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Spillover 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, through a random lottery, began the police academy together. 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 heightened risk aversion. JEL CODES: B55; D81; D83; J01, K00, K10, K42

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1. INTRODUCTION

Law enforcement is an unpredictable and hazardous occupation. Between 2004 and 2016, Chicago police officers arrested 130,000 adults and one in 175 of these resulted in an officer being injured. In 2014, police officers also reported an on-the-job injury rate four times higher than the average all other occupations (BLS, 2014) with the leading cause of these injuries being assaults or violent acts by the civilians with whom they interact (Tiesman et al., 2018).[1] Officers are cognizant of this risk: a 2016 PEW poll found that 84% of police officers worried about their safety on the job with 42% stating that they are nearly always seriously concerned (PEW, 2016). The use of physical violence against suspects to compel compliance is seen as a central means of self-protection against this risk, yet it often has tragic outcomes.

Nearly four percent of arrests involve physical force. From 2015 to 2018, police officers killed over 3,000 individuals (The Guardian, 2018; Washington Post, 2018), and there is growing evidence of externalities resulting from the use of force. The use of police force can reduce the educational performance of minority groups (Ang, 2019; Legewie and Fagan, 2019), affect attitudes toward police (Skolnick and Fyfe, 1993; Weitzer and Tuch, 2004; Brunson and Miller, 2005) and undermine police legitimacy (Tyler 2004; Ramsey and Robinson 2015; Lum and Nagin 2017; Nagin and Manski 2017). In response, there has been a surge of research trying to understand the use of force by officers and the policies aiming to curb it.

The decision to use force is a consequence of officer-civilian interactions. In

[1]While police officers face an on-the-job risk higher than the average profession, felony killings of police have decreased by fifty percent from 1992 to 2013 (National Law Enforcement Officers Memorial Fund).

each encounter with a civilian, officers face the risk of injury. In the language of Ehrlich and Becker (1972), since officers cannot fully insure against the risk of injury, they use force to satisfy their demand for self-protection. However, past researchers have found evidence suggesting officers may deviate from use-of-force policies of using force due to multiple factors: incomplete training or monitoring (Prendergast 2001; Prendergast 2003; Shi 2009; Drover and Ariel, 2015; Ready and Young, 2015; Ariel et al., 2017; Owens, 2018; Ba, 2018; Ba and Rivera, 2019); the technology at their disposal (Ba and Grogger, 2018); or discriminatory preferences against civilian suspects (Legewie, 2016; Fryer 2018; Cesario et al., 2018).

This paper examines the role of peer injuries in an officer's decision to use force. We first show that there is a strong correlation between officer injuries and the propensity of others to use force (Figure 3). We then exploit the random lottery that determines which officers begin the police academy in the same month to identify exogenously assigned peer groups. Using officers assigned to different units after graduating from the academy, we conduct an event study on the frequency of uses of force following a former peer on-duty injury.

Our peer group definition follows Ager, Bursztyn and Voth (2019). We exclude peers still serving in the same police district to avoid contamination from correlated shocks to civilian non-compliance. This research design allows for exogenous variation in peer characteristics that is uncorrelated with individual characteristics, avoiding common issues in the estimation of endogenous peer effects or contextual effects (Manski, 1993; Angrist, 2014).

We show that peer injuries substantially increase the use of force in the week following a former peer's on-duty injury, leading to more civilian injuries.

Consistent with homophily, we find that injuries to more demographically similar peers have stronger effects.

The question of whether there are spillovers in police force resulting from on-the-job injuries has important implications for policies meant to reduce improper use of force. Policies which 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) finds that assaults against officers increased after the introduction of body cameras. Given our findings, this suggests that failing to account for injury-risk externalities associated with peer injuries will cause policy makers to under-state the impact of body cameras on the use of force.

Body cameras themselves might make officers less likely to use force, but if body cameras cause increased peer injuries, as Ariel et al. (2017) find, then the officers' resulting decrease in use of force would be smaller or even negative 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. [2] If the available technology decreases the propensity of an officer to be injured, their peers will respond by reducing force-use.

To aid in the interpretation of these results, we go on to investigate potential drivers of the behavior. We consider three potential channels: social learning (Banergee et al, 1992; Bikhchandani et al., 1992), peer effects in misconduct (Manski, 1993; Ouellet, 2019) and transitory emotional shocks that increase risk-aversion (Lowenstein et al., 2015; Shum and Xin, 2019). We find limited evidence of social learning and no evidence of peer effects

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

in misconduct. We attempt to determine whether the transitory emotional shocks officers face cause an increase in preferences for certainty (Cullen et al., 2014) or frustration (Aizer, 2009; Card and Dahl, 2011; Munyo and Rossi 2011; Eren and Mocan, 2016). A substantial increase in the probability of receiving a complaint for failing-to-provide service in the week following a peer injury suggests that primed risk-aversion may be driving these results.

This paper contributes to the burgeoning literature on the determinants of police force (e.g., Fryer, 2018; Owens, 2018, Rivera and Ba, 2019; Ba, Knox, Mummolo and Rivera, 2019; Annan-Phan and Ba, 2019) by providing evidence that police officers respond to the risk faced by their peers. This paper also contributes to a growing literature demonstrating that traumatic events can influence decisions under uncertainty.

Callen et al. (2014) find that individuals experiencing violence have increased certainty premiums, while Cameron and Shah (2013) find similar increases in risk aversion among individuals affected by natural disasters. Additionally, Imas, Kuhn, and Mironova (2018) find that violence can lead to increased impatience while Moya (2018) and Brown et al (2018) find that recent violence can trigger increases in risk aversion.[3] The paper most related to this work is Shum and Xin (2019) find that "near-miss" accidents increase risk-aversion by three to thirty percent. We find that police officers increase their propensity to use force without a significant decrease in the propensity to be injured after a peer experiences an injury. This is consistent with the officers having temporarily heightened risk aversion.

[3] Not all of the evidence points to risk aversion increasing after traumatic experiences. Voors et al. (2012) and Eckel et al. (2009) find that traumatic experiences can decrease risk aversion. Exposure to violence has been shown to negatively impact emotional regulation (Osofsky, 1995)

We contribute to the literature on the effect of emotions on decision making. Outside of this paper, much of the research on this topic has centered on the effect of the weather (Busse et al., 2015) and unexpected losses of local sports teams (Card and Dahl, 2011; Munyo and Rossi 2011; Eren and Mocan, 2016). We extend this literature to show that peer injuries may act as a transitory emotional shock influencing the emotional state of economic agents in a high-stakes setting.

Our findings also have broader implications for the peer effects literature. Similar to Hjort (2014) and Agarwal et al. (2018), we find that individuals respond to the *outcomes* their peers experience, but do not directly respond to their actions. Similar to Bayer et al., (2009) we also find that peers can influence the decisions of individuals long after the observed network has dissipated. This result suggests that researchers must account for the effects of negative events that may coincide with peer actions when interpreting the channel through which peers respond. For example, Carrell and Hoekstra (2010) find negative spillovers from children in troubled families, using domestic violence at home as an instrument. They argue that these peer effects operate through the reduced achievement or increased disruption of the affected child, supporting a "bad apples" model of peer effects. Similarly, in a quasi-experimental study with the military Murphy (2018) attributes contemporaneous misconduct as peers responding to the poor behavior of their peers. The findings in our paper suggest that these results may partially driven by individuals responding directly to the negative outcomes realized by their peers.

The rest of this paper proceeds as follows. Section II provides background on the formation of police networks and the Chicago Police Department's use of force policy. In Section III, we describe the data we use. Section IV describes the research design used to generate the estimates presented in Section V. Section VI discusses the potential mechanisms driving these effects. We conclude in section VII.

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[4], (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.[5] 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.

[4]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.

