ESSAYS ON THE ECONOMICS OF CRIME

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Dedicated to the students of Universal Audenried Charter High School.

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ABSTRACT

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This dissertation contains three separate papers that study the economics of crime. Chapters 1 and 2 focus on labor economics questions applied to the context of policing. Chapter 1 studies the effect of overtime on officer performance; Chapter 2 studies the effect of investigated complaints on officer performance. Finally, Chapter 3 studies the effect of legal access to alcohol on criminal victimization.

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1.1. Introduction

Shift work is common in many industries, including those that provide vital public safety services to communities such as law enforcement, fire, and emergency medical services, yet little is known about the effects of working time on performance in many of these settings. While longer shifts tend to be associated with more time off between shifts and less time spent commuting, they may also be linked to a higher likelihood of mistakes due to fatigue. On the other hand, shorter shifts may be associated with a higher likelihood of mistakes due to turnover.

Research has shown that sleep deprivation produces impairments in performance similar to those caused by alcohol, with just 17-19 hours without sleep appearing equivalent to a blood alcohol content of 0.05% (Williamson and Feyer, 2000). Further, work has shown that long hours and overtime are associated with poorer perceived general health, increased injury rates, and more illnesses. (Caruso et al., 2006, 2004). In a hospital setting, nurses make significantly more errors when they work individual shifts longer than 12 hours or work spells over 40 hours a week (Rogers et al., 2004). Medical residents, who often work shifts longer than 24 hours, are significantly more likely to have a car accident on the way home from a shift than on the way to a shift (Steele et al., 1999) as well as more likely to experience a percutaneous injury during extended work[1] than during non-extended work (Ayas et al., 2006). Empirical economic work on extended work hours and performance is limited due to the non-random nature of scheduling. One study by Brachet and others uses a difference-in-differences design to study the effect of long hours on performance of emergency medical services (EMS) providers. They find that fatigue worsens performance towards the end of a shift and that the deterioration in performance amounts to a roughly

[1] "Extended work" is defined as working overnight.

0.76 percent increase in 30-day mortality for users of EMS (Brachet et al., 2012).

Similar to medicine, policing is a high-stakes field. Fatigue in police officers can affect the work of those officers and in turn the daily lives of the civilians whom they patrol. Survey evidence shows that police officers are very likely to have a sleep disorder and to report that fatigue affects their ability to perform their jobs (Vila, 1996; Vila et al., 2002; Rajaratnam et al., 2011). A randomized control trial of the impact of shift length (8- vs. 10- vs. 12-hour) on police officer performance and well-being in Detroit, MI and Arlington, TX found no significant differences in work performance across the three shift lengths. However, the study did find significant improvements in sleep length and quality for 10-hour shifts compared to both 8- and 12- hour shifts as well as significantly higher amounts of overtime worked by officers assigned to 8-hour shifts than those assigned to 10- or 12-hour shifts (Amendola et al., 2011). More recently, Chalfin and Goncalves have found that Dallas police officers actively avoid extension of tour overtime by making fewer arrests at the ends of their shifts (2020). Taken as a whole, the evidence on working time and performance in policing is mixed and none of the research to date finds a causal relationship between overtime and performance.

The relationship between overtime hours and performance is especially important due to the financial incentive created by the additional pay that comes with overtime. Employees in the U.S. covered by the Fair Labor Standards Act (FLSA) are entitled to overtime pay of at least 1.5 times standard pay for all hours worked in excess of 40 hours per week. Given this, employers must weigh the variable cost of overtime pay against the fixed cost of hiring and training additional employees when making important staffing decisions (Lazear and Oyer, 2007). These decisions are especially critical in policing where, as stated previously, any changes in the performance of police officers due to extended working hours can have direct impacts on the civilians whom they patrol.

This research leverages special events in the city of Chicago as an instrument for working overtime. The key assumption is that a given officer is more likely to work special event overtime the more manpower the event requires. Using a leave-out mean of available officers working overtime as an instrument, I find that working special event overtime causes decreases in arrests of 10% to 27% depending on the timing of overtime in a work spell. Effects are driven by changes in misdemeanor, in particular, drug arrests. I find no evidence that the changes are due to fatigue using three separate tests that exploit the nature of scheduling in the CPD. Rather, it appears that they are the result of behavioral modifications on the part of the officer and suggest that officers may operate under reference-dependent preferences.

The rest of the paper is organized as follows. Section 2 provides institutional background on the Chicago Police Department and its overtime practices. Section 3 describes the data. Section 4 describes my empirical strategy. Section 5 presents the central findings and section 6 discusses potential mechanisms behind the results. Finally, Section 7 concludes.

1.2. Background

Comprised of roughly 12,000 sworn employees and 2,000 other employees, the Chicago Police Department (CPD) is the second largest municipal police department in the United States and serves the third largest city in the United States. The Bureau of Patrol breaks the city into 22 geographic units or districts for the purposes of policing. The crime rate in Chicago is significantly higher than the US average for both large cities and municipalities of any size. According to the FBI's Uniform Crime Reports, Chicago ranked number 17 in violent crime and number 10 in murder and non-negligent manslaughter per 100,000 people in 2017. Crime is most concentrated in the city's south and west sides.[2]

1.2.1. Overtime

There are many reasons why a CPD officer may work overtime including court appearances during off-duty hours, special events, training, extension of tour, and call backs. From 2012

[2]See Appendix Figures B1 and B2 for a visualization of the distribution of crime and arrests across the city.

to 2015, overtime was logged using a paper-based record-keeping system.[3] The Office of the Inspector General's "Chicago Police Department Overtime Controls Audit" describes the many issues with CPD's overtime practices during this time period. Of particular interest to this paper is the lack of sufficient monitoring of overtime to prevent unnecessary spending or overtime abuse at the officer level. Table 1 shows that the CPD over-spent a minimum of 29% to a maximum of 168% of its overtime budget during the sample period.

Officers earn overtime for "all time in excess of the hours worked in the normal work day and normal work week" ... "at a rate of time and one-half on the basis of completed fifteen-minute segments in either compensatory time or payment" (CPD Department Directives System). Officers who elect to earn overtime in compensatory time store hours at the rate of time and one-half that they are able to use for paid vacation days in the future. There are some differences in pay or compensatory time availability depending on the bargaining unit that represents the officer in question.[4,5] Regardless of preferences over additional pay and compensatory time, overtime creates clear incentives for officers to complete work above and beyond that required by their standard work schedules.

1.2.2. Special Events

According to CPD Employee Resource E07-01, the Special Events and Liaison Section (Unit 136) is responsible for staffing, among other things, voluntary special work opportunities (VSWOs) in the city. This unit coordinates, schedules, and notifies officers of their assignments to four types of VSWOs: voluntary special employment[6] (VSE), voluntary overtime initiative[7] (VOI), voluntary cancelled day-off for preplanned events[8] (VCDO), and Depart-

[3]Designated timekeepers logged overtime information into the CPD's electronic CLEAR system after hours were submitted on paper by officers.

[4]For example, employees not exempt from the FLSA receive two times the regularly hourly rate of pay for overtime worked on a Sunday when the Sunday was not a part of the officer's originally scheduled work week.

[5]The vast majority of sworn officers are represented by the Fraternal Order of Police, Chicago Lodge No. 7.

[6]Examples of this include posts at the Chicago Housing Authority or the Chicago Transit Authority.

[7]This type of overtime is worked for larger special events in the city such as the Chicago Marathon.

[8]This is worked when an officer chooses to forgo a regular day off to work CPD initiatives such as Operation Safe City.

ment procured outside employment[9] (DPOE). During the sample period, any overtime worked for VSWO was labeled as special event overtime in the department's CLEAR system.

Although the overtime logging system does not record the specific type of VSWO overtime worked, other data sources provide a basic understanding of the nature of special events in Chicago. The Special Event Evaluation Form (CPD Form 11.466) is completed after every special event in the city that requires a permit and CPD officers present. Figure B3 provides an example of one such form completed between 2012 and 2015. A public records request for each of these forms filled out between 2012 and 2015 returned a total of 163 unique special event-days.[10] The responsive records were unlikely complete because forms for certain annual events (eg, Lollapalooza) were present for some but not all years in the sample. An amended list of special events that includes each year of an event that appeared at least once in the CPD's response returned a total of 472 unique special event-days during the sample period. Further, the "Additional Assignment Info" field in CPD's attendance data provides an additional source of information on types of assignments as this field oftentimes includes strings related to VSWO employment for events that do not appear in the responsive records for CPD Form 11.466. Examples of types of assignments that fall under VSWO as given by the attendance data include detail for CPD-employee funerals, visits of important figures such as the President, polling places on election days, and teacher's strikes. As indicated by the title, officers voluntarily work VSWO when their schedules allow. These posts are staffed first by need and second by seniority, with more senior officers having the first choice of whether or not to work VSWO for a given detail. Table 2 shows average special event size[11] by shift. Shift 3 officers work the smallest events which require an average of 1.7 officers to staff and shift 4 officers work the largest which require an average of 6.19 officers. As is clear in the table, some events require significantly

[9]DPOE overtime is worked when officers participate in non-city/non-government sponsored events such as garden walks and 5K runs.

[10]Some events spanned multiple days.

[11]Size is defined as the number of officers working special event overtime in the same unit on the same calendar day.

more officers, with a maximum over all four shifts of 53 officers for one event.

1.3. Data

This research seeks to understand if and how working overtime affects the performance of police officers. Because there is no publicly available data on police officer schedules, overtime, and performance measures, I construct a unique dataset on police officer work histories and performance outcomes using administrative data from Chicago that spans the years 2012-2015. The data were gathered over three years of public records requests and linked across officers using the officer's full name. Because of this, any names that belong to more than one officer in a given year are dropped from the data.[12]

The data cover roughly 8500 officers from each of Chicago's 25 police districts.[13] Officers in these districts police within a fixed geographic boundary and are assigned to one of four shifts. Shifts 1-3 are fixed in time and shift 4 has a start time that fluctuates based on the needs of the city.[14] Attendance data for each officer is linked to overtime, arrest, and complaint data to construct a dataset that gives shift-level[15] information on police officer performance. Table 3 contains descriptive statistics at the officer level. As the table indicates, the vast majority of officers are male and are on average in their 40s with just over a decade of service with the CPD. Half of officers are White, 23% are Black or Hispanic, and the remaining officers are Asian/Pacific Islander. It is important to note that the demographic makeup of the CPD, while diverse compared to many other police departments, is not fully reflective of the demographic makeup of the city of Chicago. Again, Table 3 shows that, on average, officers make roughly one arrest and work 2.2 hours of overtime per shift. Complaints are much less frequent and are logged an average of 1.5 times per 100 shifts.

[12] Roughly 3% of officers have the same name in each of the four years of the data.

[13] In 2012, districts 13, 21, and 23 closed and were absorbed into existing districts. District by quarter fixed effects will be used in all models to control for changes caused by district closures.

[14] For example, shift 4 may start at 5pm in December but at 7pm in August due to crime's relationship with weather and ambient lighting.

[15] As some shifts span two days, outcome information is attached to the date on which a shift starts for each officer by shift observation.

As is clear in Table 3, there is a large amount of variance in arrests, complaints, and overtime hours across officers. Figure 1 contains kernel distributions of arrests, overtime, and complaints at the officer level. Panels on the left-hand side of the figure contain kernel density plots of the raw data. Because these measures are likely correlated with both location and shift, I residualize the data to control for this in the panels on the right-hand side. All figures show that there are very high performing officers on all metrics, even when controlling for time (via shift) and place (via district). Figure 2 displays the raw data in percent form. The x-axis of each plot represents the percent of district officers and the y-axis of each plot represents the percent of arrests, overtime hours, or complaints earned by a given percent of officers. If each officer contributed equally to the total, we would see a $y = x$ line. As seen in the figure, a relatively small percent of officers are responsible for a large portion of these outcomes. Taking 80% as a benchmark, we see that approximately 40% and 45% of officers are responsible for 80% of felony and misdemeanor arrests, respectively. Approximately 18% of officers are responsible for 80% of special event overtime while around 35% of officers are responsible for the same portion of extension of tour and court overtime. It is also important to note that about 45% of officers work no special overtime between 2012 and 2015. Panel C of Figure 2 shows that roughly 35% of officers are responsible for 80% of complaints and about 40% of officers have no complaints logged against them during the sample period.

Focusing on outcomes for all district units, Table 4 contains arrests by type of crime for the years 2012-2015. Arrests for drug crimes are the most common, making up about 23% of all district-level arrests. Notably, arrests for violent and property crime make up only about 16% and 12% of arrests, respectively. Table 5 contains the breakdown of overtime hours by type. Court overtime makes up about 45% of all overtime hours. Special event and extension of tour each make up 25% of hours, leaving 5% of overtime in the "other" category. See Appendix Figure B4 for the distribution of overtime across districts. A visual comparison of Appendix Figures B1, B2, and B4 shows that special events tend to be concentrated in lower-crime areas of the city.

Table 8 contains information on complaints made both by employees of CPD and civilians. About 90% of complaints against CPD officers are made by individuals external to CPD. Of these complaints, about 48% are for improper searches or use of force. Notably 34% of external complaints are for operations/personnel violations. This same type of complaint makes up over 50% of internal complaints. Other common internal complaint categories are use of force, lockup procedures, and conduct unbecoming. See Appendix Tables 9 and A2 for information on final findings and disciplinary outcomes for these complaints. Further, Appendix Figure B5 displays the distribution of complaints across the city.

1.4. Methods

This paper will use an instrumental variables model exploiting special events in Chicago that induce some officers to work overtime.

1.4.1. Baseline Ordinary Least Squares

District officers in Chicago work a 4-2 schedule, meaning they work four days in a row followed by two days off. This creates a scheme where officers work an average of 5 days in a given week and have 2 continuous days off that rotate throughout the week over time. Given this scheduling, an officer could work a special event before his first, second, third, or fourth shift. I will model the relationship between overtime and performance using the following regression

$$Y_{icst} = \alpha + \beta OT_{i,t-d} + \Gamma X_{ict} + \nu_{ict} \tag{1.1}$$

where i is officer, c is unit, s is shift, and t is the calendar day on which a shift begins. Because a special event likely changes the environment faced by officers, only days without special events are included in analysis. Y_{icst} is a performance measure, $OT_{i,t-d}$ is an indicator variable that is 1 if officer i worked overtime on day $t-d$ and 0 otherwise. X_{ict} is a matrix of fixed effects and linear controls. Fixed effects included are unit by quarter, shift time,[16] shift number,[17] day of week, and officer. Unit by quarter fixed effects control for variation

[16]Shift time refers to the starting time of a shift.
[17]Shift number n refers to the n-th consecutive shift worked before a day off.

in a given location across time as well as for the three district closures that occur in the first year of the data. Shift time and day of week fixed effects control for the relationship between crime and day of the week and time of day, respectively. Shift number fixed effects absorb any non-linearities in performance across a work spell. Finally, officer fixed effects control for the large amount of variation in outcomes across officers. Linear controls for reported index and non-index crimes are also included. While our estimates from the standard OLS models will not be causal due to selection into working overtime, they provide benchmark against which to compare our instrumental variables estimates.

1.4.2. Instrumental Variables

To identify the effects of working overtime on performance of officers who are likely to police special events only when they require a large amount of manpower, I will instrument for working overtime using a measure of the size of a special event. More specifically, I will use a leave-out mean of the number of officers who worked special event overtime at the district by shift by date level as an instrumental variable for whether or not an individual officer worked overtime on that day. This amounts to the two-stage least squares (2SLS) model below:

$$Y_{icst} = \alpha + \beta_0 OT_{ics,t-d} + \Gamma X_{ict} + \nu_{ict} \tag{1.2}$$

$$OT_{ics,t-d} = \rho_0 \frac{1}{n-1} \sum_{j \in cs, j \neq i} OT_{j,t-d} + \Gamma X_{ict} + \epsilon_{ict} \tag{1.3}$$

where $OT_{j,t-d}$ is an indicator variable that is 1 if officer j worked overtime on date $t-d$ and n is the total number of officers assigned to unit by shift cs on date $t-d$ who were *not* working a regularly scheduled shift that day and as such available to police a special event.

In order for this 2SLS model to uncover causal estimates of the local average treatment effect (LATE), the instrument must satisfy three criteria: excludability, relevance, and monotonicity (Angrist and Imbens, 1994). Although excludability cannot be tested directly, it is likely the case that $\frac{1}{n-1} \sum_{j \in cs, j \neq i} OT_{j,t-d}$ influences Y_{icst} only through $OT_{ics,t-d}$ due to the inclusion of controls for reported crime and the restriction that no special events can occur on a given unit by shift by date. The controls for reported crime will absorb any lingering effects of a past special event on the current environment faced by an officer. To support this claim, I test for correlation between overtime and reported crime at the unit by shift level in Table 7 using the following OLS regression

$$Y_{cst} = \alpha + \beta_0 OT_{cs,t-d} + \Gamma X_{ct} + \nu_{ct}, \tag{1.4}$$

where Y_{cst} is the count of reported index or non-index crimes at the unit by shift by calendar date level. $OT_{cs,t-d}$ is an indicator variable that is 1 if there was special event overtime worked by any officer assigned to unit c and shift s on day $t-d$ and 0 otherwise. X_{ct} includes unit by quarter, shift, and day of week fixed effects and linear controls for non-index or index reported crime, respectively.[18] The results in Table 7 indicate that the special events, even in the past, may impact the crime environment faced by an officer. For example, the table shows that reported index crimes increase by an average of 1.6% when there has been special event overtime overtime on shift $t-1$ compared to when there has been no such overtime. This in itself does not violate excludability; rather, it is precisely why controls for reported crime included are despite the requirement that days with special events are excluded from analysis. Table 8 contains parameter estimates for β_0 in (4) when Y_{cst} is the unit by shift by date level count of arrests or complaints. In this case, X_{ct} includes linear controls for both reported index and non-index crimes in all regressions. While these results are not causal, they provide suggestive evidence that, conditional on fixed effects and linear controls for reported crime, special events are not associated with significant changes in the environment faced by officers.

My instrument satisfies relevance if it is that case that, on average, an officer is more likely to work a special event if that event requires more manpower. This relationship is clearly true and supported by the strong first stage results in Table 9. The table shows a statistically significant relationship between the leave-out mean and overtime indicator for overtime

[18] More specifically, if the dependent variable, Y_{cst}, is the count of reported index crimes, a linear control for reported non-index crimes is included and vice versa.

worked from one to four shifts in the past. Effect sizes are very large, but keep in mind the denominator in the leave out mean as well as the mean of the dependent variable. One additional officer working overtime leads to an increase in overtime likelihood of roughly 80%. This amounts to the mean overtime indicator increasing from 0.01 to 0.018. Given that the minimum Kleibergen-Paap rk Wald F statistic is 92, the instrument is strong. Finally, for the instrument to satisfy monotonicity, it must *not* be the case that for a given change in the value of the instrument, some officers increase overtime likelihood while others decrease it. While this criteria is difficult to test explicitly, its violation is only logically possible by senior officers who have the first choice at working overtime. This is because only those officers are able to select into small events but out of large events. Figure 3 plots average years of service for each event size in the data with a local polynomial regression in gray. While not causal,[19] this figure shows a slight negative relationship between event size and years of service, potentially violating monotonicity. As a robustness check, officers in the top 10% or 20% of seniority are removed from each district by shift grouping and the interpretation of results is unchanged.

1.5. Results

Ordinary least squares estimates for the relationship between existence of special event overtime and performance are in Table 10. Referring back to Table 5, note that 70% of overtime worked in Chicago is either court or extension of tour. Both of these types of overtime are directly related to performance and it is likely that officers who work these types of overtime most frequently do so because they are high performing officers. Because of this, it is important to focus on a type of overtime, that worked for special events, for which the nature of assignment allows for a causal analysis. OLS results for special event overtime in Table 10 are inconsistent but show some evidence of overtime being related to a decrease in arrests for drug crimes of around 15%.[20] Although it is not the case that

[19] An important consideration that the figure does not take into account is the concentration of senior officers in lower crime districts that also happen to have larger and more frequent special events.

[20] Appendix Table A3 presents estimates for the relationship between special event overtime hours and performance. Results are similarly inconsistent.

officers who work special events are likely high-performing officers (nor are they likely to be low-performing officers), the issue of selection into this overtime remains and prevents the OLS estimates from being causal.

1.5.1. Instrumental Variables

Recall our strong first stage from Table 9. Conditional on the satisfaction of excludability, relevance, and monotonicity, the 2SLS estimates for β_0 represent the local average treatment effect (LATE) of working special event overtime on performance. More specifically, β_0 represents the causal effect of working special event overtime on performance for complier officers, or those who are more likely to work special events the larger the events are. Table 11 contains IV LATE estimates for β_0 from (2). Special event overtime causes 27% to 10% decreases in arrests for events worked one to three shifts in the past, respectively. Changes in arrests are being driven by misdemeanor arrests, with significant decreases from 32% to 12%, depending on shift number. Within misdemeanor arrests, drug arrests fall significantly from between 45% and 90%. Importantly, arrests for serious crimes, or felony arrests, show no significant relationship with overtime. Additionally, there is modest evidence that complaints per arrest decrease after working overtime. Taken as a whole, the evidence suggests that working overtime leads to decreases in low-level productivity without increases in mistakes as proxied by complaints.[21,22] Appendix Table A6 contains estimates for β_0 from (2) with a more saturated set of controls for the crime environment. These regressions include unit by quarter, unit by shift, and day of week fixed effects as well as linear controls for reported index and non-index crimes. A leave-out mean of the dependent variable is included in lieu of officer fixed effects due to power limitations. Results in this table are consistent with those in Table 11, bolstering support that the changes are caused by working overtime and not the effect of special events on the environment faced by officers.

[21] Appendix Table A4 contains 2SLS estimates for the LATE of special event overtime hours on performance. Results are consistent with those in Table 11.

[22] See Appendix Table A5 for estimates of β_0 from (2) for other common arrest types.

1.6. Mechanisms

A natural mechanism through which overtime may affect officer performance is fatigue. Additional overtime hours may make an officer more tired than he normally would be on a given shift, causing his productivity to decrease. Although there are not direct ways to test this channel, I will use shift timing, the timing of overtime in a work spell, and arrest times to indirectly speak to this mechanism.

1.6.1. Shift Timing

Given that policing is an around the clock operation, some officers routinely work during the night and others do so during the day. Given that special events tend to happen during the day, one can expect that officers assigned to night shift who work special events are *more likely* to have their sleep patterns affected by overtime. If fatigue via loss of sleep were the main mechanism at play, one would expect the productivity of night shift workers to be *more* affected by special event overtime than that of day shift workers. Defining night shift workers as those whose shifts span two calendar days (eg, a shift starts Monday night and ends the following Tuesday morning), I run (2) and (3) on day shift and night shift workers separately. Figure 4 shows the distribution of overtime and arrests by shift timing. The figure shows that day and night shift workers contribute fairly equally to special event overtime but that day shift workers tend to make more felony and misdemeanor arrests than night shift workers do. Given that both shifts work a fairly equal amount of overtime, one should not expect sample size to pose a power issue for the regressions run on either shift.

Tables 12 and 13 contain LATE estimates for the effect of working special event overtime on performance for day and night shift workers, respectively. As is clear in the tables, the productivity of night shift workers is not significantly affected by working special event overtime, indicating that changes in productivity of day shift workers drive the relationship shown in Table 11. These results provide suggestive evidence that fatigue via loss of sleep is not the mechanism behind the relationship between overtime and performance.

1.6.2. Special Event Timing

Next, I exploit the timing of special event overtime in a work spell to assess whether fatigue via increased working hours seems to be a mechanism behind the relationship between overtime and performance. Given that officers primarily work 4 shifts of a fixed length in a row before any days off, one could imagine that fatigue affects officers more the further they are in a work spell. For example, an officer has had two consecutive days off before his first shift in a spell but has worked for three days in a row before his fourth shift. Because of this, one would expect that overtime before a fourth consecutive shift will have a larger effect on productivity on average than overtime before the first shift in a work spell if fatigue is causing the decreases in productivity seen in Table 11. Figure 5 shows parameter estimates for β_0 from (2) conditional both on the shift number in a work spell and overtime timing. For example, the leftmost parameter in Panel B in the figure is the estimate of β_0 when an officer is in his first regular shift and worked overtime on date $t-2$. If fatigue were the mechanism at play, one would expect the a negative sloped line to result from connecting the parameters in each panel. Although there are some power issues that come into play when one conditions on both shift number in a work spell and overtime timing, it is clear that there is not a negative relationship between the parameters in any of the four panels in the figure, providing additional evidence that fatigue via increased hours of work is not causing the changes in productivity that follow special event overtime.

1.6.3. Arrest Timing

As a third and final test of the mechanism of fatigue, I study the extent to which special event overtime causes changes in arrest timing. If this overtime does affect performance through fatigue, one would expect that, on average, arrests would happen later in the shift if overtime was worked recently than if it were not worked. To test this, I run (2) and (3) conditional on an arrest being made where the outcome variable is the number of hours into a shift at which an arrest occurs. Table 14 contains LATE estimates of β_0 from (2) where Y_{aicst} is the number of hours into shift s that arrest a made by officer i in unit c whose shift begins on date t. Table 14 shows a largely insignificant relationship between special event overtime and arrest timing. Misdemeanor arrests made one shift after special event overtime occur roughly 15 minutes later in the shift on average compared to the same arrests when special event overtime was not worked one shift in the past. This parameter is significant at the 10% level, however the magnitude of the effect is small despite its being 25% of the mean. Being shifts are nine hours long, a 15 minute change in arrest timing amounts to less than 3% of the length of a shift. This provides further evidence against fatigue as the mechanism behind the changes in productivity caused by overtime.

1.6.4. Behavioral Modifications

Given the lack of evidence of fatigue as the mechanism at play, one might wonder if the nature of work changes on regular shifts after overtime has been worked. It could be the case that police officers simply patrol less on shifts after overtime, causing a decrease in arrests unrelated to fatigue. In order to rule this out, I run (2) only including shifts where the officer is assigned to a patrol car. Results for these regressions are in Table 15 and show the same relationship between special event overtime and performance, although effect sizes are smaller. Due to the smaller effect sizes, one can assume that some but not all of the relationship between overtime and performance is due to changes in assignment.

The remaining effects of special event overtime on officer performance may be the result to behavioral modifications on the part of the officer. It could be the case that officers operate with reference-dependent preferences and view productivity via special event overtime as a substitute for productivity via arrests. If this were the case, one would expect to see decreases in arrests that follow overtime without evidence of fatigue.

1.7. Conclusion

Given the association between long work hours and overtime and poorer perceived general health, increased injury rates, and more illnesses, it is important to understand how long hours effect performance in high-stakes fields like policing. Survey evidence shows that police officers are very likely to have a sleep disorder and to report that fatigue affects their ability to perform their jobs (Vila, 1996; Vila et al., 2002; Rajaratnam et al., 2011). In addition, a randomized control trial of the impact of shift length (8- vs. 10- vs. 12-hour) on police officer performance and well-being in Detroit, MI and Arlington, TX found no significant differences in work performance across three shift lengths. However, the study did find significant improvements in sleep length and quality for 10-hour shifts compared to both 8- and 12- hour shifts as well as a significantly higher amount of overtime worked by officers assigned to 8-hour shifts than those assigned to 10- or 12-hour shifts (Amendola et al., 2011).

The relationship between additional hours due to overtime and performance is especially important due to the financial incentive created by the additional pay that comes with overtime. Given this, employers must weigh the variable cost of overtime pay against the fixed cost of hiring and training additional employees when making key staffing decisions (Lazear and Oyer, 2007). These decisions are especially important in policing where any changes in the performance of police officers due to extended working hours can have direct impacts on the civilians whom they patrol.

This research provides the first causal evidence on the effects of overtime on police performance by leveraging special events in the city of Chicago as an instrument for working overtime. By exploiting the fact that a given officer is more likely to work special event overtime the more manpower the event requires, I find that working special event overtime causes decreases in arrests of 10% to 27% depending on the timing of overtime in a work spell. Effects are driven by changes in misdemeanor, in particular, drug arrests. I find no evidence that the mechanism behind this relationship is fatigue by completing simple tests using shift, overtime, and arrest timing which is unsurprising given that special event overtime leads to a relatively small increase in working hours. Rather, it appears that they are the result of behavioral modifications on the part of the officer that could be the result of reference-dependent preferences. Without information on the objective function of a given police department, it is unclear whether allowing this type of overtime is preferred to hiring additional officers.

1.8. Tables

Table 1: Overtime Spending

Year	Actual Spending	Appropriated Spending	%
2012	61,270,928	36,934,000	166
2013	107,133,125	39,934,000	268
2014	103,043,397	79,599,000	129
2015	115,324,438	79,624,000	145

Source: City of Chicago Financial Management and Purchasing Systems, nominal dollars.

Table 2: Special Event Size by Shift

Shift	Min	p25	p50	p75	Max	Mean	SD	N
1	1	1	1	1	49	1.90	4.25	4692
2	1	1	1	2	49	2.19	3.62	7614
3	1	1	1	2	53	1.69	1.83	8729
4	1	1	1	9	51	6.19	4.16	5231

This table contains summary statistics of the average count of officers assigned to special event overtime (conditional on at least one assignment) for district units from 2012-2015.

Table 3: Officer Descriptive Statistics

Percent Male	77.68
Percent White	50.71
Percent Black	22.74
Percent Hispanic	22.85
Mean Age	40.74
	(8.71)
Mean Years of Service	12.18
	(7.48)
Mean Arrests Per 100 Shifts	18.78
	(15.92)
Mean Complaints Per 100 Shifts	0.30
	(0.50)
Mean Overtime Hours Per 100 Shifts	37.88
	(55.18)
Total Officers	8554

This table contains descriptive statistics for district unit officers with unique full names. Standard deviations in parentheses.

