs) decrease by about 7.5% in the week a peer is injured.

Next, we look at the change in the probability of making an arrest and changes in the composition of crimes for which the officer makes arrests. If officers only reduce their response to low-level incidents that are unlikely to require force, then the increase in FPS complaints may be due to reductions in work effort. We address this by examining potential changes in arrests the week following a peer injury.

Difference-in-differences estimates of these effects using variations of Equation 1 are displayed in Table 11. The outcome variable for each equation is equal to 1 if the officer makes any arrest of a given type in a given week. Overall, we find no changes in the probability that an officer arrests a suspect in the week after a peer injury. There are also no clear changes in the composition of arrests following peer injures. Arrests for traffic violations and drug crimes increase slightly; however, the magnitude of these increases is less than three percent of the baseline mean. Taken together, this suggests that officers increase the probability of using force in the wake of peer injuries due to heightened risk-aversion rather than retaliation or effort substitution.

7. CONCLUSION

Police officers face the difficult mandate of safely arresting suspects who pose a risk to themselves or others. While the state empowers officers with the legal use of violence, this violence has negative externalities on society.

This paper explores the role networks play in exacerbating injury risk to officers.

Using novel data from the Chicago Police Department, we construct an exogenously formed network of officers who later worked in different areas of the city. Injuries-on-duty cause former peers to increase both their propensity to use force by 7% and increase the probability they injure a suspect by 10% in the following week. This effect is larger when restricting the definition of peers to same-race officers who attended the academy together. These effects are likely a lower bound on how officers respond to peer injuries, as connections to former academy peers working in different units are likely weaker than those with whom they currently work.
Our finding that the risk of injury-on-duty is statistically unaffected by peer injuries suggests that social learning is not the primary driver of these effects. Similarly, we find that when officers are not injured, peers do not respond to instances of force use, suggesting that police officers are not mimicking the behavior of their peers. Our finding that there is a large increase in the propensity to get a complaint about the failure to provide service after a peer injury suggests that primed risk-aversion may be the primary driver of the effects.

The existence of spillovers in police force resulting from on-the-job injuries has important implications for policies meant to reduce improper use of force. Policies that increase the risk of injury to officers will have a muted effect on force-use when officers respond to risk by increasing force. For example, Ariel et al. (2017) find that assaults against officers increased after the introduction of body cameras. Given our findings, this suggests that failing to account for externalities associated with peer injuries will cause policymakers to under-estimate the impact of body cameras on the use of force.

Body cameras might have a direct reduction in the propensity to use force. Still, if cameras increase the injury risk, as Ariel et al. (2017) find, then the resulting change in force-use may be smaller or even positive when implemented at scale. Alternatively, policies that reduce the risk to officers may have positive externalities on force use. For example, Ba and Grogger (2018) find that the introduction of Tasers reduced injuries to police officers.[20] If the available technology decreases the propensity of an officer to be injured, their peers will respond by reducing force-use.

Policymakers should take these externalities into account when determining the optimal way to reduce improper use of force. Focusing on interventions that reduce injury risk may reduce the threat to officers and will have the added benefit of reducing their propensity to use force. Any policy meant to reduce force use that does not change or increase the risk to officers may have limited effects.

[20] However, they did not find any change in civilian injury rates or the use of firearms.

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7.1 Tables

Table 1: Police Entrance Lotteries

Exam	Dates of Administration	Attended	Passed	Classes	Officers	P-Value
2002	1/12/2002	3150	No info	16	322	0.002
2003	11/22/2003	No	No info	5	52	0.350
2004	11/20/2004	4163	No info	7	352	0.620
2005	2/18/2006; 2/19/2006	4061	3338	3	181	0.850
2006-1	6/4/2006	1508	1255	2	145	0.260
2006-2	8/6/2006	1025	863	3	191	0.00
2006-3	11/5/2006	1795	1487	14	853	0.222
2010	12/11/2010 makeups: 3/12/2011; 6/11/2011; 9/25/2011; 12/3/2011; 6/2/2013; 12/1/2012; 3/9/2013	8621	7689	22	1300	0.770
2013	12/14/2013 military makeups 6/28/2014; 12/7/2014; 6/13/2015; 12/6/2015	14788	12877	8	651	0.457

Note: Sample includes every officer who started at the police academy between January 2002 and December 2013. A multinomial logit is run separately for each of the entrance exam dates where the outcome measure is a categorical variable representing the starting month and the right-hand side variables include the sex, age, and race of the police officer. P-value is from a chi-squared test under the null hypothesis that all of the regression coefficients are simultaneously equal to zero.