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

Referred applicants proceed to Step 4 where they take further examinations. These include a physical test, background check, psychological evaluation and drug test. After the officer passes these examinations, they start 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 4 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 8 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 1,000 hours of basic training over six months. Training includes instruction on 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 [6] [7] (Chicago Police Union Agreement, 2017). During the initial twelve months of active duty, the CPD assigns probationary officers to districts at their discretion. Duty assignments can change day-

[6] Nearly all officers who begin training graduate from the police academy with fewer than 3% of officers failing (Chicago Tribune, 2017).

[7] The probationary period consists of eighteen months of active duty. The six months in the academy and the next twelve in the probationary assignments. Time absent from duty does not apply toward completion of the probationary period.

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.

Police districts operate under the Bureau of Patrol. They are segmented 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, etc. 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 (5). The composition of each unit is made up of many different cohorts (7).

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).

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 handcuffs or applying pressure to sensitive areas. Officers are also permitted to use higher level responses with or without weapon; 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.

In general, the CPD requires officers to use force that is "objectively reasonable, necessary, and proportional to the actions of subject (Chicago Police Department General Order G03-02, 2017). There is no formal definition of "objectively reasonable." However, officers are required to take into consideration 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 use of force, the CPD explicitly takes into account the fact that department members may not have perfect information regarding the compliance status of the suspect and that these decisions are made quickly and under tense circumstances.

The proportionality requirement is in reference to the officers contemporaneous beliefs about the threat he or she faces. This may be different than the threat determined by an objective observer. Officers are only only allowed to use force which is likely to cause death or great bodily harm to subjects when it is the subject poses an imminent threat of death or great bodily harm to the officer or others in the surrounding area or when a person who had committed a forcible felony that involved the threatened infliction of great bodily harm was trying to avoid arrest or run from the police. This type of force is meant to be a last resort when all other de-escalation methods have failed.

3. DATA

To answer our research we use four sources of administrative data from the Chicago Police Department. Data on the use of force and injuries comes from the CPD's Tactical Response Reports (TRR) for non-juvenile suspects. Tactical Response Reports are required in all incidents where a member of the police department uses firearms, impact munitions, Tasers, acoustic devices, impact weapons, mechanical actions/techniques, or chemical weapons.[8] Minor levels of force, such as holds, handcuffing, and the force necessary to overcome passive resistance do not require a TRR. However, members also have to fill out TRRs when a suspect alleges an injury, if the suspect resists arrest in a manner more severe than fleeing or in situations where suspects use force against someone in the encounter, regardless of the officer's decision to use force. (General Order G03-02-02).

Our data encompass over 16,000 instances of force by the CPD between January 2005 and October 2016. We supplement the data with employment records that include unit assignments. 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.[9] Second, the data contain detailed information about the time and location of the incident along with suspect, officer, and interaction characteristics. Third, the data report the start date of the officer, which is critical to our identification strategy.[10]

The data also describe the suspects' actions during the encounter from the

[8]The police department also requires TRRs for force involving canines, but canine units are excluded from the analysis.

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

[10]Any police officer without a recorded start data is excluded from the analysis.

perspective of the officer. The reports allow officers to classify suspects into different resistance categories. Cooperative suspects comply with the officers absent the use of force. Non-cooperative suspects are classified as active resisters, passive resisters, or assailants. Passive resisters fail to comply with verbal or other direction. An active resister attempts to create distance between herself and the officer. Assailants either threaten or use violence against the officer or a third party.

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). This data contains all allegations of misconduct filed by civilians or other officers during our sample period. These data include the individual(s) who filed the complaint, the individual(s) against whom the complaints were levied, the date of the action about which the individual complained about, and the details of the action (See Ba (2017) for more details). Attached to each officer week, we include an indicator variable equal to 1 if the officer has any compliant filed against them regarding an incident that occurred the week after a peer injury. We also investigate three specific types of arrests: force and verbal abuse, false search or arrest, and failure to provide service.

The data do have some flaws and limitations. The CPD use of force model does not require TRRs for situations that involve minor force. For this reason, it is likely that there is underreporting of this type of force and interactions with cooperative suspects. Second, we observe the presence of an injury, or alleged injury, to officers and civilians. However, the nature, extent, severity, or cause of the injuries are not observed in our data.[11] Since

[11]The CPD refused to provide this information in the FOIA citing HIPPA privacy reglua-tions.

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 the officers claim were assailants. While we cannot learn the nature or extent of officer injuries from the data, there is some literature on the type of injuries officers sustain suggesting that the primary cause of injury is violence experienced on duty.[12]

Even under this restriction, we cannot know with certainty that the suspect caused the injury or even that the suspect exerted a high level of resistance. Officers may twist and ankle or sustain an injury by accident during interactions with assailing civilians and these will be mis-classified as treatment events in the analysis. Moreover, suspects and officers may have different opinions about the amount of force used by the suspects. There is no independent check on the accuracy of the officer's description of a suspect's resistance. As such, there is measurement error in the provision of this information. This mis-classification means that in some instances, we will be treating control periods (no former peer injuries) as treated periods (at least one former peer injury). This means that the mis-classification will attenuate our treatment effects.

Lastly, CPD officer unit assignment data records officers as being a part of the "academy" unit until they finish their probationary period. This means

[12]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 or injuries by persons or animals (BLS, 2014). The next most common injury category was falls, slips, and trips; this category accounted for 25.3% of injuries. Overexertion followed, accounting for 21.4%. Using data from the National Electronic Injury Surveillance System-Occupational Supplement, Tiesman et al. (2018) 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. Next most common were bodily reactions and exertions as well as transportation incidents. Most 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.

that we cannot observe the geographic assignment in the year between when an officer graduates the academy and when the officer is assigned a permanent unit. Since local non-compliance shocks are a major threat to identification, we drop entries from this period of time from the data set.

The limitations of our data force us to place some restrictions on the sample. We observe an officer's unit assignment, but not the tasks they perform on an individual day. For this reason, we restrict the sample to officers who enter one of 22 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.[13].

A total of 3,548 officers start the academy in our sample. After excluding officers who do no enter into a geographic district, we are left 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.

4. RESEARCH DESIGN

The goal of the empirical analysis is to identify the causal effect of a police officer's on-the-job injury on one's network. There are four major challenges

[13]See Appendix for more information.

to identifying this effect, which this definition of peers helps us overcome. Foremost, we need to be able to observe the relevant network for officers in our sample. 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 the data: all officers, officers in the same unit, or officers who attended the police academy together. Following Ager, Bursztyn and Voth (2019), the network definition we use is officers who, through the random lottery, attended the police academy together but no longer work 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.[14]

This definition is used to overcome two threats to identification. Force-use and officer injuries are co-determined. If more aggressive officers are able to sort into more aggressive networks, then the correlated probability of injuries and force-use will spuriously look like peer effects in the data. The CPD assignment process rules out the possibility that officers sorted into more aggressive cohorts. Second, there may be district-level common shocks to civilian non-compliance within a district. If some shock increases the probability that civilians in a given police district are non-compliant, then 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 officers will be compared to other individuals who face the same non-compliance rate.

Finally, we must be able to overcome the simultaneity between an individ-

[14]This means that individuals who attended the police academy together and still work in the same police district are considered to be untreated if an individual from their network is injured.

ual's actions and their peer's actions referred to as the reflection problem (Manski, 1993). Angrist (2014) shows that designs relying on ”cohort variation”, like that of the random lottery, do not overcome the reflection problem because identification relies on finite-sample fluctuations in treatment assignment. The ideal research design for measuring spillover effects would randomly assign subjects to treatment with an assignment probability that is independently distributed across groups (Crepon et al., 2013). We approximate this research design by combining the random assignment of cohort with the quasi-random timing of injuries to officers.

In practice, we construct the counterfactual outcomes within district d and week t variation, using injuries to police academy classmates as the treatment. The effect of an officer injury is identified through an event study comparing 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 occurrs at time $t = E_i$ for individual i. We denote individual fixed effects as λ_i, district-week fixed effects as λ_{dt}. The main estimating equation used to recover the causal effect of peer injuries is

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

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, 2019). The treatment, $\mathbb{1}[t = E_{g,-d} + 1]$, is 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. 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. injured officers attended the police academy with 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. Event time in this regression is denoted τ. We omit the dummy for the week before a former peer is injured, set period -5 to be equal to one if the week 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.[15]

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

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-

[15]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.

compliant civilians or signal interpretation, but identification may still be threatened by unobserved post-treatment shocks specific to members of a particular police academy cohort.