Table 4: Arrests by Type

Crime Type	No.	% of Total
Violent		
Homicide	243	0.09
Rape/Sexual Assault	283	0.10
Robbery	2127	0.77
Assault	41,724	15.08
Property		
Burglary	2480	0.90
Larceny/Theft	20,793	7.52
Motor Vehicle Theft	2454	0.89
Other Property	6251	2.26
Other		
Drug	62,909	22.74
Public Nuisance	30,113	10.89
Traffic	18,026	6.52
DUI	8558	3.09
Vice	4165	1.51
Warrant	36,084	13.04
Weapon	7316	2.64
Other	33,115	11.97
Total	276,641	100.00

Table 5: Overtime Hours by Type

Type	Portion of Total
Court	0.45
Extension of Tour	0.25
Special Event	0.25
Other	0.05

Categories included in other: call back, CAPS, staff meeting, and other.

Table 6: Complaints: Categories

	Complainant Type					
Complaint Category	External		Internal		Total	
	No.	%	No.	%	No.	%
Operation/Personnel Violations	3769	34.12	633	51.97	4402	35.90
Improper Search	3326	30.11	33	2.71	3359	27.39
Use of Force	1976	17.89	98	8.05	2074	16.91
Lockup Procedures	757	6.85	141	11.58	898	7.32
Traffic	271	2.45	12	0.99	283	2.31
Conduct Unbecoming	102	0.92	134	11.00	236	1.92
Verbal Abuse	225	2.04	8	0.66	233	1.90
Other	619	5.60	159	13.05	778	6.34
Total	11,045	100.00	1218	100.00	12,263	100.00

Categories included in other are alcohol abuse, bribery/official corruption, criminal misconduct, substance abuse, supervisory responsibilities, and unknown. Each of these comprises less than 1% of total complaints.

Table 7: Test for Conditional Random Assignment: Crime

	(1)	(2)	(3)	(4)	(5)
	t	t-1	t-2	t-3	t-4
Index	0.250**	0.224***	0.113	-0.0326	-0.0310
	(0.0985)	(0.0818)	(0.0712)	(0.0798)	(0.0718)
Mean of DV	13.71	13.70	13.71	13.71	13.71
Effect Size (% of Mean)	1.822	1.635	0.822	-0.238	-0.226
Non-Index	-0.0236	-0.0717	-0.108*	-0.0799	-0.104
	(0.0699)	(0.0611)	(0.0563)	(0.0597)	(0.0643)
Mean of DV	8.898	8.895	8.897	8.898	8.899
Effect Size	-0.266	-0.806	-1.216	-0.898	-1.170

This table contains parameter estimates for the relationship between special event overtime and reported crime. All regressions include unit by quarter, day of week, and shift fixed effects and linear controls for index and non-index reported crime where appropriate. Standard errors clustered at the unit by shift level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Test for Conditional Random Assignment: Arrests and Complaints

	(1) t	(2) t-1	(3) t-2	(4) t-3	(5) t-4
All	0.0916	0.0612	0.0652	0.0703	0.0856
	(0.0588)	(0.0558)	(0.0561)	(0.0540)	(0.0569)
Mean of DV	3.273	3.274	3.275	3.275	3.276
Effect Size (% of Mean)	2.799	1.868	1.991	2.147	2.614
Felony	0.0205	0.000554	-0.00884	0.000187	0.0160
	(0.0152)	(0.0148)	(0.0142)	(0.0129)	(0.0149)
Mean of DV	0.653	0.654	0.654	0.654	0.654
Effect Size	3.143	0.0848	-1.352	0.0286	2.444
Misdemeanor	0.0711	0.0606	0.0740	0.0701	0.0697
	(0.0509)	(0.0477)	(0.0486)	(0.0474)	(0.0488)
Mean of DV	2.620	2.621	2.621	2.621	2.622
Effect Size	2.713	2.313	2.825	2.676	2.657
Complaint	0.00476	-0.00700	-0.0153	-0.0157	0.00883
	(0.0193)	(0.0190)	(0.0185)	(0.0167)	(0.0187)
Mean of DV	0.673	0.673	0.673	0.673	0.673
Effect Size	0.707	-1.040	-2.270	-2.335	1.312

This table contains parameter estimates for the relationship between special event overtime and arrests. All regressions include unit by quarter, day of week, and shift fixed effects and linear controls for index and non-index reported crime. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Special Event Overtime Indicator and Performance (First Stage)

	(1)	(2)	(3)	(4)
	Overtime On Shift			
	t-1	t-2	t-3	t-4
Leave-Out Mean	0.295***	0.369***	0.355***	0.337***
	(0.0304)	(0.0289)	(0.0315)	(0.0350)
Mean of DV	0.00951	0.00955	0.00925	0.00917
Effect Size (% of Mean)	3104.7	3862.1	3839.2	3674.7
Mean Pool Size	38.99	38.99	38.99	38.99
F-Statistic on Excluded IV	94.49	162.9	127.2	92.42

This table contains parameter estimates for the first stage of the effect of the existence of special event overtime on performance. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of the portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses. Pool size refers to the average number of off-duty officers in each unit by shift by date cell. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Special Event Overtime Indicator and Performance, OLS

	(1)	(2)	(3)	(4)
	Overtime On Shift			
	t-1	t-2	t-3	t-4
All	-0.00591	-0.00194	0.00525	0.00876*
	(0.00598)	(0.00441)	(0.00536)	(0.00455)
Mean of DV	0.182	0.182	0.182	0.182
Effect Size (% of Mean)	-3.248	-1.065	2.885	4.809
Felony	-0.0000913	-0.000177	0.00526*	0.00136
	(0.00210)	(0.00249)	(0.00268)	(0.00256)
Mean of DV	0.0428	0.0428	0.0428	0.0428
Effect Size	-0.213	-0.415	12.29	3.190
Misdemeanor	-0.00582	-0.00176	-0.00000365	0.00740*
	(0.00509)	(0.00383)	(0.00399)	(0.00444)
Mean of DV	0.139	0.139	0.139	0.139
Effect Size	-4.179	-1.264	-0.00262	5.306
Drug	-0.00565***	-0.00644**	-0.00490	-0.00512**
	(0.00174)	(0.00271)	(0.00341)	(0.00244)
Mean of DV	0.0379	0.0379	0.0379	0.0379
Effect Size	-14.90	-16.99	-12.91	-13.50
Complaint	-0.000343	-0.000181	0.00000670	-0.000165
	(0.000324)	(0.000338)	(0.000354)	(0.000348)
Mean of DV	0.00251	0.00251	0.00251	0.00251
Effect Size	-13.66	-7.208	0.267	-6.579

This table contains parameter estimates for the effect of the existence of special event overtime on performance. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. Standard errors clustered at the unit by shift level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Special Event Overtime Indicator and Performance, IV

	(1)	(2)	(3)	(4)
		Overtime On Shift		
	t-1	t-2	t-3	t-4
All	-0.0500***	-0.0262***	-0.0177*	-0.0153
	(0.0182)	(0.00781)	(0.00984)	(0.0123)
Mean of DV	0.182	0.182	0.182	0.182
Effect Size (% of Mean)	-27.44	-14.36	-9.707	-8.377
First Stage F	94.49	162.9	127.2	92.42
Felony	-0.00435	-0.00236	-0.000653	0.00911
	(0.00856)	(0.00528)	(0.00545)	(0.00694)
Mean of DV	0.0428	0.0428	0.0428	0.0428
Effect Size	-10.16	-5.515	-1.526	21.28
First Stage F	94.49	162.9	127.2	92.42
Misdemeanor	-0.0448***	-0.0231***	-0.0169*	-0.0239**
	(0.0138)	(0.00771)	(0.00946)	(0.0107)
Mean of DV	0.139	0.139	0.139	0.139
Effect Size	-32.19	-16.59	-12.11	-17.14
First Stage F	94.49	162.9	127.2	92.42
Drug	-0.0347***	-0.0342***	-0.0258***	-0.0177**
	(0.00899)	(0.00512)	(0.00748)	(0.00741)
Mean of DV	0.0379	0.0379	0.0379	0.0379
Effect Size	-91.54	-90.23	-68.06	-46.70
First Stage F	94.49	162.9	127.2	92.42
Complaint	0.000144	-0.00146*	-0.00119*	-0.000917
	(0.00103)	(0.000798)	(0.000692)	(0.000817)
Mean of DV	0.00251	0.00251	0.00251	0.00251
Effect Size	5.754	-58.11	-47.40	-36.53
First Stage F	94.49	162.9	127.2	92.42

This table contains parameter estimates for the effect of the existence of special event overtime on performance. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Special Event Overtime Indicator and Performance, IV: Day Shift Workers Only

	(1) t-1	(2) t-2	(3) t-3	(4) t-4
All	-0.0830**	-0.0471***	-0.0269	-0.0379
	(0.0380)	(0.0169)	(0.0188)	(0.0243)
Mean of DV	0.208	0.208	0.208	0.208
Effect Size (% of Mean)	-39.84	-22.60	-12.93	-18.17
Felony	-0.0147	0.00695	-0.0152*	0.0105
	(0.0207)	(0.0118)	(0.00882)	(0.0141)
Mean of DV	0.0484	0.0484	0.0484	0.0484
Effect Size	-30.35	14.37	-31.46	21.81
Misdemeanor	-0.0683**	-0.0540***	-0.0117	-0.0484**
	(0.0290)	(0.0189)	(0.0195)	(0.0202)
Mean of DV	0.160	0.160	0.160	0.160
Effect Size	-42.72	-33.77	-7.322	-30.26
Drug	-0.0663***	-0.0446***	-0.0478***	-0.0293*
	(0.0180)	(0.00689)	(0.0148)	(0.0156)
Mean of DV	0.0462	0.0462	0.0462	0.0462
Effect Size	-143.4	-96.44	-103.3	-63.36
Complaints Per Arrest	-0.000267	-0.00372**	-0.00200	-0.00518**
	(0.00387)	(0.00186)	(0.00239)	(0.00199)
Mean of DV	0.00257	0.00258	0.00257	0.00257
Effect Size	-10.37	-144.5	-77.78	-201.2
First Stage F	91.76	180.2	84.40	82.96

This table contains parameter estimates for the effect of the existence of special event overtime on performance of day shift officers. Day shift officers are those whose shifts start and end on the same calendar day. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Special Event Overtime Indicator and Performance, IV: Night Shift Workers Only

	(1)	(2)	(3)	(4)
	t-1	t-2	t-3	t-4
All	0.0111	0.0311	-0.00882	-0.0468
	(0.0233)	(0.0280)	(0.0302)	(0.0382)
Mean of DV	0.130	0.130	0.130	0.130
Effect Size (% of Mean)	8.549	23.97	-6.789	-36.00
Felony	0.00448	0.0179	0.0198	-0.0156
	(0.0180)	(0.0224)	(0.0375)	(0.0145)
Mean of DV	0.0302	0.0302	0.0302	0.0302
Effect Size	14.85	59.40	65.69	-51.81
Misdemeanor	0.00662	0.0132	-0.0286	-0.0311
	(0.0193)	(0.0269)	(0.0288)	(0.0326)
Mean of DV	0.0997	0.0997	0.0997	0.0997
Effect Size	6.644	13.24	-28.72	-31.22
Drug	-0.0166	-0.0147	0.0117	-0.00942
	(0.0142)	(0.0221)	(0.0224)	(0.0160)
Mean of DV	0.0152	0.0152	0.0152	0.0152
Effect Size	-109.3	-96.90	77.23	-61.95
Complaints Per Arrest	0.00160	-0.00145	-0.00124	-0.000571
	(0.00303)	(0.00194)	(0.00261)	(0.00138)
Mean of DV	0.00264	0.00264	0.00264	0.00264
Effect Size	60.68	-54.98	-46.96	-21.66
First Stage F	50.02	76.22	84.68	73.34

This table contains parameter estimates for the effect of the existence of special event overtime on performance of night shift officers. Night shift officers are those whose shifts start and end on the consecutive calendar days. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Special Event Overtime and Arrest Timing

	(1)	(2)	(3)	(4)
	Overtime on Shift			
	t-1	t-2	t-3	t-4
All	0.391*	-0.0916	0.0957	-0.144
	(0.204)	(0.0841)	(0.122)	(0.0935)
Mean of DV	4.474	4.474	4.474	4.474
Effect Size (% of Mean)	39.08	-9.158	9.571	-14.37
Felony	0.787	-0.178	-0.0637	-0.126
	(0.677)	(0.161)	(0.205)	(0.201)
Mean of DV	4.509	4.509	4.509	4.509
Effect Size	78.68	-17.85	-6.373	-12.62
Misdemeanor	0.256**	-0.0593	0.169	-0.167*
	(0.117)	(0.0727)	(0.142)	(0.0920)
Mean of DV	4.463	4.463	4.463	4.462
Effect Size	25.63	-5.933	16.92	-16.70
Drug	0.483	0.0127	0.0468	-0.243
	(0.519)	(0.114)	(0.132)	(0.209)
Mean of DV	4.400	4.400	4.400	4.400
Effect Size	48.31	1.269	4.682	-24.27
First Stage F	24.12	64.33	94.05	31.80

This table contains parameter estimates for the effect of the existence of special event overtime on arrest timing. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. A leave-out mean of portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

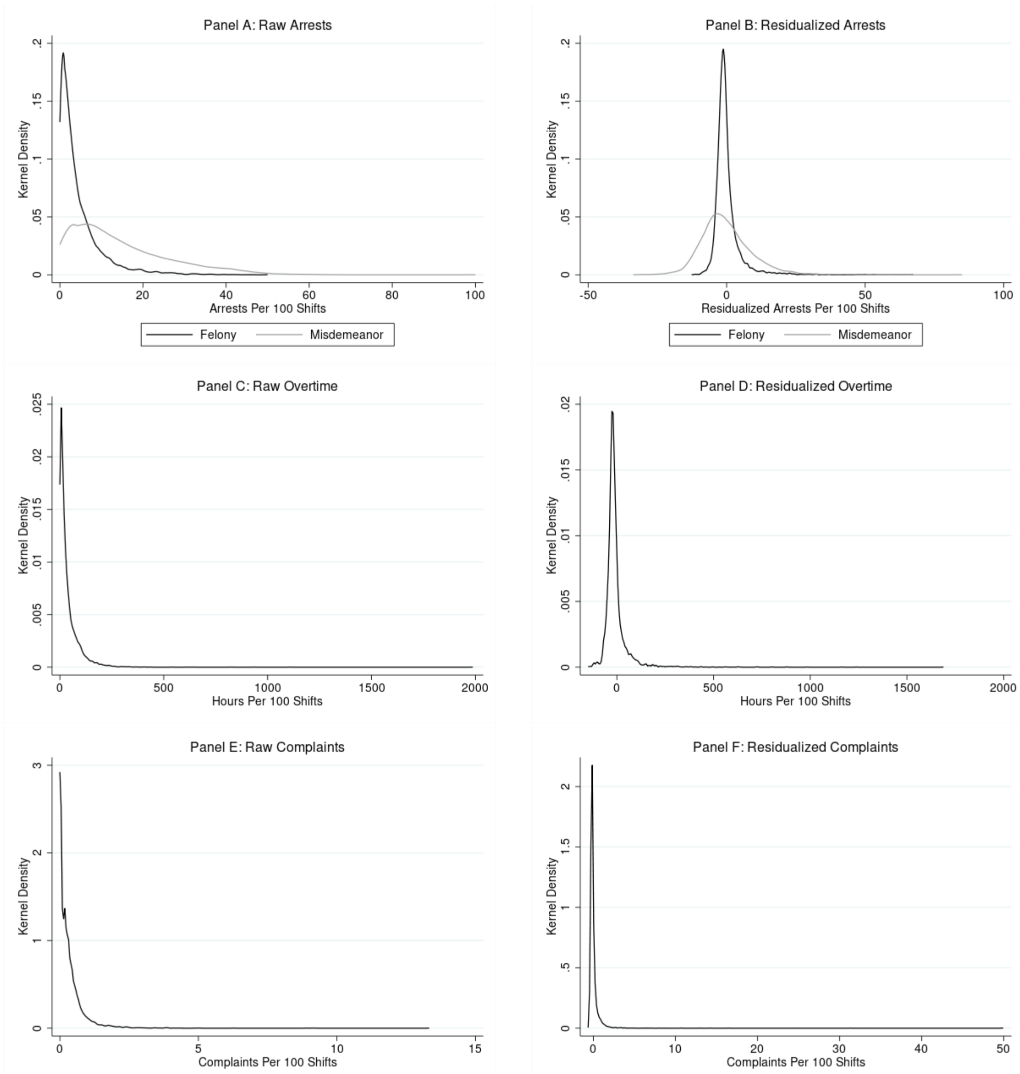
Table 15: Special Event Overtime Indicator and Performance, IV: Patrol Car Only

	(1)	(2)	(3)	(4)
	t-1	t-2	t-3	t-4
All	-0.0457**	-0.0223***	-0.0158	-0.0113
	(0.0191)	(0.00770)	(0.00991)	(0.0116)
Mean of DV	0.212	0.212	0.212	0.212
Effect Size (% of Mean)	-21.53	-10.49	-7.455	-5.309
First Stage F	88.10	154.3	121.7	78.74
Felony	-0.00628	-0.00173	-0.00378	0.0114
	(0.00949)	(0.00639)	(0.00478)	(0.00712)
Mean of DV	0.0498	0.0498	0.0498	0.0498
Effect Size	-12.61	-3.476	-7.583	22.97
First Stage F	88.10	154.3	121.7	78.74
Misdemeanor	-0.0390***	-0.0204**	-0.0120	-0.0225**
	(0.0141)	(0.00920)	(0.00956)	(0.0109)
Mean of DV	0.162	0.162	0.162	0.162
Effect Size	-24.01	-12.53	-7.384	-13.88
First Stage F	88.10	154.3	121.7	78.74
Drug	-0.0329***	-0.0307***	-0.0252***	-0.0148**
	(0.00792)	(0.00406)	(0.00664)	(0.00574)
Mean of DV	0.0447	0.0447	0.0447	0.0447
Effect Size	-73.53	-68.63	-56.45	-33.13
First Stage F	88.10	154.3	121.7	78.74
Complaints Per Arrest	0.000660	-0.00149*	-0.00102	-0.000918
	(0.00119)	(0.000865)	(0.000763)	(0.000876)
Mean of DV	0.00281	0.00281	0.00281	0.00281
Effect Size	23.51	-53.17	-36.26	-32.67
First Stage F	88.09	154.3	121.7	78.74

This table contains parameter estimates for the effect of the existence of special event overtime on performance for officers assigned to patrol cars only. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

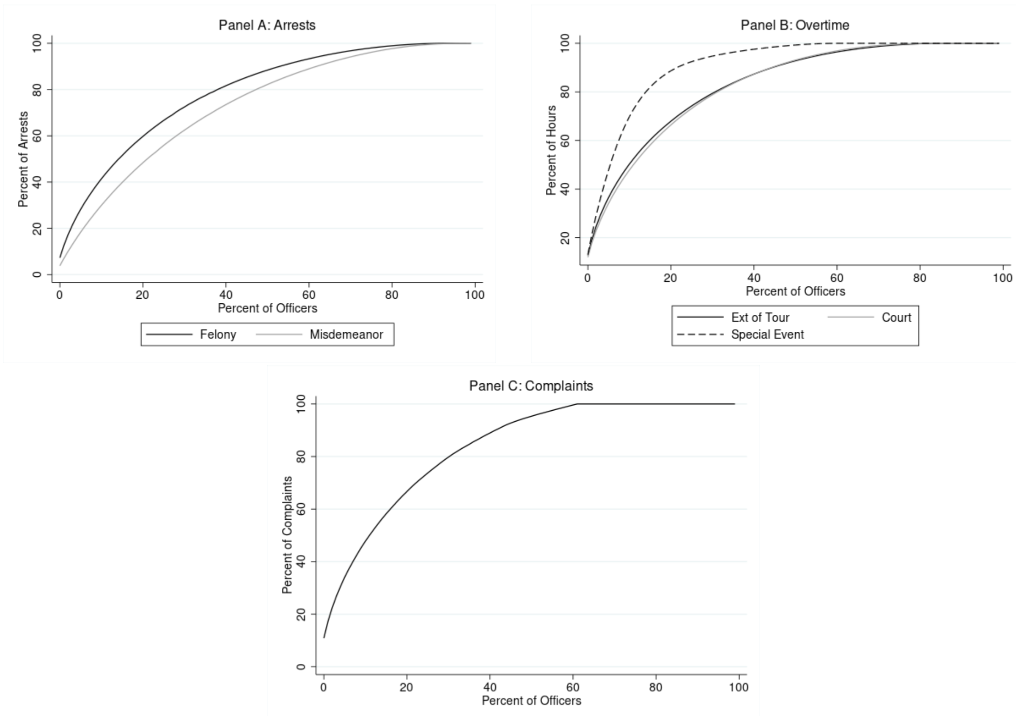
1.9. Figures

Figure 1: Distribution Across Officers



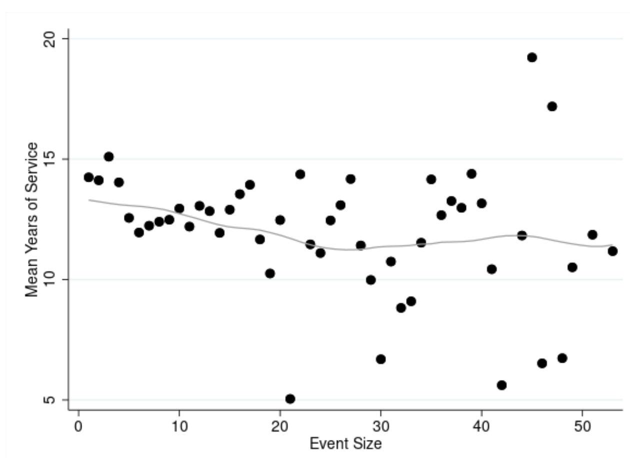
This figure displays kernel distributions of arrests, overtime, and complaints. All plots on the left are of the raw data; those on the right are residualized, removing district and shift effects.

Figure 2: Distribution Across Officers by Percent



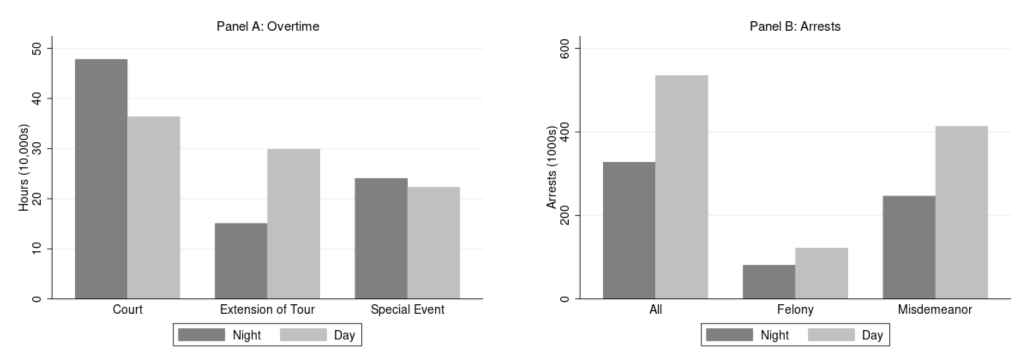
This figure displays distributions of arrests, overtime, and complaints across officers. The x-axis in each plot represents the percent of officers and the y-axis in each plot represents the percent of total arrests, overtime hours, or complaints.

Figure 3: Relationship Between Event Size and Experience



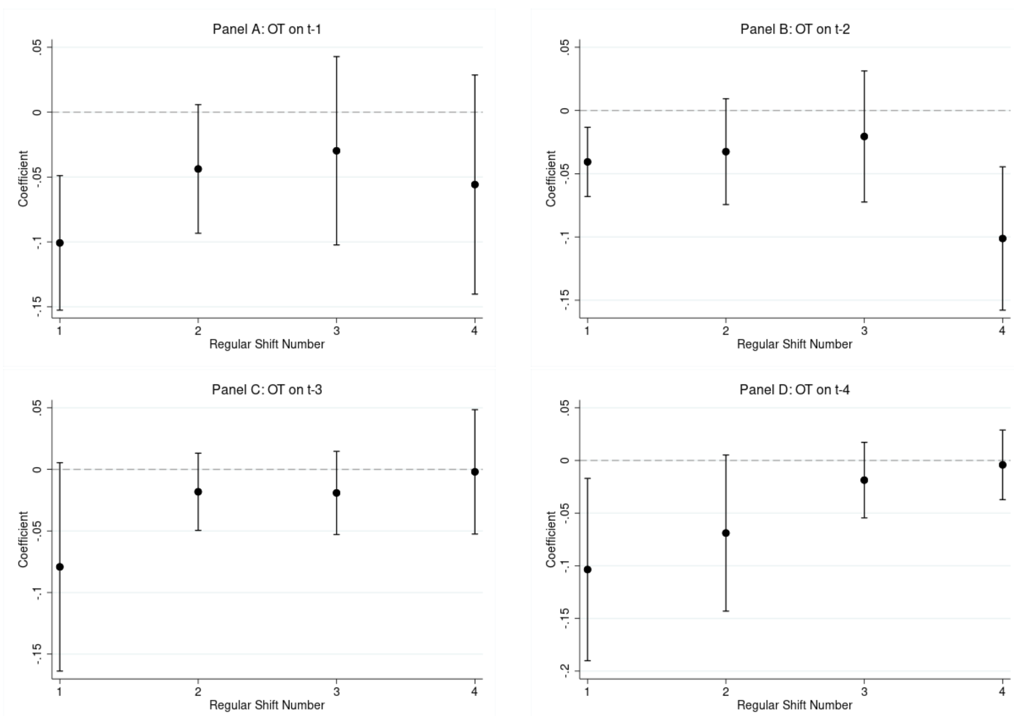
This figure displays average years of service for each event size in the data. A local polynomial regression is shown in gray.

Figure 4: Productivity by Shift Timing



This figure displays the distribution of overtime (Panel A) and arrests (Panel B) by shift timing. Day shifts are defined as those that start and end in the same calendar day whereas night shifts are defined as those that start on date X and end on date $X + 1$.

Figure 5: LATE Parameters by Timing of Overtime



This figure displays LATE parameter estimates for IV regressions of the effect of special event overtime on performance for each shift and overtime placement combination. Panel A shows estimates for β from (2) when overtime was one shift in the past for each regular shift option. Panels B, C, and D show the same information for overtime 2, 3, and 4 shifts in the past, respectively. 95% confidence intervals included.

