

Peer Effects in Police Use of Force[†]

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We study the link between police officers' on-duty injuries and their peers' force use using a network of officers who attended the police academy together through a random lottery. On-duty injuries increase the probability of officers using force by 7 percent in the subsequent week. Officers are also more likely to injure suspects and receive complaints about neglecting victims and violating constitutional rights. The effect is concentrated in a narrow time window following the event and is not associated with significantly lower injury risk to the officer. Together, these findings suggest that emotional responses drive the effects rather than social learning. (JEL H76, J28, K42)

Excessive use of force by police is a critical social and economic issue in the United States. Aggressive policing results in human capital and educational losses among affected populations (Legewie and Fagan 2019; Ang 2021), in addition to the hundreds of police killings each year (Lartey 2015) and substantial costs to taxpayers through misconduct settlements (Thomson-DeVeaux, Bronner, and Sharma 2021). The social cost of police behavior and tactics has led the Biden administration to view addressing systemic misconduct as a key policy objective.¹ However, there is little empirical evidence on the factors contributing to officers' use of force. This paper argues that peer injuries on duty and negative emotional shocks can play a significant role in an officer's decision to use force.

We use detailed administrative data on the Chicago Police Department (CPD) to show that officers are more likely to use force and injure civilians shortly after a peer is injured on duty. Identifying such peer effects is generally difficult due to

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¹See President Biden's remarks on the Derek Chauvin Trial (<https://www.whitehouse.gov/briefing-room/speeches-remarks/2021/04/20/remarks-by-president-biden-on-the-verdict-in-the-derek-chauvin-trial-for-the-death-of-george-floyd/>).

the nature of social and professional networks and the difficulty in observing the network to which an officer belongs. Officers may choose peers who have similar preferences for the use of force. Officers who work together may also face common risks of injuries and returns to using force. We overcome these challenges by exploiting the fact that the CPD police academy draws in new officers using random lottery numbers, restricting officers' ability to choose their academy peer group, and after training is complete new officers are generally sent to different areas across the city. We denote officers who went to the academy together and later worked in different areas as "former peers."² Former peers provide us with an observable network of peers who are not chosen by the officer and who do not face the same local shocks to use of force or injury risk. We exploit the exogenous timing of a former peer's injury using a difference-in-difference design that compares officers who had a former peer injured to other officers who work in the same district but did not have a former peer injured.³

We find that a former peer's injury during a use-of-force incident increases an officer's propensity to use force by 7 percent in the subsequent week, leading to a 10 percent increase in the likelihood of injuring a civilian. Officers are also more likely to receive a complaint about a false arrest or improper search in the week following a former peer's injury. These findings suggest that officers respond to the injury of a peer by violating suspects' constitutional rights. We also find a higher probability of receiving a complaint about the failure to provide service, suggesting officers sort out of helping potential victims after their former peer is injured.

These results likely understate the full effect of officer injuries for two reasons. First, the estimation strategy excludes peers in the same geographic police district because these officers may experience correlated shocks to civilian noncompliance. Injuries to these contemporaneous peers are likely to be at least as impactful due to plausibly lower social distance between the coworkers relative to former peers. For example, when we consider another group with plausibly lower social distance—cohort members of the same race—the effect doubles in magnitude. Second, we dropped the first year of data for all officers in our sample because we could not observe the officers' geographic district during that time. Effects are likely larger during this period because officers have less experience and are likely closer to their academy peers.

To better understand these effects, we investigate mechanisms driving peer responses. We find evidence consistent with officers increasing force due to an emotional response, as the increase in police violence is immediate and quickly disappears two weeks after an injury. This pattern is consistent with other studies on negative emotional shocks leading to short-term increases in violence (Card and Dahl 2011; Munyo and Rossi 2015). Furthermore, as in Guryan, Kroft, and

²Shue (2013) and Ager, Bursztyn, and Voth (2017) use a similar definition for their peer groups.

³Identifying the effect of an officer injury on a peer's decision requires exogeneity in the officer's injury with an assignment probability independently distributed across groups (Angrist 2014). We approximate this ideal using an approach similar to a partial population design (Duflo and Saez 2003; Hirano and Hahn 2010) by combining quasi-random assignment of officers to groups with a difference-in-difference design exploiting exogenous timing of officer injuries. However, in our main specification, former peers' role for identification is to provide an observable network of officers who do not experience common local shocks, which is not related to trends in injury risk.

Notowidigdo (2009), we find that professional experience attenuates social influences such as peer effects, and this moderating effect of tenure is similar to Ta, Lande, and Suss (2021), who find that a police officer's emotional reactivity is lower in more experienced police officers. We also rule out several alternative explanations. We find no evidence that officers are simply "mimicking" their former peers' use of force, nor do they significantly reduce their injury likelihood through social learning or updated beliefs. Lastly, by linking officers to arrest records, we rule out the possibility that they are reducing their effort (Mas 2006; Ba and Rivera 2019).

This article contributes to three literatures. First, it contributes to the growing literature on police discretion by documenting that police decision-making is influenced by peer injuries. Much of the existing literature on crime and policing focuses on crime prevention and incapacitation effects,⁴ while a growing body of work has focused on police as individual agents who exercise discretion, resulting in differences in outcomes such as arrests, stops, and uses of force.⁵ More recent studies have documented how aggressive policing can reduce the educational performance of minority groups, negatively affect attitudes toward the state, and undermine police legitimacy.⁶ By identifying a causal determinant in the decision to use force, this paper builds upon the burgeoning literature attempting to unpack the black box of police productivity by providing evidence that police officers respond to their peers' outcomes.⁷ These findings introduce 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. Alternatively, policies that reduce the risk to officers may have positive externalities on force use.

Second, it contributes to the literature on peer effects by documenting evidence that individuals in the workplace respond to peer outcomes rather than choices (Mas and Moretti 2009; Cornelissen, Dustmann, and Schönberg 2017). This result suggests that direct responses to peer outcomes may partially drive the results in other studies that find negative spillovers. For example, 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 soldiers. Our finding provides new mechanisms for exploring such results. We also confirm that one's peer group can affect individuals' choices and outcomes long after the group dissipates (Bayer, Hjalmarsson, and Pozen 2009; Shue 2013).

Finally, this paper contributes to the literature on the effects of exposure to violence by showing that a peer's exposure to violence affects an individual's behavior in high-stakes decisions. Lab and artificial field experiments have uncovered

⁴See Levitt (1997); Di Tella and Schargrodsky (2004); Evans and Owens (2007); Draca, Machin, and Witt (2011); Chalfin and McCrary (2018); and Morales-Mosquera (2020) for papers documenting the effect of policing on crime.

⁵See Knowles, Persico, and Todd (2001); Lum and Nagin (2017); Fryer (2020); Knox, Lowe, and Mummolo (2020); Ba et al. (2021); and Rivera (2022).

⁶See Skolnick and Fyfe (1993); Tyler (2004); Weitzer and Tuch (2004); Brunson and Miller (2005); Lum and Nagin (2017); Manski and Nagin (2017); Legewie and Fagan (2019); and Ang (2021).

⁷See Fryer (2019); Owens et al. (2018); Ba and Rivera (2019); Ba et al. (2021); Annan-Phan and Ba (2019); and Zimring (2019) for other work attempting to uncover the determinants of police force.

evidence that exposure to violence can increase preferences for certainty and impatience, while decreasing emotional regulation.⁸ Outside of the lab, Bauer et al. (2016) find that people exposed to war behave more cooperatively and altruistically toward their in-group. In contrast, we find that those exposed to violence behave less altruistically toward out-group members. Similarly, Hjort (2014) finds that ethnic conflict increases animus against out-group members, and Aizer (2009) finds that exposure to violence can reduce future productivity.

The remainder of the paper proceeds as follows. Section I describes the Chicago Police Department's institutional background, providing information on network formation and the policy governing officers' use-of-force decisions. Section II describes the relevant datasets, sample definitions, and summary statistics. Section III explains the research design used to generate the estimates provided in Section IV. Section V sheds light on the mechanisms suggested by auxiliary data analysis. Section VI concludes.

I. Background

A. Formation of Police Networks

The recruitment process generally follows five steps: (1) a recruitment call,⁹ (2) an entrance exam, (3) a referral lottery, (4) a battery of physical and mental tests, and finally, (5) the prospective officer attends the police academy.

The CPD regularly issues recruitment calls. Online Appendix Table A1 displays the nine recruitment calls made between 2002 and 2013. After applying, the CPD administers an exam to evaluate the officer's cognitive and noncognitive abilities. In step 3, the CPD adds all applicants who pass the exam to an eligibility list and assigns each a lottery number. These applicants are referred to the CPD academy in lottery order as vacancies become available, with veterans receiving priority in the randomization. Applicants remain on the lottery list until it is either exhausted or retired. This application process ensures that individuals do not select into specific cohorts based on their propensity to use force, be injured, or respond to peer injuries with violence.

Our data contain officers' start dates and the dates that the CPD held recruitment tests. However, we do not observe an individual officer's test date (the entrance exam referred to in step 2), as the CPD is unable to supply this information (see the online Appendix for more information). In its place, we use the date of the most recent test before the officer started at the academy as a proxy for their test date. We

⁸See Callen et al. (2014); Imas, Kuhn, and Mironova (2018); Moya (2018); Brown et al. (2019) for lab and artificial field evidence that violence can affect preferences and behavior. Osofsky (1997) finds that violence can decrease emotional regulation.

⁹Officers who apply to be part of the Chicago Police Department must fulfill age and citizenship requirements. Applicants must also have a combination of postsecondary and/or army training: applicants must have at least 60 semester hours from an accredited university, 3 years of active duty in the armed forces, or 30 semester hours and 1 continuous year of active duty (Chicago Police Department 2021).

overcome any measurement error in our proxy test dates by using individual fixed effects in our main specifications.¹⁰

Applicants whose lottery numbers are called proceed to step 4, where they must pass 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 at the police academy. We refer to all officers starting the academy together in the same month as an academy cohort. Panel A of Figure 1 shows the dates these cohorts start at the police academy throughout the sample period. On average, police academy cohorts are 78 percent male, 49 percent White, 17 percent Black, and 34 percent Hispanic. The mean age of new officers in our sample is 29 years. Panel B of Figure 1 presents a histogram of the cohort sizes during the sample period; cohorts have, on average, 42.81 individuals, though cohort sizes range considerably.

Once applicants enter the academy, the Education and Training Division provides over 900 hours of basic training over 6 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 additional training on gangs, drugs, law, ethics, report writing, vehicle stops, and driving.

After completing the academy, officers complete roughly 12 months of probationary field training.¹¹ During the 12 months of field training, the CPD assigns probationary officers to districts at their discretion. Duty assignments can change day-to-day during this period. Unfortunately, the unit assignment data do not record probationary assignments. After the probationary period is over, officers move to more permanent police units based on the needs of the CPD rather than the preferences of the police officer. As discussed earlier, we define an officer's "former peers" as the members of their academy cohort who are working in a different geographic unit in a given week.

Most police officers work in units tasked with policing a specific geographic area, known as a police district. In 2013, there were 22 police districts in Chicago, with officers often assigned to police specific beats (about 1 square mile) within the district.¹² We focus our analysis on these geographic units. In an average week, we observe 94 officers who joined between 2002 and 2013 in each of the 22 units. The distribution of unit sizes is shown in panel C of Figure 1, and each unit consists of many different cohorts (see panel D of Figure 1). In an average week, officers have 60 former peers.

B. *Use-of-Force Policy*

The CPD defines force as physical contact by a department member used to compel a subject's compliance. The department's policy is that officers should attempt to

¹⁰ These individual fixed effects allow us to overcome this issue because whatever date the officer took the test is time-invariant and subsumed by the individual fixed effect.

¹¹ Nearly all officers who begin training graduate from the police academy, with fewer than 3 percent of officers failing (Hinkel 2017). The probationary period consists of 18 months of active duty. Officers spend the first 6 months in the academy and the final 12 months in probationary district assignments. Time absent from duty does not apply toward completion of the probationary period.