We expect the results from this regression to be attenuated. Dropping the data from the probationary year prevents us from observing the effect of many officer's first event. If the effect of treatment is decreasing in its frequency, then we would expect this to bias the effects downward. Similarly, the social cohesion within a cohort is likely the largest right after the academy ends. As time passes, police officers may fall out of contact with each other or develop different social networks. Thus, we should expect this year to have the largest treatment effect on subjects. We expect the coefficients from these regressions to be a lower bound of the average effect because we will classify some innocuous injuries resulting from falls as civilian attacks. This means that there will be some control events misclassified as treatment events.

It is also unclear how quickly officers learn about peer injuries. If officers learn about these injuries at different rates, we may be missing some of the dynamics. Finally, since injuries can happen at any time, there are periods of time that are both within five weeks after an injury and five weeks before another injury. This means that there are some periods where the pre-period for one injury is the post-period for another injury. This will bias the effect in the pre-period upwards and at the same time the post-period downwards.

Lastly, 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 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

5.1 MAIN RESULTS

We first examine the effects of former peers' injuries on the propensity to use any type of force by estimating Equation (2). As cohorts join throughout the sample period, the estimation equation will drop individuals who are not employed for five weeks before and after exposure. We estimate the base rate of force use as the constant term from a regression of (2) without individual or unit-week fixed effects. Figure (9) 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. We display the magnitude of this ratio for each period on the vertical axis with the time until exposure presented on the horizontal axis.

The effects of peer injuries are not significantly different from zero 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, 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 week. It also will take time for the officers to learn about their peer's injury status.

In the week following a peer injury, use of force increases by more than five percent relative to the period before a peer is injured. The treatment effects dissipate quickly over time, immediately losing significance after the first week post exposure. This pattern is consistent with Card and Dahl (2011) and Munyo and Rossi (2013) who find that incidental emotional shocks have a short-lived effect on violence.

Table 4 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. Columns (3)-(5) estimate different alternative specifications which control for the number of former peers on duty in a given week and entrance-exam cohort fixed effects. Our preferred specification is in Column (2). Across models we estimate large and statistically significant effects around 7 percent of the baseline mean.

The 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. The CPD's policy of hiring eligible applicants in lottery order supports this assumption, as the randomization should lead to these groups being equal on average. However, the randomization is conducted on the level of the cohort. Therefore, 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.

While individual fixed effects remove the time-invariant factors, we attempt to rule out time-variant factors that are influenced by changes in the composition of individuals across different testing periods. Columns (4) and (5) of Table 4 shows 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 does 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 the increased 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 (10) and Table (6). The baseline rate of suspect injury is 0.55% in a given week. Injuries to former peers increases the propensity for suspects to be injured during interactions with the police by 9.78% of the baseline mean ($p < 0.087$).

5.2 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. This definition relies on the assumptions that these individuals were acquainted with each other and maintained their bonds after the academy ended. However, since peer groups are large it is unlikely that all of the group members satisfy these requirements. Indeed, Carrel et al. (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 individuals with those who are not actually treated. To further test whether these effects are driven by information spread by *former* peers, we wish to 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 (11) 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 (11) 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% ($p < 0.01$) more likely to use force in the week after a peer is injured, with a baseline probability of using force of 1.6%.

5.3 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.

5.3.1 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 higher level of 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 take down, open hand strike, punches, kicking, 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 are the weapons 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 and present the results in Table 8.

Since the baseline use of any force is low, the baseline force use for each type of force-use is very low 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 less underreporting for types of force that are harder to conceal (firearms and Tasers).
Similar to our main regressions, we do not find any evidence of pre-trends in any specification except for in the use of 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 suspects who offer low levels of resistance. This suggests that absent a peer injury, officers would have not considered these suspects as a threat. But, after a peer injury they now view the suspects as non-compliant. We investigate this claim more directly in the next section.

5.3.2 SUSPECT CHARACTERISTICS

Next, we look at treatment effect heterogeneity depending on the officer-reported level of resistance of the suspect, the threat posed to the officer, and the race of the suspect. Table 9 displays variants of Equation (2) on indicators equal to one if force is used against suspects with a stated resistance type and presence of a weapon.

We do not find any significant changes in the reported resistance of suspects. However, we caution the reader to interpret these findings with some skepticism since suspects may disagree with the Officer's recorded assessment of their resistance.

Columns (6) and (7) examine the effects based on a more objective measure of the risk to the officer – whether the suspect was armed. Here, we see that use of force against armed suspects increases by roughly 6%. Due to a smaller baseline, we see a larger percentage increase in use of force against armed suspects; however, this is not statistically significant.

Finally, we investigate heterogeneity based on the similarity of the suspect to the individual who injured the officer's peer. Police officers have been shown to use in previous research to incorporate race in their decision making (Fryer, 2018; Knowles, Perisco and Todd, 2011; 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 force use.

In Table 10, we present the results from nine regressions. In each of these regressions, the outcome is use of force against a member of the race stated in the column while the treatment is an indicator for a former peer being injured by an assailant of a certain 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 use of force against African American subjects significant increases in the week after an African American individual injures the officers peer. There are no significant increases in 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.

These results should also be interpreted with caution. 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 three distinct categories: social learning, traditional peer effects, and emotional responses.

6.1 SOCIAL LEARNING

The first mechanism we entertain is social learning (Banergee, 1992; Bikhachandani, Hirschleifer and Welch, 1992). If officers maintain close contact with individuals who contemporaneously attended the police academy, they may be more likely to learn about injuries-on-duty which occur on their former peers' patrols. In this way, injuries to former peers would act as a private signal of the underlying injury risk during civilian interactions. This may lead to officers updating 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 of the effects, friends of injured former peers will have a second signal about the true injury risk officers face while on duty. This should translate into a reducing in the probability that the officer experiences 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 function equal to one if the officer was injured in a given week. The results of this comparison are reported in Figure 10 and column (2) of Table 11. 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. This leads us to believe that 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 entertain is conventional peer effects. There is a large body of work showing that individual choices are influenced by the actions of one's peer group (see Brock and Durlauf, 2001) and consistent with our results that peer groups mainly operate within race (Garlick, 2018). Moreover, Murphy (2017) finds that misconduct by soldiers in the US Army tend to occur at similar times the misconduct of peers, suggesting that officers may be responding to the proper or improper use of force by their peers. Since injury risk and the decision to use force are co-determined, the data patterns we find might be spurious and result instead from officers choosing to use force because they learned about their peers' choices.

Table 3 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 use which are not accompanied by an officer injury. We use these 14,000 instances of force use to investigate whether force-use mimicry is driving these results.

Column (1) of table 11 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

Finally, we investigate whether temporary changes in the officers' preferences drive these results. Previous literature has shown negative affect can influence individual's propensity to engage in violence (see Card and Dahl, 2011; Munyo and Rossi, 2011; Eren and Mocan, 2018).[16] Moreover, there are several laboratory experiments which show that exposure to violence can affect time and risk preferences.

[16]While this literature focuses on negative affect resulting from the unexpected loss of a sports match, we expect former peer injury-on-duties to similarly induce negative affect. Osofsky (1995) shows that violence can reduce an individual's ability to regulate emotions.

Loewenstein (1996) has documented that preferences can be malleable and can be temporarily affected by emotional states. Traumatic events, such as natural disasters, have been shown to impact risk-preferences (Eckel et al., 2009; Behir and Willinger, 2013; Cameron and Shah, 2013; Hanaoka et al., 2018) and trust (Cassar, Healy and Kessler, 2017). Exposure to violence has been shown to predict preferences for more immediate and less uncertain rewards. Similarly, Hjort (2014) find that animus discrimination can increase in response to ethnic conflict and Rohlfs (2010) find 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 situations in which they may be harmed 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 complaints against officers to understand their behavior.