1.10. Supplementary Tables

Table A1: Complaints: Findings

Final Finding	Complainant Type External No.	External %	Internal No.	Internal %	Total No.	Total %
Exonerated	432	3.91	34	2.79	466	3.80
No Affidavit	6245	56.54	94	7.72	6339	51.69
Not Sustained	1584	14.34	174	14.29	1758	14.34
Sustained	133	1.20	513	42.12	646	5.27
Unfounded	1050	9.51	111	9.11	1161	9.47
Unknown	1601	14.50	292	23.97	1893	15.44
Total	11,045	100.00	1218	100.00	12,263	100.00

Table A2: Complaints: Disciplinary Outcomes

Disciplinary Outcome	Complainant Type External No.	External %	Internal No.	Internal %	Total No.	Total %
Days of Leave, < 30	76	0.69	273	22.41	349	2.85
Not Served	2	0.02	4	0.33	6	0.05
Reinstated by Board	0	0.00	1	0.08	1	0.01
Reprimand	22	0.20	149	12.23	171	1.39
Resigned	3	0.03	10	0.82	13	0.11
Separation	0	0.00	4	0.33	4	0.03
Unsustained	9309	84.28	413	33.91	9722	79.28
Unknown	1601	14.50	291	23.89	1892	15.43
Suspended Over 30 Days	0	0.00	2	0.16	2	0.02
Violation Noted	32	0.29	71	5.83	103	0.84
Total	11,045	100.00	1218	100.00	12,263	100.00

Table A3: Special Event Overtime Hours and Performance, OLS

	(1)	(2)	(3)	(4)
	Overtime On Shift			
	t-1	t-2	t-3	t-4
All	-0.00220***	-0.000288	0.000835	0.00127***
	(0.000576)	(0.000402)	(0.000510)	(0.000423)
Mean of DV	0.182	0.182	0.182	0.182
Effect Size (% of Mean)	-1.208	-0.158	0.458	0.699
Felony	-0.000287	-0.0000964	0.000541*	0.000397
	(0.000270)	(0.000292)	(0.000292)	(0.000312)
Mean of DV	0.0428	0.0428	0.0428	0.0428
Effect Size	-0.672	-0.225	1.264	0.929
Misdemeanor	-0.00191***	-0.000192	0.000294	0.000875*
	(0.000494)	(0.000411)	(0.000388)	(0.000447)
Mean of DV	0.139	0.139	0.139	0.139
Effect Size	-1.373	-0.138	0.211	0.628
Drug	-0.00119***	-0.000654**	-0.000619*	-0.000466
	(0.000252)	(0.000294)	(0.000370)	(0.000337)
Mean of DV	0.0379	0.0379	0.0379	0.0379
Effect Size	-3.130	-1.725	-1.633	-1.228
Complaint	-0.0000388	-0.00000704	-0.00000672	-0.00000745
	(0.0000482)	(0.0000339)	(0.0000369)	(0.0000360)
Mean of DV	0.00251	0.00251	0.00251	0.00251
Effect Size	-1.545	-0.280	-0.268	-0.297

This table contains parameter estimates for the effect of special event overtime hours on performance. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. Standard errors clustered at the unit by shift level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Special Event Overtime Hours and Performance, IV

	(1)	(2)	(3)	(4)
		Overtime On Shift		
	t-1	t-2	t-3	t-4
All	-0.00572**	-0.00197**	-0.00123	-0.00150
	(0.00240)	(0.000808)	(0.000928)	(0.00109)
Mean of DV	0.182	0.182	0.182	0.182
Effect Size (% of Mean)	-3.142	-1.081	-0.675	-0.825
First Stage F	72.82	89.00	69.79	49.23
Felony	-0.000846	-0.000230	0.0000639	0.000991
	(0.00110)	(0.000498)	(0.000541)	(0.000683)
Mean of DV	0.0428	0.0428	0.0428	0.0428
Effect Size	-1.979	-0.538	0.149	2.317
First Stage F	72.81	89.00	69.80	49.23
Misdemeanor	-0.00479***	-0.00169**	-0.00128	-0.00244**
	(0.00174)	(0.000722)	(0.000873)	(0.000942)
Mean of DV	0.139	0.139	0.139	0.139
Effect Size	-3.439	-1.214	-0.922	-1.750
First Stage F	72.82	89.00	69.79	49.23
Drug	-0.00487***	-0.00339***	-0.00238***	-0.00189**
	(0.00118)	(0.000499)	(0.000730)	(0.000733)
Mean of DV	0.0379	0.0379	0.0379	0.0379
Effect Size	-12.84	-8.939	-6.280	-4.991
First Stage F	72.81	89.00	69.80	49.22
Complaint	0.0000340	-0.000110	-0.000107*	-0.0000484
	(0.000123)	(0.0000725)	(0.0000559)	(0.0000670)
Mean of DV	0.00251	0.00251	0.00251	0.00251
Effect Size	1.355	-4.390	-4.250	-1.930
First Stage F	72.82	89.00	69.79	49.22

This table contains parameter estimates for the effect of special event overtime hours on performance. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of special event overtime hours at the unit by shift by date level instruments for special event overtime hours. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Special Event Overtime Hours and Performance, IV: Other Arrest Types

	(1)	(2)	(3)	(4)
	Overtime on Shift			
	t-1	t-2	t-3	t-4
Violent	-0.00764	0.00744	0.00457	0.000152
	(0.00886)	(0.00561)	(0.00587)	(0.00423)
Mean of DV	0.0362	0.0362	0.0363	0.0363
Effect Size	-21.08	20.53	12.62	0.419
Assault	-0.00650	0.00650	0.00509	0.00290
	(0.00763)	(0.00479)	(0.00478)	(0.00416)
Mean of DV	0.0313	0.0313	0.0314	0.0314
Effect Size	-20.74	20.75	16.23	9.240
Property	-0.00787	-0.00182	0.000577	0.00384
	(0.00650)	(0.00639)	(0.00430)	(0.00498)
Mean of DV	0.0271	0.0271	0.0271	0.0271
Effect Size (% of Mean)	-28.98	-6.704	2.125	14.15
Theft	-0.00604*	-0.00169	-0.00106	0.00237
	(0.00331)	(0.00431)	(0.00279)	(0.00323)
Mean of DV	0.0146	0.0146	0.0146	0.0146
Effect Size	-41.28	-11.57	-7.217	16.21
Public Nuisance	-0.00740	-0.0135***	0.00143	-0.00807
	(0.00883)	(0.00467)	(0.00748)	(0.00621)
Mean of DV	0.0164	0.0164	0.0164	0.0164
Effect Size	-45.22	-82.34	8.726	-49.24
Warrant	0.00311	0.00714*	0.0126**	0.0138**
	(0.00682)	(0.00373)	(0.00545)	(0.00592)
Mean of DV	0.0206	0.0206	0.0206	0.0206
Effect Size	15.09	34.66	61.22	67.15
Weapon	0.0162***	0.0119***	0.00274	0.00389
	(0.00546)	(0.00427)	(0.00457)	(0.00429)
Mean of DV	0.00758	0.00758	0.00758	0.00759
Effect Size	214.2	156.9	36.11	51.28
Other	-0.0100	0.00301	-0.00396	-0.00788
	(0.00767)	(0.00498)	(0.00581)	(0.00523)
Mean of DV	0.0200	0.0200	0.0200	0.0200
Effect Size	-50.18	15.04	-19.77	-39.36
First Stage F	94.49	162.9	127.2	92.42

This table contains parameter estimates for the effect of the existence of special event overtime on performance. All regressions include unit by quarter, day of week, shift, and officer fixed effects and linear controls for index and non-index reported crime. A leave-out mean portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Special Event Overtime Indicator and Performance, IV Specification 2

	(1)	(2)	(3)	(4)
	Overtime On Shift			
	t-1	t-2	t-3	t-4
All	-0.0498*	-0.0253*	-0.0192	-0.0120
	(0.0271)	(0.0138)	(0.0152)	(0.0156)
Mean of DV	0.182	0.182	0.182	0.182
Effect Size (% of Mean)	-27.37	-13.87	-10.53	-6.608
First Stage F	94.07	164.0	127.7	92.41
Felony	-0.00364	0.00346	-0.00887	0.0103
	(0.0128)	(0.00953)	(0.00788)	(0.0105)
Mean of DV	0.0428	0.0428	0.0428	0.0428
Effect Size	-8.518	8.097	-20.74	24.12
First Stage F	94.07	164.0	127.6	92.43
Misdemeanor	-0.0462**	-0.0287**	-0.0103	-0.0224
	(0.0211)	(0.0138)	(0.0144)	(0.0161)
Mean of DV	0.139	0.139	0.139	0.139
Effect Size	-33.19	-20.62	-7.409	-16.09
First Stage F	94.02	163.9	127.6	92.39
Drug	-0.0537***	-0.0430***	-0.0349***	-0.0197**
	(0.0118)	(0.00661)	(0.0114)	(0.00833)
Mean of DV	0.0380	0.0380	0.0380	0.0380
Effect Size	-141.5	-113.4	-91.87	-51.97
First Stage F	93.98	163.9	127.6	92.41
Complaint	0.000100	-0.00256**	-0.00175	-0.00177
	(0.00159)	(0.00126)	(0.00127)	(0.00134)
Mean of DV	0.00251	0.00251	0.00251	0.00251
Effect Size	4.002	-101.8	-69.63	-70.47
First Stage F	93.92	163.9	127.6	92.42

This table contains parameter estimates for the effect of the existence of special event overtime on performance. All regressions include unit by quarter, unit by shift, and day of week fixed effects and linear controls for index, non-index reported crime, and an officer-level leave-out mean of the dependent variable in lieu of officer fixed-effects. All regressions with complaints as the outcome also include a control for arrests. A leave-out mean of portion of officers working overtime at the unit by shift by date level instruments for existence of overtime. Standard errors clustered at the unit by shift level in parentheses.

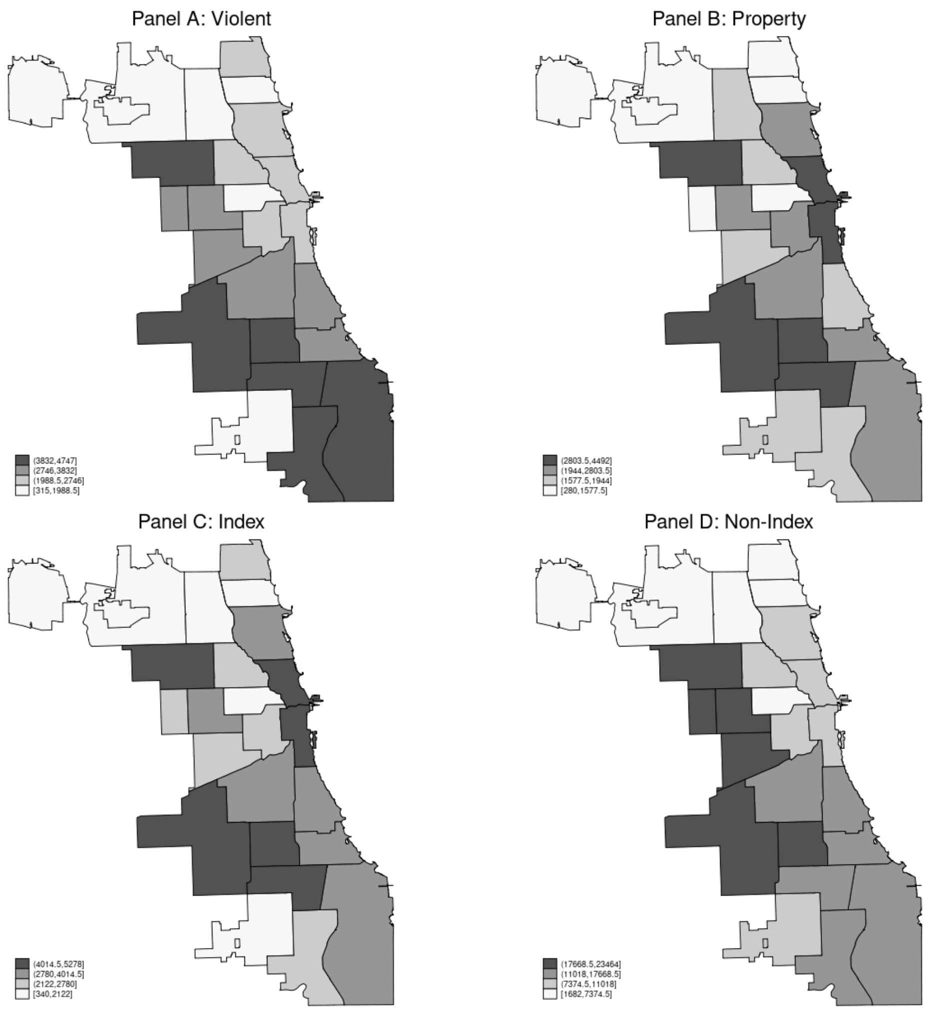
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.11. Supplementary Figures

Figure B1: Distribution of Crime Across Districts

This figure displays maps of the distribution of crime across districts in Chicago. Regions shaded darker have a higher concentration t of crime. Panel A shows violent crime; Panel B shows property crime; Panel C shows index crime; and Panel D shows non-index crime.

Figure B2: Distribution of Arrests Across Districts



This figure displays maps of the distribution of arrests across districts in Chicago. Regions shaded darker have a higher concentration of arrests. Panel A shows arrests for violent crime; Panel B shows arrests for property crime; Panel C shows arrests for index crime; and Panel D shows arrests for non-index crime.

Figure B3: Sample Special Event Evaluation Report (CPD Form 11.466)

SPECIAL EVENT EVALUATION REPORT / CHICAGO POLICE DEPARTMENT

PREPARE IN DUPLICATE

NAME OF EVENT	LOCATION	DISTRICT
9th Ward Back to School Parade	12400 S Michigan Ave to 301 E 111th St	005

DATE & TIME OF EVENT	TIME OF POLICE DETAIL	TOTAL
18 Aug 2012 FROM 0900 HRS TO 1730 HRS	[redacted]	

EVENT SPONSOR (NAME, ADDRESS & PHONE NO.)

9th Aldermanic Ward Office, 32 E 112th PL, [redacted]

SPONSOR'S REPRESENTATIVE (NAME, ADDRESS & PHONE NO.)

[redacted]

PERMIT NO.	EXPECTED ATTENDANCE	POLICE ESTIMATE
unk	over 1000	over 1000

GROUPS INVOLVED	GROUPS PREDOMINANTLY
9th Ward Office, CPS, CTA, Local Schools/Church Organizations	☒ ADULT ☐ MINOR

TOTAL NO. OF POLICE	DEPUTY CHIEFS	CAPTAINS	LIEUTENANTS	SERGEANTS	FOOT POLICE OFFICERS
[redacted]	0	0	1	[redacted]	

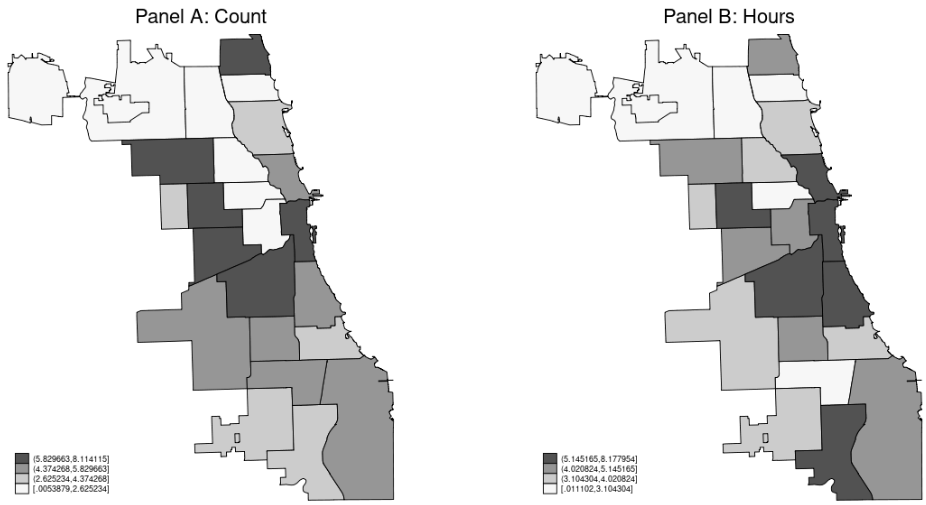
3/W M.C.	SQUADS	SQUADROLS	CIV. DRESS	YOUTH OFF.	OTHER
0	[redacted]	0	[redacted]		All In Uniform

TOTAL MAN HOURS	WEATHER CONDITIONS	ARRANGEMENTS SATISFACTORY?
[redacted]	☐ RAIN ☐ SNOW ☒ CLEAR ☐ CLOUDY ☐ WARM ☐ COLD ☐ (OTHER)	[redacted]

EVALUATE THE COVERAGE OF THIS EVENT, AND GIVE RECOMMENDATIONS, IF ANY.

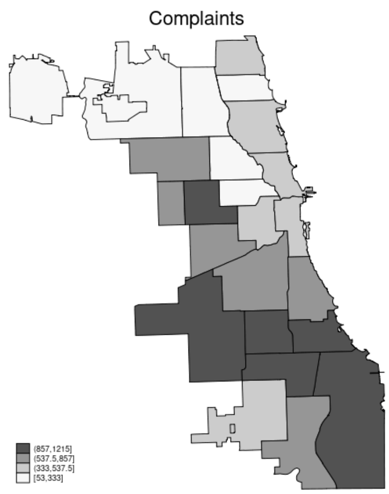
[redacted]

Figure B4: Distribution of Special Event Overtime Across Districts



This figure displays maps of the distribution of special event overtime across districts in Chicago. Regions shaded darker have a higher concentration of overtime. Panel A shows the count of special event overtime worked and Panel B shows the number of hours of special event overtime worked.

Figure B5: Distribution of Complaints Across Districts



This figure displays maps of the distribution of complaints across districts in Chicago. Regions shaded darker have a higher concentration of complaints.

CHAPTER 2 : Complaints and Performance: Evidence from Complaints Against Chicago Police Officers

2.1. Introduction

On a daily basis, police officers must use discretion in interactions with civilians in an attempt to protect and serve. They seek to strike the right balance between force and restraint when faced with threatening environments. Officers are frequently forced to make quick, critical decisions with very little notice or information, weighing their own personal safety with that of the individuals whom they patrol. Between 2011 and 2013, the average law enforcement training program for officers in the United States was about 840 hours or 21 weeks, an average of only 21 hours of which focused exclusively on use of force and de-escalation (Reaves, 2016).

In addition to lessons on use of force and de-escalation, police training academies include a discussion on the potential consequences of use of insufficient force or no force at all. A widely used example of these consequences is the murder of Deputy Kyle Dinkheller in Georgia in 1998. Officers in training are often shown the three and one half minute dash cam video of the traffic stop in which Deputy Dinkheller was eventually shot to death by a mentally ill veteran armed with a rifle. New recruits are frequently led to believe that Deputy Dinkheller was recently disciplined for misconduct and that said discipline may have made him hesitate before using lethal force.[1] In some training programs, recruits even complete a simulation of the Dinkheller stop in which they are given the opportunity to use lethal force against the veteran before he shoots his rifle (CalibrePress, Copyright 2020). After training academy that often includes examples such as Dinkheler's, officers enter field training where they transition into applying the skills they learn in the academy

[1] This is not actually the case. Months before his death, Deputy Dinkheller was forced to write an apology letter to an important community member who complained to the sheriff after Dinkheller "spoke harshly" to him when he failed to yield as Dinkheller approached the scene of a traffic accident. Still, Dinkheller's colleagues were "convinced it was somewhere in the back of his mind during his final traffic stop" (Lake, 2017).

to real-stakes interactions with civilians.

Many of these interactions lead to seeming abuses of power by the police. The first highly publicized interaction of this sort was assault of Rodney King in Los Angeles in 1992, which resulted in large-scale protests and riots. More recently, the rise of cell phone cameras in the US has been coupled with an increase in accounts of situations in which police officers seem to abuse power while on the job. The murders of numerous Black Americans have sparked the Black Lives Matter movement which campaigns against, among other things, inappropriate policing practices such as police brutality and racially motivated policing.

The increased attention on the negative consequences of policing has led to a heightened need for an understanding of the inner workings of police departments, in particular, of how officer behavior responds to monitoring by superiors. While there is a large literature on how monitoring or performance evaluations affect employee behavior in other fields,[2] there is little empirical research on the monitoring of police officer performance, despite the large effect of police on society as a whole. The existing research on the internal workings of police departments largely focuses on the effects of bias on various outcomes including award nominations (Ba et al., 2020, 2019) and the racial composition of arrests (Bulman, 2019). One study by Rivera and Ba (2019) finds that internal accountability via self- and public-monitoring improves community relations without increasing crime. Additionally, Dharmapala et al. (2018) find that adoption of union bargaining agreements by sheriffs' offices in Florida is associated with increases in violent misconduct.

In most police departments, employees and civilians are able to file complaints of misconduct against police officers if they believe to have been the victim of or witness to officer behavior that is inappropriate. These complaints are a potentially useful tool for managing officers as they give superiors the opportunity to see officer behavior through another set of eyes. In this sense, they mitigate the principal agent problem between managers and officers as it is not possible for sergeants, lieutenants, or captains to observe the behavior of patrol

[2]For example, see Taylor and Tyler (2012), Rockoff and Speroni (2010), and Kluger and DeNisi (1996).

officers at most times. Despite the potential utility of these complaints, very few are fully investigated by the CPD (Ba, 2018). While much research has been done on complaints and discipline (for example, Kane and White (2009) and Ba (2018)) and the predictive power of current complaints on future misconduct (Rozema and Schanzenbach, 2019; Harris and Worden, 2014), no research to date has explored the ways in which complaints might have a causal effect on police officer performance.

Using detailed administrative data from the Chicago Police Department, I study the extent to which complaint investigations affect police officer behavior. I leverage the conditionally random timing of complaint submissions. Using a difference-in-differences design that compares officer behavior for complaints with and without investigatory interviews before and after complaint submission, my analysis finds little to no evidence that complaints with investigations have a differential impact on officer behavior than those without investigations. The evidence suggests that there are not significant differences in the nature of complaints with and without investigation. Changes in assignment or relegation to desk duty do not appear to be confounding results. Taken as a whole, this provides an opportunity for future research to study the causal effect of a complaint (regardless of investigation) on officer performance.

2.2. Background

Comprised of roughly 12,000 sworn employees and 2,000 other employees, the Chicago Police Department (CPD) is the second largest municipal police department in the United States and serves the third largest city in the United States. The Bureau of Patrol breaks the city into 22 geographic units or districts for the purposes of policing. The crime rate in Chicago is significantly higher than the US average for both large cities and municipalities of any size. According to the FBI's Uniform Crime Reports, Chicago ranked number 17 in violent crime and number 10 in murder and non-negligent manslaughter per 100,000 people in 2017. Crime is most concentrated in the city's south and west sides.

2.2.1. Complaints

Both private citizens and employees of the CPD are able to file a complaint for misconduct against an employee of the department. Complaints can be filed anonymously; however, filing without personally identifiable information and a signed affidavit dramatically reduces the likelihood that a complaint will be investigated (Ba, 2018; Dep, 2017). According to the CPD, formal investigations are completed "in all cases where an allegation of misconduct, if proven true, would constitute a violation of the departments conduct rules" (Chi, 2017). However, complaint investigators are only required to attempt to make contact with complainants three times if a complaint is submitted without a signed affidavit[3] (Chi, 2017).

If a complaint warrants an investigation, either the Bureau of Internal Affairs (BIA) at the CPD or the Independent Police Review Authority[4] (IPRA) is responsible for said investigation. The IPRA investigates allegations of use of force, coercion, domestic violence, and bias-based verbal abuse whereas the BIA handles all other complaints[5] (Chi, 2017). Per the Fraternal Order of Police Chicago Log No. 7 Collective Bargaining Agreement (CBA), disclosure of the complainant(s)'s name(s) and nature of the allegations is mandated prior to the questioning of an accused officer. The officer under investigation will also be informed of "the identities of the person in charge of the investigation, the designated primary interrogation officer, the designated secondary interrogation officer, if any, and all persons present during the interrogation and shall be advised whether the interrogation will be audio recorded." (Fra, 2012). According to the Department of Justice's (DOJ) report on the CPD, accused officers are provided with "detailed notice of the misconduct charges as well as copies of all relevant police records" that "allow the accused officer to sufficiently prepare before being questioned." (Dep, 2017)

[3]If no contact has been made and the investigator believes the complaint warrants investigation, he or she can submit an affidavit override request to the head of the agency investigating the complaint (Dep, 2017).

[4]The IPRA was replaced by the Civilian Office of Police Accountability in 2017 (Dep, 2017).

[5]The BIA itself investigates complaints related to criminal misconduct, corruption, substance abuse, and driving under the influence. All other complaints are delegated to the accused officer's assigned district for investigation ((Chi, 2017).

When criminal prosecution is not being sought against a department member, the Administrative Proceedings Rights (Statutory) form is signed electronically during the interview. This electronic acknowledgement has the full effect as that of the member's signature. When criminal prosecution is being sought against a department member, the officer in question completes both a criminal rights form and a Notification of Charges/Allegations form[6] (Chi, 2017). As described in the Data section, there is much variation in the length of complaint investigations as well as the time between complaint filing and associated investigation. These delays are often unpopular with officers as well; the DOJ report mentions that "accused officers who have not engaged in misconduct are burdened with the scrutiny of being under investigation, and may be stuck doing desk duty for years while investigations languish" (Dep, 2017)

2.3. Data

This research seeks to understand if and how investigated complaints affect the performance of police officers. Because there is no publicly available data on police officer schedules linked with complaints and performance measures, I construct a unique dataset on police officer work histories and performance outcomes using administrative data from Chicago that spans the years 2012-2015. The data were gathered over three years of public records requests and linked across officers using the officer's full name. Because of this, any names that belong to more than one officer in a given year are dropped from the data.[7]

The data cover roughly 8500 officers from each of Chicago's 25 police districts.[8] Officers in these districts police within a fixed geographic boundary and are assigned to one of four shifts. Shifts 1-3 are fixed in time and shift 4 has a start time that fluctuates based on the needs of the city.[9] Attendance data for each officer is linked to arrest, complaint, and

[6] A FOIA request for dates of these electronic signatures returned "no records."

[7] Roughly 3% of officers have the same name in each of the four years of the data.

[8] In 2012, districts 13, 21, and 23 closed and were absorbed into existing districts. Indicator variables will be used in all models to control for changes caused by district closures.

[9] For example, shift 4 may start at 5pm in December but at 7pm in August due to crime's relationship with weather and ambient lighting.

injury on duty data [10] to construct a dataset that gives day-level information on police officer performance. Table 7 contains descriptive statistics at the officer level based on complaint and interview status. Differences across each category are significant at the 1% level except for those marked with a †. These two comparisons are significantly different at only the 10% level. There are clear differences in characteristics of all officers, those with at least one complaint, and those with at least one complaint-related interview. As seen in the Table, officers who are interviewed related to a complaint at least one time are significantly more male, have served on the force longer, are on average more productive, and are injured more often than all officers and those with at least one complaint.

Not only do the type of officer who completes a complaint investigation differ from the average, characteristics about complaints based on interview status also show clear differences. Table 8 shows complaint categories by interview status. As is clear in the Table, there are significant differences in the composition of complaints based on interview status. For example, use of force complains make up 17% of no interview complaints and only 11.7% of interview complaints. Table 9 contains investigation findings by interview status and also shows clear differences across status. Most notable is the fact that 0% of complaints with associated interviews are labeled as "No Affidavit" whereas 55% of complaints without interviews are given this finding. Because complaints with associated interviews are actually investigated, we see higher likelihoods of the following categories: Not Sustained,[11] Sustained,[12] or Unfounded.[13] Finally, Table 10 contains information on disciplinary outcomes by interview status. Unsurprisingly, officers are significantly more likely to be disciplined for complaints with associated interviews.

[10]Injuries are measured via the date at which an officer enters "injury on duty" status. If he or she is working that day, he or she is recorded injured on that day. Otherwise the injury is recorded as occurring on his or her most recent working shift.

[11]Complaints that are not sustained are "not supported by sufficient evidence which could be used to prove or disprove the allegation" (Chi, 2017).

[12]Complaints that are sustained are "supported by sufficient evidence to justify disciplinary action" (Chi, 2017).

[13]Complaints that are unfounded are "not based on facts as shown by the investigation, or the reported incident did not occur" (Chi, 2017).

2.4. Empirical Strategy

2.4.1. Identification

For my estimates of the effect of complaints on officer performance to be considered causal, it must be the case that the timing of complaint logging is random conditional on included controls. This paper exploits the fact that it is highly unlikely that a police officer receives a complaint *each* time he makes an action that *could* result in a complaint. Rather, it is likely the case that, conditional on some set of controls, the precise timing of a complaint is random. In this section I now address several concerns which potentially threaten this identifying assumption.

A primary concern of this research design is that my outcome measures could be correlated with characteristics of the officers or environments that they face. For example, one might expect arrests and complaints are correlated even if the relationship is not causal because both arrests and complaints require civilian interactions. To address this concern, I condition on officer fixed effects which remove all officer-invariant characteristics that may appear to drive the relationship between complaints and my outcome measures. Another officer-specific concern is that behavior may change with experience in a manner that is unique to each individual officer. For example, some officers may make fewer arrests as they gain more experience. If one of these officers receives a complaint, he or she may appear to reduce productivity in response to the complaint when in reality the effect is due to experience. Because of this, I study outcomes in a narrow window of one to four months around interviews. In addition, I include complaint type, unit by quarter,[14] unit by shift, and shift by day of week fixed effects. Complaint type fixed effects remove any responses to complaints that are common to a specific complaint type. Unit by quarter, unit by shift, and shift by day of week fixed effects control for the fact that crime can vary by location throughout time (historical time or time of day) and by time of day differentially by day of

[14] Recall that, in 2012, districts 13, 21, and 23 closed and were absorbed into existing districts. Unit by quarter fixed effects control for changes caused by each of these closures.

week. Finally, linear controls for reported index and non-index crimes are included in all models to remove effects of calendar-day specific crime changes.

In order to support my claim that the timing of complaints and their officer interview dates are conditionally random, I conduct a test for correlation between complaint timing and officer characteristics. This test takes the form

$$COMP_{aicst} = \sum_j \beta_j DEMO_{it}^j + X_{acst}\Gamma + \epsilon_{aisct} \tag{2.1}$$

where $COMP_{aicst}$ in (2.1) is the complaint filing date for complaint a logged against officer i in unit c assigned to shift s. $DEMO_{it}^j$ contains demographic information on officer i at date t for j demographic characteristics.[15] X_{cst} is a matrix containing unit by quarter, unit by shift, shift by day of week, and complaint type fixed effects. Table 11 contains results from the test. As is clear in the table, there is no evidence of a relationship between officer demographics and complaint timing. All individual parameters and the joint F-test are insignificant at conventional levels.

A second concern is that, if complaints with associated interviews are very different from those without interviews, officer behavior prior to the submission of a complaint may differ in both levels and trends based on complaint type. Differences in trends in behavior prior to complaint submission amounts to a violation of the parallel trends assumption. It is logically possible if it were the case that officer behavior prior to a complaint-incident escalates in the case of complaints with interviews but not in the case of complaints without interviews. The parallel trends assumption requires that behavior of officers related to complaints with and without interviews would follow similar trends in the absence of complaint submission. Table 12 tests whether arrests, complaints, or injuries differ for officer-complaints with and without interviews in the pre-period. There is modest evidence that treated officers make fewer overall in the pre-period when it is defined as one or four months long. However, as

[15] The t subscript is only relevant when the demographic information is the officer's age or experience as this changes with time.

long as the differences are stable over time in the pre-period, there is no violation in parallel trends and my difference-in-differences will be unbiased and correctly identified.

The event study in Figure 6 provides a check for parallel trends. A violation of parallel trends would manifest as a nonzero slope in the months prior to complaint submission, while an exogeneity violation would show an uptick (or downtick) in the point estimate in the month or two prior to complaint submission. The graphs plot the point estimates from (2.2) for four months pre- and post-complaint submission.

$$Y_{icst} = \alpha + \sum_{k=-4}^{4} \beta_k TREATED_{ai} \times RelMonth_k + X_{icst}\Gamma + \epsilon_{icst} \tag{2.2}$$

Panels A, B, C, D, and E plot β from (2.2) when the outcome variable is all arrests, felony arrests, misdemeanor arrests, complaints per arrest, and injuries per arrest, respectively. As is clear in the figure, there only evidence of a violation of parallel trends is in panels (b) and (c) where β_{-4} is significantly different from 0.

A final concern is that an officer may no be aware of the complaint logged against him on the exact date which the complaint is submitted. My data allow me to identify the dates upon which a complaint is received, interviews are conducted, and the corresponding investigation is closed. None of these dates perfectly identify the date on which an officer becomes aware of a complaint logged against him. Because of this, my results may be compromised by measurement error in the treatment variable. In order to best estimate the difference-in-differences parameter given this potential mis-measurement, I include controls for the weeks around an incident and complaint submission date.

In each model, standard errors are clustered at the unit by shift level in order to account for spatial autocorrelation amongst officers in the same working group[16] These working groups contain 66 officers on average. To the extent that serial correlation exists not only within observations for a given police officer but also amongst officers within the working group,

[16]Officers who work the same shift in the same unit face a similar environment and interact with each other more frequently than do officers in some other grouping.

clustering standard errors at the higher level of aggregation accounts for this feature of the data (Bertrand et al. (2004), Bester et al. (2011)).