¹² A smaller share of officers work in specialized units, such as Canine, Marine/Helicopter, SWAT, or Bomb Squad units. Since these specialized units operate across geographic districts, we omit them from the analysis.

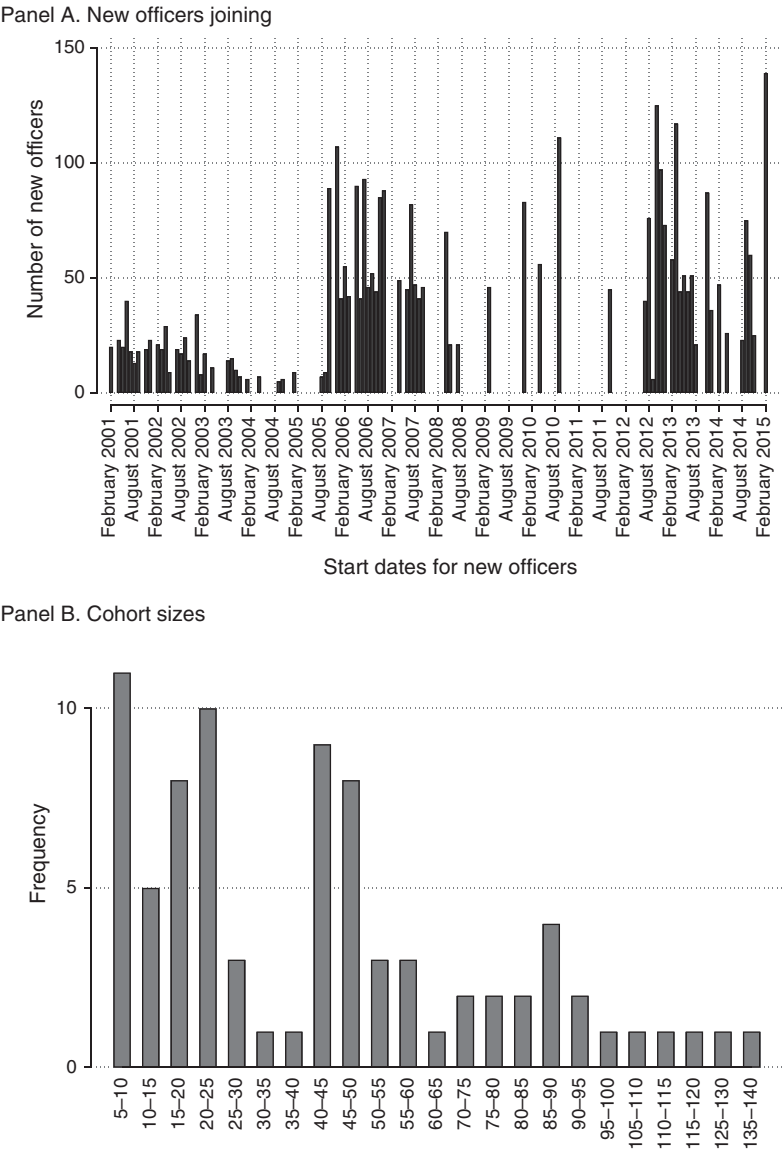
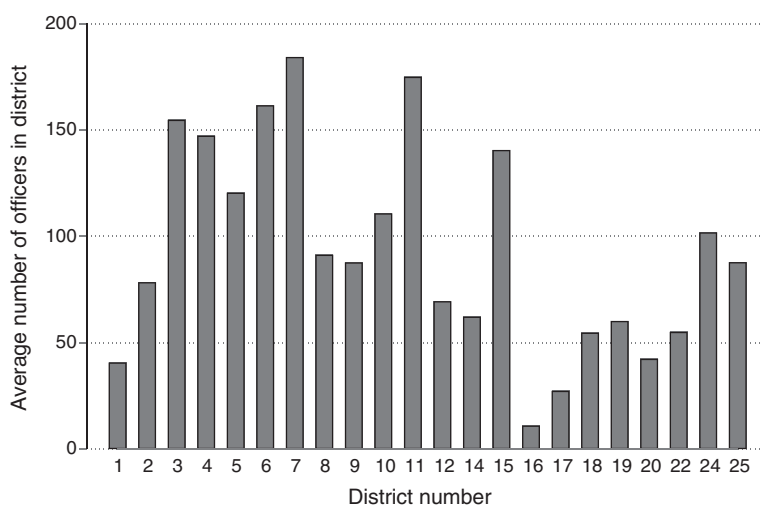


FIGURE 1. DISTRICT AND COHORT SIZES THROUGHOUT THE SAMPLE PERIOD (continued)

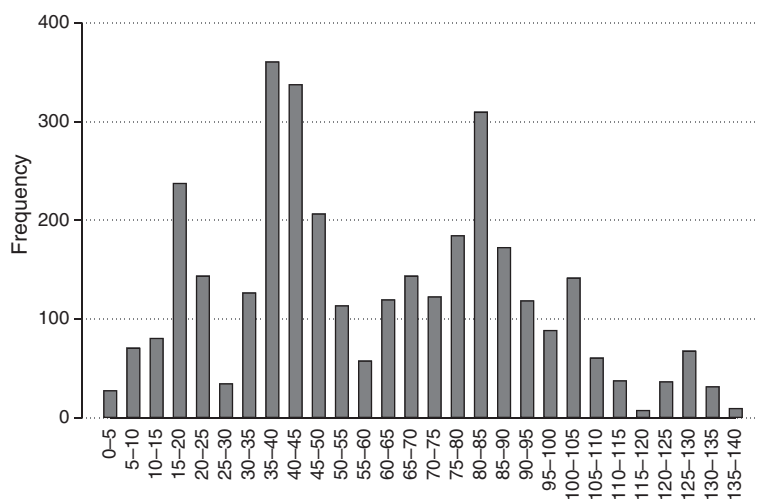
gain the voluntary compliance of subjects when possible. However, officers are not required to take actions that endanger themselves or third parties (Chicago Police Department, General Order G03-02).¹³

¹³ 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

Panel C. Unit sizes



Panel D. Number of former peers

FIGURE 1. DISTRICT AND COHORT SIZES THROUGHOUT THE SAMPLE PERIOD (*continued*)

Notes: Panel A displays the number of new officers in each month of the sample period. Panel B displays the frequency of each cohort size observed in our dataset. Panel C displays the average number of officers in each of the geographic police units. Panel D displays the number of academy mates who work in different geographic districts.

Under Chicago Police Department General Order G03-02, the CPD requires officers to use force that is “objectively reasonable, necessary, and proportional” to the subject’s actions. However, there is no formal definition of “objectively reasonable.”

of physical violence. Lastly, the CPD permits officers to use Tasers, pepper spray, batons, and firearms under some circumstances.

The CPD instructs officers 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, the CPD explicitly accounts for imperfect information regarding the suspect's compliance and officers making decisions quickly in tense circumstances.

The requirement of proportional force relies on the officer's contemporaneous beliefs about the threat they face. These beliefs may differ from those determined by an objective observer. The guidelines permit deadly force when the officer believes that the suspect poses an imminent threat of great bodily harm. Officers may also use deadly force when the suspect has committed a forcible felony, threatened the infliction of great bodily harm, and attempted to avoid arrest. 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.

Despite these guidelines, a 2015 probe by the Justice Department found that the CPD engaged in a "pattern or practice" of unconstitutional use of force. The Justice Department's report noted that CPD officers regularly engage in behavior that endangers themselves, resulting in unnecessary and avoidable force use. It attributed the unconstitutional force to deficiencies in accountability and insufficient support for officers' wellness and safety. The findings further noted that the unconstitutional force fell mostly on Black and Latino neighborhoods. See Department of Justice (2017) for more details.

II. Data and Summary Statistics

We use four sources of administrative data from the Chicago Police Department.¹⁴ Use of force and injury data come from the Chicago Police Department's Tactical Response Reports (TRR) for nonjuvenile suspects. The CPD requires that officers fill out Tactical Response Reports after an officer uses more than a minor level of force.¹⁵ While minor levels of force do not require a TRR, officers must fill out TRRs when a suspect alleges an injury or the suspect resists arrest or in situations where the suspect uses physical violence (Chicago Police Department General Order G03-02-02).

Our data encompass over 16,000 instances of force use by the CPD between January 1, 2005, and October 31, 2016. These data have numerous strengths relative to other existing datasets. They cover almost every instance of police use of force in Chicago, regardless of whether the officer injures or kills the suspect.¹⁶ 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

¹⁴Data were obtained by or in collaboration with the Invisible Institute. See Holz, Rivera, and Ba (2022) for replication data and code.

¹⁵This 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 CPD also requires TRRs for force involving canines, but we exclude canine units from the analysis.

¹⁶The data exclude incidents involving juveniles and subjects of unknown ages because juvenile records are not subject to Freedom of Information Act requests.

officer employment records that include unit assignments and report the officer's start date because it is critical to our identification strategy.¹⁷

To help understand how a former peer's injury influences officer behavior, we supplement these data with data on complaints issued against officers and data on arrests.¹⁸ The complaint data contain all allegations of misconduct filed by civilians or other officers from January 1, 2000 until June 17, 2016, including the date of the incident and details of the actions resulting in a complaint. We match these data to the officer data using the complainant's self-reported incident date. We investigate three specific types of complaints: force and verbal abuse, improper search or arrest, and failure to provide service. The arrest data contain all CPD arrests of adults during our sample period, including crime type, arrestee demographics, and arrest date and time (see the online Appendix for more details).

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.¹⁹ We restrict our treatment definition to injuries that occur during interactions with suspects who allegedly attacked the officer.²⁰ While it is unlikely an officer is injured by a violent suspect but reports the suspect as nonaggressive, misclassification may occur when an officer is injured due to an accident and a suspect was aggressive (i.e., the suspect did not cause the injury). This misclassification would lead to measurement error. We will consider control periods (no former peer was injured due to a suspect) as treated periods (at least one former peer injury due to a suspect). However, this attenuates our effects as long as the covariance between former peer injuries and measurement error does not exceed the variance of the measurement error itself (Aigner 1973; Black, Berger, and Scott 2000).

Lastly, CPD officer unit assignment data records officers as part of the academy unit until they finish their probationary period, rather than until they graduate from the academy. We use the sample of officers who start at the academy after January 2001. We cannot observe the officers' geographic assignments in the year between graduation and the end of the probationary period. Since local civilian noncompliance shocks constitute a significant threat to identification, we exclude every officer's probationary year from the analysis. We dropped officers who leave the academy before six months or who graduate after our sample period.

A total of 4,429 officers start the academy between 2001 and 2013 (see online Appendix Table A2), and we study these officers between 2004 and 2016. Among other filters, we drop officers who do not enter into a geographic district after leaving the academy, leaving us with 3,461 officers (see online Appendix Table 3) and

¹⁷ We exclude all police officers without a recorded start date from the analysis because we cannot link these officers to a police academy cohort.

¹⁸ See Ba (2017) for a detailed discussion of these data.

¹⁹ The CPD refused to provide this information in the FOIA request, citing HIPPA privacy regulations.

²⁰ The data do not include information about the nature or extent of officer injuries. However, Tiesman et al. (2018) reports that the most common cause of injuries on duty is violence. Most of these injuries were to the hands, legs, neck, head, or shoulders. About 40 percent of injuries were contusions, abrasions, lacerations, fractures, or dislocations. The remaining 60 percent were sprains, strains, or other injuries. In their sample, assault-related injuries grew between 2003 and 2011. The Bureau of Labor Statistics also reports that of the 27,660 on-the-job injuries reported in their 2014 sample, violence caused 27 percent of injuries. The next most common category was falls, slips, and trips; this category accounted for 25.3 percent of injuries. Overexertion followed, accounting for 21.4 percent of injuries.