We supplement the primary data set with a data set on complaints against officers used in Ba (2017).[17] Table displays the results from variants of Equation (2) using different types of complaints as outcomes. This is an indicator equal to one if the officer committed an action that week resulting in a complaint from either a civilian or a fellow officer.[18] Column (1) displays the results for any type of complaint, column (2) displays the results for complaints for 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 be a 15% increase in the probability of receiving a complaint for failure to provide service in the week after a peer is injured. Arrests for improper search or arrest also increase after a peer is injured suggesting that officers may be searching individuals because they viewed them as more likely to be a threat. Importantly, complaints for improper force use or verbal usage (for example, racial slurs) decrease by about 7.5% in the week a peer is injured. Together, this suggests that officers increase their force use in the wake of peer injuries due to heightened risk-aversion rather than frustration or retaliation.

7. CONCLUSIONS

Police officers face the difficult mandate of safely arresting suspects who pose a risk to themselves or others. While the state empowers officers with

[17] See Ba (2017) for a more detailed discussion of the complaints.

[18] 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.

the legal use of violence, this violence has negative externalities on society. It can erode the legitimacy of law enforcement (Tyler 2004; Ramsey and Robinson 2015; Lum and Nagin 2017; Nagin and Manski 2017), lead to potentially violent protests (Basu, Yan and Ford, 2014) and harm the members of the society that officers are entrusted to protect (Ang, 2019).

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 and the probability they injure a suspect in the following weeks. This effect is larger when restricting the definition of peers to same-race officers who attended the academy together. Our finding that the risk of injury-on-duty is statistically unaffected by peer injuries suggests that social learning is not the main 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 for failure to provide service after a peer injury suggests that primed risk-aversion may be the primary driver of the effects.

These findings suggest that the risk of injury can be an important motivator for the use of force and have externalities on other officers. Policy makers must take these externalities in to 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. Policies meant to reduce force use which do not change or increase the risk to officers may have limited effects.

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9. Appendix

9.0.1 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.[19] 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.

[19] 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.

9.1 Figures

Figure 1: First Page of Tactical Response Report

TACTICAL RESPONSE REPORT / Chicago Police Department

INCIDENT

DATE OF INCIDENT | TIME | ADDRESS OF OCCURRENCE | LOCATION CODE | BEAT/OCCUR. | VIDEO RECORDED INCIDENT ☐ BWC ☐ IN-CAR VIDEO ☐ OTHER VIDEO

BUSINESS NAME | ☐ DNA EXACT AREA WITHIN LOCATION (E.G., BASEMENT, STAIRWAY ,BEDROOM) | ASSIGNMENT TYPE ☐ ON-VIEW ☐ OTHER ☐ SUPERVISOR DIRECTED ☐ CALL FOR SERVICE

EVENT NO. | RD NO. | IUCR CODE | IR NO. | CB NO. | INVOLVED A PURSUIT? ☐ NO ☐ FOOT ☐ VEHICLE ☐ OTHER

LIGHTING ☐ DAYLIGHT ☐ DARKNESS ☐ DUSK ☐ DAWN ☐ ARTIFICIAL | WEATHER ☐ CLEAR ☐ CLOUDY ☐ RAIN ☐ SNOW/ICE ☐ FOG | PATROL TYPE? ☐ POLICE CAR ☐ FOOT ☐ BICYCLE ☐ MOTORCYCLE/ PAPV ☐ SQUADROL ☐ VAN/BUS ☐ OTHER: | MEMBER WAS? ☐ ALONE ☐ WITH PARTNER | ASSIST UNITS ON SCENE? ☐ YES ☐ NO | INCIDENT ☐ INDOOR ☐ OUTDOOR

INVOLVED MEMBER

RANK | LAST NAME | FIRST NAME | EMPLOYEE NO. | WATCH | SEX ☐ M ☐ F | RACE | AGE | HT. | WT.

DATE OF APPT. | UNIT & BEAT OF ASSIGN. | DUTY STATUS ☐ ON ☐ OFF | IN UNIFORM? ☐ YES ☐ NO | TYPE OF MEMBER INJURY ☐ None / None Apparent ☐ Minor Swelling ☐ Minor Contusion/Laceration ☐ Complaint of Substantial Pain ☐ Significant Contusion ☐ Laceration Requiring Sutures ☐ Broken/Fractured Bone(s) ☐ Heart Attack/Stroke/Aneurysm ☐ Gun Shot ☐ Fatal ☐ Other (Explain)

SUBJECT INFORMATION ☐ DNA

LAST NAME | FIRST NAME | M.I. | SEX ☐ M ☐ F | RACE | D.O.B. | HT. | WT.

ADDRESS | TELEPHONE NO. | CONDITION ☐ Apparently Normal ☐ Injured by Member ☐ Injured Not by the Member's Force ☐ Alleges Injury by Member ☐ Under Influence of Alcohol ☐ Under Influence of Drugs ☐ Mental Illness / Emotional Disorder ☐ Disability ☐ OTHER (Specify)

MEDICAL TREATMENT? ☐ Refused Medical Aid ☐ Offered/EMS Requested ☐ Performed by Member ☐ Performed by CFD EMS ☐ Taken to Hospital (Specify) ☐ OTHER (Specify) | SUBJECT INJURY BY MEMBER'S USE OF FORCE? ☐ None/None Apparent ☐ Subject Alleged Injury ☐ Non-Fatal - Minor Injury ☐ Non-Fatal - Major Injury ☐ UNK ☐ Fatal

SUBJECT'S ACTIONS (Check all that apply) ☐ DNA ☐ UNK

☐ DID NOT FOLLOW VERBAL DIRECTION ☐ UNABLE TO UNDERSTAND VERBAL DIRECTION ☐ VERBAL THREATS ☐ STIFFENED (DEAD WEIGHT) ☐ PULLED AWAY ☐ FLED ☐ IMMINENT THREAT OF BATTERY - NO WEAPON

☐ PHYSICAL ATTACK WITHOUT WEAPON. (SPECIFY) ☐ HAND/ARM/ELBOW STRIKE ☐ KNEE/LEG STRIKE ☐ MOUTH/TEETH/SPIT ☐ PUSH/SHOVE/PULL ☐ GRAB/HOLD/RESTRAIN ☐ WRESTLE/GRAPPLE ☐ OTHER (DESCRIBE)

☐ THROWN OBJECT (DESCRIBE) ☐ IMMINENT THREAT OF BATTERY WITH WEAPON ☐ ATTEMPT TO OBTAIN MEMBER'S WEAPON ☐ PHYSICAL ATTACK WITH WEAPON ☐ USED FORCE LIKELY TO CAUSE DEATH OR GREAT BODILY HARM ☐ OTHER (DESCRIBE)

WAS SUBJECT ARMED WITH WEAPON? ☐ NO ☐ YES, DESCRIBE BELOW: ☐ BLUNT OBJECT (DESCRIBE) ☐ CHEMICAL WEAPON ☐ TASER/STUN GUN ☐ VEHICLE ☐ KNIFE/CUTTING INSTRUMENT ☐ SEMI-AUTO PISTOL ☐ REVOLVER ☐ RIFLE ☐ SHOTGUN ☐ EXPLOSIVE DEVICE ☐ OTHER (DESCRIBE)

☐ WEAPON/OBJECT PERCEIVED AS:

WEAPON USE: ☐ DNA ☐ Possessed ☐ Displayed, Not Used ☐ Used - Attempt to Attack Member ☐ Used - Attacked Member ☐ Obtained Member's Weapon ☐ Member at Gunpoint ☐ Member Shot/Shot At

DID THE SUBJECT COMMIT AN ASSAULT OR BATTERY AGAINST THE INVOLVED MEMBER PERFORMING A POLICE FUNCTION? ☐ NO ☐ YES | SUBJECT ACTIVITY Drug-Related? ☐ YES ☐ NO | Gang-Related? ☐ YES ☐ NO