2.4.2. Empirical Models

To study the effect of *investigated* complaints of misconduct on police officer performance, I compare productivity outcomes in a one to four month window around complaint logging. To the extent that fewer than one to four months of information are available in the pre- or post- period, I include the maximum number of observations possible. For example, imagine that the sample period ends when an officer is 1.5 months into his post period. For this officer, and all of those in similar situations, 1.5 months will comprise his post period for two to four month bandwidth settings. I study arrests, complaints, and injuries at the officer-by-shift level using the following difference-in-differences model:

$$Y_{icst} = \alpha + \beta_0 TREATED_{ia} \times POST_{iat} + \beta_1 TREATED_{ia} + \beta_2 POST_{iat} + X_{icst}\Gamma + \epsilon_{icst} \quad (2.3)$$

where Y_{icst} in (2.3) is the count of arrests, complaints, or injuries made by officer i in unit c working shift s that begins on date t. $TREATED_{ia}$ is an indicator variable that equals 1 if complaint a associated with officer i was investigated (as indicated by an interview) and 0 otherwise. $POST_{iat}$ is an indicator variable that is 1 if date t is $after$ complaint a was logged against officer i and 0 otherwise. X_{icst} is a matrix containing officer, complaint type, unit by quarter, unit by shift, and shift by day of week fixed effects, linear controls for reported index and non-index crimes, and indicator variables for the week around complaint incidents, submissions, or interviews if they appear in the bandwidth. In all main specifications, only shifts during which an officer is present and assigned to patrol car[17] are included in analysis to account for the high likelihood officers are assigned to desk duty during some portions of the window between complaint submission and investigation closure.

[17]Attendance data contain information on patrol cars but no information specific to foot or bike beat assignments.

2.5. Results

Parameter estimates for β in (2.3) where the post-period is determined by complaint submission date are in Table 13. Investigated complaints, or those with interviews, have no differential effect on police officer performance when compared to complaints without investigation. There is minor evidence of a decrease in felony arrests and an increase in misdemeanor arrests in the 4-month bandwidth. These results are consistent with the evidence in Figure 6. Further, Tables 14 and 15 contain estimates for β in (2.3) for internal and external complaints, respectively. Table 14 shows some evidence of a decrease in injuries per arrest after investigated complaints are submitted whereas Table 15 shows only a significant differential effect of investigated complaints on future complaints, with officers who receive investigated complaints earning 15-30% fewer future complaints based on bandwidth. Finally, I run (2.3) separately for officers based on experience. Tables 16 and 17 contain parameter estimates for officers below and above mean experience, respectively. Table 16 shows significant decreases in felony arrests in the one and four month bandwidth specifications. While changes in misdemeanor arrests are significant, the effect sizes are quite small in magnitude. Table 17 shows no significant effect of complaints with investigations on performance of officers with above mean experience.Tables 18 and 19 contain results for internal complaints separately by experience and show no significant effect of investigated complaints on performance. Tables 20 and 21 contain results for external complaints separately by experience. These tables show no evidence of a relationship between investigated complaints and performance except that below mean experienced officers with investigated complaints appear to decrease future complaints by roughly 30% one and two months after complaint submission.

2.5.1. Threats to Validity

Given that some officers are relegated to desk duty during the complaint investigation process, one might be concerned that changes in assignment may be driving results in some way. Although my main results condition on assignment to a patrol car, it could be the case that the type of officer who is removed from active duty is different from the type of officer who is not and that these differences may be causing the lack of perceived changes in performance for officers who receive complaints that are investigated. To address this concern, I test for changes in assignment after complaint log dates. More specifically, I run (2.3) where the outcome variable is an indicator for being present or an indicator for being assigned to a patrol car conditional on being present for duty. As is clear in the table, there is no evidence of investigated complaints causing a differential change in the likelihood an officer is present. There is some evidence that officers with investigated complaints are less likely to be assigned to a patrol car one month after a complaint is submitted; however, magnitudes are quite small.

2.6. Conclusion

On a daily basis, police officers must use discretion in interactions with civilians in an attempt to protect and serve and are forced to make quick, critical decisions with very little notice or information, weighing their own person safety with that of the individuals whom they patrol. After training programs that last an average of 21 weeks, officers enter field training where they transition into applying the skills they learn in the academy to real-stakes interactions with civilians.The increased attention on the negative consequences of policing due to the rise in cell phone cameras has led to a heightened need for an understanding of the inner workings of police departments, in particular, of how officer behavior responds to monitoring by superiors.

In most police departments, employees and civilians are able to file complaints of misconduct against police officers if they believe to have been the victim of or witness to officer behavior that is inappropriate. These complaints are a potentially useful tool for managing officers as they give superiors the opportunity to see officer behavior through another set of eyes. In this sense, they mitigate the principal agent problem between managers and officers as it is not possible for sergeants, lieutenants, or captains to observe the behavior of patrol officers at most times. Despite this, most complaints submitted to the Chicago Police Department are not fully investigated.

Using detailed administrative data from the Chicago Police Department, I study the extent to which complaint investigations affect police officer behavior. I leverage the conditionally random timing of complaint submissions. Using a difference-in-differences design that compares officer behavior for complaints with and without investigatory interviews before and after complaint submission, my analysis finds little to no evidence that complaints with investigations have a differential impact on officer behavior than those without investigations. The evidence suggests that there are not significant differences in the nature of complaints with and without investigation. Changes in assignment or relegation to desk duty do not appear to be confounding results. Taken as a whole, this provides an opportunity for future research to study the causal effect of a complaint (regardless of investigation) on officer performance.

2.7. Tables

Table 7: Officer Descriptive Statistics

	All Officers	At Least One Complaint	At Least 1 Interview
Percent Male	77.68	80.17	83.33
Percent White	50.71	50.18	49.03
Percent Black	22.74	24.01	26.13
Percent Hispanic	22.85†	22.14†	20.75
Mean Age	40.74†	40.88†	41.01
	(8.71)	(8.22)	(8.09)
Mean Years of Service	12.18	12.39	12.84
	(7.48)	(6.93)	(6.84)
Mean Arrests Per 100 Shifts	18.78	21.25	23.08
	(15.92)	(17.04)	(19.32)
Mean Complaints Per 100 Shifts	0.30	0.49	0.66
	(0.50)	(0.56)	(0.70)
Mean Injuries Per 100 Shifts	0.10	0.11	0.14
	(0.32)	(0.56)	(0.24)
Total Officers	8774	5347	945

This table contains descriptive statistics for district unit officers with unique full names. Standard deviations in parentheses. Differences across categories are significant at the 1% level unless indicated with a †. These differences are significant at the 10% level.

Table 8: Complaints: Categories

	Interview Status					
Complaint Category	No		Yes		Total	
	No.	%	No.	%	No.	%
Operation/Personnel Violations	4547	35.97	421	38.98	4968	36.20
Improper Search	3433	27.16	230	21.30	3663	26.69
Use of Force	2173	17.19	126	11.67	2299	16.75
Lockup Procedures	936	7.40	81	7.50	1017	7.41
Traffic	312	2.47	14	1.30	326	2.38
Conduct Unbecoming	186	1.47	83	7.69	269	1.96
Verbal Abuse	244	1.93	28	2.59	272	1.98
Other	811	6.42	97	8.98	908	6.62
Total	12642	100.00	1080	100.00	13722	100.00

Categories included in other are alcohol abuse, bribery/official corruption, criminal misconduct, substance abuse, supervisory responsibilities, and unknown. Each of these comprises less than 1% of total complaints. Differences in means by interview type are significant at the 1% level unless indicated with a †. Those differences are significant at the 10% level.

Table 9: Complaints: Findings

	Interview Status					
Final Finding	No		Yes		Total	
	No.	%	No.	%	No.	%
Exonerated	474	3.75	52	4.81	526	3.83
No Affidavit	7008	55.43	0	0.00	7008	51.07
Not Sustained	1649	13.04	322	29.81	1971	14.36
Sustained	465	3.68	288	26.67	753	5.49
Unfounded	1092	8.64	233	21.57	1325	9.66
Unknown	1954	15.46	185	17.13	2139	15.59
Total	12642	100.00	1080	100.00	13722	100.00

Differences in means by interview type are significant at the 1% level for all final findings.

Table 10: Complaints: Disciplinary Outcomes

	Interview Status					
Disciplinary Outcome	No		Yes		Total	
	No.	%	No.	%	No.	%
Administrative Termination	2	0.02†	0	0.00†	2	0.01
Days of Leave, <30	224	1.77	162	15.00	386	2.81
Not Served	11	0.09	8	0.74	19	0.14
Reinstated by Board	1	0.01	1	0.09	2	0.01
Reprimand	128	1.01	66	6.11	194	1.41
Resigned	17	0.13	13	1.20	30	0.22
Separation	0	0.00	4	0.37	4	0.03
Unsustained	10221	80.85	605	56.02	10826	78.90
Unknown	1956	15.47	184	17.04	2140	15.60
Suspended Over 30 Days	1	0.01	1	0.09	2	0.01
Violation Noted	81	0.64	36	3.33	117	0.85
Total	12642	100.00	1080	100.00	13722	100.00

Differences in means by interview status are significant at the 1% level unless indicated with a †. Those differences are not significant.

Table 11: Test for Sorting on Demographics, Officer

	(1)
Male	-0.837
	(3.099)
White	-4.496
	(6.722)
Black	-3.276
	(6.895)
Hispanic	-3.001
	(6.622)
Age (Years)	-0.000571
	(0.248)
Experience (Quarters)	0.0487
	(0.0843)
Observations	5081
Joint F-Statistic	0.541
$P > F$	.776

This table contains parameter estimates for β in (2.1). All regressions include complaint type, unit by quarter, unit by shift, and shift by day of week fixed effects. Standard errors clustered at the unit by shift level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Balance Table for Outcome Variables

	(1)	(2)	(3)	(4)
	Length of Pre-Period (Months)			
	1	2	3	4
All Arrests	-0.0270**	-0.0112	-0.00871	-0.0140*
	(0.0131)	(0.00977)	(0.00908)	(0.00830)
Mean of DV	0.330	0.316	0.310	0.309
Effect Size (% of Mean)	-8.185	-3.540	-2.812	-4.545
Felony Arrests	0.00649	0.00462	0.00691	0.00392
	(0.0161)	(0.0110)	(0.00835)	(0.00649)
Mean of DV	0.0892	0.0856	0.0840	0.0835
Effect Size (% of Mean)	7.282	5.391	8.233	4.700
Misdemeanor Arrests	-0.00649	-0.00462	-0.00691	-0.00392
	(0.0161)	(0.0110)	(0.00835)	(0.00649)
Mean of DV	0.911	0.914	0.916	0.917
Effect Size (% of Mean)	-0.713	-0.505	-0.755	-0.428
Complaints Per Arrest	-0.00246	0.000951	0.000361	-0.000595
	(0.00429)	(0.00238)	(0.00165)	(0.00140)
Mean of DV	0.00583	0.00487	0.00492	0.00472
Effect Size (% of Mean)	-42.20	19.53	7.347	-12.59
Injuries Per Arrest	0.00121	-0.000837	-0.000151	0.000499
	(0.00414)	(0.00200)	(0.00147)	(0.00108)
Mean of DV	0.00404	0.00272	0.00238	0.00211
Effect Size (% of Mean)	29.98	-30.75	-6.324	23.67
Observations	26762	55864	81914	107091

This table contains estimates of β from $Y_{icst} = \alpha + \beta TREATED_{ai} + X_{aicst}\Gamma + \epsilon_{icst}$ where $TREATED_{ai}$ is 1 if complaint a logged against officer i has an interview. All regressions include officer, unit by quarter, unit by shift, shift by day of week, and complaint type fixed effects and linear controls for reported index and non-index crimes. Indicator variables for the day before, day of, and day after the complaint-generating incident are also included. Regressions with complaints or injuries as the outcome include an additional control for arrests. Only pre-period observations are included in analysis. Standard errorsclustered at the unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Complaint Log Date and Performance, Patrol Car Only

	(1)	(2)	(3)	(4)
		Bandwidth		
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.00722	-0.00317	-0.00213	-0.00455
	(0.00720)	(0.00591)	(0.00453)	(0.00367)
Mean of DV	0.303	0.300	0.299	0.298
Effect Size (% of Mean)	-2.382	-1.055	-0.713	-1.526
Felony Arrests	-0.00683	-0.00236	-0.00269	-0.00491**
	(0.00550)	(0.00300)	(0.00238)	(0.00222)
Mean of DV	0.0819	0.0809	0.0809	0.0808
Effect Size	-8.338	-2.916	-3.330	-6.076
Misdemeanor Arrests	0.00683	0.00236	0.00269	0.00491**
	(0.00550)	(0.00300)	(0.00238)	(0.00222)
Mean of DV	0.918	0.919	0.919	0.919
Effect Size	0.744	0.257	0.293	0.534
Drug Arrests	0.00590	0.00280	0.00188	0.00156
	(0.00593)	(0.00376)	(0.00319)	(0.00250)
Mean of DV	0.0848	0.0838	0.0832	0.0829
Effect Size	6.955	3.340	2.267	1.887
Complaints Per Arrest	-0.00255	-0.000196	0.000251	0.000858
	(0.00193)	(0.00122)	(0.000922)	(0.000815)
Mean of DV	0.0282	0.0169	0.0129	0.0109
Effect Size	-9.058	-1.162	1.944	7.843
Injuries Per Arrest	-0.000928	0.000265	0.0000639	0.0000985
	(0.000706)	(0.000456)	(0.000363)	(0.000307)
Mean of DV	0.00176	0.00161	0.00158	0.00156
Effect Size	-52.85	16.43	4.042	6.330
Observations	367,461	702,461	1,024,776	1,332,487

This table contains parameter estimates for the effect of complaints on officer performance. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicator variables for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Internal Complaint Log Date and Performance, Patrol Car Only

	(1)	(2)	(3)	(4)
			Bandwidth	
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.0112	0.00200	0.000431	-0.000245
	(0.0114)	(0.00964)	(0.00729)	(0.00609)
Mean of DV	0.216	0.213	0.211	0.211
Effect Size (% of Mean)	-5.194	0.938	0.204	-0.116
Felony Arrets	-0.00825	-0.00243	0.000828	0.000258
	(0.00734)	(0.00548)	(0.00378)	(0.00332)
Mean of DV	0.0560	0.0531	0.0526	0.0525
Effect Size	-14.73	-4.576	1.573	0.491
Misdemeanor Arrests	0.00825	0.00243	-0.000828	-0.000258
	(0.00734)	(0.00548)	(0.00378)	(0.00332)
Mean of DV	0.944	0.947	0.947	0.948
Effect Size	0.874	0.256	-0.0873	-0.0272
Drug Arrests	0.00870	0.00380	0.00403	0.00400
	(0.00551)	(0.00461)	(0.00397)	(0.00383)
Mean of DV	0.0465	0.0464	0.0458	0.0459
Effect Size	18.73	8.184	8.795	8.714
Complaints Per Arrest	0.00231	0.00195	0.000737	0.00175
	(0.00431)	(0.00257)	(0.00185)	(0.00159)
Mean of DV	0.0273	0.0159	0.0123	0.0103
Effect Size	8.489	12.24	6.009	16.99
Injuries Per Arrest	-0.00204*	-0.000997	-0.00111	-0.00103*
	(0.00116)	(0.000761)	(0.000709)	(0.000582)
Mean of DV	0.00180	0.00146	0.00139	0.00138
Effect Size	-113.2	-68.35	-80.21	-74.12
Observations	32,722	63,053	92,293	120,024

This table contains parameter estimates for the effect of internal complaints on officer performance. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: External Complaint Log Date and Performance, Patrol Car Only

	(1)	(2)	(3)	(4)
	Bandwidth			
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.00875	-0.00681	-0.00413	-0.00452
	(0.0109)	(0.00829)	(0.00699)	(0.00557)
Mean of DV	0.312	0.309	0.308	0.306
Effect Size (% of Mean)	-2.808	-2.205	-1.343	-1.477
Felony Arrests	-0.00843	-0.00199	-0.00358	-0.00614*
	(0.00827)	(0.00428)	(0.00361)	(0.00353)
Mean of DV	0.0846	0.0838	0.0838	0.0837
Effect Size	-9.959	-2.374	-4.275	-7.341
Misdemeanor Arrests	0.00843	0.00199	0.00358	0.00614*
	(0.00827)	(0.00428)	(0.00361)	(0.00353)
Mean of DV	0.915	0.916	0.916	0.916
Effect Size	0.921	0.217	0.391	0.670
Drug Arrests	0.00457	0.00213	0.000626	0.00113
	(0.00995)	(0.00556)	(0.00494)	(0.00376)
Mean of DV	0.0885	0.0875	0.0868	0.0865
Effect Size	5.160	2.429	0.721	1.310
Complaints Per Arrest	-0.00800***	-0.00363**	-0.00212*	-0.00136
	(0.00236)	(0.00150)	(0.00112)	(0.000950)
Mean of DV	0.0284	0.0170	0.0130	0.0110
Effect Size	-28.20	-21.35	-16.28	-12.36
Injuries Per Arrest	-0.000838	0.000471	0.000349	0.000325
	(0.00103)	(0.000666)	(0.000537)	(0.000458)
Mean of DV	0.00176	0.00163	0.00160	0.00157
Effect Size	-47.58	28.88	21.80	20.71
Observations	324,652	620,587	905,250	1,177,149

This table contains parameter estimates for the effect of external complaints on officer performance. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Complaint Log Date and Performance, Patrol Car Only: Below Mean Experience

	(1)	(2)	(3)	(4)
			Bandwidth	
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.0167	-0.00847	-0.00369	-0.00513
	(0.0107)	(0.00896)	(0.00670)	(0.00511)
Mean of DV	0.371	0.369	0.368	0.367
Effect Size (% of Mean)	-4.494	-2.295	-1.004	-1.399
Felony Arrests	-0.0169*	-0.00304	-0.00433	-0.00790**
	(0.00901)	(0.00487)	(0.00413)	(0.00362)
Mean of DV	0.106	0.105	0.105	0.105
Effect Size	-15.99	-2.898	-4.137	-7.543
Misdemeanor Arrests	0.0169*	0.00304	0.00433	0.00790**
	(0.00901)	(0.00487)	(0.00413)	(0.00362)
Mean of DV	0.894	0.895	0.895	0.895
Effect Size	1.894	0.339	0.483	0.882
Drug Arrests	0.00567	0.00221	0.00208	0.00162
	(0.00951)	(0.00665)	(0.00570)	(0.00433)
Mean of DV	0.114	0.113	0.112	0.112
Effect Size	4.991	1.961	1.861	1.451
Complaints Per Arrest	-0.00304	-0.000223	-0.000127	0.0000887
	(0.00272)	(0.00171)	(0.00124)	(0.00104)
Mean of DV	0.0285	0.0173	0.0134	0.0114
Effect Size	-10.68	-1.287	-0.945	0.776
Injuries Per Arrest	-0.00117	0.000559	0.000265	0.000112
	(0.00116)	(0.000687)	(0.000529)	(0.000433)
Mean of DV	0.00207	0.00196	0.00193	0.00191
Effect Size	-56.56	28.50	13.75	5.839
Observations	213,458	408,112	595,429	773,653

This table contains parameter estimates for the effect of complaints on officer performance of officers with below mean experience. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Complaint Log Date and Performance, Patrol Car Only: Above Mean Experience

	(1)	(2)	(3)	(4)
	Bandwidth			
	1 Month	2 Months	3 Months	4 Months
All Arrests	0.000737	0.00107	-0.000281	-0.00420
	(0.00873)	(0.00584)	(0.00531)	(0.00543)
Mean of DV	0.205	0.202	0.202	0.201
Effect Size (% of Mean)	0.359	0.528	-0.139	-2.092
Felony Arrests	0.00422	-0.00153	-0.0000527	-0.00102
	(0.00398)	(0.00334)	(0.00292)	(0.00277)
Mean of DV	0.0478	0.0468	0.0470	0.0469
Effect Size	8.839	-3.264	-0.112	-2.166
Misdemeanor Arrests	-0.00422	0.00153	0.0000527	0.00102
	(0.00398)	(0.00334)	(0.00292)	(0.00277)
Mean of DV	0.952	0.953	0.953	0.953
Effect Size	-0.443	0.160	0.00554	0.107
Drug Arrests	0.00660	0.00385	0.00295	0.00250
	(0.00423)	(0.00273)	(0.00258)	(0.00252)
Mean of DV	0.0440	0.0428	0.0424	0.0418
Effect Size	15.00	8.996	6.961	5.966
Complaints Per Arrest	-0.00125	0.000408	0.000778	0.00185
	(0.00250)	(0.00158)	(0.00127)	(0.00112)
Mean of DV	0.0276	0.0161	0.0122	0.0102
Effect Size	-4.513	2.534	6.383	18.20
Injuries Per Arrest	-0.000785	-0.0000573	-0.000205	0.0000344
	(0.000755)	(0.000467)	(0.000416)	(0.000393)
Mean of DV	0.00132	0.00112	0.00109	0.00105
Effect Size	-59.31	-5.098	-18.78	3.269
Observations	150,389	287,571	419,718	546,540

This table contains parameter estimates for the effect of complaints on officer performance of officers with above mean experience. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Internal Complaint Log Date and Performance, Patrol Car Only: Below Mean Experience

	(1)	(2)	(3)	(4)
			Bandwidth	
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.0122	0.00319	0.00431	-0.00119
	(0.0161)	(0.0152)	(0.0115)	(0.00939)
Mean of DV	0.278	0.276	0.271	0.270
Effect Size (% of Mean)	-4.383	1.157	1.590	-0.442
Felony Arrests	-0.0250*	-0.00916	-0.00163	-0.00204
	(0.0148)	(0.0102)	(0.00696)	(0.00549)
Mean of DV	0.0792	0.0752	0.0740	0.0735
Effect Size	-31.54	-12.19	-2.204	-2.772
Misdemeanor Arrests	0.0250*	0.00916	0.00163	0.00204
	(0.0148)	(0.0102)	(0.00696)	(0.00549)
Mean of DV	0.921	0.925	0.926	0.926
Effect Size	2.714	0.991	0.176	0.220
Drug Arrests	0.00729	0.00617	0.00646	0.00780
	(0.0108)	(0.00812)	(0.00712)	(0.00714)
Mean of DV	0.0704	0.0696	0.0678	0.0687
Effect Size	10.36	8.868	9.526	11.36
Complaints Per Arrest	0.00707	0.00734	0.00342	0.00258
	(0.00687)	(0.00470)	(0.00310)	(0.00254)
Mean of DV	0.0283	0.0167	0.0129	0.0109
Effect Size	24.93	43.92	26.46	23.61
Injuries Per Arrest	-0.00300	-0.00104	-0.00145	-0.000601
	(0.00241)	(0.00145)	(0.00117)	(0.00101)
Mean of DV	0.00223	0.00191	0.00171	0.00169
Effect Size	-134.8	-54.29	-84.72	-35.54
Observations	16,620	31,964	46,767	60,900

This table contains parameter estimates for the effect of internal complaints on officer performance for officers with below mean experience. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Internal Complaint Log Date and Performance, Patrol Car Only: Above Mean Experience

	(1)	(2)	(3)	(4)
	Bandwidth			
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.0139	-0.00503	-0.0103	-0.00495
	(0.0155)	(0.0124)	(0.00974)	(0.00873)
Mean of DV	0.151	0.147	0.148	0.148
Effect Size (% of Mean)	-9.240	-3.426	-6.957	-3.352
Felony Arrests	0.00307	0.00296	0.00267	0.000343
	(0.00656)	(0.00580)	(0.00490)	(0.00440)
Mean of DV	0.0310	0.0289	0.0290	0.0293
Effect Size	9.922	10.24	9.195	1.172
Misdemeanor Arrests	-0.00307	-0.00296	-0.00267	-0.000343
	(0.00656)	(0.00580)	(0.00490)	(0.00440)
Mean of DV	0.969	0.971	0.971	0.971
Effect Size	-0.317	-0.304	-0.275	-0.0353
Drug Arrests	0.00993*	0.00481	0.00350	0.00144
	(0.00549)	(0.00427)	(0.00394)	(0.00356)
Mean of DV	0.0204	0.0209	0.0214	0.0208
Effect Size	48.75	22.99	16.35	6.895
Complaints Per Arrest	-0.0000855	-0.000166	-0.00126	0.000445
	(0.00558)	(0.00357)	(0.00281)	(0.00243)
Mean of DV	0.0259	0.0150	0.0114	0.00959
Effect Size	-0.331	-1.103	-11.01	4.637
Injuries Per Arrest	-0.00150	-0.000765	-0.00107	-0.000968
	(0.00137)	(0.00109)	(0.000921)	(0.000780)
Observations	15855	30620	44846	58279
Mean of DV	0.00139	0.00101	0.00107	0.00108
Effect Size	-108.0	-75.52	-99.95	-89.55

This table contains parameter estimates for the effect of internal complaints on officer performance for officers with above mean experience. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: External Complaint Log Date and Performance, Patrol Car Only: Below Mean Experience

	(1)	(2)	(3)	(4)
			Bandwidth	
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.0147	-0.0116	-0.00624	-0.00579
	(0.0156)	(0.0121)	(0.00979)	(0.00782)
Mean of DV	0.380	0.377	0.376	0.375
Effect Size (% of Mean)	-3.879	-3.088	-1.660	-1.545
Felony Arrests	-0.0152	-0.0000549	-0.00489	-0.00877
	(0.0124)	(0.00679)	(0.00581)	(0.00540)
Mean of DV	0.109	0.108	0.108	0.108
Effect Size	-14.01	-0.0509	-4.541	-8.144
Misdemeanor Arrests	0.0152	0.0000549	0.00489	0.00877
	(0.0124)	(0.00679)	(0.00581)	(0.00540)
Mean of DV	0.891	0.892	0.892	0.892
Effect Size	1.707	0.00615	0.547	0.983
Drug Arrests	0.00508	0.00106	-0.000656	0.0000665
	(0.0141)	(0.00871)	(0.00774)	(0.00601)
Mean of DV	0.117	0.117	0.116	0.116
Effect Size	4.327	0.909	-0.566	0.0574
Complaints Per Arrest	-0.00895***	-0.00422**	-0.00248*	-0.00201*
	(0.00285)	(0.00170)	(0.00131)	(0.00104)
Mean of DV	0.0286	0.0174	0.0135	0.0115
Effect Size	-31.29	-24.26	-18.43	-17.50
Injuries Per Arrest	-0.000566	0.000839	0.000587	0.000390
	(0.00146)	(0.000910)	(0.000717)	(0.000585)
Mean of DV	0.00207	0.00197	0.00194	0.00193
Effect Size	-27.34	42.63	30.21	20.25
Observations	191,256	365,733	533,414	692,900

This table contains parameter estimates for the effect of external complaints on officer performance for officers with below mean experience. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: External Complaint Log Date and Performance, Patrol Car Only: Above Mean Experience

	(1)	(2)	(3)	(4)
			Bandwidth	
	1 Month	2 Months	3 Months	4 Months
All Arrests	-0.00307	-0.00450	-0.00303	-0.00525
	(0.0132)	(0.00851)	(0.00832)	(0.00804)
Mean of DV	0.211	0.208	0.207	0.206
Effect Size (% of Mean)	-1.451	-2.165	-1.461	-2.547
Felony Arrests	0.00132	-0.00551	-0.00144	-0.00172
	(0.00679)	(0.00505)	(0.00466)	(0.00421)
Mean of DV	0.0495	0.0487	0.0488	0.0487
Effect Size	2.668	-11.31	-2.947	-3.525
Misdemeanor Arrests	-0.00132	0.00551	0.00144	0.00172
	(0.00679)	(0.00505)	(0.00466)	(0.00421)
Mean of DV	0.951	0.951	0.951	0.951
Effect Size	-0.139	0.579	0.151	0.181
Drug Arrests	0.00398	0.00288	0.00287	0.00282
	(0.00758)	(0.00455)	(0.00442)	(0.00406)
Mean of DV	0.0464	0.0450	0.0445	0.0439
Effect Size	8.573	6.414	6.441	6.421
Complaints Per Arrest	-0.00377	-0.00105	-0.000918	0.000213
	(0.00359)	(0.00233)	(0.00163)	(0.00147)
Mean of DV	0.0280	0.0163	0.0123	0.0103
Effect Size	-13.49	-6.443	-7.456	2.075
Injuries Per Arrest	-0.00194	-0.000318	-0.000230	0.000116
	(0.00135)	(0.000856)	(0.000658)	(0.000587)
Mean of DV	0.00132	0.00114	0.00109	0.00105
Effect Size	-147.0	-27.86	-21.00	11.09
Observations	130,147	248,794	363,229	473,219

This table contains parameter estimates for the effect of external complaints on officer performance for officers with above mean experience. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

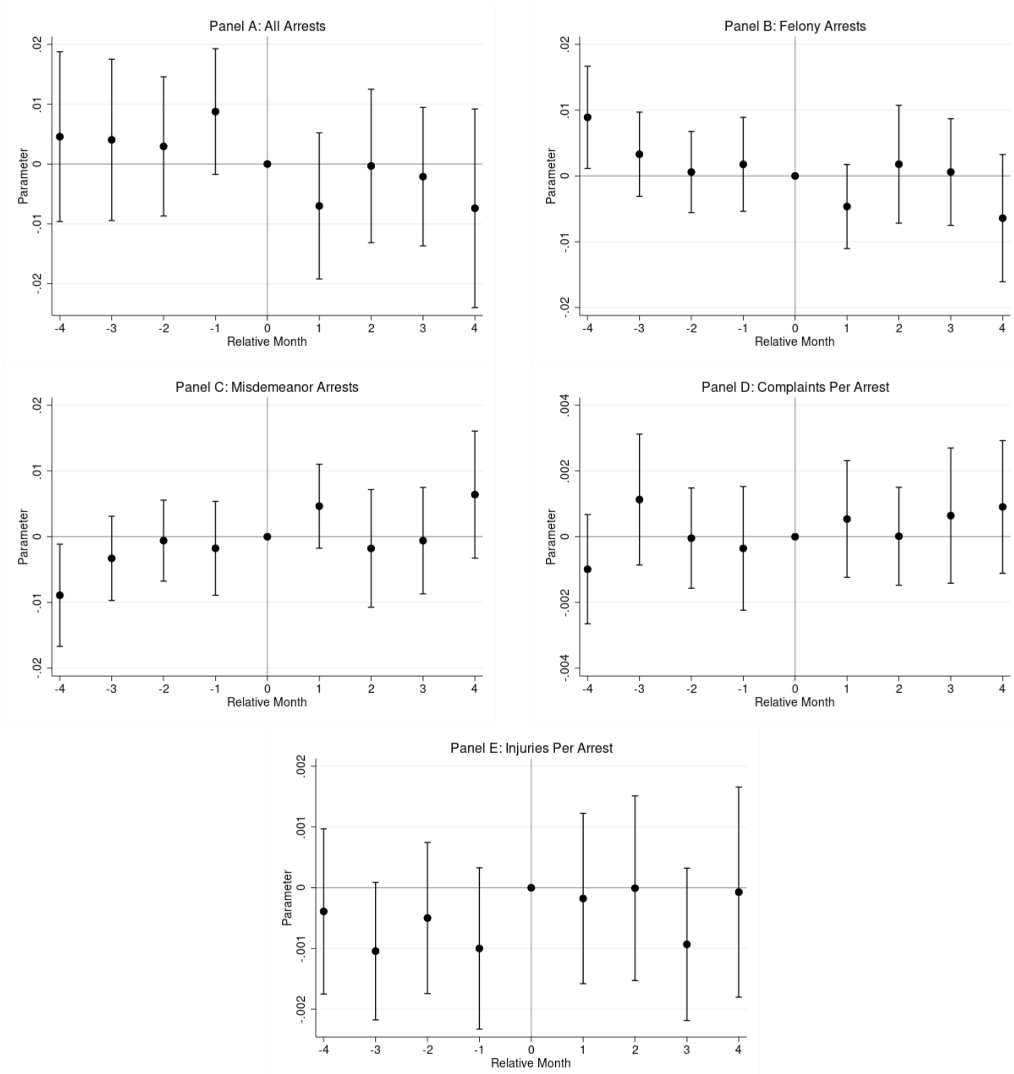
Table 22: Complaint Log Date and Assignment

	(1)	(2)	(3)	(4)
			Bandwidth	
	1 Month	2 Months	3 Months	4 Months
Present	-0.00944	-0.0100	-0.00939	-0.00839
	(0.00647)	(0.00637)	(0.00599)	(0.00589)
Mean of DV	0.540	0.529	0.524	0.520
Effect Size (% of Mean)	-1.747	-1.894	-1.793	-1.615
Patrol Car	-0.0111**	-0.00610	-0.00672	-0.00745
	(0.00449)	(0.00467)	(0.00520)	(0.00525)
Mean of DV	0.893	0.892	0.892	0.892
Effect Size	-1.247	-0.684	-0.754	-0.836

This table contains parameter estimates for the effect of complaints on officer assignment. The post period is determined by the date on which a complaint is submitted to the CPD. All regressions include complaint type, officer, unit by quarter, unit by shift, and shift by day of week fixed effects; linear controls for reported index and non-index crimes; and indicators for the week around incident, complaint logging, interview, and investigation closure dates. All regressions with complaints or injuries as the outcome also include a control for arrests. Standard errors clustered at unit by shift level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.8. Figures

Figure 6: Testing Parallel Trends



This figure plots β_k from $Y_{icst} = \alpha + \sum_{k=-4}^{4} \beta_k TREATED_{ai} \times RelMonth_k + X_{icst}\Gamma + \epsilon_{icst}$. Y_{icst} is all arrests in panel (a), felony arrests in panel (b), misdemeanor arrests in panel (c), complaints per arrest in panel (d), and injuries per arrest in panel (e). 95% confidence intervals around β_k included.