TABLE 1—FREQUENCY OF EVENTS AND OUTCOMES

| | Weeks | Mean | Standard deviation | 10th percentile | 50th percentile | 90th percentile |
|----------------------------|-------|-------|--------------------|-----------------|-----------------|-----------------|
| <i>Injuries</i> | | | | | | |
| Officer injured | 675 | 0.003 | 0.002 | 0.000 | 0.002 | 0.005 |
| Former peer injured | 675 | 0.105 | 0.074 | 0.004 | 0.094 | 0.214 |
| Cohort member injured | 675 | 0.112 | 0.078 | 0.005 | 0.100 | 0.227 |
| Any officer injured | 675 | 0.904 | 0.293 | 0.957 | 1.000 | 1.000 |
| <i>Force Use</i> | | | | | | |
| Any force use | 675 | 0.018 | 0.007 | 0.010 | 0.018 | 0.027 |
| Control | 675 | 0.011 | 0.005 | 0.005 | 0.010 | 0.017 |
| Without weapon | 675 | 0.015 | 0.006 | 0.008 | 0.014 | 0.022 |
| Nonlethal | 675 | 0.001 | 0.002 | 0.000 | 0.001 | 0.003 |
| Mitigation | 675 | 0.019 | 0.007 | 0.011 | 0.019 | 0.028 |
| Baton | 675 | 0.001 | 0.001 | 0.000 | 0.000 | 0.002 |
| Taser | 675 | 0.001 | 0.002 | 0.000 | 0.000 | 0.003 |
| Firearm | 675 | 0.000 | 0.001 | 0.000 | 0.000 | 0.001 |
| Other | 675 | 0.001 | 0.001 | 0.000 | 0.001 | 0.003 |
| Injured suspect | 675 | 0.006 | 0.003 | 0.002 | 0.005 | 0.010 |
| <i>Arrests</i> | | | | | | |
| Any crime | 675 | 0.527 | 0.268 | 0.313 | 0.420 | 1.000 |
| Municipal code | 675 | 0.034 | 0.028 | 0.015 | 0.023 | 0.076 |
| Traffic | 675 | 0.053 | 0.026 | 0.032 | 0.046 | 0.087 |
| Warrant | 675 | 0.113 | 0.061 | 0.066 | 0.093 | 0.221 |
| Drug crime | 675 | 0.173 | 0.098 | 0.084 | 0.146 | 0.346 |
| Property crime | 675 | 0.096 | 0.059 | 0.047 | 0.073 | 0.198 |
| Violent crime | 675 | 0.135 | 0.067 | 0.073 | 0.112 | 0.244 |
| Other | 675 | 0.177 | 0.103 | 0.093 | 0.140 | 0.364 |
| <i>Complaints</i> | | | | | | |
| All complaints | 675 | 0.016 | 0.008 | 0.007 | 0.015 | 0.026 |
| Force and verbal | 675 | 0.004 | 0.004 | 0.000 | 0.004 | 0.010 |
| Arrest and search | 675 | 0.006 | 0.004 | 0.000 | 0.005 | 0.010 |
| Failure to provide service | 675 | 0.003 | 0.003 | 0.000 | 0.003 | 0.006 |
| Unbecoming conduct | 675 | 0.000 | 0.001 | 0.000 | 0.000 | 0.001 |

Notes: This table reports descriptive statistics for each week in the dataset. Since officers are joining throughout the sample period, the composition of officers differs across weeks. The mean value represents the probability that each event or outcome occurred at least once in the sample week.

a total of 989,908 officer-week observations between 2004 and 2016 (the period for which we observe TRRs) and 968,111 officer-week observations with at least 1 lag period. Of these officers, 2,836 use force at least once in the sample, with 2,000 having instances accompanying an injury or alleged injury to the suspect. In our sample, 1,280 officers experience injuries. Nearly all officers (3,429) experience at least 1 injury to a member of their police academy cohort.

Table 1 displays the summary statistics of events and outcomes by week. An individual officer is very unlikely to be injured in a given week. However, 90 percent of weeks involve at least 1 officer injury, and 10 percent of weeks involve an injury to a former peer. Officers use force in about 1.8 percent of weeks. When they use force, they tend to use more than one type. Officers arrest suspects in about 53 percent of weeks and get complaints for their actions in about 1.6 percent of weeks. We show the correlation between outcomes, events, and characteristics in online Appendix Table A4. Figure 2 displays the positive correlation between officer injuries in a week and other officers' force use.

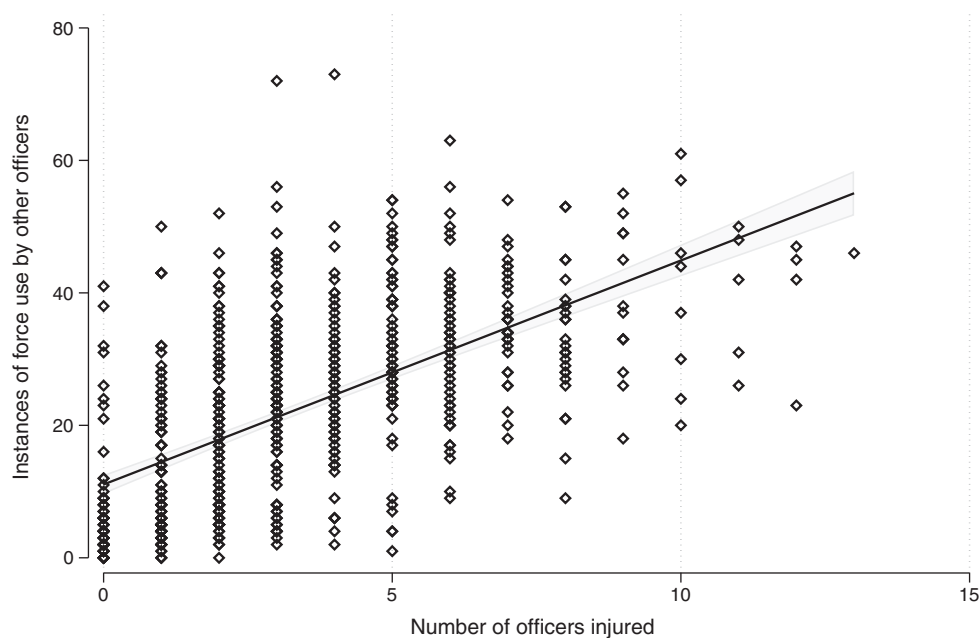


FIGURE 2. CORRELATION BETWEEN OFFICER INJURIES AND FORCE USE BY OTHERS

Notes: This 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. It uses the full sample of all officers included in Tactical Response Reports from 2004 to 2016. The black line represents the regression line of force use in a given week on the number of other injured officers in that week. Standard error bands are presented around the line.

Testing for Violations of Random Cohort Assignment.—We now evaluate whether officers' characteristics across cohorts are consistent with the random lottery procedure advertised by the CPD. We evaluate the implication of random assignment to cohorts using the procedure from Guryan, Kroft, and Notowidigdo (2009) to correct for the negative mechanical correlation between an officer's characteristics and those of their peer group.²¹ In our case, this takes the form

$$(1) \quad X_{igr} = \pi_0 + \pi_1 \bar{X}_{-i,gr} + \phi \bar{X}_{-i,r} + \epsilon_{igr}.$$

Here, X_{igr} is the average predetermined characteristic of officer i in academy cohort g chosen from test cohort r . We approximate the test cohort using cohorts that began between two test dates.²² The predetermined characteristics we observe are the officer's sex, age at appointment, and race.

²¹Guryan, Kroft, and Notowidigdo (2009) corrects the procedure in Sacerdote (2001) to show that there is a negative mechanical correlation between one's characteristics and those of one's peer group arising from the fact that peers are, in a sense, sampled without replacement. That is, peers with high values of a characteristic are chosen from a group with slightly lower mean characteristics than those with low values because removing them from the group reduces the mean of the set. They also show that this bias is decreasing in the size of the set. Randomization in our setting occurs on the entrance exam cohort level.

²²We do not know for certain whether officers in two cohorts come from the same test cohort, as the CPD does not track this information (see the online Appendix for more details). In our research design, we overcome this issue by using officer-level fixed effects, which will be collinear with test date.

TABLE 2—TESTING FOR VIOLATIONS OF THE RANDOM ASSIGNMENT OF ENTRANCE LOTTERIES

| | Officer male | Officer Black | Officer White | Officer Hispanic | Officer age |
|------------------------------------|--------------------|--------------------|--------------------|--------------------|-------------------|
| | (1) | (2) | (3) | (4) | (5) |
| <i>Class cohort leave-out-mean</i> | 0.22 (0.12) | 0.12 (0.11) | 0.04 (0.13) | −0.05 (0.14) | 0.52 (0.16) |
| <i>Test group leave-out-mean</i> | −110.70 (54.49) | −150.89 (58.86) | −120.92 (56.94) | −111.25 (55.85) | −96.25 (52.23) |
| <i>R</i> ² | 0.346 | 0.423 | 0.351 | 0.329 | 0.342 |
| Observations | 3,468 | 3,468 | 3,468 | 3,468 | 3,468 |

Notes: The table includes results from estimating equation (1) with various predetermined officer-level characteristics. The sample includes every officer who started at the police academy between January 2002 and December 2013, regardless of whether or not they worked in a geographic unit after graduation. Officer age is measured at the age of taking the entrance exam. Standard errors are in parentheses and are clustered by the test cohort. We assign officers to a test academy cohort based on the last test before they began at the police academy. However, we do not observe the officer’s actual test date.

We regress X_{igr} on $\bar{X}_{-i,gr}$, the class cohort leave-out-mean of X_{igr} , and \bar{X}_{ir} , the mean of X_{igr} for all individuals in test cohort r . Estimates of equation (1) for each of the predetermined characteristics are presented in Table 2. The lack of statistical significance on the class cohort leave-out-mean coefficients suggests that officers are not assigned based on race or sex. We find a positive correlation between an officer’s age and the age of their class. However, this difference is economically small.

Overall, these tests suggest that the Chicago Police Department did assign officers to police academy cohorts using a random lottery. However, the discrepancy in the age variable may be due to several explanations. We suspect a combination of cohorts being drawn in over time (so all members drawn in later are naturally older), miscategorizing cohorts to tests (young cohorts of one test will be mixed with older cohorts of the previous test), and veteran priority rules may be collectively driving this violation. Unfortunately, our data do not contain the officer’s actual test date, and therefore, we are unable to test with certainty why the age of the officer when starting the academy is positively correlated with the age of their class.

III. Research Design

The empirical analysis aims to identify the causal effect of an on-duty injury in an officer’s network. The ideal research design uses two stages of randomization: individuals are first randomized into a group, then, within each group, individuals are further randomly selected into treatment or control conditions (Hirano and Hahn 2010; Crépon et al. 2013; Angrist 2014; Baird et al. 2018). Following Ager, Bursztyn, and Voth (2017), we approximate this design by first choosing to define our peer group as officers who, through the lottery, attended the police academy

together and now work in a different district.²³ We then exploit the quasi-random timing of force-related injuries to those former peers.

In principle, the relevant network is the set of officers whose injury status is observable to the officer in question, including all officers or officers in the same unit. However, using endogenously formed peer groups may lead to situations where officers who are more prone to altercations are more likely to be in the same group. For example, suppose that officers sort into units based on their underlying aggression due to homophily or management decisions by the CPD. We might then expect these groups to have a higher propensity to get into conflicts with suspects, leading to more injuries and more force. The CPD recruitment process makes this type of confound implausible for academy cohorts, as the decision to enter the academy with a particular cohort is outside the control of individual officers and the CPD.

Our definition also addresses potentially confounding common shocks to civilian noncompliance, which we assume are district specific. When some shock reduces the probability that civilians comply with an officer's requests, 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 noncompliance because we compare treated officers to other officers in their own district who face the same noncompliance rate.