Table 2: Frequency of Events

	Self-injured	Former peer injured	Any officer injured
Average Per week	.24%	12.05%	95.16%
Observed career Average	.89	43.62	368.48

Note: Table uses data from all Tactical Response Reports in the data. The first row displays the percentage for each category, averaging over every week that the officer appears in the data set. The second row sums all events over the period of time in which we observe the officer.

Table 3: Effect of Injuries to Former Peers on the Propensity to use Force

	(1) Force	(2) Force	(3) Force	(4) Force	(5) Force
Former peer in previous week	0.00380***	0.00127**	0.00127**	0.00131**	0.00132**
	(0.000782)	(0.000556)	(0.000558)	(0.000580)	(0.000582)
Constant	0.0178***				
	(0.000585)				
Percent Increase	21.28	7.09	7.11	7.36	7.37
Pre-trend Test	0.000	0.822	0.822	0.660	0.664
Individual Fixed Effects	NO	YES	YES	YES	YES
Unit-Week Fixed Effects	NO	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	NO	YES
Test Cohort-Week Fixed Effects	NO	NO	NO	YES	YES
R-squared	0.000	0.042	0.042	0.045	0.045
Observations	896363	896250	896250	896250	896250

Note: Column 1 displays estimates from an OLS regression of an indicator for force on the first lag of past peer injuries. Difference-in-Differences coefficients from variations of Equation 1 displayed in columns 2 through 5. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table 4: Effect of Past Peer Injuries on Suspect Injuries

	(1) Injure Suspect	(2) Injure Suspect	(3) Injure Suspect	(4) Injure Suspect	(5) Injure Suspect
Former peer injured in previous week	0.00125***	0.000539*	0.000537*	0.000652**	0.000652**
	(0.000351)	(0.000311)	(0.000312)	(0.000293)	(0.000294)
Constant	0.00551***				
	(0.000226)				
Percent Increase	22.59	9.78	9.75	11.83	11.83
Pre-trend Test	0.017	0.300	0.301	0.160	0.160
Individual Fixed Effects	NO	YES	YES	YES	YES
Unit-Week Fixed Effects	NO	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	NO	YES
Test Cohort-Week Fixed Effects	NO	NO	NO	YES	YES
R-squared	0.000	0.032	0.032	0.036	0.036
Observations	896363	896250	896250	896250	896250

Note: Column 1 displays estimates from an OLS regression of an indicator for force on the first lag of past peer injuries. Difference-in-Differences coefficients from variations of Equation 1 where the outcome is whether a suspect reported or suffered an injury is displayed in columns 2 through 5. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table 5: Effect of Injuries to Former Peers of the Same Race

	(1)	(2)	(3)	(4)	(5)
	Force	Force	Force	Force	Force
Same-race former peer injured in previous week	0.00694***	0.00296***	0.00297***	0.00315***	0.00315***
	(0.00108)	(0.000922)	(0.000924)	(0.000946)	(0.000949)
Constant	0.0180***				
	(0.000592)				
Percent Increase	38.66	16.51	16.55	17.53	17.56
Pre-trend Test	0.000	0.613	0.605	0.953	0.951
Individual Fixed Effects	NO	YES	YES	YES	YES
Unit-Week Fixed Effects	NO	YES	YES	YES	YES
Number of Same-Race Former Peers	NO	NO	YES	NO	YES
Test Cohort-Week Fixed Effects	NO	NO	NO	YES	YES
R-squared	0.000	0.042	0.042	0.045	0.045
Observations	896363	896250	896250	896250	896250

Note: Column 1 displays estimates from an OLS regression of an indicator for force on the first lag of past peer injuries. Difference-in-Differences coefficients from variations of Equation 1 displayed in columns 2 through 5. Treatment is defined as an injury to an officer who both started the police academy in the same month and is of the same race. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table 6: Heterogeneous Effects by Type of Force