CHAPTER 3 : The Minimum Legal Drinking Age and Crime Victimization with Aaron Chalfin and Ben Hansen

3.1. Introduction

For nearly every crime committed there is both an offender and a victim. Yet, due to both data constraints and disciplinary norms, economics research on crime dating back to Becker (1968) has primarily focused on policy levers that modify the behavior of potential offenders (Nagin, 2013; Chalfin and McCrary, 2017). Perhaps this is natural because the chief policy levers at the disposal of a social planner, namely police (which change the certainty of punishment) and prisons (which change the severity of punishment), are intended to influence the actions of the agents committing crimes. Given that by offending offenders violate society's explicit social norms, this perspective is largely consistent with the normative views of the general public which does not wish to blame the victim for having been victimized (Crawford, 1977; Eigenberg and Garland, 2008).

We agree with this view — that victims are not blamed when a crime occurs — and note that, since markets require the voluntary transfer of property rights, there can be neither a market nor a price for victimization. (Carnis, 2004). At the same time, victim behavior can be an important input into the cost function of potential offenders. As such, providing information can be a critical means through which social planners can empower victims while also tailoring public safety interventions to be maximally disruptive to potential offenders (Cozens et al., 2005; MacDonald, 2015; Branas et al., 2016), in so doing, reducing the overall cost of crime control (Ben-Shahar and Harel, 1995). In this paper we provide some of the first evidence on the causal determinants of victimization, focusing on abrupt change in legal access to alcohol at age 21 in the United States.

Empirical evidence suggests that both law enforcement (Sherman and Weisburd, 1995; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Evans and Owens, 2007; Chalfin and McCrary, 2018; Weisburst, 2018; Mello, 2019) and harsher punishments (Helland and

Tabarrok, 2007; Drago et al., 2009; Hansen, 2015; Tahamont and Chalfin, 2016) can reduce offending, at least under some circumstances. However, most research suggests that the effect of police on crime is, at best, modest and that the effect of incarceration is small and has declined at the margin as the incarceration rate has risen (Liedka et al., 2006; Cullen et al., 2011; Durlauf and Nagin, 2011; Johnson and Raphael, 2012; Lofstrom and Raphael, 2016). The picture that emerges from this research is that traditional policy levers, while effective, are not necessarily cost-effective, at least at current levels. Beyond the large financial cost to the government, collateral harms associated with the use of police and incarceration, including direct economic and social impacts as well as effects on families and communities, fall disproportionately on low income, racially segregated neighborhoods, further raising the cost of traditional policy levers (Western et al., 2001; Aizer and Doyle Jr, 2015; Mueller-Smith, 2015).[1]

Given that traditional forms of crime control are costly, there are potentially important benefits to identifying alternative means of crime control that have lower financial and collateral costs. Recently, a number of alternatives have emerged, including improving the quality of labor market market opportunities (Yang, 2017; Schnepel, 2017; Agan and Makowsky, 2018), cognitive behavioral therapy (Heller et al., 2017), summer job programs for youth (Heller, 2014; Davis and Heller, 2017), and investments in local capital infrastructure like street lighting (Doleac and Sanders, 2015; Chalfin et al., 2019b) and the quality of public space (Branas et al., 2011; Kondo et al., 2016; Branas et al., 2018). Though all of these potential policy levers show promise, there are inevitable concerns regarding the scalability and external validity of studies of local programs (Davis et al., 2017).

Each of the aforementioned alternatives seeks primarily to modify the behavior of offenders. However, a factor in nearly every crime is the presence of a victim.[2] To date, the literature

[1] We further note that the low clearance rates for most serious crimes make crime reduction through deterrence particularly challenging. Low clearance rates may also reduce the scope for incapacitation to have appreciable effects on crime.

[2] Indeed Becker's original model of crime considers the role of victimization, yet ultimately his approach, and most of the literature thereafter, focuses on police and punishment as the principal policy levers.

in economics focusing on victimization is sparse and focuses primarily on a small number of investments that make committing a crime more difficult including auto theft prevention technology (Ayres and Levitt, 1998), security guards (Maheshri and Mastrobuoni, 2017) and the introduction of business improvement districts (Cook and MacDonald, 2011). However, the scope of victim precaution is large. Indeed, private markets supply everything from low tech security devices like locks, to the increasing presence of private alarms and security cameras (e.g., Amazon Ring), GPS trackers, and credit monitoring. Victim precaution also includes behavioral modifications that potentially affect the probability of victimization such as the choice to leave ones home at night, hailing a taxi versus walking while in a high-crime area, carrying valuables on ones person, or the awareness and knowledge of a new environment. As such, there is a great deal of ground for the empirical literature to cover.

Identifying the determinants of victimization is both important and promising for several reasons. First, recent literature has found that becoming a crime victim has a wide range of impacts and includes effects as diverse as mental well-being (Cornaglia et al., 2014; Dustmann and Fasani, 2015), labor market earnings and benefits receipt (Bindler and Ketel, 2019) and health outcomes for newborn infants (Currie et al., 2018). Accordingly, the costs of victimization are likely to be large and especially concentrated among the most vulnerable members of society. Second, for many crimes, especially those that have low clearance rates, abating crime through deterrence-based strategies is costly. As such, victimization-oriented strategies might reduce crime at lower cost. While the normative implications of this hypothesis are potentially controversial, given the high collateral costs of the criminal justice system, this policy option may be worth further exploration. Third, victims often have relatively little information about their probability of being victimized as well as the effectiveness of private investments in crime control. Indeed, given that the academic literature has yet to offer evidence on the causal effect of actions or policies on victimization, we might expect that individuals will have difficulty accurately forecasting this on their own. Therefore, it stands to reason that victims might not optimally invest in precaution in wide variety of situations.[3] Finally, while programmatic interventions typically have high variable costs, we note that informational interventions often have low marginal costs and, as such, are easier to scale. Because of this, there may be considerable promise in providing information to victims as well as law enforcement.

Studying the determinants of victimization has proven elusive for at least two reasons. First, it is difficult to identify policies which affect the probability of victimization without also affecting the supply side of the market. Second, victimization research is hampered by the extremely limited availability of microdata, especially U.S. microdata at the sub-national level (Gutierrez and Kirk, 2017). While a large research literature in criminology identifies some demographic and situational correlates of victimization (Gottfredson, 1986), finding exogenous variation upon which a causal claim may be made about an actionable policy lever has proven elusive.

In this paper, we study one prominent policy lever that plausibly has an outsize influence on victimization: legal access to alcohol. A large body of research has found evidence of significant social costs associated with legal access to alcohol (Grossman and Markowitz, 1999; Markowitz, 2000; Carpenter, 2005, 2007; Carpenter and Dobkin, 2009, 2011; Cook and Durrance, 2013; Heaton, 2012; Kilmer et al., 2013; Carpenter and Dobkin, 2015; Anderson et al., 2017; Carpenter and Dobkin, 2017).[4] These papers utilize both age-based discontinuities in access to alcohol and geographic variation in state or local policy. Their consensus suggests that legal access to or lower prices of alcohol are associated with increased traffic fatalities, suicides, violent behavior and injuries, including injuries among male victims which were intentionally inflicted (Carpenter and Dobkin, 2017). Despite the substantial body of evidence documenting the negative public health impacts of alcohol use and abuse, these impacts might be even larger if alcohol use also increases the risk of of becoming a victim of a crime more broadly, which none of the previous studies have been able to address.

[3] We further note that individual choices may result in externalities to others as investments in precaution may change the relative returns of crime to potential offenders.

[4] A notable exception is a paper by Lindo et al. (2016) who find no evidence of an effect of legal access to alcohol on motor vehicle accidents in Australia.

In particular, one of the most intriguing possibilities is that legal access to alcohol might be an important driver of sexual assaults, a relationship that has received wide speculation in the literature in criminology and public health (Kantor and Straus, 1989; Dembo et al., 1992; Miller et al., 1993; Abbey et al., 2001; Abbey, 2002; Champion et al., 2004; Felson and Burchfield, 2004). These studies, while suggestive, are largely correlational and lack credible research designs, though recent research has intriguingly linked sexual assaults to local culture of drinking and alcohol abuse or "college party culture." (Lindo et al., 2018).

The primary empirical challenge involved in identifying a causal impact of alcohol use on crime is that using alcohol, particularly to excess, is an endogenous choice. As a result, there are many reasons why a correlation between alcohol use and victimization might exist either among individuals or, for a given individual, over time. We study a related research question — and one which pairs naturally with a potential policy lever — and estimate the extent to which *legal access to alcohol* causes a discrete change in victimization.

In order to identify a causal effect of legal access to alcohol on victimization, we utilize the fact that legal access to alcohol in the United States changes discretely at age 21 and, using a sharp regression discontinuity design, estimate the likelihood that an individual is victimized just after her 21st birthday relative to the period before her 21st birthday. In order to estimate the model, we build a unique administrative dataset that contains the exact date of birth for all crime victims known to law enforcement in eight major U.S. cities and find strong evidence that certain types of victimization — sexual assault and burglary for women, assault and robbery for men and larceny for both genders — increase considerably at age 21. This effect is found only at age 21 (and not on prior or subsequent birthdays) and is unlikely to be driven by celebrating one's 21st birthday itself. We likewise study the effect of birthday celebrations more generally and find that victimization rise by approximately 15 percent on or near an individual's birthday, thus suggesting a second means through which alcohol use and not only alcohol access, has a causal effect on victimization.

On the whole, our estimates suggest that legal access to alcohol changes the landscape of victimization considerably and that a sizable share of serious crime could be abated by policies that change legal access to alcohol or modify the parameters of public intoxication. Our findings also provide additional insights into the complex and controversial relationship between alcohol and sexual assault (Lindo et al., 2018). In particular, while both Carpenter and Dobkin (2015) and Hansen and Waddell (2018) fail to find evidence that arrests or criminal charges for rape increase at age 21, we find sexual assault victimization at age 21 increases by nearly 25 percent in our preferred specifications. Taken together, these findings are more consistent with a model of crime in which perpetrators of sexual assault seek out vulnerable populations than with a model where sexual assault perpetrators lose control due to increased alcohol use.[5]

The remainder of this paper proceeds as follows: Section 2 provides a brief institutional history of the minimum legal drinking age and its effects on alcohol consumption. Section 3 provides detail on the unique administrative dataset collected for this study; Section 4 provides an overview of the econometric models; Section 5 presents results and Section 6 concludes.

3.2. Background

3.2.1. Private Actions and Victimization

There are many ways through which potential victims can reduce their likelihood of becoming the victim of a crime. With respect to property crimes, these include investments in traditional target-hardening strategies (e.g., locks and deadbolts) and technology (e.g., surveillance cameras and security systems) as well as labor inputs such as private security services. In the case of violent crimes which drive an outsize share of the social costs of offending (Chalfin, 2015), private precautions are, to a greater extent, driven by behavioral

[5]It is important to note that we are unable to identify the subset of victimizations that are cleared by an arrest. It is entirely possible that, in keeping with the literature on offending, there is no significant change in these victimizations after the 21st birthday. This is especially likely if there is an increase in victimizations that involve alcohol at the MLDA. See Spohn and Tellis (2012) for more information.

modifications by potential victims — modifications that are perceived to change an individual's probability of victimization. Such behavioral modifications might include avoiding leaving one's home at night, hailing a taxi instead of walking while in a high-crime area, carrying fewer valuables on one's person or maintaining a generally higher level of vigilance or situational awareness. Each of these actions has the potential to make crime less profitable to a potential offender.

While investments in private precaution are costly with often unknown benefits to potential victims, they are potentially attractive to a social planner for a number of reasons. First, an individual victim may have more information about how to successfully abate his or her risk of being victimized than law enforcement which must devise crime control strategies on the basis of typical patterns of victim and offender behavior that cannot easily tailor these strategies to a given individual's needs (Ben-Shahar and Harel, 1995; Felson and Clarke, 1995). Second, in most cities in the United States, there is approximately one sworn police officer for every 250 residents and so there are natural limits to the ability of law enforcement to deter offending. Finally, investments in private precaution may raise search costs for offenders, thus making crime less attractive overall (Shavell, 1991). Thus, private precautions, even when observable to potential offenders, may generate positive spillovers to society.

Taken as a whole, the theory suggests that it may be possible for potential victims to abate crime more efficiently than can the government – at least at the margin. Consider, for instance, crimes such as larceny or burglary which often involve belongings left unattended or homes that were unlocked at the time of the crime, both of which are extremely common and which could be abated through low-cost changes in behavior among potential victims. These crimes are only marginally responsive to police manpower (Chalfin and McCrary, 2018). Yet, for a variety of reasons — because individuals do not fully internalize the cost of victimization (Clotfelter, 1978; Ayres and Levitt, 1998), because public spending on crime control may be treated as a subsidy (Guha and Guha, 2012) or because individuals are myopic or misinformed — victims may under-invest in precaution, relative to what is socially optimal.[6] This raises the possibility that there may yet be low hanging fruit to pick with respect to addressing crime through private victim action.

3.2.2. Alcohol Use and Victimization

Literature outside of economics has linked alcohol abuse and victimization, either as a correlate of victimization risk (Champion et al., 2004; Felson and Burchfield, 2004) or as a predictor of subsequent victimization (Kantor and Straus, 1989; Widom, 2001), particularly in the context of domestic violence and sexual assault (Abbey, 2002). However, none of these studies utilizes exogenous variation to identify a causal effect of substance use. Within economics, prior research has found that emergency department visits for injuries inflicted by others increase at age 21 (Carpenter and Dobkin, 2017) and that sexual assault victimization rises during college football game days (Lindo et al., 2018), an effect which is credibly due to an increase in alcohol consumption.

While the evidence is predominantly correlational, there are a number of reasons why alcohol use and crime victimization might be causally related. First, there is evidence that the use and, particularly, abuse of alcohol causes individuals to exhibit fewer inhibitions (Mulvihill et al., 1997; Easdon and Vogel-Sprott, 2000; Fillmore and Vogel-Sprott, 2000) which may lead them to take on risks that they otherwise would not have taken (Ryb et al., 2006). Thus victimization might rise with alcohol abuse due to a change in the risk tolerance of potential victims. Second, intoxication may affect an individual's situational awareness and therefore increase the ease with which a victim can be identified and approached by a motivated offender. For instance, an intoxicated victim might be less likely to notice a risky situation (Parks and Miller, 1997) or take actions to mitigate that risk. Third, a large literature establishes that intoxication increases aggression (Giancola and Zeichner, 1995; Graham et al., 2006), which itself is a predictor of victimization, especially for assaults. We

[6] Some of the earliest thinking about the role of private precaution in the crime production function can be found in the seminal work of (Erhlich, 1973; Ehrlich, 1981) who conceives of the "derived demand" for crime as the willingness of market participants to invest in private precautions.

note, for example, that the difference between an assault victim and the perpetrator of an assault can simply be which party strikes the first blow (Chalfin et al., 2019a). Finally, intoxicated victims may be less able to defend themselves effectively, thus reducing the cost to a potential offender.

3.2.3. The MLDA and Alcohol Consumption

In the United States, the minimum legal drinking age — the age at which individuals are legally allowed to purchase alcohol – has historically oscillated between 18 and 21 years of age. Many states initially lowered their minimum legal drinking ages only to raise them again later in the 1980s. Now, essentially every state implements a minimum legal drinking age of 21.[7]

While the law does not prevent minors from securing access to alcohol (Freisthler et al., 2003), there is ample evidence that *legal access* to alcohol nevertheless increases drinking and, in particular, problematic drinking. For example, recent research uses information from National Health Interview Survey to show that drinking increases at both the extensive and intensive margins when individuals turn 21, using on local regression discontinuity based estimates (Carpenter and Dobkin, 2009). Other evidence shows that it is precisely the most problematic types of drinking that increase at age 21 — for example, binge drinking — as opposed to moderate levels of drinking (Carpenter et al., 2016). This research, and other related studies on youth zero tolerance laws (Carpenter, 2007), suggest that alcohol use, including consumption patterns consistent with alcohol abuse, increases with legal access to alcohol.

In this research, we rely on the sharp discontinuity in legal access to alcohol at age 21 to identify a causal effect of legal access to alcohol on victimization. Since data that contain the exact date of victimization for the entire United States are not available, we focus on city-level administrative records of victimizations with the exact date of birth and exact

[7]There are a few very limited exceptions. For instance, some states permit alcohol use with one's parents at restaurants (in Wisconsin for instance).

date of victimization for all crime victims in eight large cities in the United States. Our data cover the following jurisdictions: Charlotte, NC (Charlotte-Mecklenburg), Dallas, TX, Denver, CO, Houston, TX, Kansas City, MO, Milwaukee, WI, San Diego, CA and St. Louis, MO.

While this gives us a large number of victimizations upon which to draw inferences, this subsample of the United States is small enough we would ill-powered to do a subsample analysis of the National Health Interview Survey (NHIS).[8] To verify alcohol abuse increases at age 21 for our subsample, we focus on a public health outcome shown to be correlated with alcohol use: drunk driving. Indeed, prior research on the MLDA using both both differences-in-differences designs (Carpenter and Dobkin, 2011; Francesconi and James, 2019) and regression discontinuity designs (Carpenter and Dobkin, 2009) suggests that traffic fatalities increase with legal access to alcohol due to the MLDA. Moreover, both (Carpenter and Dobkin, 2015) and (Hansen and Waddell, 2018) find that drunk driving increases by 40 percent at the MLDA for two completely independent states.[9]

This motivates our proxy for alcohol use and abuse: the fraction of fatal accidents involving alcohol from the Fatal Analysis and Reporting System (FARS), a census of every fatal car crash in the United States. We focus on the fraction of fatal car crashes and its evolution over the age distribution. The FARS collects age instead of exact date of birth. As shown in Figure 7, the fraction of accidents involving alcohol increases monotonically with age among teenagers before exhibiting a discrete jump at age 21, after which it declines monotonically. A similar pattern emerges for the subset of cities that provided victimization reports. This suggests that there are similar "first stage" effects of alcohol access on alcohol use and abuse for our subset of cities and the nation as a whole.

[8] The first stage estimates in Carpenter and Dobkin are generally quite precise and well powered. However if we were to draw a 3 percent random sample of the NHIS (roughly the size of our cities relative to overall US population), it would yield all of the point estimates for their first to be insignificant even holding the point estimate fixed given the change in the standard error due to the smaller sample.

[9] These estimated increases are essentially equal in magnitude to the estimated increase in alcohol consumption found in the NHIS by Carpenter and Dobkin (2009). As such, it may be reasonable to infer that alcohol consumption and drunk driving increase at similar percentages with legal access to alcohol.

3.3. Data

This research considers whether individuals who have legal access to alcohol are more likely to become crime victims. As national microdata on crime victims are unavailable, we construct a unique dataset on crime victimization, using administrative microdata obtained from eight municipal police departments in the United States. The eight police departments are the municipal law enforcement agencies for the following cities: Charlotte, NC (Charlotte-Mecklenburg), Dallas, TX, Denver, CO, Houston, TX, Kansas City, MO, Milwaukee, WI, San Diego, CA and St. Louis, MO.[10] These departments cover a population of approximately 8 million residents, represent a number of different U.S. regions and include three of the ten largest cities in the United States — Houston, Dallas and San Diego.[11] Table 23 explores the extent to which the cities in our analytic sample differ from other U.S. cities and the population as a whole with respect to their crime rates. The cities in our sample have higher than average crime rates, approximately 50 percent higher than other large cities, depending on the crime type. St. Louis, in particular, has an extremely high crime rate and had the highest homicide rate in the United States in 2016.

In each city, the data contain information on the type of crime, the date of victimization and the victim's exact date of birth and gender. To protect victim anonymity, we do not have victim identifiers. We focus on crimes that, with a few exceptions, largely correspond to the Federal Bureau of Investigation's list of "index crimes" which are collected annually and reported in the FBI's *Uniform Crime Reports*. Specifically, we focus on the following crimes: assault,[12] burglary, homicide, larceny, motor vehicle theft, robbery and sex-related crimes which are an aggregate of rape and other sexually-related offenses, in cities for which

[10] Note that not every crime has a person-victim — for example, crimes against businesses. We focus on crimes with a person-victim.

[11] In total, we reached out to twenty-two police departments. We received no reply from municipal law enforcement agencies in the following cities: Cincinnati, OH Cleveland, OH, Detroit, MI Memphis,TN, Nashville,TN, Washington DC, Atlanta, GA, Sacramento, CA, Tuscon, AZ, Cambridge MA, Baton Rouge, LA, Seattle, WA and Las Vegas, NV. The following departments declined our request for data: Baltimore, MD, Miami, FL, Orlando, FL, Philadelphia, PA, Boston, MA, Columbus, OH, Portland, OR, Phoenix, AZ and Newark NJ.

[12] In a deviation from the index crime designation, this category includes both aggravated and simple assaults, but not sexual assaults.

they are available.[13] Overall, data cover the years 2007 through 2018 though exact years of data availability vary by department.[14] In all subsequent analyses, we aggregate the data from our eight cities in order to generate a national estimate of the effect of legal exposure to alcohol on crime.

We supplement our administrative data with microdata from the U.S. National Crime Victimization Survey (NCVS), a survey of a random sample of between 49,000 and 77,000 U.S. residents, collected by the U.S. Bureau of Justice Statistics (BJS) since 1977. The NCVS is the principal national dataset on victimization in the United States (Gutierrez and Kirk, 2017) and allows us to ensure that the reporting of crimes to law enforcement does not change discontinuously at age 21, a critical falsification check for our analysis. In the NCVS, respondents are asked to indicate whether they have been the victim of a crime during the past six months. Critically respondents are also asked whether they reported that crime to law enforcement. We use the NCVS to explore whether crime *reporting* changes at the age of 21, a story which might be true if intoxicated victims who are under the minimum legal drinking age are less likely to report a crime to law enforcement. If true, this could lead us to conclude that victimization increases at age 21 even though this effect might merely be an artifact of differential crime reporting. We consider whether crime reporting changes discontinuously at age 21 in Section 5.2.1 and conclude that there is little evidence of differential crime reporting at the MLDA.

Prior to describing our empirical models and results, we pause here to present a brief descriptive analysis of the age-victimization profile in our administrative data. In Figure 8, we present the share of victimizations by age, using a local polynomial smoother. Violent crimes are presented in Panel A; property crimes are presented in Panel B. For both crime types, we present results separately for males (using a black line) and females (using a gray line). Consistent with a longstanding empirical regularity that has been documented by

[13] While specific offense types vary by city, we include the following offenses in our sexual assault aggregate: fondling, rape, sexual assault or battery and sodomy. See Appendix C for the universe of sex offenses by police department.

[14] See Appendix D for department-specific date ranges.

scholars of victimization, crime victimization generally rises throughout childhood, peaking between the ages of 20 and 30 and falling steeply thereafter (Stafford and Galle, 1984). Several exceptions are worth noting. First, there are important gender differences with respect to the victimization-age profile of sexual assault. Among males, sexual assaults are most prevalent in childhood and the risk declines substantially thereafter. Among females, sexual assault risk peaks just prior to age 20 and declines precipitously thereafter, though victimization remains uniformly higher than among males. Second, while homicide risk peaks for both genders in the early 20s, the peak is especially large for men reflecting the ubiquity of gang violence and "vendetta-like antagonisms," often referred to colloquially as "beefs" (Kennedy, 1996). The opposite pattern holds for assaults with women experiencing an especially high degree of vulnerability in their early 20s while men's victimization risk declines more slowly throughout their lifespan.

Referring to Appendix Figure B6, readers can contrast patterns in our data, derived from crime reports, with survey data reported to the NCVS. Specifically, we present the age profile of victimization for respondents aged 12 and older using the 2006 to 2016 waves of the NCVS.[15] Violent victimization patterns are, on the whole, extremely similar to those derived from our administrative data. However, there are some notable differences with respect to property crime victimization. In contrast to the law enforcement data from our eight cities, in the NCVS, the age-crime profile for burglary and larceny suggests that crime victimization drops off less dramatically after age 30. Notably, both the NCVS and our administrative data highlight that the age period affected by the MLDA, the early 20s, is an important period for policy given high victimization rates for nearly all crimes.

3.4. Methods

We estimate the causal effect of legal access to alcohol on victimization using a regression discontinuity design, leveraging the discrete change in legal alcohol access at age 21. The primary identifying assumption is that individuals who are just below age 21 and individuals

[15]Homicide is not included in this figure as all victims are deceased.

who are just above age 21 are exchangeable — that is, they do not differ, on average, with respect to both observable and unobservable characteristics. While age is a common running variable in empirical applications in applied microeconomics (see e.g., (Lemieux and Milligan, 2008; Smith, 2009; McCrary and Royer, 2011), we discuss potential violations of this assumption in Section 5.2.3.

Because all individuals are subject to the treatment age age 21, without exception, we estimate treatment effects using a "sharp" RD design. In keeping with standard empirical practice, we estimate treatment effects using the following general specification:

$$Y_i = \alpha + \tau D_i + \beta(X_i - c) + \gamma(X_i - c)D_i + \varepsilon_i \tag{3.1}$$

In (1), Y_i is the count of victimizations occurring on relative age i, (X_i-c) is the number of days relative to a given crime victim's 21st birthday and D_i is an indicator variable for whether or not the criminal incident occurred prior to or after the victim's 21st birthday. The coefficient on D_i, τ identifies the causal effect of legal alcohol access. Because the evolution of victimization over the life cycle may be nonlinear in age relative to 21, in practice, we specify a model that also includes $(X_i - c)^j$ and the product of this term and D_i for polynomials of order j=2 and 3. These non-linearities could pick up numerous different factors which can affect victimization, such as criminality which is known to vary over the life course (Loeber and Farrington, 2014), an age gradient to alcohol consumption or likelihood that an individual lives alone. Given that all of these factors shift as individuals age, we focus on on identifying the causal effect of alcohol access at age 21. As other factors directly related to victimization are unlikely to discretely change at 21, this offers a reasonable approach to estimating the causal effect of alcohol access on victimization.

Equation (1) is estimated for a given bandwidth, h so that the regression is estimated for those observations within $c - h \leq X_i \leq c + h$. All models are estimated using robust standard errors which accommodate the possibility that there is heteroskedasticity among the individual error terms within age bins. In Section 5.4, we describe a number of robustness checks which test the sensitivity of the results to alternative modeling strategies.

In addition to estimating a standard RD effect of the impact of the minimum legal drinking age, we also estimate a "birthday effect" — that is, the change in victimization risk on a victim's birthday itself or on the following weekend when an individual might celebrate his or her birthday. Estimating this effect is important for two reasons. First, it helps to ensure that our estimates of the causal effect of alcohol access are not merely due to birthday celebration effects. Second, birthday celebration effects are interesting in their own right and serve to bolster our interpretation that legal access to alcohol explains discontinuities in the probability of victimization that we document in Section 5.1.