Finally, we use the panel structure of the data in conjunction with a difference-in-difference design to eliminate bias arising from both types of simultaneity. We use lagged peer injuries as treatment because contemporaneous peer injuries may be confounded by contemporaneous events that affect the noncompliance rate for all parties in that week. It may also take time for officers to learn about the injuries of their peers. Moreover, 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, we use the quasi-random timing of injuries to former peers as an approximation of the random assignment of injuries within an academy cohort.

In practice, 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-difference estimator. The event of a former peer's 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

$$(2) \quad Y_{idgt} = \lambda_i + \lambda_{dt} + \beta \cdot \mathbf{1}\{t = E_{g,-d} + 1\} + \epsilon_{idgt}.$$

Here, the unit of observation is an officer-week. The outcome variable, Y_{idgt} , is an indicator function, equal to one if the outcome is realized for officer i working in district d during week t who belongs to academy cohort g . For example, in our main specifications, Y_{idgt} is equal to one if officer i chose to use force in week t and zero otherwise. An advantage of this approach is that we include officer-week observations in which the officer did not use force, make an arrest, etc., which allows us to

²³ 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.

avoid endogeneity issues from conditioning on interactions with civilians (Ba et al. 2021).²⁴ The treatment, $\mathbf{1}\{t = E_{g,-d} + 1\}$, is an indicator equal to one if a former peer (an officer who attended the police academy with officer i and is working in a different district) was injured in the previous week.

The individual fixed effects, λ_i , account for time-invariant individual-level differences in the outcome. For example, they account for time-invariant differences in how each officer interprets a suspect's actions as noncompliance. They also subsume test-cohort fixed effects, which is the level of randomization to the peer group. District-week fixed effects, λ_{dt} , account for district-week-level differences in the costs and benefits of choosing $Y_{idgt} = 1$. These fixed effects control for district-specific shocks to civilian or officer aggression, such as the weather or pollution (Herrnstadt et al. 2016; Annan-Phan and Ba 2019), and control for common shocks under the partial interference assumption.

The coefficient of interest, β , estimates the change in the outcome for affected officers relative to officers in the same district who did not experience a former 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 81 cohorts. The main identifying assumption is that the change in the outcome in a given district-week is independent of whether an injured officer started the police academy in the same month as officer i . With both officer and district-week fixed effects, random assignment of officers to cohorts is not necessary for identification. However, random assignment allows us to rule out sorting into peer groups based on unobservables that simultaneously increase the likelihood of injury and propensity to use force.

To assess this assumption's plausibility and examine the dynamic effects of a peer injury, we regress the outcomes on lags and leads of injuries to a former peer. We denote event time in this regression as τ . We omit the dummy for the week before a former peer is injured so that we can interpret the coefficients relative to the week before the injury. We set period -6 to be equal to one when the event was six or more weeks before the injury and set period 6 to be one if the week is six or more weeks after the injury.²⁵

$$(3) \quad Y_{idgt} = \lambda_i + \lambda_{dt} + \sum_{\tau} \beta_{\tau} \cdot \mathbf{1}\{t = E_{g,-d} + \tau\} + \epsilon_{idgt},$$

where $\tau = \{-6+, -5, -4, -3, -2, 0, 1, 3, 4, 5, 6+\}$. In this regression, the coefficients of interest are now β_{τ} . These coefficients 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 noncompliant civilians or differ in the way they interpret signals during interactions with civilians.²⁶

²⁴ See Knox, Lowe, and Mummolo (2020) for discussions of this issue.

²⁵ The lags and leads will also alter the composition of individual-weeks that we observe. The first and last six weeks of every individual's observations in the panel will be excluded from the regression since the end periods combine many additional time periods and this adds noise to the estimates of the effects in those periods.

²⁶ However, unobserved posttreatment shocks specific to members of a particular cohort may still threaten the identification of β_{τ} . For example, if an officer is injured, it may mean a loss of manpower, which leads to other

Officers experience multiple events over their time in our sample. Standard event studies usually include one event per cross-sectional unit and mutually exclusive dummy variables representing periods before and after treatment. Our setting departs from this standard. While the probability an individual officer gets injured is one-quarter of 1 percent, over 95 percent of weeks in our sample contain at least 1 officer injury, and officers have 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. Thus, β_τ can represent the effect for a period, which is both a pretreatment period and a posttreatment 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 evidence of pre-trends.

At present, there is no accepted method of conducting event studies when there are multiple or overlapping events. The Monte Carlo simulation results in Sandler and Sandler (2014) suggest that allowing multiple event dummies to be nonzero 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, or time produces biased estimates of the treatment effect. We follow their guidance in our estimation and allow multiple event dummies to be nonzero.

IV. Results

This section demonstrates that after a former peer is injured during a violent interaction, officers substantially increase their propensity to use force shortly after a peer injury. Officers are also more likely to injure suspects and receive complaints about their conduct during the first two weeks after an injury to a former peer. Officer tenure reduces the magnitude of these effects. Moreover, the effects are moderated by social distance, as officers respond twice as strongly to the injury of former peers of the same race.

A. Propensity to Use Force and Injure Suspects

We first consider whether officers increase their propensity to use force after a former peer is injured. Table 3 displays the results of estimating equation (2) with the outcome being the use of force and each column containing different controls and specifications. Column 1 displays a strong correlation between officer injuries and the propensity to use force. Former peer injuries are associated with a 24 percent higher likelihood of using force in the following week.

However, to establish a causal relationship, we must address three main threats to identification. First, we account for local time-varying shocks to civilian noncompliance by adding district-week fixed effects in column 2. Second, we

officers working more, thus mechanically increasing their potential for use of force. However, since former peers do not work in the same district as the injured officer, this is unlikely to threaten our identification strategy.

TABLE 3—EFFECT OF INJURIES TO FORMER PEERS ON THE PROPENSITY TO USE FORCE

| | Force | | | | | |
|--|----------------------|----------------------|----------------------|----------------------|----------------------|-----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| <i>Former peer injury in previous week</i> | 0.00419 (0.00078) | 0.00321 (0.00076) | 0.00178 (0.00061) | 0.00123 (0.00058) | 0.00123 (0.00054) | 0.05805 (0.02469) |
| <i>Constant</i> | 0.01720 (0.00057) | 0.01732 (0.00057) | 0.01748 (0.00044) | 0.01754 (0.00036) | 0.01754 (0.00006) | −3.01291 (0.00337) |
| Model | OLS | OLS | OLS | OLS | OLS | Poisson |
| Percent increase | 24.370 | 18.630 | 10.370 | 7.170 | 7.160 | 5.810 |
| Unit-week fixed effects | No | Yes | Yes | Yes | Yes | Yes |
| Number of former peers | No | No | Yes | Yes | No | No |
| Test period fixed effects | No | No | No | Yes | No | No |
| Individual fixed effects | No | No | No | No | Yes | Yes |
| Pre-trend test | 0.000 | 0.000 | 0.177 | 0.793 | 0.804 | 0.915 |
| R^2 | 0.000 | 0.022 | 0.023 | 0.024 | 0.040 | |
| Observations | 986,111 | 986,088 | 986,088 | 986,088 | 986,088 | 607,688 |

Notes: Column 1 displays estimates from a linear regression of an indicator for any force used by the officer on the first lag of injuries to former peers. Column 2 controls for unit-week fixed effects. Column 3 controls for unit-week and number of former peer fixed effects. Column 4 controls for unit-week, number of former peers, and estimated test period fixed effects. Column 5 estimates equation (2), controlling for individual and unit-week fixed effects. Column 6 estimates equation (2) using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects (column 1). We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 3 displays all percent changes from column 5 in this regression testing for differences in pre-trends. Column 1 in online Appendix Table A5 displays all of the coefficients on lags and leads for column 5 in this table. Column 1 in online Appendix Table A6 displays all of the coefficients on lags and leads for column 6 in this table.

account for heterogeneity in cohort size that may have affected the quality of teaching and the probability of experiencing an event in column 3. Next, we account for sorting into more aggressive networks using our test period proxy fixed effects in column 4.²⁷ Finally, in column 5 of Table 3, we replace test fixed effects with individual fixed effects. These fixed effects allow us to control for time-invariant differences in an individual's propensity to use force and overcome any misclassification of test dates.²⁸ We find that the treatment effects do not change substantially or qualitatively with the introduction of individual fixed effects.

Estimates in columns 4 and 5 are nearly identical, with a former peer's injury increasing an officer's likelihood of using force in the following week by 7 percent relative to the baseline rate. Finally, because the outcome is rare, we include a Poisson specification as a robustness check in column 6.²⁹ The coefficient in column 6 of Table 3 can be readily interpreted as an approximation of the percent change. We find this coefficient is similar to the OLS estimate in size and significance.

To better understand the consequences of the increases in force use, we estimate equation (2) with a suspect injury as the outcome variable in Table 4. Column 1

²⁷ Changes in the applicant pool may lead to cohorts that systematically differ from one another in unobservable ways, making them more likely to experience injuries and use force.

²⁸ An officer's test cohort is time-invariant and is therefore perfectly collinear with the individual fixed effect.

²⁹ We choose to estimate the model using Poisson maximum likelihood estimation rather than with a probit model because of the incidental parameters problem (Neyman and Scott 1948; Hahn and Newey 2004). Wooldridge (1999) shows that Poisson maximum likelihood works well with a binary response variable.

TABLE 4—EFFECT OF FORMER PEER INJURIES ON SUSPECT INJURIES

| | Injure suspect | | |
|--|----------------------|----------------------|-----------------------|
| | (1) | (2) | (3) |
| <i>Former peer injury in previous week</i> | 0.00140 (0.00035) | 0.00053 (0.00030) | 0.09298 (0.04382) |
| <i>Constant</i> | 0.00528 (0.00022) | 0.00537 (0.00003) | −3.21079 (0.00607) |
| Model | OLS | OLS | Poisson |
| Percent increase | 26.490 | 10.060 | 9.300 |
| Unit-week fixed effects | No | Yes | Yes |
| Individual fixed effects | No | Yes | Yes |
| Pre-trend test | 0.000 | 0.085 | 0.097 |
| R^2 | 0.000 | 0.031 | |
| Observations | 986,111 | 986,088 | 233,326 |

Notes: Column 1 displays estimates from a linear regression of an indicator for a suspect injury on the first lag of injuries to former peers. Column 2 estimates equation (2), controlling for individual and unit-week fixed effects. Column 3 estimates equation (2) using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects (column 1). We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value for the null hypothesis that the coefficients of lead periods in the event study are simultaneously equal to zero. Panel A of Figure 4 displays all percent changes from column 2 in this regression testing for differences in pre-trends. Column 2 in online Appendix Table A5 displays all of the coefficients on lags and leads for column 2 in this table. Column 2 in online Appendix Table A6 displays all of the coefficients on lags and leads for column 3 in this table.

of Table 4 shows that the baseline rate of suspect injury per week is 0.53 percent. Based on column 2, injuries to former peers increase an officer's propensity to injure suspects by 10 percent of this baseline mean. Comparing columns 2 and 3 shows that these results are similar when using a Poisson regression.

B. Event Study

Next, we investigate the dynamics of the treatment effect and test whether there are parallel pre-trends between officers with and without a former peer injured. To do so, we estimate equation (3) where the outcome variable is whether the officer used any force in the week after a former peer was injured.³⁰ We estimate the base rate of force as the constant term from a regression of equation (3) without individual or unit-week fixed effects. Figure 3 displays the coefficient estimates from equation (3) divided by the base rate, which we assume is measured without error in computing standard errors for the ratio. This allows us to interpret the effects as percent changes from the baseline propensity to use force.

The effects of a former peer injury in the weeks before the injury are neither economically nor statistically significant. Baseline use of force is also small, with 1.78 percent of officers using any force in a given week. In the week of a former

³⁰The estimation equation will drop individuals who are not employed for five weeks before and after exposure because cohorts join throughout the sample period. As a robustness check, column 1 in online Appendix Table A6 repeats this exercise using a Poisson regression specification.