	(1) Control	(2) No Weapon	(3) Non-Lethal	(4) Baton	(5) Taser	(6) Firearm	(7) Other
Lagged Former Peer Injury	0.000723**	0.000986*	-0.00000891	-0.0000237	0.0000936	0.000110*	0.0000889
	(0.000356)	(0.000499)	(0.0000815)	(0.0000663)	(0.000135)	(0.0000648)	(0.000161)
Baseline	.0105	.0147	.0008	.0005	.0017	.0003	.0011
Percent Increase	6.89	6.71	-1.11	-4.73	5.51	36.66	8.08
Pre-Trend Test	0.539	0.659	0.005	0.165	0.725	0.930	0.882
Individual FE	YES	YES	YES	YES	YES	YES	YES
Unit-Week FE	YES	YES	YES	YES	YES	YES	YES
Observations	896250	896250	896250	896250	896250	896250	896250

Note: Difference-in-Differences coefficients from variations of Equation 1 displayed. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table 7: Heterogeneous Effects by Race

Assailant Race	Suspect Race: Black	Suspect Race: Hispanic	Suspect Race: White
	(N = 13251)	(N = 2022)	(N = 999)
Black	7.95**	6.94	5.28
(1650 Events)	[0.74,15.16]	[-10.66,24.54]	[-19.19,29.75]
Hispanic	-0.42	3.78	23.24
(281 Events)	[-15.56,14.72]	[-42.08,49.64]	[-35.47,81.94]
White	1.66	30.63	-42.26*
(146 Events)	[-21.67,24.99]	[-21.55,82.82]	[-87.71,3.18]

Note: Percent change from variations of Equation 1. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table 8: Force Use and Injuries

	Did not use Force	Used Force	Total
Not Injured	883325	14437	897762
Injured	121	2011	2132
Total	883446	16448	899894

Note: The rows display the total number of events by the decision to use force.

Table 9: Mechanisms

	(1) Force	(2) Injured	(3) FPS Complaints	(4) Arrests
Former peer used-force in previous week	0.000487			
	(0.000340)			
Former peer injured in previous week		-0.000181	0.000361**	0.00266
		(0.000166)	(0.000177)	(0.00201)
Percent Increase	3.11	-7.56	14.45	.86
Pre-Trend Test	0.218	0.545	0.652	0.947
Individual FE	YES	YES	YES	YES
Unit-Week FE	YES	YES	YES	YES
Number of Past Peers	YES	YES	YES	YES
R-Squared	0.0420	0.0281	0.0293	0.276
Observations	896250	896250	896250	866560

Note: Difference-in-Differences coefficients from variations of Equation 1 displayed. Column (4) only includes data from January 2007 through December 2016. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table 10: Effect of Past Peer Injuries on Complaints Against Officers

	(1) All Complaints	(2) Force and Verbal	(3) Arrest and Search	(4) FPS
Former peer injured in previous week	0.000719*	-0.000252	0.000405*	0.000361**
	(0.000398)	(0.000202)	(0.000206)	(0.000177)
Percent Increase	5.57	-7.59	8.19	14.74
Pre-trend Test	0.679	0.412	0.558	0.652
Individual Fixed Effects	YES	YES	YES	YES
Unit-Week Fixed Effects	YES	YES	YES	YES
Number of Former Peers	NO	NO	NO	NO
Test Cohort-Week Fixed Effects	NO	NO	NO	NO
R-squared	0.041	0.037	0.038	0.029
Observations	896250	896250	896250	896250

Note: Column 1 displays estimates from an OLS regression of an indicator for force on the first lag of past peer injuries. Difference-in-Differences coefficients from variations of Equation 1 where the outcome is whether an officer received a compliant for an action occurring the week following a peer injury displayed in columns 2 through 5. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table 11: Effect of Past Peer Injuries on Officer Arrests