In practice, we estimate (and control for) birthday celebration effects by adding a dummy variable to (1) that indicates whether date i was the victim's birthday or on the subsequent weekend. We show that the risk of victimization increases dramatically on a victim's 21st birthday as well as on other birthdays. This finding further bolsters our confidence that legal access to alcohol, and the corresponding increase in alcohol consumption previously documented at age 21, is a predominant mechanism driving these large effects.

3.5. Results

3.5.1. Main Results

We study the effect of the reaching the legal drinking age separately for violent crimes (murder, robbery, sexual assault and other assaults) and property crimes (burglary, larceny and motor vehicle theft). We also estimate models separately by gender. Tables 24 and 25 present Poisson regression estimates of the effect of legal access to alcohol on victimization for males and females, respectively.[16] In each cell, we report the incidence rate ratio (IRR) from the Poisson regression model and the robust standard error around the estimate. The first column reports coefficient estimates for the regression outlined in equation (1) using

[16]To address the concern that results may be sensitive to our choice of a Poisson link function, we present corresponding log-linear estimates of the RD treatment effect in Appendix Tables A7 and A8. Results are substantively very similar to the Poisson regression estimates reported in Tables 24 and 25.

an order 1 polynomial in age, interacted with an indicator variable for whether the victim's age was greater than or equal to 21. Columns (2) and (3) include a second order polynomial and a third order polynomial in age, respectively. In Column (4), we focus on the quadratic specification and add a dummy variable for whether an individual is victimized on his or her birthday in order to distinguish legal access to alcohol from birthday celebration effects. Recognizing that birthdays are not always celebrated on an individual's exact birthday, in Column (5), we include the birth date itself and the three following days. In column (6), we include the entire week around the individual's birthday.[17]

For males, legal access to alcohol leads to a 7 percent increase in both violent and property victimization. Effects are especially large for sex offenses (12-120 percent; 74 percent in our preferred model) though these are not precisely estimated as sexual assaults with male victims are relatively uncommon in the data. Effects are also meaningful and significant at conventional levels for robbery (8 percent), non-sexual assault (7 percent), larceny (8 percent) and motor vehicle theft (12 percent). Effects for burglary are close to zero in all specifications. For females, legal access to alcohol does not, in general, increase the likelihood of a violent victimization. However, sex assaults increase considerably — by approximately 24 percent. Property crimes likewise increase — by approximately 12 percent for burglary and larceny. Unlike for males, we find little evidence that motor vehicle theft victimization is sensitive to the MLDA for women. We further note that the estimated effects are, for the most part, not sensitive to our choice of polynomial and persist regardless of how we account for birthday effects.

The estimated effects can be seen graphically in Figure 9 which presents Poisson estimates of the age profile of victimizations with average victimization counts in fourteen-day bins for violent and property crimes, respectively.[18] Both panels in this figure provide visual evidence consistent with the results in Tables 24 and 25. As is evident from Tables 24 and

[17] We also estimate our models including an interaction between our birthday effect variables and the indicator for age over 21. Results are unchanged.

[18] Figure 9 is produced using the specification from Column (2) in Tables 24 and 25.

25, there are noticeable increases in sex assault and larceny victimizations for both men and women as well as non-sexual assault victimizations for men.[19]

3.5.2. Robustness

The results presented in Section 5.1 suggest that the probability of crime victimization changes discontinuously at age 21, an effect that we attribute to the minimum legal drinking age. In this section, we subject these results to greater scrutiny in order to establish that the change in victimization that we observe is the result of legal access to alcohol and not another feature of the social world.

Differential Reporting Behavior

A natural concern in ascribing a causal interpretation to the results reported in Section 5.1 is that these estimates could be an artifact of differential reporting behavior among individuals who have reached the minimum legal drinking age. This might be the case, for instance, if underage victims are less inclined to report a crime to law enforcement due to concerns about being arrested or detained as a result of their own illegal use of alcohol. Such a story is especially worrisome insofar as it could rationalize our principal finding — that victimization increases at age 21.

This differential reporting story is not possible to rule out using our administrative data as these data include only crimes that are known to law enforcement. In order to investigate the plausibility of differential crime reporting by age, we turn to survey data and focus our attention on 18 to 35 year old respondents to the 2006-2016 waves of the National Crime Victimization Survey, the principal source of national data on crime reporting behavior (Lauritsen et al., 2009; Gutierrez and Kirk, 2017). Leveraging the fact that the NCVS captures whether an individual was victimized as well as whether or not she reported a given crime to law enforcement, we observe the extent to which reporting rates change

[19] Using data from San Diego, Denver, St. Louis and Dallas — the cities for which we have time stamps — we explore the extent to which effects are driven by weekday versus weekend victimizations in Appendix Table A12.

discretely at age 21.

Figure 10 shows average violent and property crime reporting rates by age for males and females, respectively, focusing on the ages around age 21. We present reporting rates for violent and property crime aggregates as reporting rates for individual crime types are noisy and therefore lead to a somewhat underpowered test for discontinuous crime reporting rates as a function of the MLDA. For completeness, we present crime reporting rates for individual crime types in the NCVS in Appendix Figure B7. Looking at Figure 10 and Appendix Figure B7, there is little evidence of a discrete change in reporting behavior at 21.[20] The evidence thus provides support for our claim that the changes in victimization that we observe at the minimum legal drinking age are being driven by legal access to alcohol and not by age-graded reporting patterns among crime victims.

Bandwidth Selection

Looking across the columns of Tables 24 and 25, it is straightforward to see that our estimates of the effect of the MLDA on criminal victimization are relatively stable with respect to the polynomial in age as well as with respect to differing controls for birthday celebrations. We next turn to whether our results are sensitive to the choice of bandwidth. In order to test the sensitivity of our preferred estimates to bandwidth selection, we re-estimate outcome models (using an order 2 polynomial) for a range of bandwidths between 180 and 730 days, in 10 day increments. The results of this exercise are presented in Figure 11. In the figure, we report violent victimization estimates in Panel A and property victimization estimates in Panel B. Broadly speaking, the figure demonstrates that results are not substantively driven by a strategic choice of bandwidth. Referring to Panel A, estimates for robbery and assault are reasonably stable. For males, there is a small amount of attenuation as the bandwidth increases, suggesting that our preferred estimates are, if

[20]The NCVS records age at the time of survey, not victimization. As a result, there are likely many instances where the victim's age is mislabeled in terms of when the victimization occurred. Figure 10 and Appendix Figure B7 was produced assuming that each respondent was the same age at survey and victimization.

anything, conservative. For women, consistent with the estimates reported in Table 25, there is insufficient evidence that there are effects for these crimes. For sexual assault, estimates for women are remarkably stable. For men, the estimates attenuate considerably at larger bandwidths but this is likely driven by the large amount of imprecision in these models as reported sex offenses are uncommon among adult males. For property crimes (Panel B), there is little evidence that the victim-gender combinations for which estimates were significant at conventional levels (burglaries with female victims and larcenies for both genders) are sensitive to bandwidth selection.

Sample Selection

While our administrative dataset provides incredibly granular data on the timing of victimization, an inherent limitation is that we observe only those individuals who are victimized by a crime. As such, our estimates could potentially be compromised by sample selection bias — that is, differential selection into the sample local to the minimum legal drinking age. Given that the data we use to draw inferences are victimization counts by relative age, the most pressing concern is that sample selection bias changes the risk of entering our sample as a function of the running variable. In particular, we note the possibility that, upon reaching the minimum legal drinking age, individuals who live in outlying areas become differentially likely to travel to the cities in our sample — for instance, to consume alcohol in bars or nightclubs in the closest large city. To the extent that this is true, there would be more 21 year olds than 20 year olds available to victimize in municipal law enforcement data and, as such, we could observe increased victimization after age 21 that is an artifact of geographic selection rather than a genuine change in the vulnerability of potential victims at age 21. In this section, we offer a formal test for geographic sorting at the MLDA.

Leveraging national data from the NCVS and additional detail in our microdata from Dallas, we offer two different tests for geographic sorting at the minimum legal drinking age. First, using NCVS data, we assess the extent to which the share of victimizations that occurred in a crime victim's county of residence changes as a function of age. These data are plotted in Figure B8 and reveal no clear evidence of an age gradient local to the MLDA.[21] Next, in Dallas, we observe each crime victim's home municipality, allowing us to discern whether the crime victim is a Dallas resident or not. Given that sample selection will be predominantly driven by selection into our sample among non-residents, we focus our attention on crime victims who are *local residents*. The assumption is that, among local residents, we would not expect the number of potential victims to change discontinuously at the threshold of the running variable. This analysis thus offers us a means of testing whether the estimates presented in Tables 24 and 25 are robust to sample selection concerns. While an effect of the MLDA on victimization among this sub-sample provides evidence that sample selection is not driving our main results, we note that to the extent that local residents and non-residents themselves differ with respect to victimization risk, there is, of course, no requirement that the results of this analysis should mirror our main estimates.[22]

We present RD estimates for local residents in Dallas in Table 26. In keeping with the standard model presented in equation (1), we regress the number of victimizations in each relative age cell on an indicator variable representing the age 21 threshold in the running variable and a second order polynomial in age fully interacted with the indicator variable. We account for birthday celebration effects using the week around the individual's birthday. For each model, a positive point estimate indicates that, even among local residents, who are less subject to geographic sorting concerns, victimization rises at the minimum legal drinking age. Referring to the table, there is clear evidence for an increase in property

[21]Since exact dates of birth and victimization are not provided in the NCVS, we note that victimizations that occur at age 21 will apply to individuals who are both age 21 and age 22 at the time they were surveyed by the NCVS. There is no evidence that the share of victimizations in a victim's home county changes abrutply at these ages.

[22]There is, in fact, some evidence that non-residents have greater victimization risk than locals. We explore this possibility in Appendix Figures B9 and B10 which plot RD effects for Dallas residents (Appendix Figure B9) and non-residents (Appendix Figure B10). The available evidence suggests that property crime victimization for both genders and sexual assault victimization for females increases more at the MLDA for non-residents than it does for residents. While this analysis potentially suggests that non-residents engage in riskier behaviors at the legal drinking age, we urge caution in interpreting these results as RD effects for non-residents could potentially be driven by sample selection. Overall, approximately 84 percent of victimizations in Dallas accrue to local residents. Among victims between the ages of 19-23, it is 81 percent.

victimization among both male and female Dallas residents.

For violent offenses, the evidence is less compelling. In particular, we do not see clear evidence for the increase in violent victimization for men that is reported in Table 24 or the increase in sexual assault victimization that we reported in Table 25. That said, the estimates use data from a single city and, as such, are imprecise. There is, therefore, little evidence against the estimates reported in Tables 24 and 25. For example, among male Dallas residents, point estimates suggest an increase in robbery victimization of 5 percent and an increase in assault victimization of 4 percent. These estimates are extremely similar to those reported in Column (6) of Table 24 where we estimate that robbery and assault victimizations rise among men by 8 percent and 7 percent, respectively. For sexual assaults with female victims, our point estimate suggests that these, if anything, decline at the MLDA among Dallas residents. However, the standard error (0.258) is large and, accordingly, the estimate is not inconsistent with the 25 percent increase reported in Table 25. Taken as a whole, our reading of the evidence is that the MLDA continues to have an impact among a sub-sample of potential victims for whom sample selection bias is likely to be small.

Other Robustness Checks

We conduct several additional robustness tests. First, we consider the possibility that the results reported in Section 5.1 might be driven disproportionately by one of the eight cities in our sample and thus might be sensitive to the removal of any one of these cities from the data. In Tables A9 and A10, we re-estimate our preferred models — those that use a quadratic polynomial and are reported in column (2) of Tables 24 and 25 — dropping one city at a time from our data. In all cases, estimates are remarkably insensitive to the exclusion of any one of our eight cities. This analysis is also helpful in addressing the possibility that legal access to recreational marijuana in Denver or medical marijuana in San Diego — both of which occur at the age of 21 — is confounding our results. Dropping either (or both) of these cities from the analysis has no substantive impact on point estimates for any of the crimes we study.

Next, we show that the increase in victimization that we observe at age 21 is unique and is not present at other ages that are, to first order, unaffected by the MLDA. Figure 12 presents RD treatment effects graphically for each age between 19 and 35, using an order 2 polynomial. In the figure, age is plotted on the x-axis and the IRR from the sharp RD regression presented in equation (1) is plotted on the y-axis. Graphs are presented for estimates that were significant at conventional levels in Tables 24 and 25. In each graph, the treatment effects cluster around an IRR of 1, indicating that there is no average treatment effect of legal access to alcohol at ages other than 21. This is exactly what we would expect given that legal alcohol access does not change at any of the other ages. Critically, in all cases, the treatment effect at age 21 is the largest among all of the ages estimated which indicates that the RD effect at age 21 is unusual and therefore provides key support for the prior estimates.

Randomization Inference

Finally, we conduct a randomization inference exercise in order to provide further support for the estimates derived from our parametric models. We hold the observation window fixed at two years on either side of the MLDA cutoff at age 21. We perform randomization inference using 1,000 replications for each of the crime type by gender combination for which we estimate a significant effect of the MLDA on victimization. Because the slope in age for victimization is often large, we residualize our log victimization counts prior to randomly assigning ages to each count.[23] For each replication, we randomly shuffle age relative to the MLDA and estimate log-linear RD model on the residualized log count of victims.[24]

Figure 13 presents histograms of the distributions of the t-ratios for each estimate of the

[23]We residualize by estimating our log-linear model with a second order polynomial in age interacted with an indicator for age over 21. This model includes all explanatory variables except D_i, our indicator for age over 21.

[24]This corresponds to Column 2 in Tables A7 and A8.

RD effect with a dashed gray line indicating the 95th percentile of the distribution of randomization and a black line indicating the t-ratio for our true model. The ranking of the true t-ratio in the distribution of placebo estimates yields an implied p-value based on the simulated sampling distribution. It is evident in Figure 13 that our true t-ratio is well to the right of the 95th percentile of the distribution of randomization for all sub-plots in each panel indicating that these results are unlikely to be due to chance.

3.5.3. Extensions

Having considered the robustness of our main results, we next consider two extensions which provide further context for these results. First, we consider the potential mechanisms through which legal access to alcohol increases crime victimization. Next, we consider the extent to which crime victimization rises, in general, around an individual's birthday — whether that birthday is an individual's 21st or not. The latter analysis provides further support for the salience of alcohol consumption (both legal and illegal) in creating opportunities for victimization.

Location of Victimization

There are two primary mechanisms through which legal access to alcohol might affect victimization: exposure and vulnerability. By exposure, we are referring to the change in alcohol *access* that occurs at age 21, understanding that even though minors regularly access alcohol before reaching the minimum legal drinking age, the ease through which alcohol can be accessed changes at age 21 (Carpenter and Dobkin, 2009). By vulnerability, we are referring to the change in the ways in which alcohol is consumed at the MLDA, independent of any change in exposure. In particular, does legal access to alcohol increase victimization by shifting the location of problematic drinking (e.g., drinking in bars of nightclubs) or does legal alcohol access operate primarily by increasing alcohol use?

In order to better understand the mechanisms through which the MLDA affects victimization, we estimate treatment effects separately for crimes that occur in residential versus non-residential locations. [25] Figure 14 reports the percentage of victimizations for each age, gender, and crime type combination that occur in residential locations. The share of victimizations that occur in residential locations varies considerably by gender and crime type. Robberies are uncommon in residential locations, a finding which is, in large part, an artifact for how robberies are defined by the Federal Bureau of Investigation and accordingly by municipal law enforcement agencies. For assaults, around one third of assaults with a male victim and just over half of assaults with a female victim occur in residential locations. Sexual offenses are evenly split between residential and non-residential locations. For property crimes, among individuals who are proximate in age to the MLDA, the majority of larcenies and motor vehicle thefts occur in non-residential settings. Burglaries, by definition, occur in residential locations.[26]

In Table 27, we report effects for residential and non-residential crimes both with (columns 3 and 4) and without (columns 1 and 2) a control variable for the birthday celebration effect. As in previous analyses, we further disaggregate results by crime type and gender. For males, effects on violent victimization are, on the whole, driven by crimes that occur in non-residential locations. This is especially true for assaults and also, to a lesser extent, for robberies.[27] Effects on property victimization vary less by location type. For females, the large effects for sex offenses are likewise driven by non-residential locations while effects for larceny are equally large in both location types. Taken as a whole, the data suggest that increases in victimization are, at least to an extent, driven by the fact that alcohol use is more likely to occur in non-residential settings after individuals have reached the legal drinking age.

More broadly, some types of crimes, for instance burglary, might largely capture the ex-

[25]Location information was shared by the following 5 police departments in our sub-sample: Dallas, TX, Denver, CO, Houston, TX, Milwaukee, WI and St. Louis, MO. Appendix E contains the department-specific location tags that we consider to be "residential."

[26]We exclude commercial burglaries from our data as victim information is often unavailable and, when it is, it is unclear what relationship the indicated victim has to the event. In many cases, we suspect the indicated victim may be a business manager.

[27]The same is true for sex offenses though sparse data means that the results are estimated with only limited precision.

posure or away from effect. While the effect of the MLDA is large and significant for burglary, this point estimate is considerably smaller than the point estimate for the most social costly crimes. This suggests while exposure could account for some of the impacts on sexual assaults, it likely would not account for the majority of the estimated impacts.

Birthday Celebration Effects

A large literature in public health establishes that individuals are more likely to consume alcohol in both public and private on their birthdays — especially at age 21 (Neighbors et al., 2005; Brister et al., 2010). Until this point, we have conditioned on the period of time just around a victim's birthday in order to more reliably identify the effect of the MLDA. In this section, we recognize that birthday effects are interesting in their own right. We therefore investigate whether there are "birthday effects," that is, a general increase in victimization on or around an individual's birthday, independent of an intercept shift in the incidence of victimization that occurs at age 21 and endures in the ensuing weeks and months.

We estimate birthday effects by adding three different sets of birthday-related indicators to our main RD models — an indicator for an individual's birthday, an indicator for a an individual's birthday and the following three days and an indicator for the week around an individual's birthday.[28] Table 28 presents Poisson estimates of the change in the likelihood of victimization on or around an individual's birthday, an effect we describe as a birthday celebration effect and as distinct from the broader and more enduring effect of legal access to alcohol.[29]

In Table 28, the first three columns present estimates for males, the next three columns for females. Each column corresponds to a different definition of the birthday celebration window. We report incidence rate ratios and robust standard errors in parentheses. Birth-

[28] All birthday effects are estimated with a quadratic polynomial in age interacted with an indicator for being 21 or older at the time of victimization.

[29] Appendix Table A11 reports log-linear estimates of the birthday effect.

day celebration effects are very large — overall, men are nearly 30 percent more likely to suffer a violent victimization and 10 percent more likely to suffer a property victimization on or around their birthdays. Effects are similar for women. Both genders are more likely to be assaulted. For women, sexual assault effects are particularly large with a 60 percent increase in the likelihood of suffering a sexual assault on one's birthday.

In order to investigate whether the birthday celebration effect is unique to age 21, we re-estimate birthday celebration effects (using the exact birthday) for all ages between 19 and 35. These estimates are presented in Figure 15, which plots incidence rate ratios on the y-axis against the victim's birthday in years on the x-axis. These figures support the idea that birthday celebration effects are not unique to age 21 and are instead universal, persisting throughout an individual's life. Indeed, celebration effects at age 21 are not of unusual magnitude and fall roughly in the middle of the distribution of estimated effects. This finding thus provides further support for the idea that alcohol use is a driver of crime victimization more generally and that this effect persists beyond age 21.

3.6. Conclusion

A large body of research has explored the causal determinants of criminality. While victimization is an equally important side of the same coin, due to data constraints, this topic has received far less attention in the literature. Given that recent media attention and research related to criminal justice highlight the high social costs of over-policing (Fagan et al., 2016) and the widespread application of harsh prison sentences (Aizer and Doyle Jr, 2015), there is increasing appeal to understanding whether other policies can affect crime while potentially imposing fewer costs.

In this paper, we study one prominent policy lever that operates through private precaution and which could plausibly have an outsize influence on victimization: legal access to alcohol. We construct a novel administrative dataset that contains the exact date of birth and date of victimization for crime victims in eight large cities in the United States and use a regression discontinuity design to estimate the change in victimization that occurs at age 21, the minimum legal drinking age in the United States.

We find evidence that victimization increases at age 21 for for both males and females, though in subtly different ways. Males experience a greater number of assaults and robberies; females experience a large increase in the risk of a sexual assault. Victims of both genders experience a modest increase in the incidence of property crimes. Results are robust to empirical specification, bandwidth selection and controls for birthday celebration effects. We explore the overall impact of legal access to alcohol in Table A13. Using age-specific victimization rates from the NCVS, we apply our estimated treatment effects to victimization rates for 21-22 year olds. We estimate that legal access to alcohol local to the cutoff leads to an increase of approximately 300,000 victimizations per year, an increase of between one and two percent.

The likely mechanisms behind these increases in victimization are varied and include differences in the amount of alcohol that is consumed after reaching the legal drinking age and differences in the environment in which alcohol is consumed. Given that effects are largest in non-residential locations, there is some evidence for the latter of these two mechanisms. Effects do not appear to be an artifact of increased reporting of crimes at age 21. [30]

This research provides some of the first causal evidence that alcohol increases crime victimization. Our findings suggest that prior estimates based on arrests (Carpenter and Dobkin, 2015) or criminal charges (Hansen and Waddell, 2018) likely underestimate the effect of alcohol on total crime. Our findings can also potentially reconcile the reason why regression discontinuity based estimates of arrests using the minimum legal drinking age are typically

[30] The effects of legal access to alcohol that we report in this paper are qualitatively large and empirically important. However, these estimates potentially point to an even larger role of *alcohol use* in crime victimization. To see this, consider that the reduced form estimates reported in this paper can be seen as intent-to-treat estimates of the effect of alcohol use, understanding that alcohol will tend to change discontinuously, albeit imperfectly, with legal access to alcohol. The magnitude of the effect of alcohol *use* on victimization will thus depend on the first stage relationship between the MLDA and alcohol use. Thus, subject to an assumption that the MLDA affects victimization only through the increased use of alcohol, our results can be seen a conservative estimate of the aggregate impact of problematic alcohol use on crime victimization.

smaller than recent differences-in-difference estimates (Anderson et al., 2017). The local average treatment effect (LATE) of the former is based on criminality, and the LATE of the latter is based on the combination of victimization and criminality. Our LATE, identifies the impact of the MLDA on victimization alone. Finally, these findings provide additional insights into the complex and controversial relationship between alcohol and sexual assault (Lindo et al., 2018). In particular, while both Carpenter and Dobkin (2015) and Hansen and Waddell (2018) fail to find evidence that arrests or criminal charges for rape increase at age 21, we find sexual assault victimization at age 21 increases by nearly 25 percent. Taken together, these findings are more consistent with a model of crime in which perpetrators of sexual assault seek out vulnerable populations than with a model where sexual assault perpetrators lose their self control due to increased alcohol use.

More generally, this research highlights the possibility that information interventions that educate the public about its increased risk of victimization and encourage individuals to invest in private precautions to prevent victimization may help mitigate the effects of alcohol access on criminal victimization. Behavioral changes such as remaining cautious of one's surroundings, avoiding walking home alone or taking a taxi in lieu of walking, avoiding violence when faced with conflict, locking one's door immediately after returning home and being particular about the degree to which one associates with strangers while drinking all have the potential to reduce criminal victimization. To be clear, we are not suggesting a campaign of victim-blaming. On the contrary, information is a means of empowering potential victims to better protect themselves. It is also a means through which public safety interventions can be optimally tailored to achieve maximum impact on social welfare.

The possibility of raising the drinking age to reduce the social cost of alcohol use is a possibility that should be taken with caution as it is unclear whether the United States' unique cultural relationship with alcohol is a by-product of its drinking age being 21.[31] As it stands, our estimates suggest that the increased consumption of alcohol at age 21 is met

[31] If this is the case, a policy that raises the drinking age might have a negative general equilibrium impact on the binge-drinking culture that is common in the US.

with additional costs previously not considered. Moreover, there are a number of other policies with considering that may interact with both exposure and alcohol consumption mechanisms which shift at age 21. These include zoning and licensing, operating hours restrictions, and alcohol taxes. Future research could investigate how these or other policies.

Moreover, the choices and precautions of individuals could carry externalities to others. As an individual engages in precautions, this has a small effect on the returns and costs to engaging in crime for potential offenders. Aggregated, this would suggest the private supply precautions would be under-supplied relative to what is socially optimal, even if we assumed individuals were privately optimizing. Thus private precautions like locks, private security cameras, alarms or GPS anti-theft trackers might merit subsidies. Moreover, this is further justification for taxes on alcohol, which have remained largely unchanged in nominal value since the 1990s and whose externality offsetting effects have likely been eroded by inflation (Cook and Durrance, 2013). Future research could investigate whether other alcohol control policies such as taxes are also effective in reducing victimization.

3.7. Tables

Table 23: Crime Rates in the Study Sample (2016)

	Study Sample	Cities > 250K Population	United States
Population	7,850,000	75,300,000	321,000,000
Violent Crimes			
Murder	14.6	9.9	4.4
Rape	62.0	49.2	26.6
Robbery	356.7	226.3	101.3
Assault	1213.8	388.3	229.2
Property Crimes			
Burglary	730.7	542.3	537.2
Larceny	2621.8	2135.0	1821.5
Motor vehicle theft	624.3	388.9	215.4

Note: Data were obtained from the compilation of the Federal Bureau of Investigation's Uniform Crime Reports made available on ICPSR by Kaplan (2019).

Table 24: Poisson Male RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	1.054***	1.069***	1.098***	1.067***	1.064***	1.068***
	(0.0165)	(0.0251)	(0.0343)	(0.0250)	(0.0241)	(0.0246)
Homicide	0.998	0.781	0.705	0.781	0.769	0.779
	(0.121)	(0.148)	(0.165)	(0.148)	(0.145)	(0.147)
Sex Offenses	1.119	1.744*	2.204*	1.753*	1.762*	1.742*
	(0.234)	(0.531)	(0.948)	(0.534)	(0.539)	(0.529)
Robbery	1.048*	1.078*	1.139**	1.078*	1.080*	1.078*
	(0.0278)	(0.0424)	(0.0585)	(0.0424)	(0.0426)	(0.0425)
Assault	1.058***	1.067**	1.083*	1.063**	1.058*	1.065**
	(0.0212)	(0.0325)	(0.0452)	(0.0321)	(0.0309)	(0.0317)
Property						
All	1.023	1.074***	1.077***	1.069***	1.066***	1.069***
	(0.0142)	(0.0222)	(0.0302)	(0.0221)	(0.0219)	(0.0217)
Burglary	1.003	1.047	0.976	1.021	1.023	1.023
	(0.0307)	(0.0493)	(0.0620)	(0.0493)	(0.0496)	(0.0495)
Larceny	1.032*	1.079***	1.108***	1.066**	1.063**	1.066**
	(0.0181)	(0.0280)	(0.0385)	(0.0297)	(0.0294)	(0.0293)
Motor Vehicle Theft	1.007	1.091	1.089	1.121**	1.113**	1.121**
	(0.0402)	(0.0682)	(0.0914)	(0.0542)	(0.0534)	(0.0533)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 25: Poisson Female RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	1.008	1.017	1.002	1.014	1.013	1.016
	(0.0122)	(0.0190)	(0.0251)	(0.0189)	(0.0187)	(0.0188)
Homicide	1.274	1.437	2.007	1.428	1.418	1.429
	(0.409)	(0.685)	(1.389)	(0.676)	(0.669)	(0.674)
Sex Offenses	1.170***	1.249***	1.137	1.233**	1.226**	1.240**
	(0.0649)	(0.108)	(0.137)	(0.105)	(0.102)	(0.105)
Robbery	1.014	1.034	1.015	1.033	1.034	1.033
	(0.0360)	(0.0568)	(0.0771)	(0.0569)	(0.0572)	(0.0567)
Assault	0.998	1.002	0.992	1.000	0.999	1.002
	(0.0129)	(0.0198)	(0.0258)	(0.0198)	(0.0196)	(0.0198)
Property						
All	1.036***	1.108***	1.091***	1.103***	1.099***	1.106***
	(0.0135)	(0.0216)	(0.0289)	(0.0207)	(0.0200)	(0.0204)
Burglary	1.010	1.116***	1.068	1.117***	1.115***	1.119***
	(0.0259)	(0.0424)	(0.0525)	(0.0443)	(0.0444)	(0.0442)
Larceny	1.049***	1.117***	1.114***	1.119***	1.115***	1.124***
	(0.0166)	(0.0270)	(0.0371)	(0.0265)	(0.0256)	(0.0265)
Motor Vehicle Theft	0.988	1.022	0.976	1.021	1.015	1.018
	(0.0413)	(0.0638)	(0.0803)	(0.0482)	(0.0479)	(0.0477)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on female victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 26: Poisson RD Effects For Local Residents, Dallas Subsample

	Male		Female	
	(1)	(2)	(3)	(4)
Violent				
All	1.045	1.045	0.973	0.973
	(0.0605)	(0.0604)	(0.0404)	(0.0404)
Homicide	0.816	0.819	0.475	0.510
	(0.526)	(0.528)	(1.355)	(1.461)
Sex Offenses	6.726	6.627	0.910	0.912
	(7.999)	(7.857)	(0.256)	(0.258)
Robbery	1.050	1.050	0.686**	0.686**
	(0.121)	(0.121)	(0.122)	(0.122)
Assault	1.038	1.038	0.997	0.997
	(0.0695)	(0.0694)	(0.0437)	(0.0436)
Property				
All	1.141*	1.141*	1.191***	1.190***
	(0.0818)	(0.0818)	(0.0723)	(0.0723)
Burglary	1.106	1.107	1.420***	1.420***
	(0.158)	(0.159)	(0.158)	(0.158)
Larceny	1.380**	1.374**	1.258*	1.257*
	(0.222)	(0.221)	(0.156)	(0.155)
Motor Vehicle Theft	1.100	1.101	1.029	1.029
	(0.0963)	(0.0964)	(0.0882)	(0.0881)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime type for residents of Dallas. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Even numbered columns include indicator variables for the week around birthdays. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 27: Poisson RD Effects – Residential vs. Non-Residential