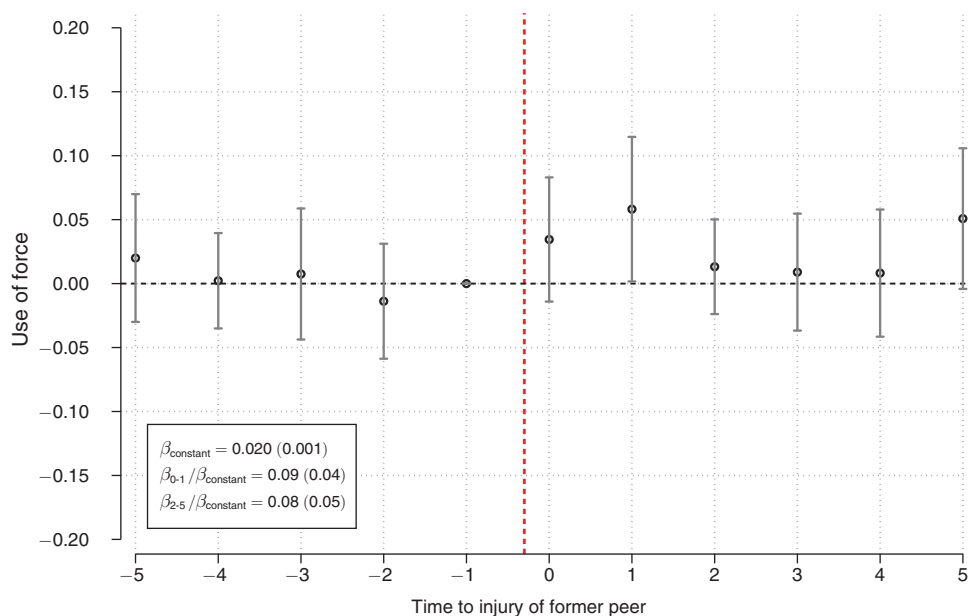


FIGURE 3. THE EFFECT OF FORMER PEER INJURIES ON POLICE USE OF FORCE

Notes: The graph shows coefficient estimates using equation (3) divided by the baseline rate of force use and 95 percent confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on treatment lags and leads without fixed effects. Standard errors are clustered by academy cohort ($G = 81$). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. The red vertical line represents the injury week.

peer injury, the use of force increases by around 3 percent of the baseline mean. We view this period as partially treated, as some officers experience injuries toward the beginning of the week. The weeks after the injury are fully treated, as injuries may happen toward the end of the week and it may take a few days for officers to learn of the injury.

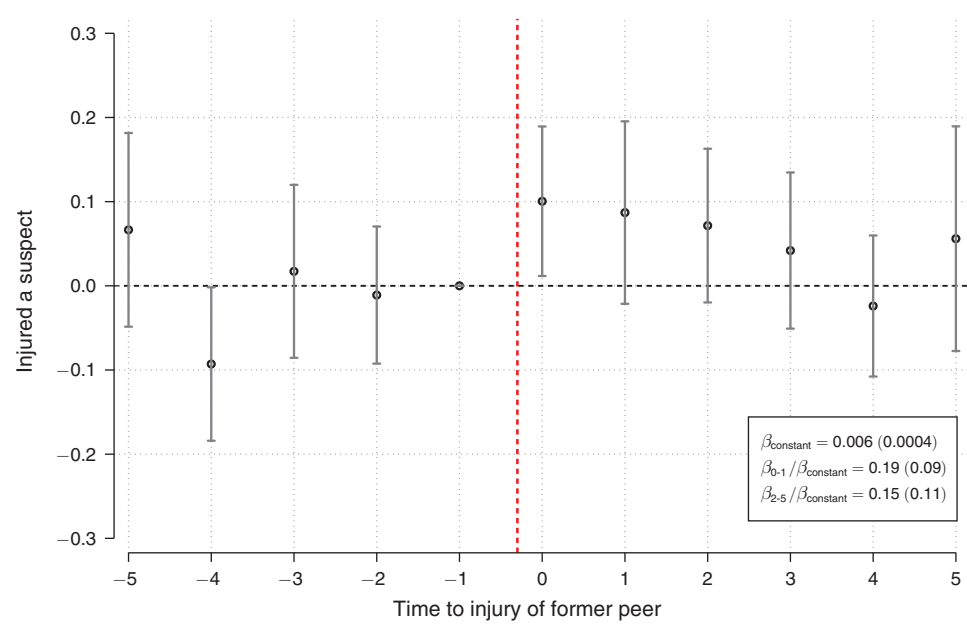
In the week after a former peer is injured, officers increase their use of force by roughly 7 percent of the baseline mean. The treatment effects dissipate quickly, immediately losing significance after the first week after exposure. This pattern suggests that the effect of an officer injury is fleeting and concentrated around the event. Similar effects are displayed in panel A of Figure 4 by estimating equation (3) with suspect injuries as the outcome.

C. Types of Force Used by Officers

Next, we investigate changes in the different types of force officers use. The CPD's use-of-force model governs the choice of which type of force to use, and the level of force an officer is permitted to use increases with the level of resistance they face.

The lowest level of force is called control tactics. It includes actions such as escort holds, wrist locks, emergency handcuffing, and arm bars. Above that are physical

Panel A. Suspect injuries



Panel B. Complaints

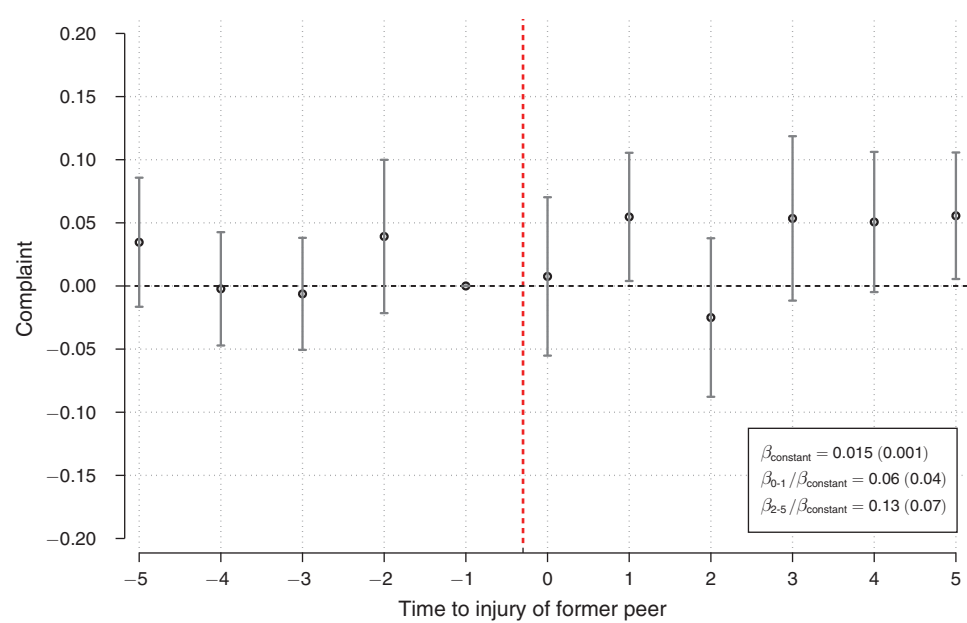


FIGURE 4. EFFECT OF FORMER PEER INJURIES ON OTHER OUTCOMES

Notes: These graphs show coefficient estimates using equation (3) divided by the baseline rate of suspect injuries or complaints and 95 percent confidence intervals. The baseline rate of each variable is calculated as the constant term from a regression on treatment lags and leads without fixed effects. Standard errors are clustered by academy cohort ($G = 81$). The regression includes individual and unit-week fixed effects. In both panels, treatment is an injury of a former peer. The red vertical line represents the injury week.

TABLE 5—HETEROGENEOUS EFFECTS BY TYPE OF FORCE

| | Control | No weapon | Nonlethal | Baton | Taser | Firearm | Other |
|--|----------------------|----------------------|-----------------------|-----------------------|----------------------|----------------------|----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| <i>Former peer injury in previous week</i> | 0.00072 (0.00035) | 0.00095 (0.00048) | −0.00002 (0.00008) | −0.00004 (0.00007) | 0.00009 (0.00013) | 0.00012 (0.00006) | 0.00007 (0.00015) |
| <i>Constant</i> | 0.01030 (0.00004) | 0.01438 (0.00005) | 0.00086 (0.00001) | 0.00050 (0.00001) | 0.00167 (0.00001) | 0.00027 (0.00001) | 0.00108 (0.00002) |
| Model | OLS | OLS | OLS | OLS | OLS | OLS | OLS |
| Percent increase | 8.108 | 7.627 | −1.914 | −8.818 | 6.907 | 46.104 | 0.007 |
| Unit-week fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Pre-trend test | 0.684 | 0.742 | 0.010 | 0.407 | 0.495 | 0.923 | 0.545 |
| R^2 | 0.034 | 0.039 | 0.029 | 0.022 | 0.024 | 0.022 | 0.023 |
| Observations | 986,088 | 986,088 | 986,088 | 986,088 | 986,088 | 986,088 | 986,088 |

Notes: Columns 1 through 7 display coefficients from estimates of equation (2) where the outcome variable is an indicator representing whether the officer used a specific type of force. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects. We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

strikes, such as takedowns, open-hand strikes, punches, kicks, or elbows, which do not involve more than the officer's body. The CPD classifies force involving weapons as nonlethal if it involves a chemical weapon and classifies other force involving a weapon by weapon type (i.e., baton or impact weapon, Taser, or firearm). We categorize all other uncommon types of force as "Other."

We estimate equation (2) on indicators for using each type of force separately and present the results in Table 5. Most instances of force recorded in these data involve force with no weapon, control tactics, and Tasers; the rarest type of force involves firearms, followed by impact weapons such as batons. Note that there is likely much lower underreporting for types of force that are harder to conceal (firearms and Tasers) because of the multiple CPD regulations for reporting their usage.

Similar to our main results, we do not find evidence of pre-trends in any specification except for nonlethal force. We find that officers primarily respond to a former peer's injury by increasing control tactics and force without weapons by about 8 percent relative to baseline. There is a substantial increase in officers using a firearm in the week after a former peer is injured, amounting to a 46 percent increase relative to baseline. However, this represents a very small percentage point increase due to the rarity of firearm usage. Assuming that officers are using force in alignment with the CPD's 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.

D. Complaints against Officers

Next, we investigate the effect of injuries to former peers on allegations of misconduct by the officers. When a police officer acts outside the confines of the US Constitution or other relevant regulations, there are limited ways to detect this

violation. Any civilian who believes an officer violated their constitutional rights may sue the law enforcement agency or report the violation to an oversight agency. Complaints are generally easier to file than lawsuits and are the most readily available form of civilian feedback a police department can access (Walker and Macdonald 2008; Ba 2017; Ba and Rivera 2019).

In this section, we consider whether officers are more likely to act in a way that generates a complaint in the week following a former peer's injury. Table 6 and panel B of Figure 4 show estimates of equations (2) and (3), respectively, with an indicator for having any complaint as the outcome. In the week following a former peer injury, we find that officers are 7 percent more likely to engage in behavior that leads to a complaint of any type (column 1 of Table 6).

We then consider four types of complaints and present estimates of equation (2) with the outcome being an indicator for each type of complaint in columns 2 through 5 of Table 6. The first two, excessive force or verbal abuse (column 2) and improper search or arrest (column 3), constitute violations of an individual's constitutional rights during an officer's attempt to enforce the law. We also consider failure to provide service (FPS) complaints (column 4) and unbecoming conduct complaints (column 5). Analyzing different types of complaints enables us to distinguish between neglecting a civilian who desired help (for example, a potential victim of a crime), which would be an FPS complaint, and violations of a civilian's constitutional rights. Finally, complaints about unbecoming conduct cover actions like providing a false statement, being drunk and disorderly in public or on base, or insulting another officer.