	(1) Any Arrest	(2) Municipal Code	(3) Traffic	(4) Warrant	(5) Drug	(6) Property	(7) Violent	(8) Other
Lagged Former Peer Injury	0.00266	0.000341	0.00118*	0.00156	0.00231*	0.000278	-0.000356	0.00187
	(0.00201)	(0.000465)	(0.000632)	(0.00115)	(0.00121)	(0.000982)	(0.000945)	(0.00140)
Baseline	.308	.018	.04	.0698	.0942	.0492	.0788	.0985
Percent Increase	.86	1.89	2.95	2.24	2.45	.56	-.45	1.89
Pre-Trend Test	0.886	0.325	0.925	0.124	0.986	0.351	0.263	0.740
Individual FE	YES	YES	YES	YES	YES	YES	YES	YES
Unit-Week FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	866560	866560	866560	866560	866560	866560	866560	866560

Note: Difference-in-Differences coefficients from variations of Equation 1 displayed. Only includes data from January 2007 through December 2016. The percent increase is calculated by dividing the coefficient by the baseline in a regression without fixed effects. Standard errors are clustered by the academy cohort (G = 73). Pre-trend test presents the p-value from an F test where the null hypothesis is that the coefficients are the lead periods from equation 2 are simultaneously equal to zero. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

7.2 Figures

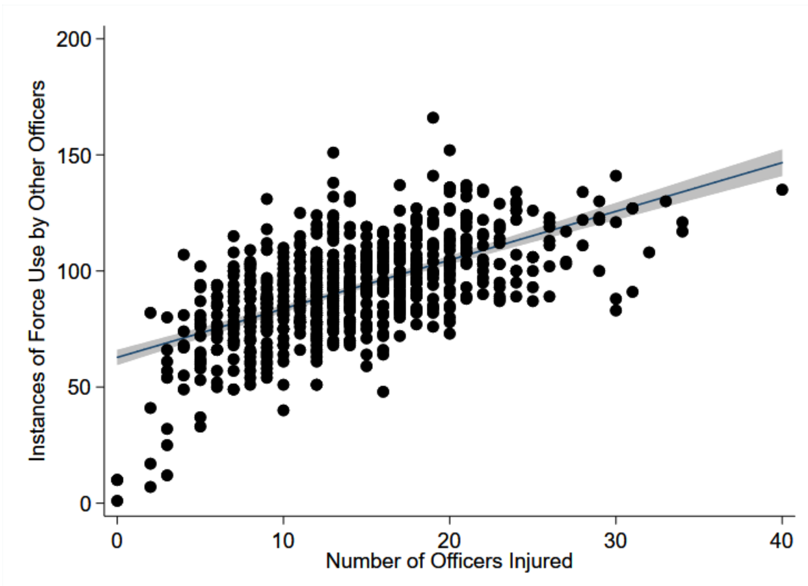

Figure 1: Correlation Between Officer Injuries and Force Use by Others

Note: Graph displays the relationship between the number of officers injured in a given week and the number of uninjured officers who use force in that same week. Uses the full sample of all officers included in Tactical Response Reports from 2004 to 2016. The blue line represents the regression line of force use in a given week on the number of other officers who are injured in that week. Standard error bands are presented around the line.