	(1)	(2)	(3)	(4)
	No Birthday Control		Birthday Effect Control	
	Residential	Non-Residential	Residential	Non-Residential
	Panel A: Males			
Violent				
All	0.989	1.098***	0.988	1.097***
	(0.0493)	(0.0342)	(0.0486)	(0.0340)
Homicide	0.652	0.780	0.653	0.780
	(0.310)	(0.178)	(0.306)	(0.178)
Sex Offenses	0.431	5.905***	0.431	5.957***
	(0.251)	(2.843)	(0.252)	(2.886)
Robbery	1.172	1.085*	1.170	1.086*
	(0.159)	(0.0536)	(0.157)	(0.0540)
Assault	0.976	1.103**	0.974	1.102**
	(0.0507)	(0.0470)	(0.0502)	(0.0464)
Property				
All	1.087*	1.056*	1.087*	1.054*
	(0.0494)	(0.0295)	(0.0493)	(0.0285)
Burglary	1.086		1.086	
	(0.0629)		(0.0630)	
Larceny	1.089	1.026	1.085	1.025
	(0.103)	(0.0346)	(0.102)	(0.0340)
Motor Vehicle Theft	1.089	1.126**	1.095	1.123**
	(0.268)	(0.0589)	(0.271)	(0.0578)
	Panel B: Females			
Violent				
All	1.020	1.014	1.020	1.013
	(0.0288)	(0.0295)	(0.0289)	(0.0293)
Homicide	0.337	3.565*	0.333	3.447*
	(0.344)	(2.421)	(0.343)	(2.283)
Sex Offenses	1.124	1.248	1.121	1.247
	(0.158)	(0.171)	(0.156)	(0.170)
Robbery	1.111	1.000	1.113	0.999
	(0.161)	(0.0611)	(0.161)	(0.0606)
Assault	1.013	1.001	1.013	1.001
	(0.0294)	(0.0337)	(0.0295)	(0.0337)
Property				
All	1.108***	1.113***	1.106***	1.111***
	(0.0417)	(0.0305)	(0.0414)	(0.0293)
Burglary	1.131**		1.128**	
	(0.0545)		(0.0540)	
Larceny	1.093	1.140***	1.091	1.137***
	(0.0724)	(0.0355)	(0.0722)	(0.0341)
Motor Vehicle Theft	0.742	1.034	0.748	1.032
	(0.194)	(0.0515)	(0.198)	(0.0512)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime and location type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Columns (3) and (4) include indicator variables for the week around birthdays. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 28: Poisson Birthday Effects

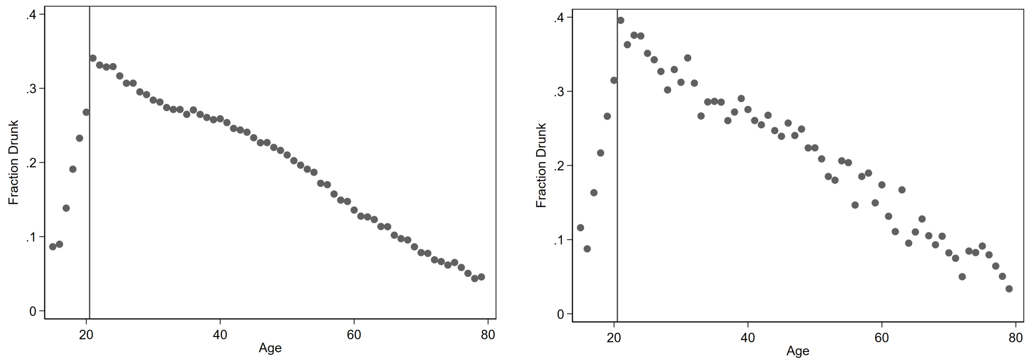
	(1)	(2)	(3)	(4)	(5)	(6)
		Male			**Female**	
	Birthday 1	Birthday 2	Birthday 3	Birthday 1	Birthday 2	Birthday 3
Violent						
All	1.148***	1.128***	1.074**	1.167***	1.093***	1.054**
	(0.0437)	(0.0421)	(0.0341)	(0.0547)	(0.0332)	(0.0246)
Homicide	0.850	1.429	1.243	1.567	1.417	1.439
	(0.519)	(0.369)	(0.244)	(1.430)	(0.762)	(0.737)
Sex Offenses	0.637	0.777	1.045	1.661***	1.377**	1.207*
	(0.535)	(0.340)	(0.370)	(0.252)	(0.174)	(0.131)
Robbery	0.945	0.942	0.950	1.075	1.007	1.048
	(0.132)	(0.0652)	(0.0525)	(0.219)	(0.0899)	(0.0695)
Assault	1.261***	1.218***	1.131***	1.149***	1.087***	1.045*
	(0.0515)	(0.0522)	(0.0425)	(0.0434)	(0.0342)	(0.0264)
Property						
All	1.133**	1.114***	1.099***	1.368***	1.232***	1.142***
	(0.0623)	(0.0400)	(0.0278)	(0.0481)	(0.0387)	(0.0331)
Burglary	1.194***	1.010	1.042	1.291***	1.148***	1.132***
	(0.0426)	(0.0819)	(0.0697)	(0.0534)	(0.0573)	(0.0440)
Larceny	1.088	1.099**	1.086***	1.461***	1.279***	1.162***
	(0.0951)	(0.0441)	(0.0281)	(0.0585)	(0.0504)	(0.0422)
Motor Vehicle	1.281***	1.322***	1.253***	0.730***	1.002	1.005
Theft	(0.0524)	(0.110)	(0.0707)	(0.0581)	(0.0801)	(0.0722)

This table contains IRR estimates for the birthday effect on male and female victimization rates for each crime type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.8. Figures

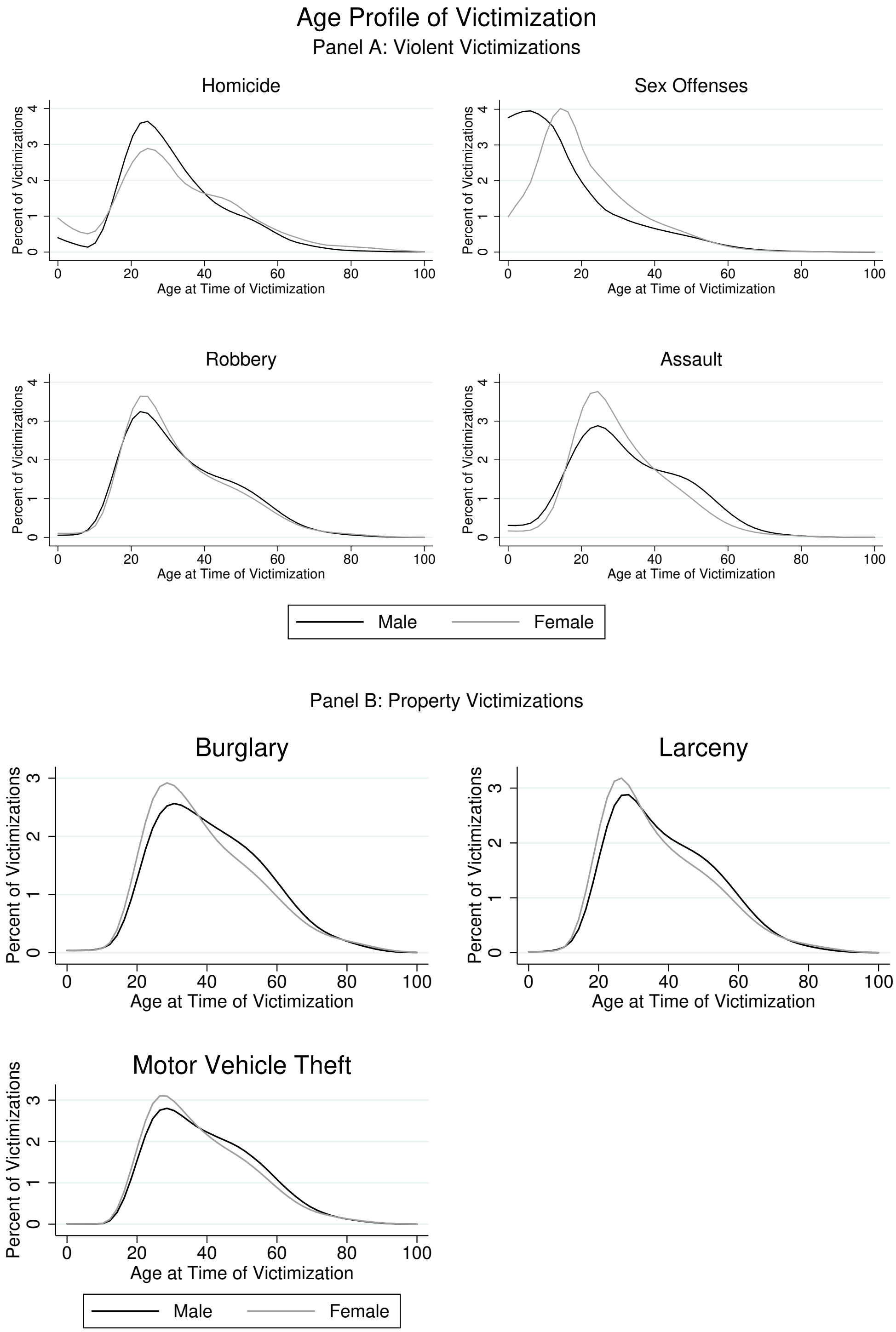
Figure 7: First Stage

A. Nationwide B. Victimization Sub-sample



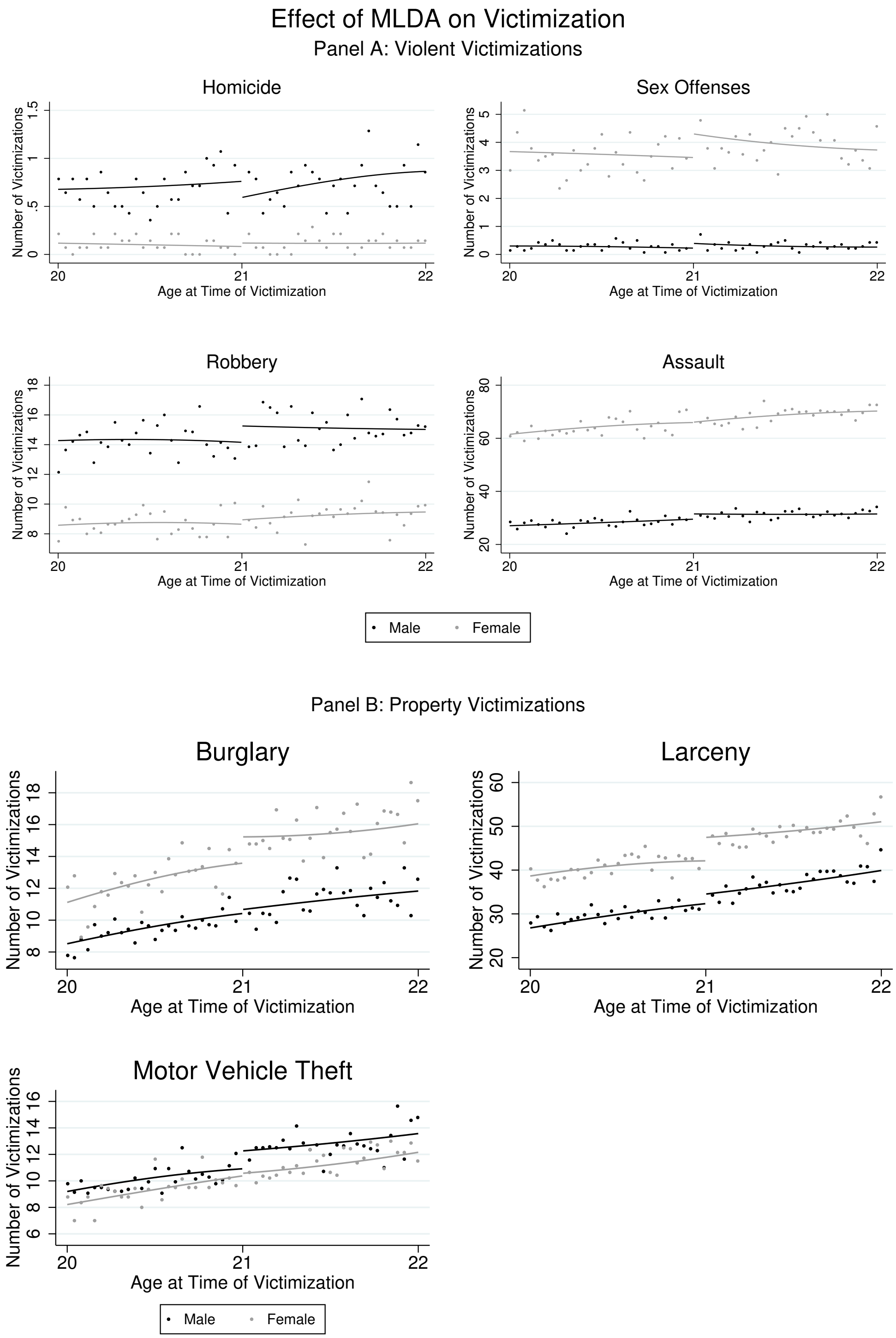
This figure plots the fraction of fatal accidents involving alcohol both nationwide (Panel A) and for the sub-sample of cities in our data (Panel B). Both series of data are based on fatal accidents from 2000 to 2016.

Figure 8: Age Profile of Victimization



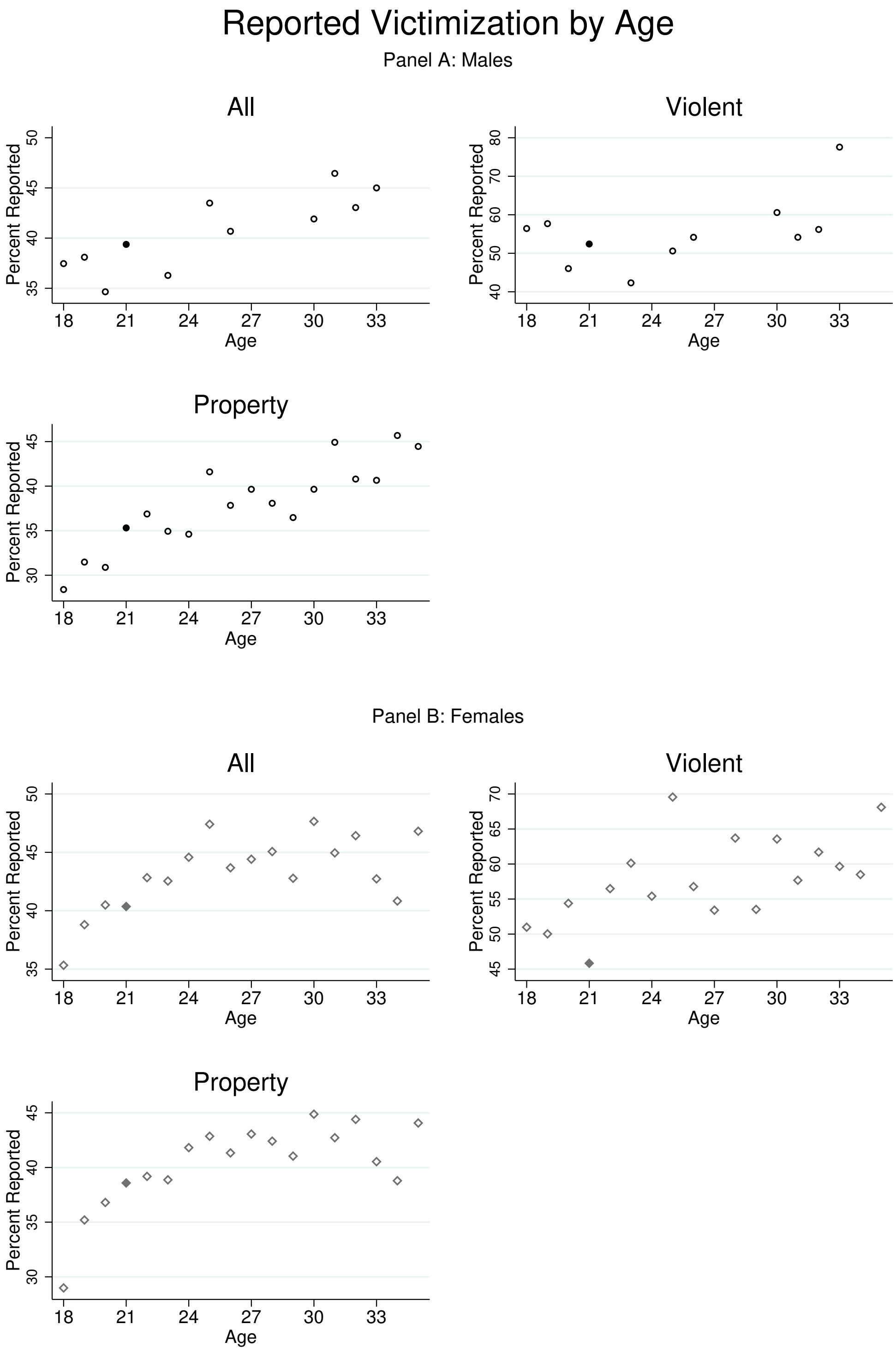
This figure presents local polynomial regressions of age on the share of victimizations in the data. Each observation is the share of all victimizations within crime type and gender that occur at a given age.

Figure 9: Main Results



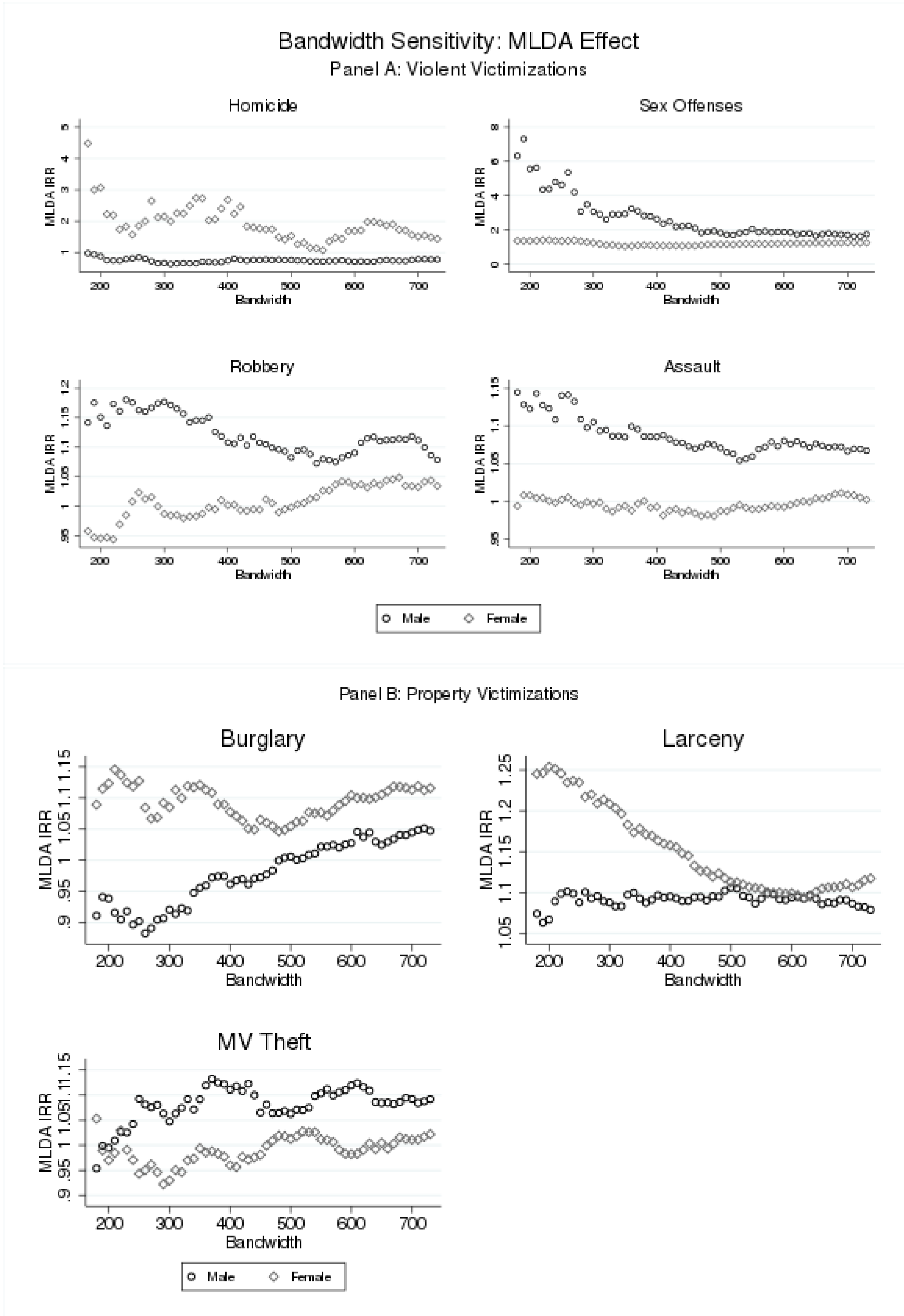
This figure contains fitted Poisson estimates and average victimization counts in 14 day bins. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

Figure 10: Reporting Behavior



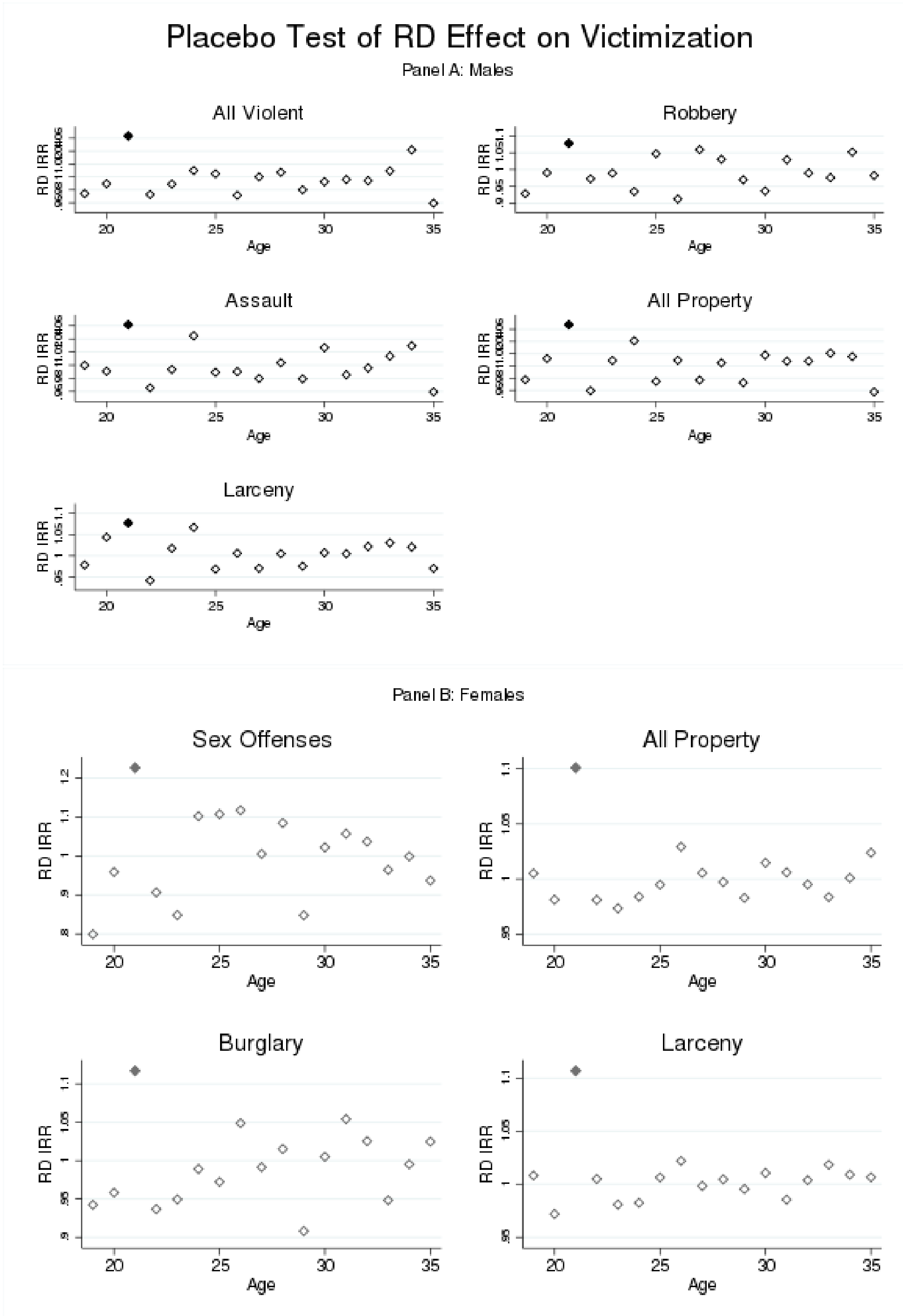
This figure presents the share of victimizations reported to law enforcement among respondents to the 2006-2016 waves of the National Crime Victimization Survey.

Figure 11: Bandwidth Sensitivity



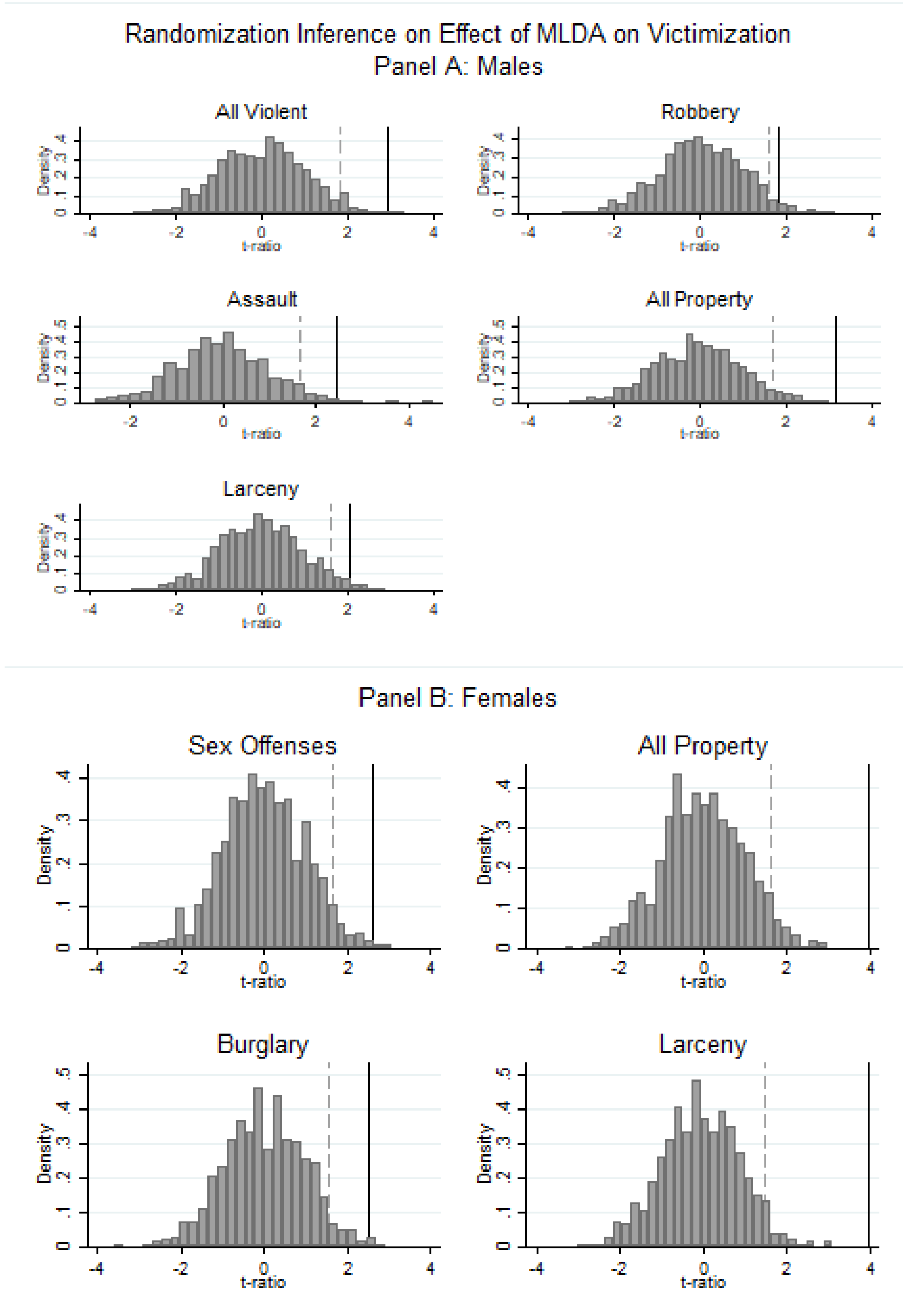
This figure presents estimates of the sensitivity of the RD estimates presented in Tables 2 and 3 to the choice of bandwidth. In the figures, the bandwidth is plotted on the x-axis; incident rate ratios are reported on the y-axis. In each case, regressions include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