We find no change in the use of force or verbal complaints following a former peer injury. However, complaints of a false arrest or improper search increase by nearly 11 percent in the week following a peer injury. This result is consistent with officers increasing the rate at which they violate a civilian's constitutional rights after a former peer is injured. We also find evidence that officers are more likely to neglect civilians requesting help. Complaints citing a failure to provide service increase by 15.69 percent in the week following a peer injury. We do not find any evidence of additional complaints about unbecoming conduct.

E. Alternative Definitions of Peer Injury

Thus far, our definition of a former peer has included all officers who attended the academy with an officer and now work in other police districts. For peer injuries to have an effect, these individuals must have been acquainted and maintained their bonds after the academy ended. Given that even randomly assigned groups still produce homophilic friendships (Carrell, Sacerdote, and West 2013), we expect stronger bonds to be between individuals of the same race (Marmaros and Sacerdote 2006). Such homophily will attenuate effects in previous results, as we will be pooling strongly and weakly treated officers. 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. We perform the same analysis as before, but we now define a (former) peer group as members of the same academy cohort now working in different districts who are all the same race.

TABLE 6—EFFECT OF FORMER PEER INJURIES ON COMPLAINTS AGAINST OFFICERS

| | All complaints (1) | Force and verbal (2) | Arrest and search (3) | Failure to provide service (4) | Unbecoming conduct (5) |
|--|--------------------------|----------------------------|-----------------------------|--------------------------------------|------------------------------|
| <i>Former peer injury in previous week</i> | 0.00083 (0.00039) | −0.00013 (0.00020) | 0.00048 (0.00020) | 0.00036 (0.00018) | 0.00001 (0.00006) |
| <i>Constant</i> | 0.01309 (0.00004) | 0.00339 (0.00002) | 0.00496 (0.00002) | 0.00250 (0.00002) | 0.00026 (0.00001) |
| Percent increase | 6.838 | −4.340 | 10.468 | 15.541 | 0.001 |
| Unit-week fixed effects | Yes | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes | Yes |
| Pre-trend test | 0.412 | 0.516 | 0.117 | 0.468 | 0.550 |
| R^2 | 0.039 | 0.033 | 0.037 | 0.027 | 0.027 |
| Observations | 986,088 | 986,088 | 986,088 | 986,088 | 986,088 |

Notes: Columns 1 through 5 display coefficients from estimates of equation (2) where the outcome variable is an indicator representing types of complaints against the officer. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects (column 1). We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table 7 and panel A of Figure 5 repeat our analysis from the previous section using this definition of peer groups. We find that officers respond twice as strongly to the injury of a same-race former peer. Officers increase their propensity to use force by 16.5 percent in the week after a same-race former peer is injured with a similar baseline probability of using force. This result is in line with existing literature that shows peer effects mainly operate within race (Garlick 2018).³¹

As a robustness check, we investigate officers' propensity to respond to former peers' injuries during interactions with suspects who displayed low levels of resistance—i.e., when a former peer reported that the suspect did not attack them during the encounter. While we do not know the cause of these injuries, the results of Tiesman et al. (2018) suggest that they may be due to falls, slips, or trips. If officers respond to a perceived threat, we would expect officers to not respond with increased force use to these injuries. Panel B of Table 7 and panel B of Figure 5 display estimates of the effect of these injuries on an officer's propensity to use force. In line with our expectations, we find no evidence that officers respond to injuries of former peers that were unlikely to be caused by suspects.

F. Heterogeneity

To better understand how officers respond to peer injuries, we investigate heterogeneity based on officer tenure, the number of past events, and suspect characteristics.

³¹ We redid the same analysis for officers of the same gender, and we find the effects to be only slightly larger than the main results. Additional results are available by contacting the authors.

TABLE 7—EFFECT OF INJURIES USING DIFFERENT TREATMENT DEFINITIONS

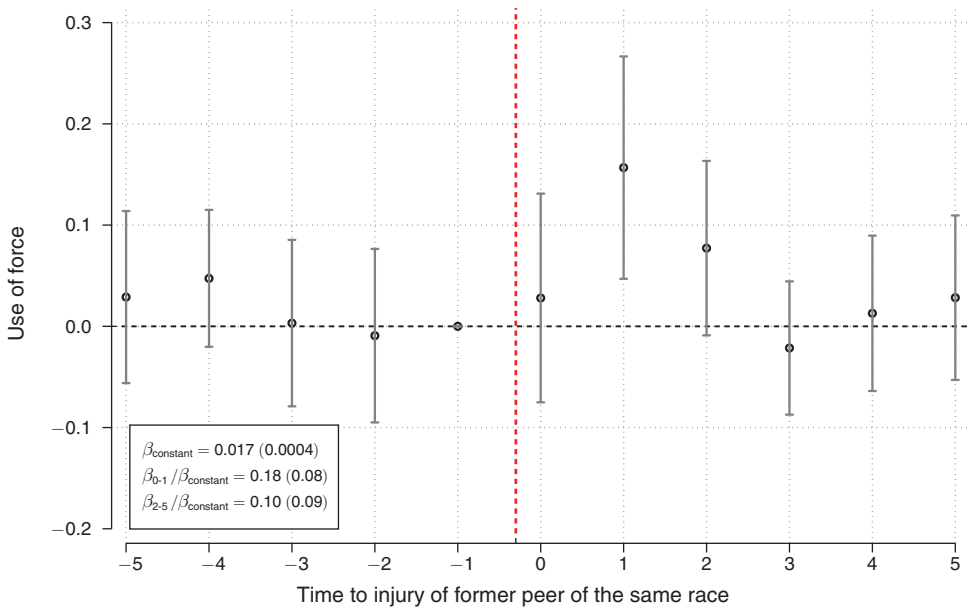
| | Officer used force against a suspect | | |
|---|--------------------------------------|------------------------|-----------------------|
| | (1) | (2) | (3) |
| <i>Panel A. Effect of injuries to former peers of the same race</i> | | | |
| <i>Same-race former peer injured in previous week</i> | 0.00723 (0.00106) | 0.00286 (0.00089) | 0.12505 (0.03579) |
| <i>Constant</i> | 0.01734 (0.00058) | 0.01755 (0.00004) | −3.01792 (0.00230) |
| Percent increase | 41.711 | 16.487 | 12.505 |
| Pre-trend test | 0.000 | 0.363 | 0.587 |
| <i>Panel B. Effect of low-resistance injuries to former peers</i> | | | |
| <i>Former peer injury in previous week</i> | 0.00335 (0.000763) | 0.000366 (0.000565) | 0.0161 (0.0286) |
| <i>Constant</i> | 0.0175 (0.000591) | 0.0177 (0.0000341) | −3.006 (0.00204) |
| Percent increase | 19.18 | 2.1 | 1.61 |
| Pre-trend test | 0.000 | 0.079 | 0.101 |
| Model | OLS | OLS | Poisson |
| Unit-week fixed effects | No | Yes | Yes |
| Individual fixed effects | No | Yes | Yes |
| Observations | 986,111 | 986,088 | 607,688 |

Notes: In panel A, column 1 displays estimates from a regression of an indicator for any force used by the officer on the first lag of injuries to same-race former peers. Column 2 estimates equation (2), controlling for individual and unit-week fixed effects. Column 3 estimates equation (2) using Poisson maximum likelihood estimation. Analogous regressions are shown in panel B, where treatment is the first lag of injuries to former peers for which the officer reported that the suspect did not use physical resistance. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects (column 1). Standard errors are clustered on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 5 displays all percent changes from column 2 in the panel A regression testing for differences in pre-trends. Column 6 in online Appendix Table A5 displays all of the coefficients on lags and leads for column 2 in panel A. Column 6 in online Appendix Table A6 displays all of the coefficients on lags and leads for column 3 in panel A. Panel B of Figure 5 displays all percent changes from column 2 in the panel B regression testing for differences in pre-trends.

G. Moderating Effects of Officer Experience

We consider whether professional experience lessens the social influences in force use. Guryan, Kroft, and Notowidigdo (2009) suggests that professional experience attenuates social influences such as peer effects. Similarly, there is a large experimental literature suggesting that market experience contributes to individual rationality (List 2011; Tong et al. 2016). These findings suggest that as officers gain more experience on the job, they learn how to avoid responding emotionally to their former peers' injuries.

Panel A. The effect of same-race former peer injuries on force use



Panel B. The effect of low-resistance injuries to former peers on force use

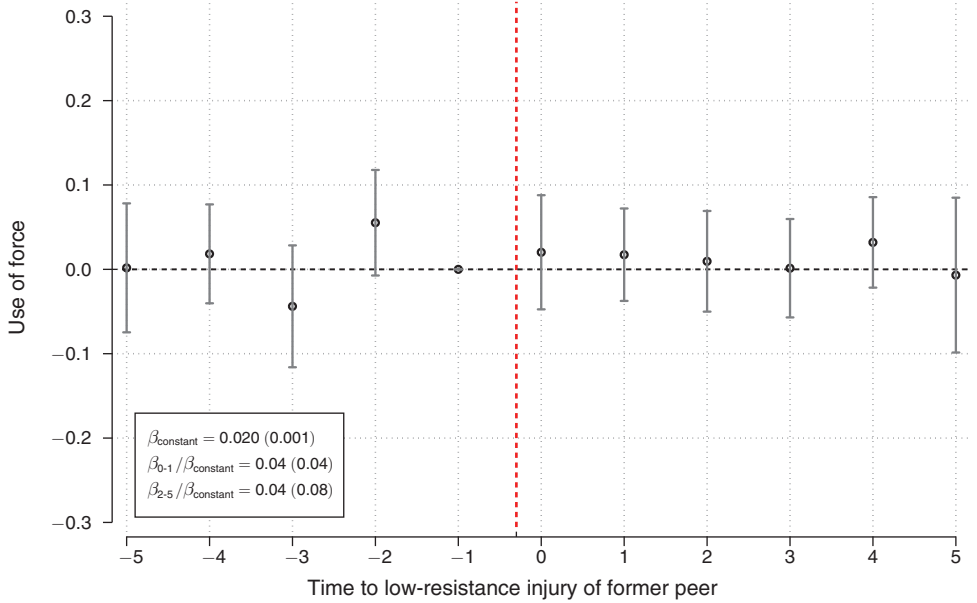


FIGURE 5. DIFFERENT DEFINITIONS OF PEER INJURY

Notes: These graphs show coefficient estimates using equation (3) divided by the baseline rate of force use and 95 percent confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort ($G = 81$). The regression includes individual and unit-week fixed effects. In panel A, treatment is an injury of a former peer of the same race. In panel B, treatment is an injury of a former peer. The red vertical line represents the injury week.