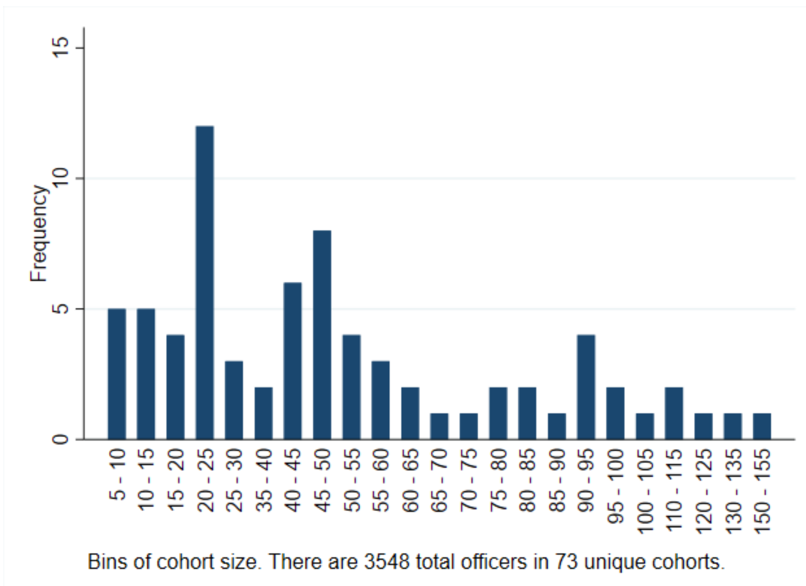

Figure 2: Distribution of Cohort Sizes

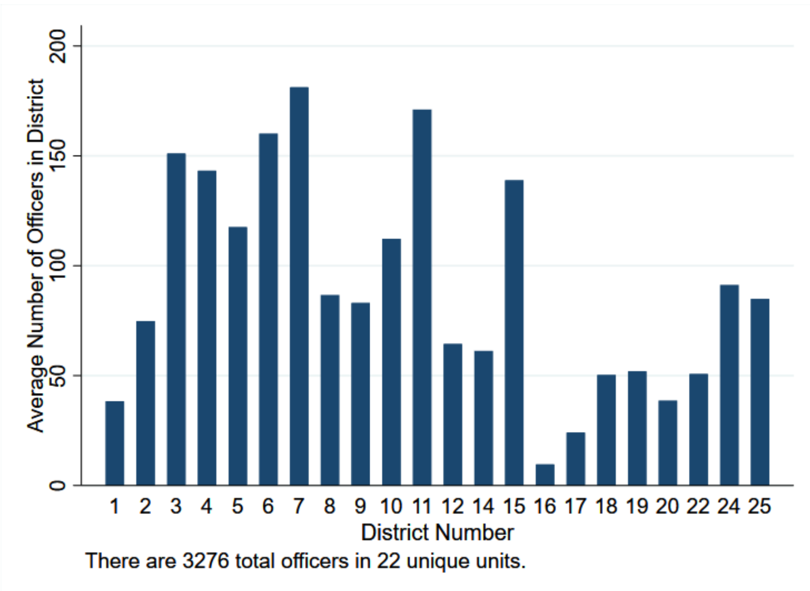

Figure 3: Distribution of Unit Sizes

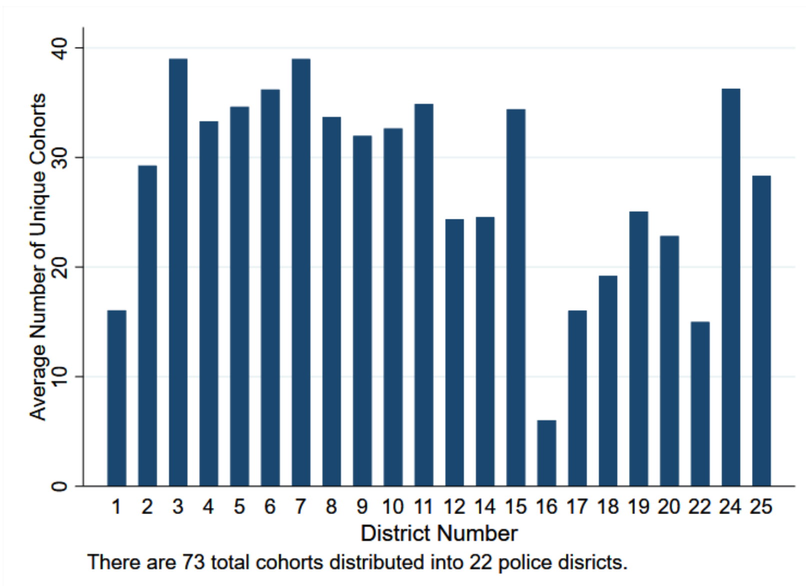

Figure 4: Distribution of the Number of Former Peers

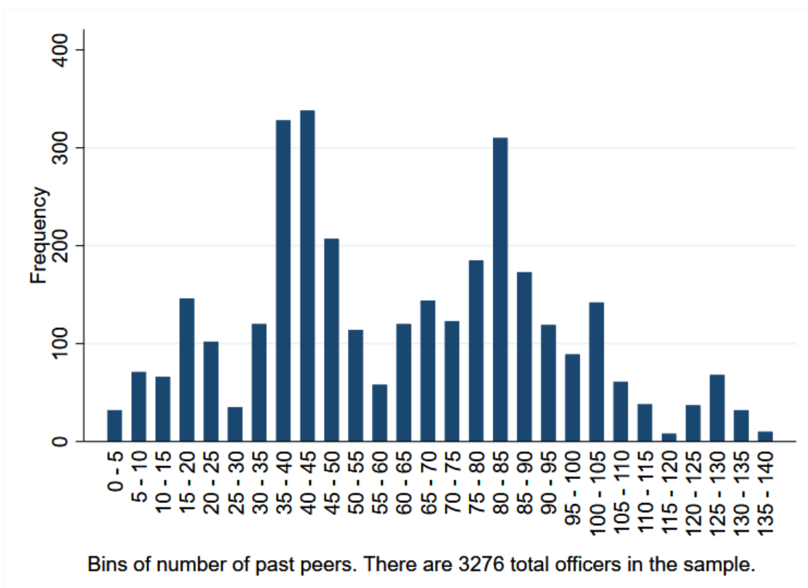