TYPE OF ACTIVITY ☐ Ambush - No Warning ☐ Traffic Stop ☐ Disturbance - Domestic ☐ Investigatory Stop ☐ Man with a Gun ☐ Disturbance - Mental Health ☐ Disturbance - Riot/Mob Action/Civil Disorder ☐ Disturbance - Other ☐ Other - Describe in Narrative ☐ Processing/Transporting/Guarding Arrestee ☐ Pursuing/Arresting Subject

MEMBER 'S RESPONSE (Check all that apply) ☐ DNA ☐ UNK

REASON FOR RESPONSE? ☐ Defense of Self ☐ Defense of Department Member ☐ Defense of Member of Public ☐ Overcome Resistance or Aggression ☐ Stop Self-Inflicted Harm ☐ Fleeing Subject ☐ Subject Armed with Weapon ☐ Unintentional

FORCE MITIGATION EFFORTS

☐ MEMBER PRESENCE ☐ ZONE OF SAFETY ☐ MOVEMENT TO AVOID ATTACK ☐ TACTICAL POSITIONING ☐ NONE ☐ VERBAL DIRECTION/ CONTROL TECHNIQUES ☐ SPECIALIZED UNITS ☐ ADDITIONAL UNIT MEMBERS ☐ OTHER

CONTROL TACTICS

☐ ESCORT HOLDS ☐ WRISTLOCK ☐ ARMBAR ☐ CONTROL INSTRUMENT ☐ PRESSURE SENSITIVE AREAS ☐ EMERGENCY HANDCUFFING ☐ HANDCUFFS/PHYSICAL RESTRAINTS ☐ OTHER

RESPONSE WITHOUT WEAPONS

☐ OPEN HAND STRIKE ☐ TAKE DOWN ☐ ELBOW STRIKE ☐ CLOSED HAND STRIKE/ PUNCH ☐ KNEE STRIKE ☐ KICKS ☐ OTHER

RESPONSE WITH WEAPONS

☐ OC/CHEMICAL WEAPON ☐ OC/CHEMICAL WEAPON W/ AUTHORIZATION* ☐ LRAD W/ AUTHORIZATION* ☐ TASER ☐ CANINE ☐ BATON/EXPANDABLE BATON ☐ LESS LETHAL SHOTGUN (DESCRIBE BELOW) ☐ OTHER IMPACT MUNITIONS (DESCRIBE BELOW) ☐ REVOLVER ☐ RIFLE ☐ OTHER ☐ SEMI-AUTO PISTOL ☐ SHOTGUN

*AUTHORIZED BY (NAME) | RANK | STAR NO. | UNIT NO.

WAS ANY REPORTABLE FORCE USED AGAINST THE SUBJECT WHILE HANDCUFFED OR OTHERWISE IN PHYSICAL RESTRAINTS? ☐ NO ☐ YES IF YES, DESCRIBE SUBJECT'S ACTIONS AND MEMBER'S RESPONSE IN THE NARRATIVE SECTION.

WEAPON DISCHARGE ☐ DNA

WEAPON TYPE: ☐ CHEMICAL WEAPON ☐ TASER ☐ SEMI-AUTO PISTOL ☐ REVOLVER ☐ RIFLE ☐ SHOTGUN ☐ OTHER | NO. OF DISCHARGES OF THE WEAPON. | WEAPON SERIAL NO. | WEAPON CERT. NO.

DID THIS WEAPON CONTRIBUTE TO A SUBJECT INJURY? ☐ YES ☐ NO | DID THE DISCHARGE RESULT IN A SELF-INFLICTED INJURY? ☐ NO ☐ YES-SUBJECT ☐ YES-MEMBER | WAS SUBJECT VEHICLE USE AS A WEAPON? ☐ NO ☐ YES - AGAINST MEMBER ☐ YES - AGAINST OTHER PERSON

WAS DISCHARGE ONLY TO DESTROY/DETER AN ANIMAL? ☐ YES ☐ NO | WAS THIS AN UNINTENTIONAL DISCHARGE DURING A NON-CRIMINAL INCIDENT? ☐ YES ☐ NO | PERSON/OBJECT(S) STRUCK BY THE DISCHARGE OF MEMBER'S WEAPON (CHECK ALL THAT APPLY): ☐ SUBJECT ☐ OTHER PERSON ☐ DEPARTMENT MEMBER ☐ ANIMAL ☐ VEHICLE ☐ NONE ☐ UNKNOWN ☐ OTHER OBJECT

TASER DISCHARGE ONLY | TASER CARTRIDGE ID NO.(S) | PROPERTY INVENTORY NO. | CARTRIDGES DISCHARGED ☐ 1 ☐ 2 ☐ DNA ☐ OTHER | ADDITIONAL ENERGY CYCLES ☐ TRIGGER ☐ DNA ☐ 1 ☐ 2 ☐ OTHER ☐ ARC ☐ DNA ☐ 1 ☐ 2 ☐ OTHER | CONTACT STUN ☐ 1 ☐ 2 ☐ DNA ☐ OTHER | SPARK DISPLAY ☐ 1 ☐ 2 ☐ DNA ☐ OTHER

FIREARM DISCHARGE ONLY | WHO FIRED FIRST SHOT? ☐ MEMBER ☐ OFFENDER ☐ OTHER (Specify) | TOTAL NO. OF SHOTS MEMBER FIRED | WAS FIREARM RELOADED DURING INCIDENT? ☐ YES ☐ NO | MAKE/ MANUFACTURER | MODEL | DID MEMBER FIRE AT A VEHICLE? ☐ NO ☐ YES

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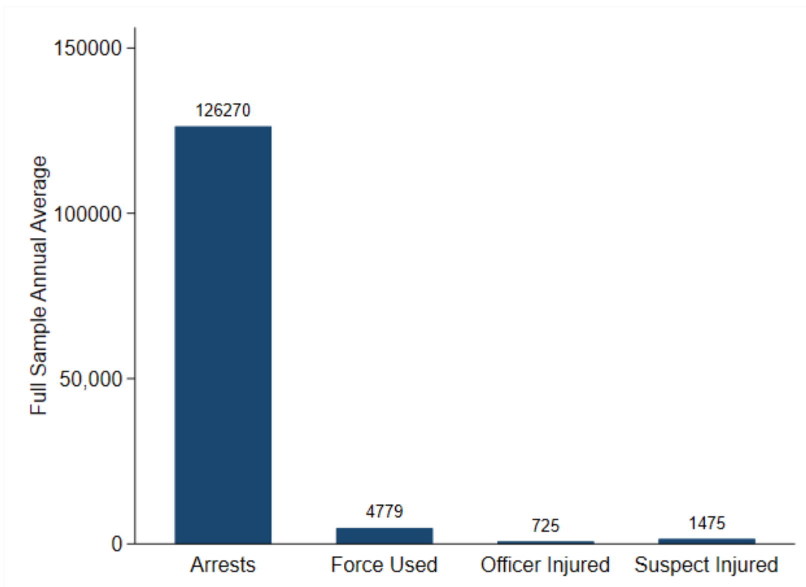
Figure 2: Average Annual Averages of Outcomes

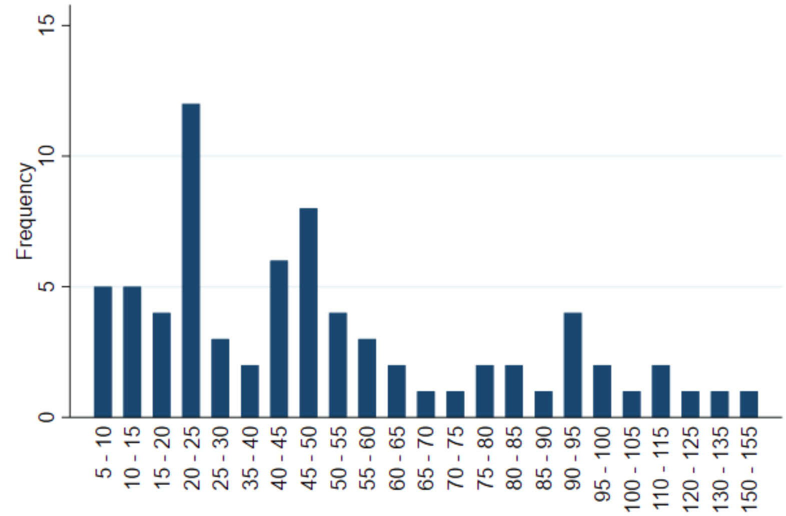


Note: Figure shows the average value of each variable per year for 2004 through 2016. The data used in the figure includes the full sample of CPD data and non-juvenile subjects.