TABLE 8—HETEROGENEOUS EFFECTS BY TENURE AND NUMBER OF EVENTS

| | Force (1) | Injure suspect (2) | Arrest (3) | Officer injured (4) | Complaint (5) |
|---|--------------|--------------------------|---------------|---------------------------|------------------|
| <i>Panel A. log tenure</i> | | | | | |
| <i>Former peer in previous week</i> | −0.00262 | −0.00127 | −0.00038 | −0.00001 | −0.00119 |
| × <i>log Tenure (months)</i> | (0.00112) | (0.00065) | (0.00337) | (0.00033) | (0.00069) |
| <i>Former peer in previous week</i> | 0.01149 | 0.00551 | 0.00532 | −0.00013 | 0.00550 |
| | (0.00460) | (0.00268) | (0.01377) | (0.00133) | (0.00270) |
| <i>Constant</i> | 0.01753 | 0.00537 | 0.37589 | 0.00236 | 0.01308 |
| | (0.00006) | (0.00003) | (0.00025) | (0.00002) | (0.00004) |
| Pre-trend test | 0.742 | 0.159 | 0.493 | 0.070 | 0.491 |
| Observations | 986,088 | 986,088 | 953,262 | 986,088 | 986,088 |
| <i>Panel B. log number of past events</i> | | | | | |
| <i>Former peer in previous week</i> | −0.00094 | −0.00031 | 0.00425 | 0.00002 | −0.00002 |
| × <i>Number of Previous (months)</i> | (0.00051) | (0.00032) | (0.00218) | (0.00017) | (0.00040) |
| <i>Former peer in previous week</i> | 0.00479 | 0.00167 | −0.01478 | −0.00028 | 0.00081 |
| | (0.00213) | (0.00136) | (0.00911) | (0.00070) | (0.00154) |
| <i>Constant</i> | 0.01739 | 0.00533 | 0.37374 | 0.00236 | 0.01298 |
| | (0.00006) | (0.00004) | (0.00028) | (0.00002) | (0.00004) |
| Pre-trend test | 0.742 | 0.159 | 0.493 | 0.070 | 0.491 |
| Observations | 947,579 | 947,579 | 921,341 | 947,579 | 947,579 |
| Model | OLS | OLS | OLS | OLS | OLS |
| Unit-week fixed effects | Yes | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes | Yes |

Notes: Panel A, columns 1 through 5 display coefficients from estimates of equation (2) with various indicators and an interaction term between a lagged injury to a former peer and the officer tenure. Log officer tenure is a continuous variable representing the log of the number of months since the officer started at the police academy. Panel B columns show analogous results using the logged number of previous events + 1. We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Panel A of Table 8 displays the effect of former peer injuries as well as an interaction term between former peer injuries and the log of the officer’s tenure in months since they started at the police academy for the outcomes considered so far. More experienced officers are less responsive to former peers’ injuries in terms of using force, injuring suspects, or acting in a manner that causes a civilian to issue a complaint.³² This finding is consistent with the evidence on experience and rationality and the general finding that social influence decreases with experience. This finding is also consistent with those in Ta, Lande, and Suss (2021), who use body camera data to show that a police officer’s emotional reactivity is lower in more experienced police officers.

Next, we consider another form of experience: the number of times an officer has experienced an injury to a former peer. As mentioned in Section III, officers experience multiple events over the time horizon in our sample. In general, the effect that repeated exposure has on an officer’s responses is ambiguous. Officers

³²Online Appendix Table A7 conducts the same analysis using tenure in levels. We find qualitatively similar results.

may become increasingly agitated or risk-averse as they observe more peers being injured. Conversely, they may get used to learning about injuries to their former peers and respond less strongly. We investigate these effects in panel B of Table 8. Similar to the effect of tenure, we find that repeated exposure to the injuries of former peers attenuates the effect of peer injuries.³³

H. *Suspect Characteristics*

Next, we investigate heterogeneity based on suspect race by estimating equation (2) with the outcome being the force use against a White or minority suspect. We display the results in Table 9, which shows that officers are significantly more likely to use force against minority suspects. There are no significant increases in the probability of using force against White suspects.

Readers should use caution when interpreting these results. Roughly 81 percent of force uses and 80 percent of officer injuries result from interactions with Black suspects. As such, our results may be driven by the relatively small number of events observed for White suspects.

V. Mechanisms and Alternative Interpretations

Having established that police officers respond to peer injuries, we now attempt to understand what mechanisms might be driving this behavior. We begin by ruling out potentially confounding effects that would challenge our interpretation of the main finding. These include officers mimicking the force use of their peers through traditional peer effects and officers increasing their effort after a peer is injured. Then, we investigate whether officers respond to peer injuries because these injuries provide some information about their injury risk or transitory emotional responses.

A. *Officers Mimicking Peer Force Use*

First, we investigate whether officers are responding to their former peer's decision to use force rather than their former peer's injury. A large body of work shows that the actions of one's peers influence decision-making (Brock and Durlauf 2001). For example, Murphy (2019) finds that misconduct by soldiers in the US Army tends to occur at similar times as the misconduct of peers. In our setting, a similar effect would be officers increasing their propensity to use force because a peer does so. This effect could potentially confound our results because in 94 percent of instances where officers were injured, they also used force against the suspect.

We investigate this potential confounding effect using over 14,000 instances of force unaccompanied by an officer injury. We use these instances to investigate whether force use mimicry is driving these results by estimating equations (2) and

³³ Online Appendix Table A8 conducts the same analysis using number of past events in levels. We find qualitatively similar results.

TABLE 9—HETEROGENEOUS EFFECTS BY SUSPECT CHARACTERISTICS

| | White suspect (1) | Minority suspect (2) |
|--|----------------------|-------------------------|
| <i>Former peer injury in previous week</i> | 0.00008 (0.00012) | 0.00114 (0.00049) |
| <i>Constant</i> | 0.00112 (0.00001) | 0.01619 (0.00006) |
| Model | OLS | OLS |
| Percent increase | 6.88 | 7.2 |
| Unit-week fixed effects | Yes | Yes |
| Individual fixed effects | Yes | Yes |
| Pre-trend test | 0.697 | 0.835 |
| R^2 | 0.036 | 0.039 |
| Observations | 986,088 | 986,088 |

Notes: Columns 1 and 2 display coefficients from estimates of equation (2) where the outcome variable is an indicator representing whether the officer used a specific type of force against a White or minority suspect. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects. We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

TABLE 10—EFFECT OF FORMER PEER FORCE USE ON OFFICER FORCE USE

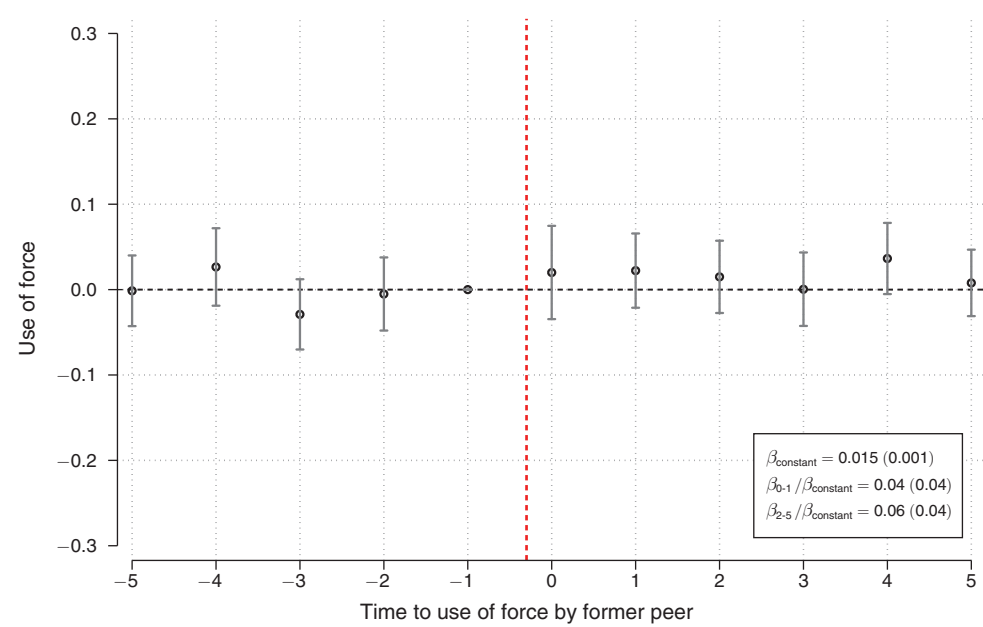
| | Force | | |
|--|----------------------|----------------------|-----------------------|
| | (1) | (2) | (3) |
| <i>Former peer injury in previous week</i> | 0.00501 (0.00061) | 0.00045 (0.00033) | 0.02629 (0.01754) |
| <i>Constant</i> | 0.01520 (0.00052) | 0.01745 (0.00016) | −3.01996 (0.00992) |
| Model | OLS | OLS | Poisson |
| Percent increase | 32.948 | 2.980 | 2.629 |
| Unit-week fixed effects | No | Yes | Yes |
| Individual fixed effects | No | Yes | Yes |
| Pre-trend test | 0.000 | 0.657 | 0.733 |
| R^2 | 0.000 | 0.040 | |
| Observations | 986,111 | 986,088 | 607,688 |

Notes: Columns 1 through 3 display coefficients from estimates of equation (2) where the outcome variable is an indicator representing whether the officer used force and the event is whether the officer's former peer used force but was not injured in the previous week. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects (column 1). We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Panel A of Figure 6 displays all percent changes from column 2 in this regression testing for differences in pre-trends.

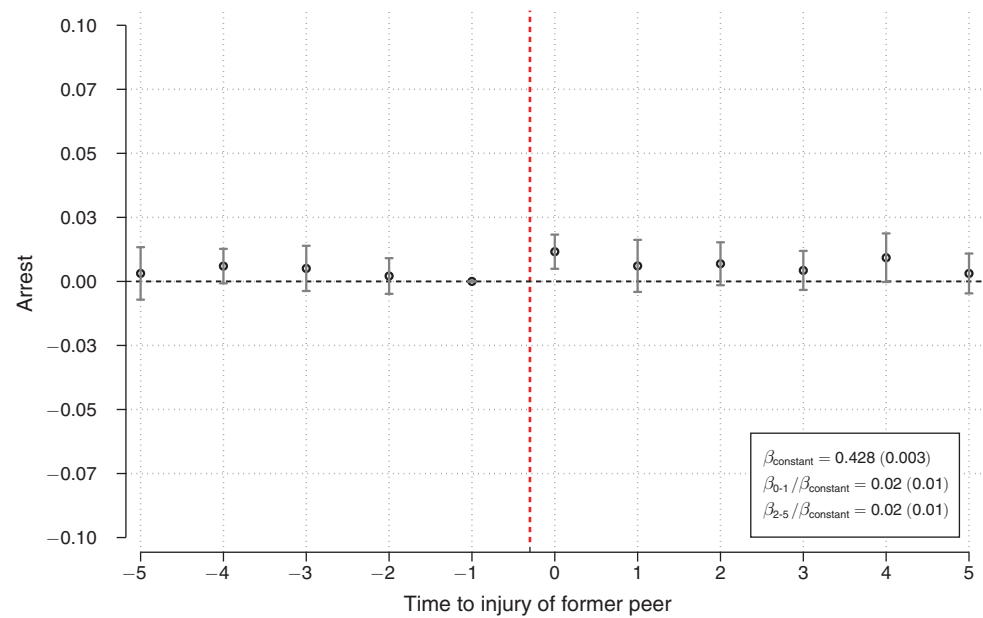
(3) with the outcome being force use and the treatment being an officer's former peer using force.

Column 1 of Table 10 shows that there is a strong correlation between an officer's use of force and the force use of former peers that was unaccompanied by an officer

Panel A. The effect of former peer force use on officer force



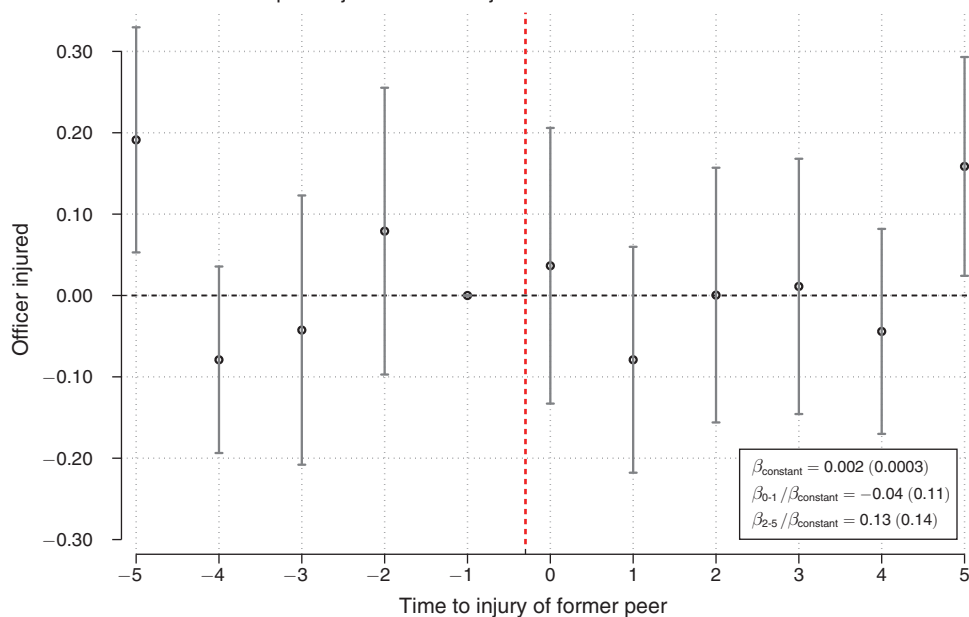
Panel B. The effect of former peer injuries on arrests



(continued)

FIGURE 6. MECHANISMS

Panel C. The effect of former peer injuries on own injuries

FIGURE 6. MECHANISMS (*continued*)

Notes: These graphs show coefficient estimates using equation (3) divided by the baseline rate of the outcome and 95 percent confidence intervals. The baseline rate of the outcome is calculated as the constant term from a regression of the outcome on treatment lags and leads without fixed effects. Standard errors are clustered by academy cohort ($G = 81$). In panel A, treatment is defined as the force use of a former peer that was not coincident with an officer injury, and the outcome is the officer's propensity to use force. In panel B, treatment is an injury to a former peer, and the outcome is the officer's propensity to make an arrest. In panel C, treatment is an injury to a former peer, and the outcome is the officer's propensity to be injured. The red vertical line represents the injury week.

injury. However, after controlling for individual and district-week fixed effects, we find no significant relationship between the two variables. Column 2 of Table 10 and panel A of Figure 6 show that there is a small and insignificant effect of former peers' force use on an officer's force use. Therefore, we can conclude that our results are not driven by officers mimicking the use of force by their former peers.