Figure 5: Distribution of the Number of Former Peers

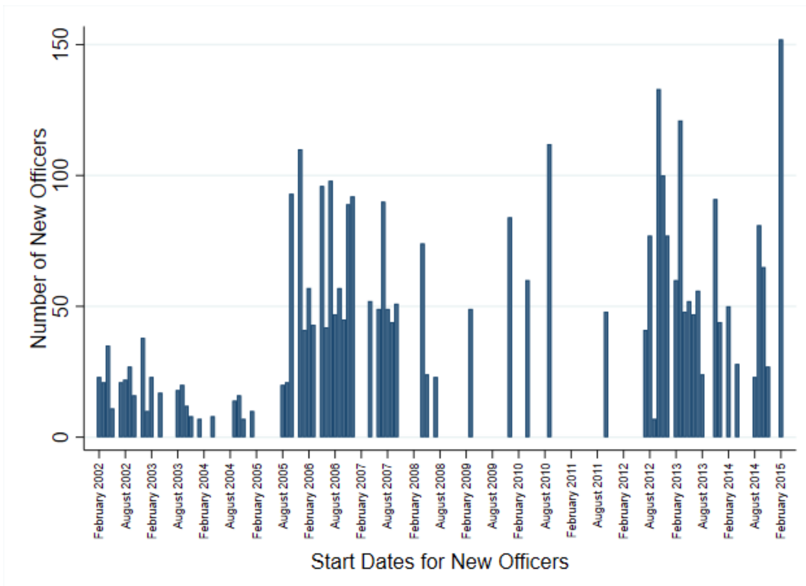

Figure 6: Addition of New Officers Throughout Sample Period

Note: Vertical bars display the number of new officers added in each month throughout the sample period.