Figure 3: Correlation Between Officer Injuries and Force Use by Others

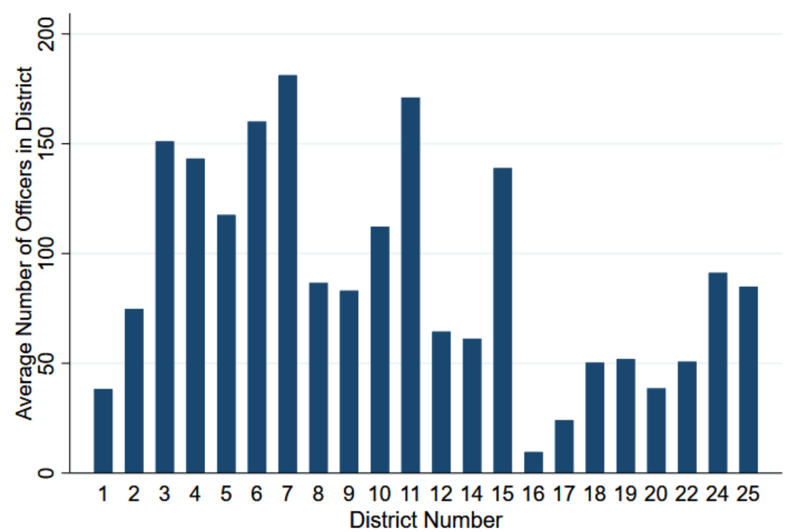
Note: Graph displays the relationship between the number of officers injured in a given week and the number of uninjured officers who use force in that same week. Uses the full sample of all officers included in Tactical Response Reports from 2004 to 2016. The blue line represents the regression line 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 4: Distribution of Cohort Sizes



Bins of cohort size. There are 3548 total officers in 73 unique cohorts.

Figure 5: Distribution of Unit Sizes



There are 3276 total officers in 22 unique units.

Figure 6: Average Number of Former Peers by Police District

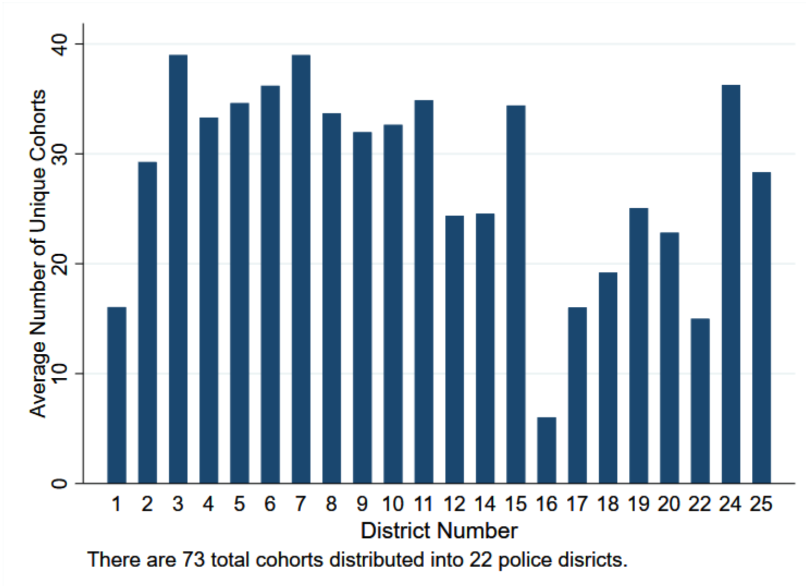


Figure 7: Distribution of the Number of Former Peers

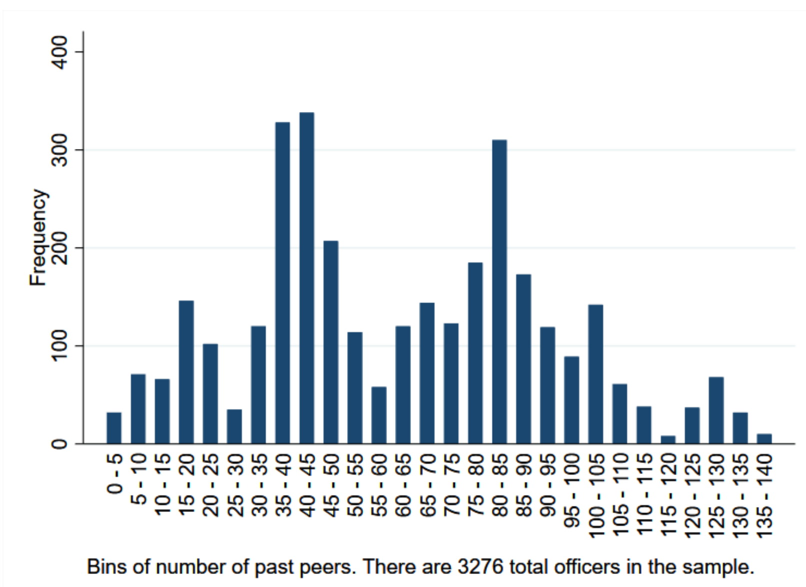
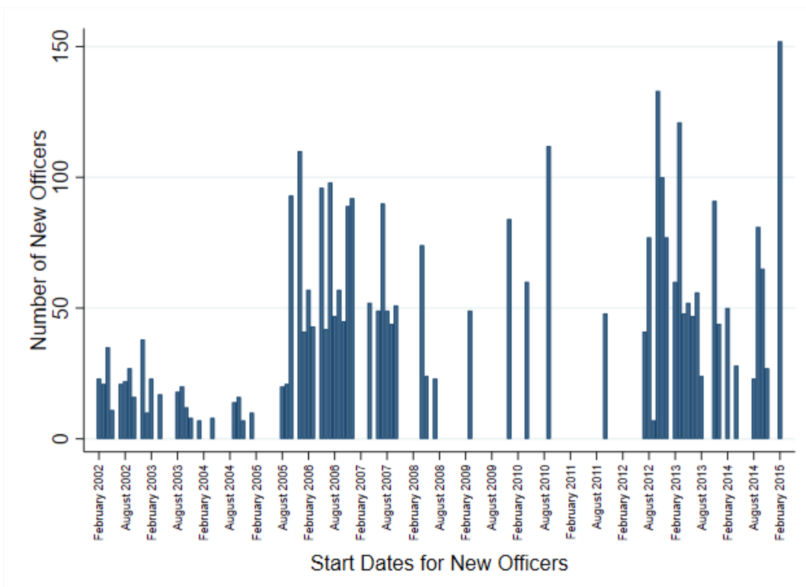
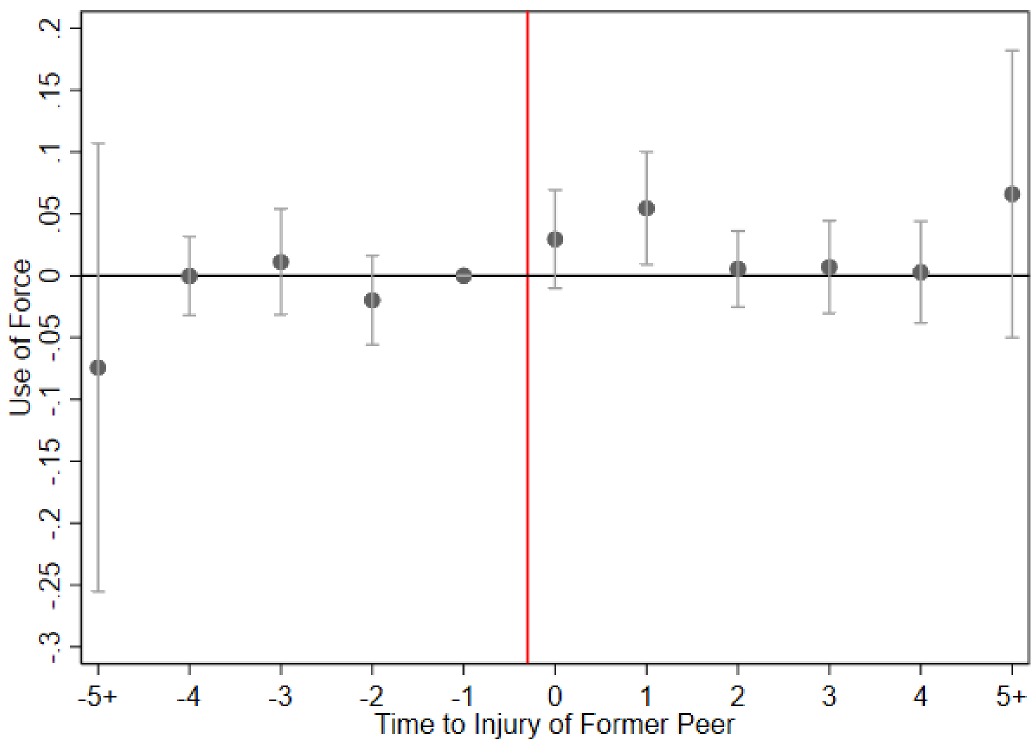