B. Officers Increasing Effort

Next, we investigate whether officers increase their time working or effort following an injury to a former peer. A potential issue is that officers have to work more hours after a peer injury because the peer has been removed from the pool of eligible workers. This is unlikely for two reasons. First, we are using events that occur to former peers. That means that the injured officers work in a separate police district. Second, there is no reason to expect that cohort mates of the injured officer working in different districts should be differentially affected by such events relative to other officers in different districts. Alternatively, officers may reduce their effort on the job for fear of being injured.

To investigate potential changes in police effort and time working, we use arrests as a measure of officer effort, following Mas (2006) and Ba and Rivera (2019).

TABLE 11—EFFECT OF FORMER PEER INJURIES ON OFFICER ARRESTS

| | Any arrest (1) | Nonindex crime (2) | Property crime (3) | Violent crime (4) |
|--|----------------------|--------------------------|--------------------------|-------------------------|
| <i>Former peer injury in previous week</i> | 0.00384 (0.00218) | 0.00397 (0.00171) | 0.00096 (0.00099) | −0.00007 (0.00125) |
| <i>Constant</i> | 0.37589 (0.00025) | 0.21881 (0.00020) | 0.06364 (0.00011) | 0.09899 (0.00014) |
| Model | OLS | OLS | OLS | OLS |
| Percent Increase | 1.034 | 1.835 | 1.532 | −0.007 |
| Unit-week fixed effects | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes |
| Pre-trend test | 0.423 | 0.675 | 0.052 | 0.106 |
| R^2 | 0.262 | 0.250 | 0.077 | 0.078 |
| Observations | 953,262 | 953,262 | 953,262 | 953,262 |

Notes: Columns 1 through 4 display coefficients from estimates of equation (2) where the outcome variable is an indicator representing arrests for various types of crime. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects. We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Column 1 in Table 11 shows the impact of former peer injuries on the probability of arresting a suspect for any reason in the following week. Panel B of Figure 6 displays the dynamics. Overall, we find that there is a small positive impact on officers' effort as measured by arrests.³⁴

Columns 2 through 4 in Table 11 show the impact of former peer injuries on different types of arrests. The arrest types correspond to the crime types: violent, property, and nonindex crimes. We find that nonindex crime arrests increase by 1.8 percent in the week following a peer injury. In contrast, property and violent crime arrests remain unaffected, indicating officers are unlikely to be working more. Furthermore, officers appear to be increasing their discretionary (nonindex) arrests, suggesting that they are not attempting to avoid potentially dangerous situations. These results suggest that officers do not materially decrease their effort after they experience a peer injury.

C. Social Learning and Emotional Responses

Now that we have ruled out officers increasing their time spent working, decreasing their effort on the job, or mimicking their peers' force use, we seek to determine why officers respond to peer injuries. We consider whether social learning (Banerjee 1992; Bikhchandani, Hirshleifer, and Welch 1992) or emotional responses (Rick and Loewenstein 2008; Kőszegi 2006) are more likely to drive the results.

Social learning will drive the results when officers are more likely to learn about their former peers' injuries and use this information in their future decisions to use

³⁴ In order to avoid issues with mismeasuring the week of the arrest due to the lack of precise arrest dates pre-2010, we redo the analysis on observations from 2010 to 2016 in online Appendix Table A9. The results are highly similar.

TABLE 12—EFFECT OF FORMER PEER INJURIES ON OFFICER INJURIES

| | Injured | | |
|--|----------------------|-----------------------|-----------------------|
| | (1) | (2) | (3) |
| <i>Former peer injury in previous week</i> | 0.00021 (0.00019) | −0.00017 (0.00016) | −0.07594 (0.07055) |
| <i>Constant</i> | 0.00231 (0.00007) | 0.00236 (0.00002) | −3.10241 (0.00859) |
| Model | OLS | OLS | Poisson |
| Percent increase | 8.860 | −7.260 | −7.590 |
| Unit-week fixed effects | No | Yes | Yes |
| Individual fixed effects | No | Yes | Yes |
| Pre-trend test | 0.000 | 0.085 | 0.085 |
| R^2 | 0.000 | 0.026 | |
| Observations | 986,111 | 986,088 | 88,582 |

Notes: Columns 1 through 3 display coefficients from estimates of equation (2) where the outcome variable is an indicator representing whether the officer experienced an injury. We calculate the percent increase by dividing the column's coefficient by the mean of the outcome variable for untreated officers, the constant term from a regression without fixed effects (column 1). We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p -value from an F -test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Column 5 in online Appendix Table A5 displays all of the coefficients on lags and leads for column 2 in this table. Column 5 in online Appendix Table A6 displays all of the coefficients on lags and leads for column 3 in this table.

force. Injuries to former peers may act as a private signal of the underlying injury risk. The signal could cause officers to update their beliefs about the probability of a noncompliant civilian injuring them, increasing their propensity to use force.

On the other hand, previous literature has also documented 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 (Tanaka, Camerer, and Nguyen 2010; Hanaoka, Shigeoka, and Watanabe 2018). Alternatively, 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.

Two main predictions separate these mechanisms. First, if social learning is driving the results, officers would have a lower chance of experiencing an injury themselves in the week following a peer injury, as they have better information about their true injury risk while on duty. We investigate this by estimating equations (2) and (3), with the outcome being an indicator representing the officer's injury status. We report these results in panel C of Figure 6 and Table 12. We find that injury risk falls by 7.26 percent in the week after a former peer is injured (column 2 of Table 12). However, the results are noisy and not statistically significant.

Second, the two mechanisms will also differentially affect the resulting dynamics. Under social learning, former peers learn about the treatment effects more quickly than those who are not former peers. In that case, the treatment group

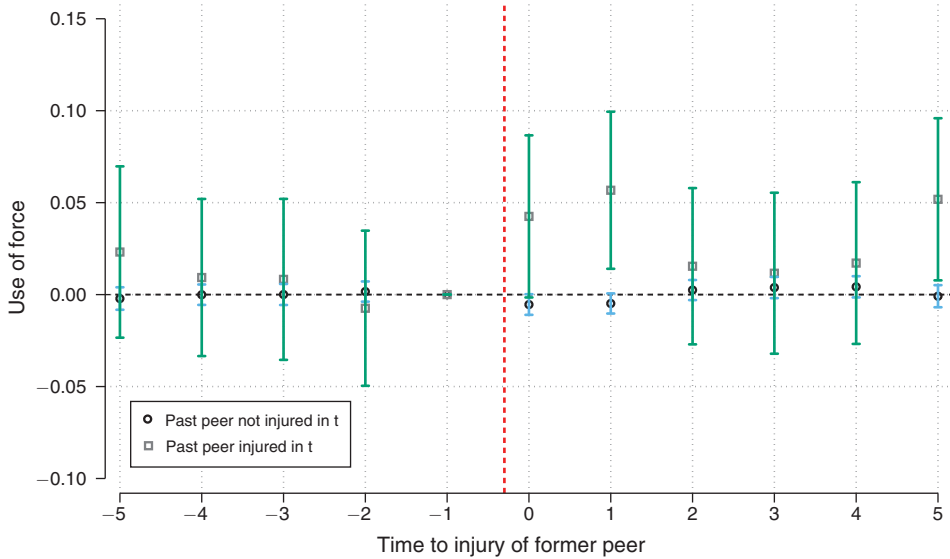


FIGURE 7. THE EFFECT OF FORMER PEER INJURIES ON USE OF FORCE

Notes: This figure displays the propensity to use force in periods around officer injuries relative to the week before an officer injury. The comparison group is comprised of individuals who did not have a former peer injured. The values are calculated by first regressing force use on individual and unit-week fixed effects. The residuals from this regression are then averaged within indicators representing weeks since an officer injury. We also present 95 percent confidence intervals using standard errors clustered at the individual level.

should have a constant effect, while the comparison group’s propensity to use force should increase to match that of the treatment group. In contrast, under an emotional response mechanism, the effects should dissipate within a few weeks. Loewenstein and O’Donoghue (2007) point out that the temporal proximity to the event greatly impacts emotional responses. Similarly, Card and Dahl (2011) and Munyo and Rossi (2015) both find that the emotional responses to sports losses are concentrated in a narrow time window after the game.

We display the effects separately for those experiencing an injury to a former peer (treatment) and those not experiencing an injury to a former peer (comparison) in Figure 7. In line with the emotional response mechanism, the control officers’ propensity to use force does not increase. In contrast, the treatment officers’ propensity to use force increases in the weeks immediately after the event and then returns to the baseline level of force use soon after that. Although we cannot fully rule out a role for social learning, these results strengthen the case for the emotional response interpretation.

VI. Conclusion

This article shows that on-duty injuries to police officers can have spillovers onto how other officers interact with civilians. Following a force-related injury to a police academy classmate working in a different district, officers increase their propensity to use force by 7 percent and increase their propensity to injure a suspect by

10 percent in the following week. Furthermore, these effects double in magnitude when the injured peer is of the same race. Given that we focus on peers acquired through the police academy and do not consider effects from peers currently in the same unit, these results likely underestimate the total effect of peer injuries.

We do not find evidence that these effects are driven by officers mimicking their peers' force use, changing their effort, or behaving differently due to social learning. Rather, we show that the increase in force use is concentrated in a narrow time window around the event and that the effects fade with officer tenure. Collectively, these results suggest that the effects are driven by an emotional response, with officers becoming more aggressive during interactions.

The existence of spillovers in police force use resulting from on-the-job injuries has important implications for policies meant to reduce improper use of force. Policies that have been shown to decrease officer injuries, such as increased patrol sizes (Kirchmaier et al. 2021), may have additional benefits for civilian and officer safety by avoiding spillovers. Other policies, such as police militarization, which can increase officer injuries, may have negative ripple effects by leading to more dangerous encounters for civilians (Masera 2021). Policymakers should consider these externalities 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 increases the risk to officers may have limited effects. Moreover, providing counseling or other support services to traumatized officers after a peer is injured (or a different incident occurs) may reduce instances of force use (Owens et al. 2018). To the best of our knowledge, there is very little research on the impact of support services on officers when they experience trauma. However, (Owens et al. 2018) show promising results indicating that low-intensity supervisory programs can lead officers to resolve incidents without making any arrest or using force, hence, without increasing the risk of injuries for either the officer or the civilian. Future research should attempt to identify interventions that can help prevent officer injuries without increasing the risk of police violence faced by civilians.

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