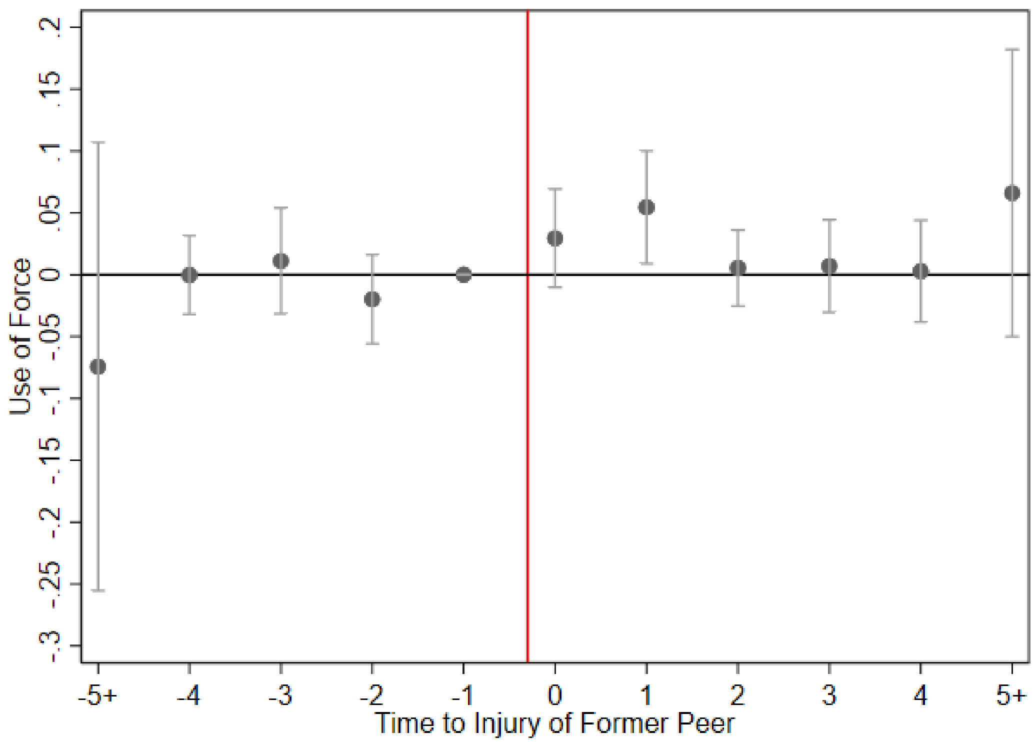

Figure 7: The Effect of Past Peer Injuries on Police Use of Force

Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 divided by baseline rate of force use and 90% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on lags and leads of treatment without fixed effects. Standard errors clustered by academy cohort (G = 73). Includes individual and district-week fixed effects. Treatment defined as injury of a former peer. Red vertical line represents treatment.

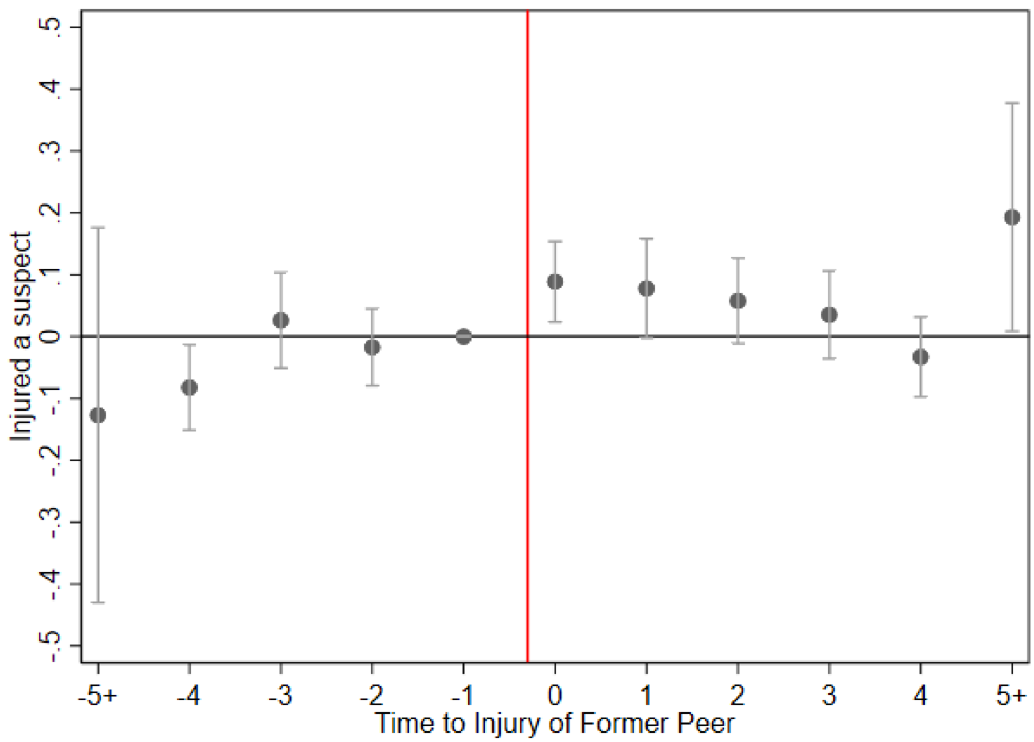

Figure 8: The Effect of Past Peer Injuries on Suspect Injuries

Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 divided by baseline rate of suspect injuries and 90% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on lags and leads of treatment without fixed effects. Standard errors clustered by academy cohort (G = 73). Includes individual and district-week fixed effects. Treatment defined as injury of a former peer. Red vertical line represents treatment.