Figure 8: Addition of New Officers Throughout Sample Period



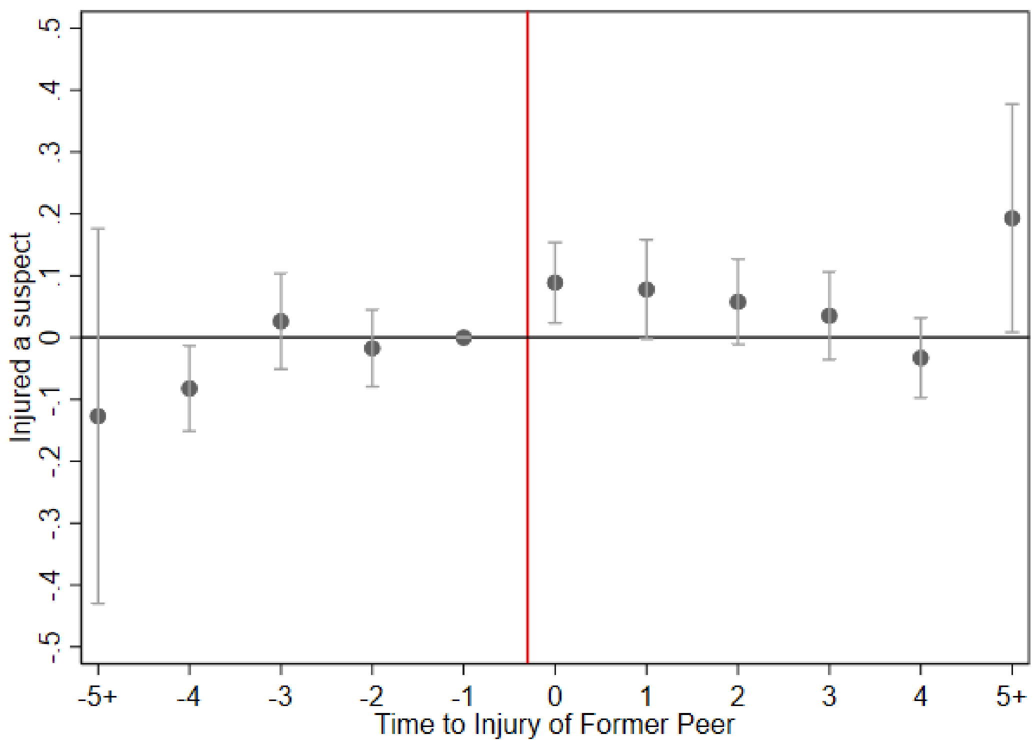
Note: Vertical bars display the number of new officers added in each month throughout the sample period.

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



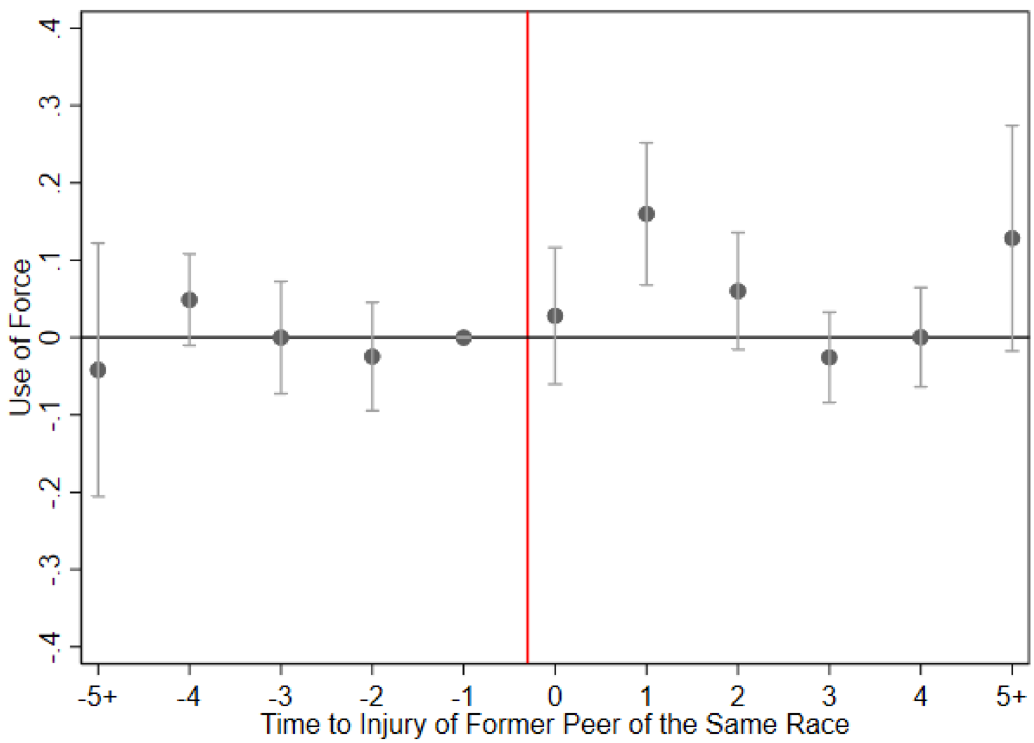
Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 divided by baseline rate of force use and 90% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on lags and leads of treatment without fixed effects. Standard 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 10: The Effect of Past Peer Injuries on Suspect Injuries