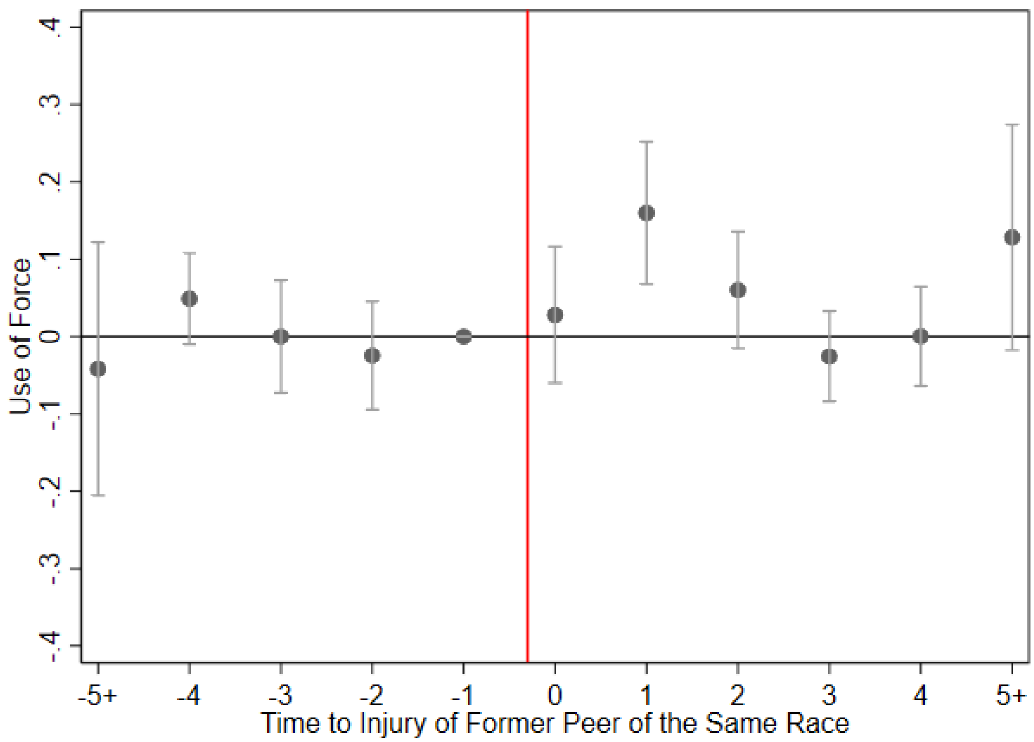

Figure 9: The Effect of Same Race Past Peer Injuries on Force Use

Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 divided by baseline rate of force use and 90% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on lags and leads of treatment without fixed effects. Standard errors clustered by academy cohort (G = 73). Includes individual and district-week fixed effects. Treatment defined as injury of a past peer who is of the same race as the officer. Red vertical line represents treatment.

A. Appendix

A.0.1 Construction of Peer Groups

Summary statistics for these entrance lotteries appears in Table 1. On average, 85% of test takers pass the entrance exam and 20% of these enter the police academy.[21] We evaluate the balance of the lotteries by performing a multinomial logisitic regression of start month group on the police officers' age, race, and sex. We then use a chi-squared test to determine whether any of the characteristics can predict entrance to a certain police academy cohort. There appears to be some imbalance in two of the nine test-cohorts. This imbalance would be concerning if we were explicitly looking at the effect of contextual effects in police force. However, since the empirical strategy uses a difference-in-differences design the imbalance in these two cohorts will not bias the treatment estimates.

For this reason, we restrict the sample to officers who enter one of 25 geographic districts after graduating from their probationary period. This means that we drop non-standard units such as the canine unit or S.W.A.T. team, who move between geographic districts from day to day. We also drop officers who leave the police academy before six months or individuals who never are registered as leaving the police academy in our sample. We cannot link these data to academy cohorts or the TRR data and cannot be used in the analysis. We also drop thirty-three indiviudals who have cohort start dates with five or fewer people.

[21] There is substantial heterogeneity in the portion of eligible people who enter the academy, ranging from three percent in 2013 to 64% in the first 2006 exam.