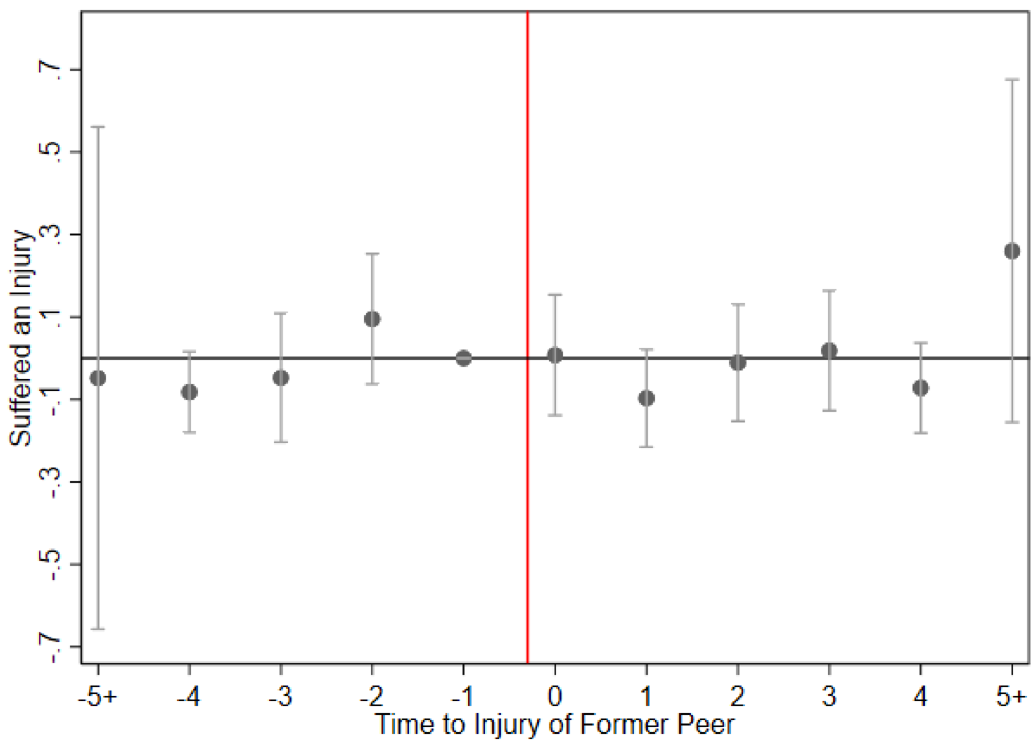
Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 divided by baseline rate of suspect injuries and 90% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on lags and leads of treatment without fixed effects. 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 11: The Effect of Same Race Past Peer Injuries on Force Use



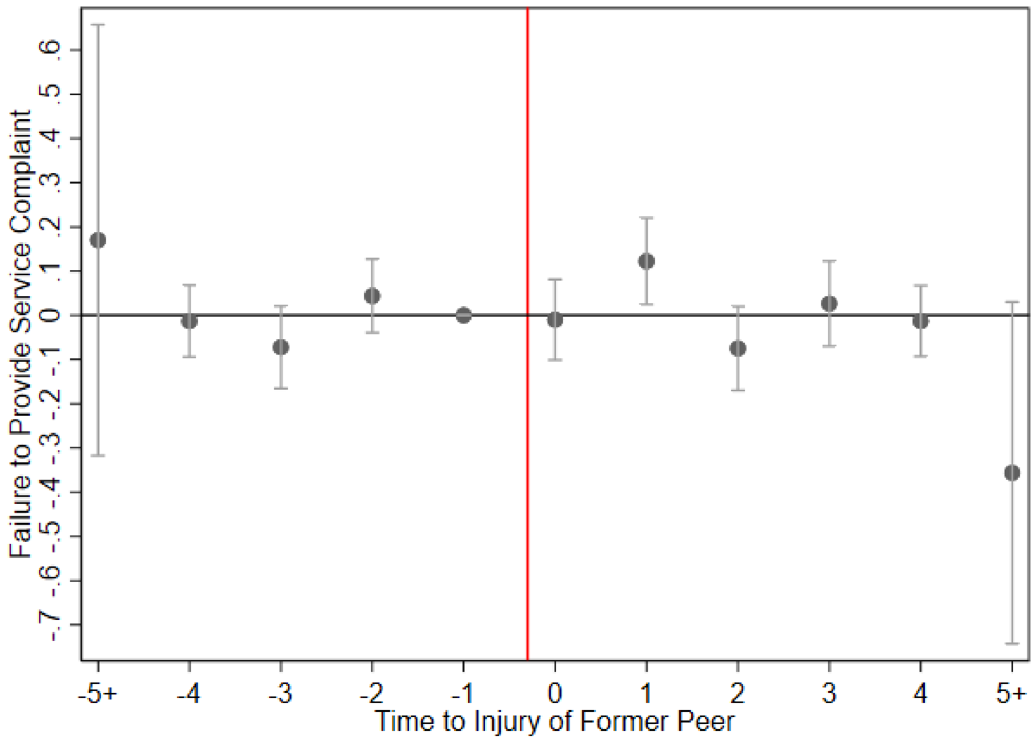
Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 divided by baseline rate of force use and 90% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on lags and leads of treatment without fixed effects. Standard 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.

Figure 12: The Effect of Past Peer Injuries on Police Injuries



Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 without a control for whether the individual was injured in a given week divided by baseline rate of injury and 90% confidence intervals. The baseline rate of injury 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 13: The Effect of Past Peer Injuries on Complaints for Failure to Provide Service



Note: Graph shows Difference-in-Differences coefficients estimated using Equation 2 without a control for whether the individual was injured in a given week divided by baseline rate of injury and 90% confidence intervals. The baseline rate of injury 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.

9.2 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	.002
2003	11/22/2003	No	No info	5	52	.35
2004	11/20/2004	4163	No info	7	352	.62
2005	2/18/2006; 2/19/2006	4061	3338	3	181	.85
2006-1	6/4/2006	1508	1255	2	145	.26
2006-2	8/6/2006	1025	863	3	191	0.00
2006-3	11/5/2006	1795	1487	14	853	.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	.77
2013	12/14/2013 military makeups 6/28/2014; 12/7/2014; 6/13/2015; 12/6/2015	14788	12877	8	651	.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: 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 4: 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	.000	.822	.822	.660	.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 5: Effect of Injuries to Former Peers of the Same Race

	(1) Force	(2) Force	(3) Force	(4) Force	(5) 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	0.00	.613	.605	.953	.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: 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	.017	.300	.301	.160	.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 7: Effect of Past Peer Injuries on Complaints Against Officers

	(1) Force and Verbal	(3) Arrest and Search	(4) FPS
Former peer injured in previous week	-0.000252	0.000405*	0.000361**
	(0.000202)	(0.000206)	(0.000177)
Percent Increase	-7.59	8.19	14.74
Pre-trend Test	.412	.558	.652
Individual Fixed Effects	YES	YES	YES
Unit-Week Fixed Effects	YES	YES	YES
Number of Former Peers	NO	NO	NO
Test Cohort-Week Fixed Effects	NO	NO	NO
R-squared	0.037	0.038	0.029
Observations	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 8: 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	.539	.659	.005	.165	.726	.930	.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 9: Heterogeneous Effects by Suspect Characteristics

	(1) Passive	(2) Active	(3) Assault	(4) Battery	(5) Deadly	(6) Unarmed suspect	(7) Armed suspect
Lagged Former Peer Injury	0.0000650	0.000777	0.0000752	0.000275	0.000132	0.00104**	0.000198
	(0.0000619)	(0.000505)	(0.000168)	(0.000251)	(0.0000817)	(0.000485)	(0.000179)
Baseline	.0002	.0088	.0038	.005	.0004	.0162	.0017
Percent Increase	32.52	8.83	1.98	5.5	33.06	6.4	11.67
Pre-Trend Test	.2611	.098	.2197	.116	.4158	.6841	.7834
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: 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).Standard errors presented in brackets below the percent change.

Table 10: 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)	[.74,15.16]	[-10.66,24.54]	[-19.19,29.75]
Hispanic	-.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-values presented in brackets under the percent change.

Table 11: Mechanisms

	(1) Force	(2) Injured	(3) FPS Complaints
Former peer used-force in previous week	0.000487		
	(0.000340)		
Former peer injured in previous week		-0.000181	0.000361**
		(0.000166)	(0.000177)
Percent Increase	3.11	-7.56	14.45
Pre-Trend Test	.218	.545	.652
Individual FE	YES	YES	YES
Unit-Week FE	YES	YES	YES
Number of Past Peers	YES	YES	YES
R-Squared	0.0420	0.0281	0.0293
Observations	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. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.