Figure 12: RD Effects



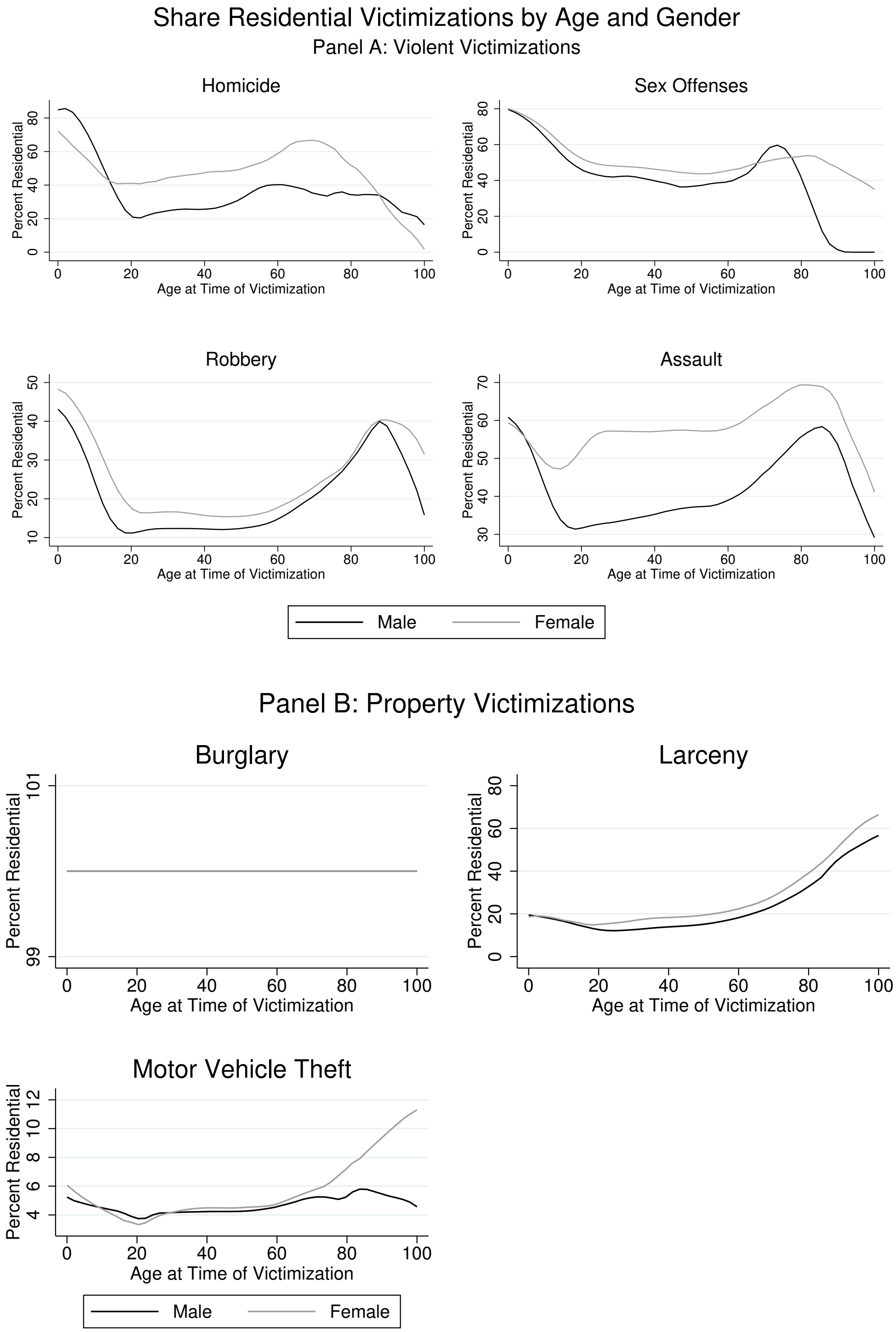
This figure contains IRR estimates for the RD effect of the MLDA on victimization for each age from 19 to 35. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

Figure 13: Randomization Inference



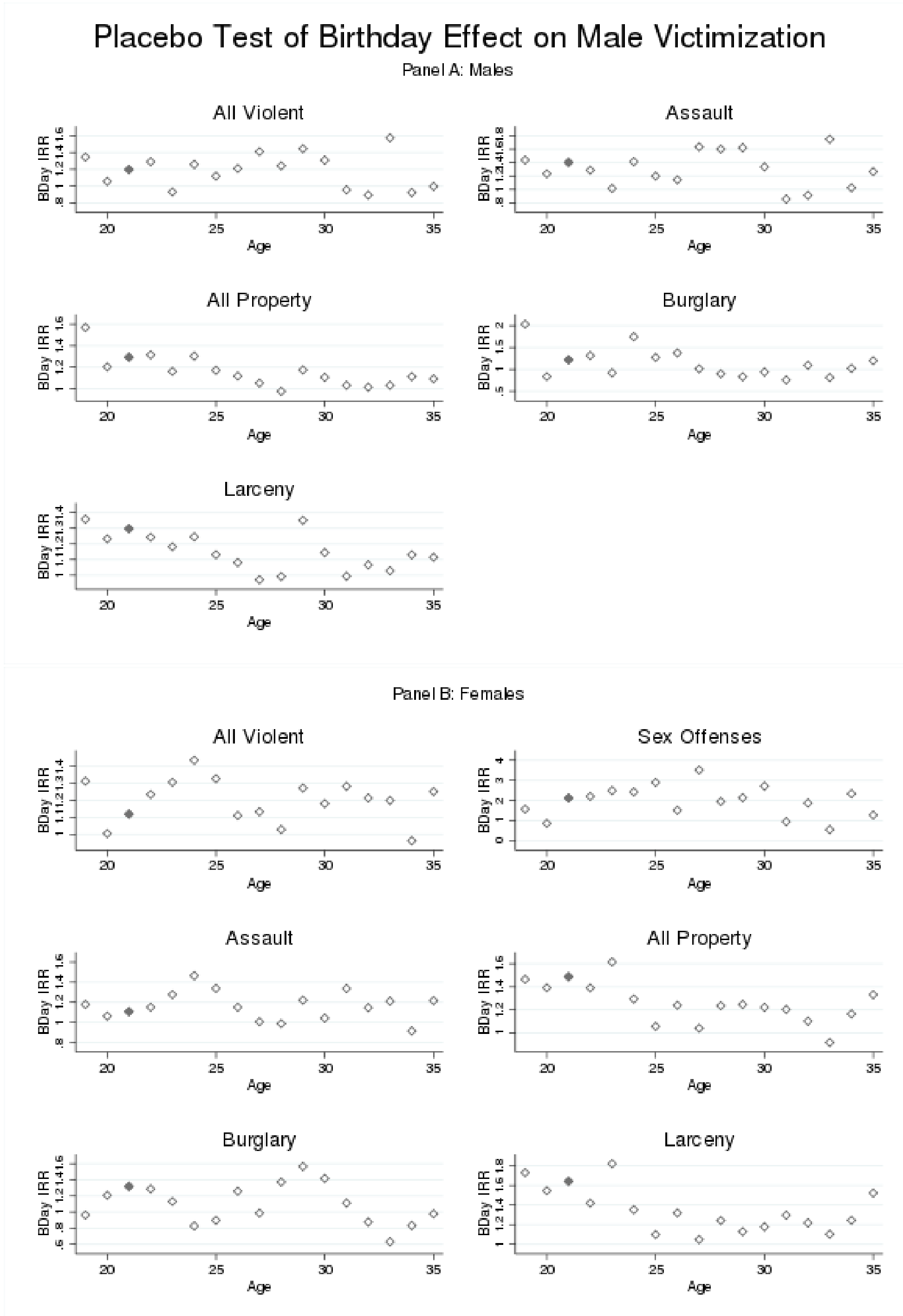
This histogram contains t-ratios from 1000 randomization inference replications of the effect of the MLDA on victimization. The 95th percentile of the distribution is indicated with a dashed gray line and the t-ratio from the true model is indicated with a black line.

Figure 14: Share of Victimizations by Age



This figure contains local polynomial regressions of age on the share of victimizations. Each observation is the percentage of victimizations at a given age that occur in a residental location.

Figure 15: Birthday Effects



This figure contains IRR estimates for the birthday celebration effect for each age from 19 to 35. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

3.9. Supplementary Tables

Table A7: Log-Linear Male RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	0.0533***	0.0695***	0.0897***	0.0677***	0.0653***	0.0687***
	(0.0158)	(0.0235)	(0.0309)	(0.0234)	(0.0229)	(0.0232)
Homicide	-0.00837	-0.0893	-0.124	-0.0887	-0.0947	-0.0903
	(0.0428)	(0.0647)	(0.0863)	(0.0648)	(0.0646)	(0.0646)
Sex Offenses	0.0154	0.0796*	0.105*	0.0803*	0.0808*	0.0795*
	(0.0298)	(0.0445)	(0.0602)	(0.0446)	(0.0447)	(0.0445)
Robbery	0.0473*	0.0758*	0.139**	0.0765*	0.0782*	0.0765*
	(0.0282)	(0.0415)	(0.0544)	(0.0416)	(0.0417)	(0.0417)
Assault	0.0620***	0.0733**	0.0791*	0.0703**	0.0662**	0.0719**
	(0.0203)	(0.0302)	(0.0407)	(0.0300)	(0.0294)	(0.0298)
Property						
All	0.0162	0.0661***	0.0733**	0.0644***	0.0625***	0.0651***
	(0.0144)	(0.0211)	(0.0284)	(0.0209)	(0.0209)	(0.0207)
Burglary	-0.0285	0.0137	-0.0637	0.0108	0.0142	0.0136
	(0.0345)	(0.0513)	(0.0685)	(0.0513)	(0.0515)	(0.0514)
Larceny	0.0181	0.0592**	0.0965**	0.0576**	0.0550*	0.0580**
	(0.0197)	(0.0291)	(0.0386)	(0.0290)	(0.0288)	(0.0286)
Motor Vehicle Theft	0.0397	0.121**	0.115*	0.119**	0.116**	0.120**
	(0.0341)	(0.0512)	(0.0677)	(0.0511)	(0.0512)	(0.0507)

This table contains estimates for the RD effect of the minimum legal drinking age on male victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicators for exact birthdays. Birthday 2 includes indicators for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the natural log of the total number of victims in each age (days) relative to the 21st birthday. Adjustment from Chalfin and McCrary (2018) used when necessary. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Log-Linear Female RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	0.00516	0.0160	0.00125	0.0139	0.0127	0.0153
	(0.0123)	(0.0187)	(0.0252)	(0.0187)	(0.0185)	(0.0186)
Homicide	0.0132	0.0186	0.0340	0.0181	0.0176	0.0183
	(0.0175)	(0.0254)	(0.0348)	(0.0252)	(0.0251)	(0.0252)
Sex Offenses	0.174***	0.236***	0.153	0.229**	0.225**	0.234***
	(0.0588)	(0.0909)	(0.125)	(0.0904)	(0.0900)	(0.0906)
Robbery	0.000943	0.0411	0.00140	0.0405	0.0414	0.0407
	(0.0387)	(0.0591)	(0.0813)	(0.0593)	(0.0597)	(0.0591)
Assault	-0.00380	0.00246	-0.00726	0.000585	-0.000605	0.00193
	(0.0131)	(0.0198)	(0.0262)	(0.0198)	(0.0197)	(0.0198)
Property						
All	0.0335**	0.106***	0.0840***	0.103***	0.0996***	0.105***
	(0.0133)	(0.0196)	(0.0264)	(0.0190)	(0.0185)	(0.0186)
Burglary	0.00367	0.109**	0.0660	0.105**	0.103**	0.107**
	(0.0294)	(0.0432)	(0.0573)	(0.0431)	(0.0432)	(0.0430)
Larceny	0.0576***	0.123***	0.116***	0.118***	0.115***	0.121***
	(0.0165)	(0.0248)	(0.0338)	(0.0240)	(0.0234)	(0.0237)
Motor Vehicle Theft	-0.0416	0.0303	-0.0401	0.0320	0.0266	0.0293
	(0.0367)	(0.0537)	(0.0694)	(0.0538)	(0.0538)	(0.0536)

This table contains estimates for the RD effect of the minimum legal drinking age on female victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicators for exact birthdays. Birthday 2 includes indicators for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the natural log of the total number of victims in each age (days) relative to the 21st birthday. Adjustment from Chalfin and McCrary (2018) used when necessary. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Poisson Male RD Effects, Excluding One City

	Excluded City							
	(1) Charlotte	(2) Dallas	(3) Denver	(4) Houston	(5) Kansas City	(6) Milwaukee	(7) San Diego	(8) St. Louis
Violent								
All	1.115	1.083***	1.071	1.071	1.103*	1.071	1.007	1.071
	(0.0873)	(0.0295)	(0.0559)	(0.0559)	(0.0598)	(0.0559)	(0.0645)	(0.0559)
Homicide	0.318	0.823	0.952	0.952	1.024	0.952	1.644	0.952
	(0.245)	(0.178)	(0.481)	(0.481)	(0.565)	(0.481)	(0.976)	(0.481)
Sex Offenses	1.065	1.411	1.867	1.867	2.041	1.867	2.664	1.867
	(1.188)	(0.463)	(1.247)	(1.247)	(1.367)	(1.247)	(2.234)	(1.247)
Robbery	1.042	1.081*	1.002	1.002	1.025	1.002	0.961	1.002
	(0.185)	(0.0468)	(0.0983)	(0.0983)	(0.104)	(0.0983)	(0.110)	(0.0983)
Assault	1.160*	1.087**	1.093	1.093	1.126*	1.093	1.012	1.093
	(0.104)	(0.0381)	(0.0662)	(0.0662)	(0.0711)	(0.0662)	(0.0763)	(0.0662)
Property								
All	1.112	1.059**	1.093*	1.093*	1.081	1.093*	1.098*	1.093*
	(0.0757)	(0.0251)	(0.0505)	(0.0505)	(0.0520)	(0.0505)	(0.0623)	(0.0505)
Burglary	0.864	1.003	0.903	0.903	0.889	0.903	0.957	0.903
	(0.112)	(0.0535)	(0.0789)	(0.0789)	(0.0790)	(0.0789)	(0.108)	(0.0789)
Larceny	1.236**	1.069**	1.179***	1.179***	1.168**	1.179***	1.159**	1.179***
	(0.110)	(0.0326)	(0.0696)	(0.0696)	(0.0708)	(0.0696)	(0.0832)	(0.0696)
MV Theft	1.213	1.096	1.118	1.118	1.107	1.118	1.100	1.118
	(0.337)	(0.0681)	(0.157)	(0.157)	(0.180)	(0.157)	(0.155)	(0.157)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male victimization rates for each rates for each crime type. All regressions include second order polymonials in age fully interacted with an indicator for age over 21. Column labels indicate the city whose data is excluded from the regressions. Each observation is the total number of victims in each age (days) relative to the 21st birthday. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Poisson Female RD Effects, Excluding One City

	Excluded City							
	(1) Charlotte	(2) Dallas	(3) Denver	(4) Houston	(5) Kansas City	(6) Milwaukee	(7) San Diego	(8) St. Louis
Violent								
All	1.056	1.029	1.016	1.016	1.014	1.016	1.000	1.016
	(0.0764)	(0.0227)	(0.0428)	(0.0428)	(0.0442)	(0.0428)	(0.0489)	(0.0428)
Homicide	6.778	1.755	1.076	1.076	0.956	1.076	0.592	1.076
	(11.88)	(0.908)	(0.928)	(0.928)	(0.820)	(0.928)	(0.505)	(0.928)
Sex Offenses	1.292	1.268**	1.426**	1.426**	1.495**	1.426**	1.453	1.426**
	(0.276)	(0.126)	(0.241)	(0.241)	(0.260)	(0.241)	(0.342)	(0.241)
Robbery	1.089	1.084	1.134	1.134	1.180	1.134	1.099	1.134
	(0.273)	(0.0663)	(0.156)	(0.156)	(0.169)	(0.156)	(0.172)	(0.156)
Assault	1.021	1.006	0.978	0.978	0.967	0.978	0.972	0.978
	(0.0789)	(0.0235)	(0.0447)	(0.0447)	(0.0463)	(0.0447)	(0.0509)	(0.0447)
Property								
All	1.146**	1.104***	1.098**	1.098**	1.092**	1.098**	1.081	1.098**
	(0.0778)	(0.0242)	(0.0465)	(0.0465)	(0.0484)	(0.0465)	(0.0519)	(0.0465)
Burglary	1.175	1.093**	1.091	1.091	1.073	1.091	1.054	1.091
	(0.130)	(0.0473)	(0.0887)	(0.0887)	(0.0924)	(0.0887)	(0.109)	(0.0887)
Larceny	1.122	1.118*	1.107*	1.107*	1.113**	1.107*	1.094	1.107*
	(0.100)	(0.0298)	(0.0583)	(0.0583)	(0.0595)	(0.0583)	(0.0645)	(0.0583)
MV Theft	1.301	1.032	1.023	1.023	0.883	1.023	1.053	1.023
	(0.388)	(0.0649)	(0.185)	(0.185)	(0.195)	(0.185)	(0.197)	(0.185)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on female victimization rates for each rates for each crime type. All regressions include second order polymonials in age fully interacted with an indicator for age over 21. Column labels indicate the city whose data is excluded from the regressions. Each observation is the total number of victims in each age (days) relative to the 21st birthday. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Log-Linear Birthday Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Male			**Female**	
	Birthday 1	Birthday 2	Birthday 3	Birthday 1	Birthday 2	Birthday 3
Violent						
All	0.146***	0.116***	0.0671**	0.164***	0.0891***	0.0532**
	(0.0372)	(0.0349)	(0.0315)	(0.0506)	(0.0313)	(0.0232)
Homicide	-0.0465	0.147	0.0880	0.0361	0.0258	0.0263
	(0.184)	(0.123)	(0.0850)	(0.0906)	(0.0469)	(0.0436)
Sex Offenses	-0.0544	-0.0313	0.00877	0.560***	0.300**	0.147
	(0.0825)	(0.0518)	(0.0550)	(0.173)	(0.129)	(0.104)
Robbery	-0.0618	-0.0669	-0.0635	0.0459	-0.0105	0.0298
	(0.125)	(0.0704)	(0.0580)	(0.224)	(0.115)	(0.0855)
Assault	0.248***	0.196***	0.122***	0.152***	0.0844***	0.0446*
	(0.0396)	(0.0383)	(0.0363)	(0.0413)	(0.0325)	(0.0260)
Property						
All	0.146**	0.0995**	0.0891***	0.297***	0.188***	0.125***
	(0.0616)	(0.0452)	(0.0315)	(0.0460)	(0.0345)	(0.0279)
Burglary	0.239***	-0.0122	0.0102	0.287***	0.150**	0.143***
	(0.0455)	(0.0985)	(0.0724)	(0.0504)	(0.0628)	(0.0437)
Larceny	0.113	0.111***	0.0923***	0.368***	0.221***	0.138***
	(0.0790)	(0.0395)	(0.0277)	(0.0519)	(0.0434)	(0.0347)
Motor Vehicle	0.349***	0.202	0.206**	-0.283***	0.0357	0.00772
Theft	(0.0611)	(0.131)	(0.0818)	(0.0648)	(0.0867)	(0.0790)

This table contains estimates for the birthday effect on male and female victimization rates for each crime type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Poisson RD Effects – Weekend vs. Weekday

	(1)	(2)	(3)	(4)
	No Birthday Control		Birthday Effect Control	
	Weekday	Weekend	Weekday	Weekend
	Panel A: Males			
Violent				
All	1.101*	1.154***	1.102*	1.154***
	(0.0594)	(0.0601)	(0.0593)	(0.0601)
Homicide	0.630	1.095	0.630	1.096
	(0.275)	(0.416)	(0.275)	(0.419)
Sex Offenses	2.004	1.164	1.995	1.140
	(1.037)	(0.676)	(1.030)	(0.646)
Robbery	1.107	1.132	1.107	1.134
	(0.111)	(0.118)	(0.111)	(0.119)
Assault	1.104	1.164**	1.105	1.163**
	(0.0699)	(0.0697)	(0.0700)	(0.0697)
Property				
All	1.066	1.137**	1.066	1.138**
	(0.0445)	(0.0625)	(0.0445)	(0.0625)
Burglary	0.977	1.001	0.978	1.002
	(0.0915)	(0.131)	(0.0922)	(0.131)
Larceny	1.111*	1.156**	1.110*	1.156**
	(0.0639)	(0.0821)	(0.0641)	(0.0820)
Motor Vehicle Theft	1.053	1.195*	1.053	1.199*
	(0.0902)	(0.121)	(0.0903)	(0.122)
	Panel B: Females			
Violent				
All	1.023	1.056	1.022	1.057
	(0.0384)	(0.0453)	(0.0383)	(0.0454)
Homicide	2.163	2.890	2.154	3.074
	(2.081)	(2.915)	(2.060)	(3.167)
Sex Offenses	0.995	1.407**	0.995	1.405**
	(0.158)	(0.227)	(0.158)	(0.227)
Robbery	1.005	1.219	1.005	1.218
	(0.136)	(0.164)	(0.135)	(0.164)
Assault	1.025	1.008	1.025	1.009
	(0.0409)	(0.0467)	(0.0408)	(0.0468)
Property				
All	1.133***	1.160***	1.134***	1.162***
	(0.0451)	(0.0549)	(0.0451)	(0.0548)
Burglary	1.060	1.212*	1.061	1.214*
	(0.0904)	(0.128)	(0.0903)	(0.128)
Larceny	1.212***	1.250***	1.212***	1.250***
	(0.0655)	(0.0779)	(0.0657)	(0.0780)
Motor Vehicle Theft	1.031	0.938	1.032	0.941
	(0.0852)	(0.0893)	(0.0855)	(0.0899)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime type by weekend versus weekday. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Columns (3) and (4) include indicator variables for the week around birthdays. Weekend victimizations occur from Friday at 8 PM through Monday at 6 AM. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

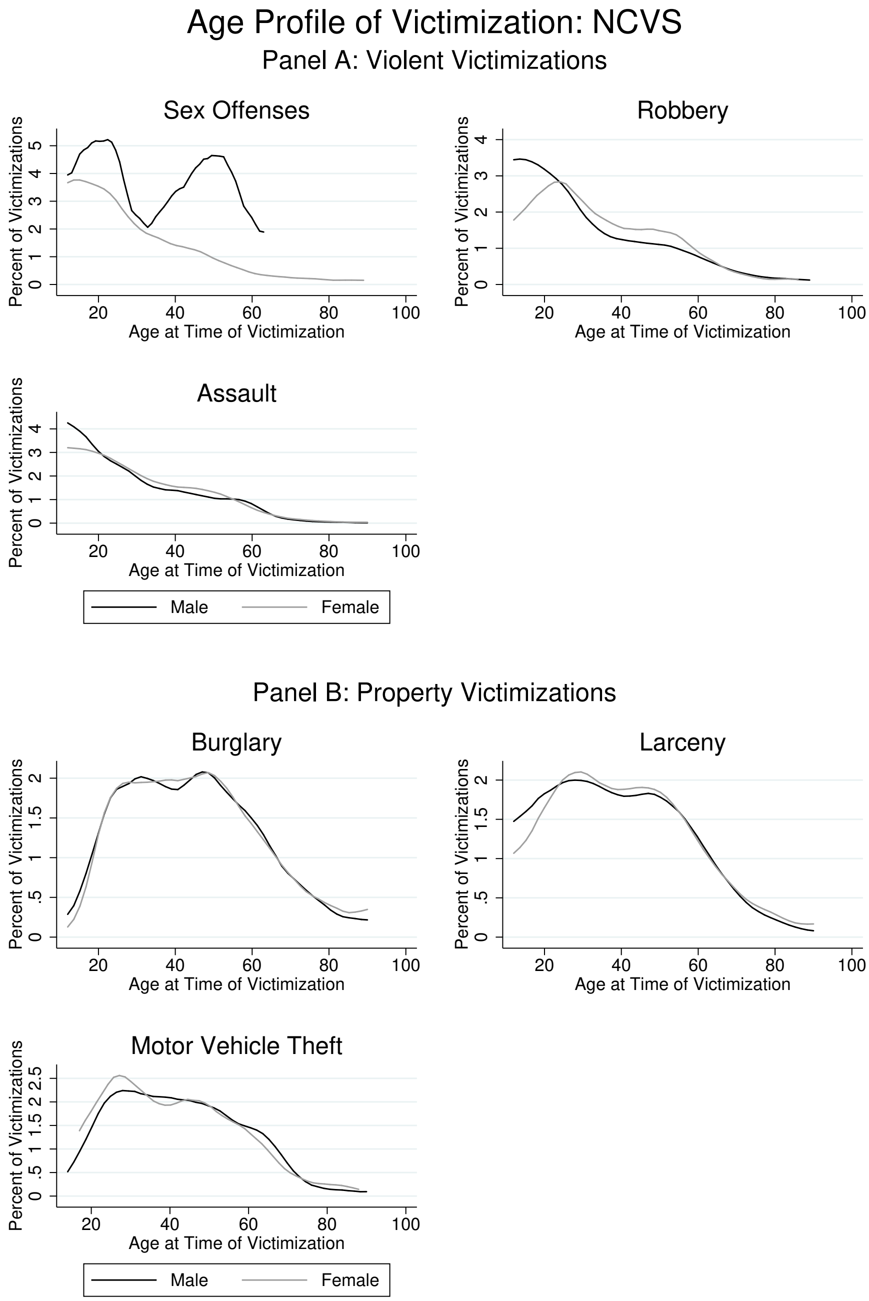
Table A13: Estimated Increases in Victimization

Crime Type	Victimization Rate	IRR	Study Sample Increase	Cities > 250K Increase	U.S. Increase	U.S. Total
			Panel A: Males			
Violent						
All	2.092	1.069	291	2793	11,908	1,248,185
Sex Offenses	0.032	1.744	48	461	1964	130,603
Robbery	0.364	1.078	57	549	2342	322,198
Assault	1.696	1.067	229	2199	9,374	803,007
Property						
All	6.985	1.071	1001	9597	40,913	7,919,035
Burglary	1.103	1.024	53	512	2184	1,515,096
Larceny	5.514	1.024	745	7149	30,478	5,638,455
MV Theft	0.368	1.124	92	883	3765	765,484
			Panel B: Females			
Violent						
All	2.071	1.016	67	641	2734	1,248,185
Sex Offenses	0.237	1.244	117	1119	4771	130,603
Robbery	0.255	1.034	17	168	715	322,198
Assault	1.579	1.002	6	61	261	803,007
Property						
All	7.430	1.109	1634	15,673	66,812	7,919,035
Burglary	1.340	1.121	327	3138	13,376	1,515,096
Larceny	5.609	1.127	1437	13,785	58,766	5,638,455
MV Theft	0.373	1.019	14	137	585	765,484

This table contains estimates for the increase in victimization counts of 21 to 23 year olds associated with the MLDA.Victimization rates are estimated using the 2006-2016 waves of the NCVS. We assume that 2.57% of the population in each column of Table 1 is between 21 and 23 based on the 2016 ACS. U.S. Total Victimizations for 2016 are from the FBI's Uniform Crime Reports and are not gender-specific. Total victimizations for assault are aggravated assaults only.

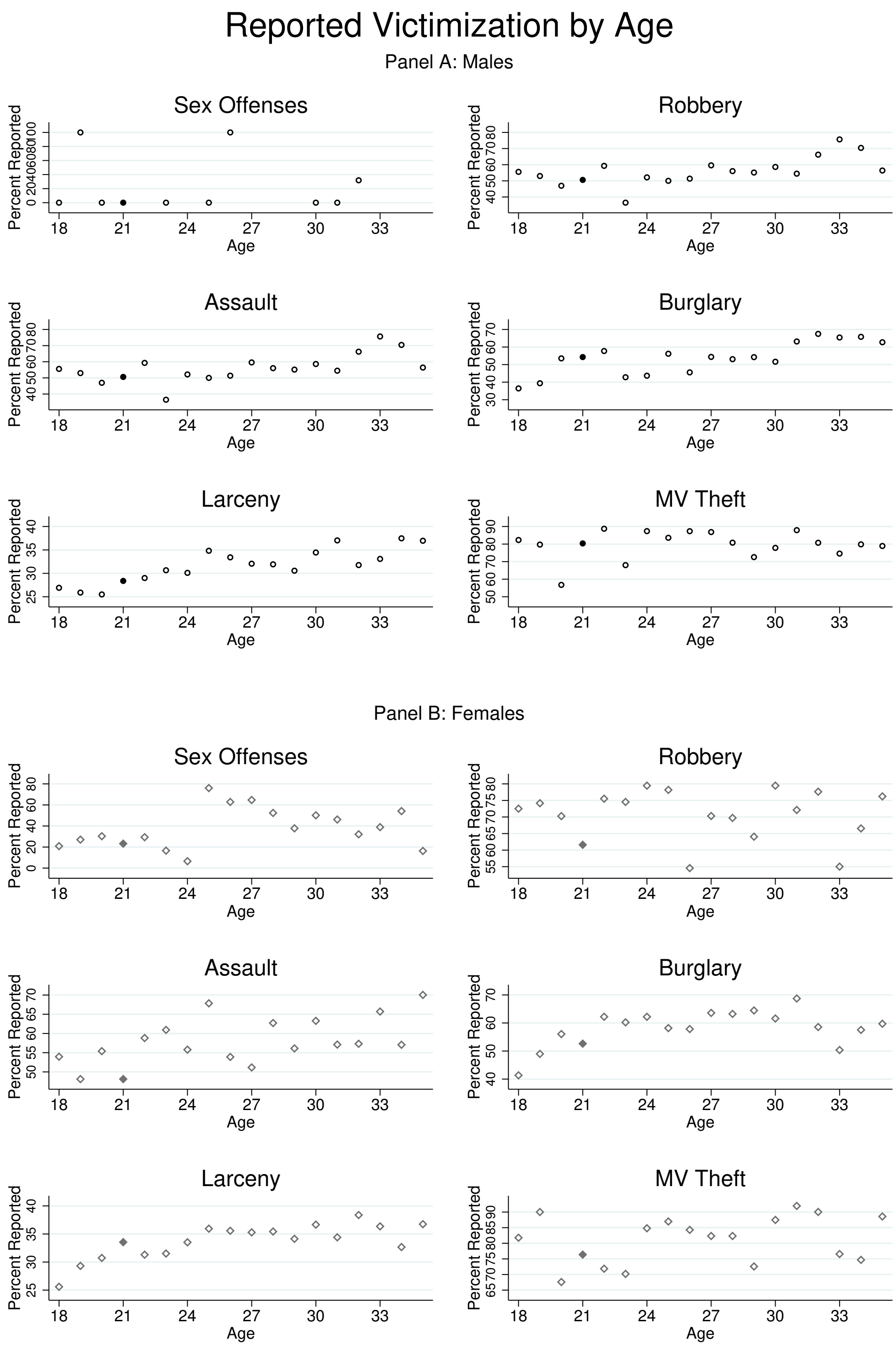
3.10. Supplementary Figures

Figure B6: Age Profile of Victimization



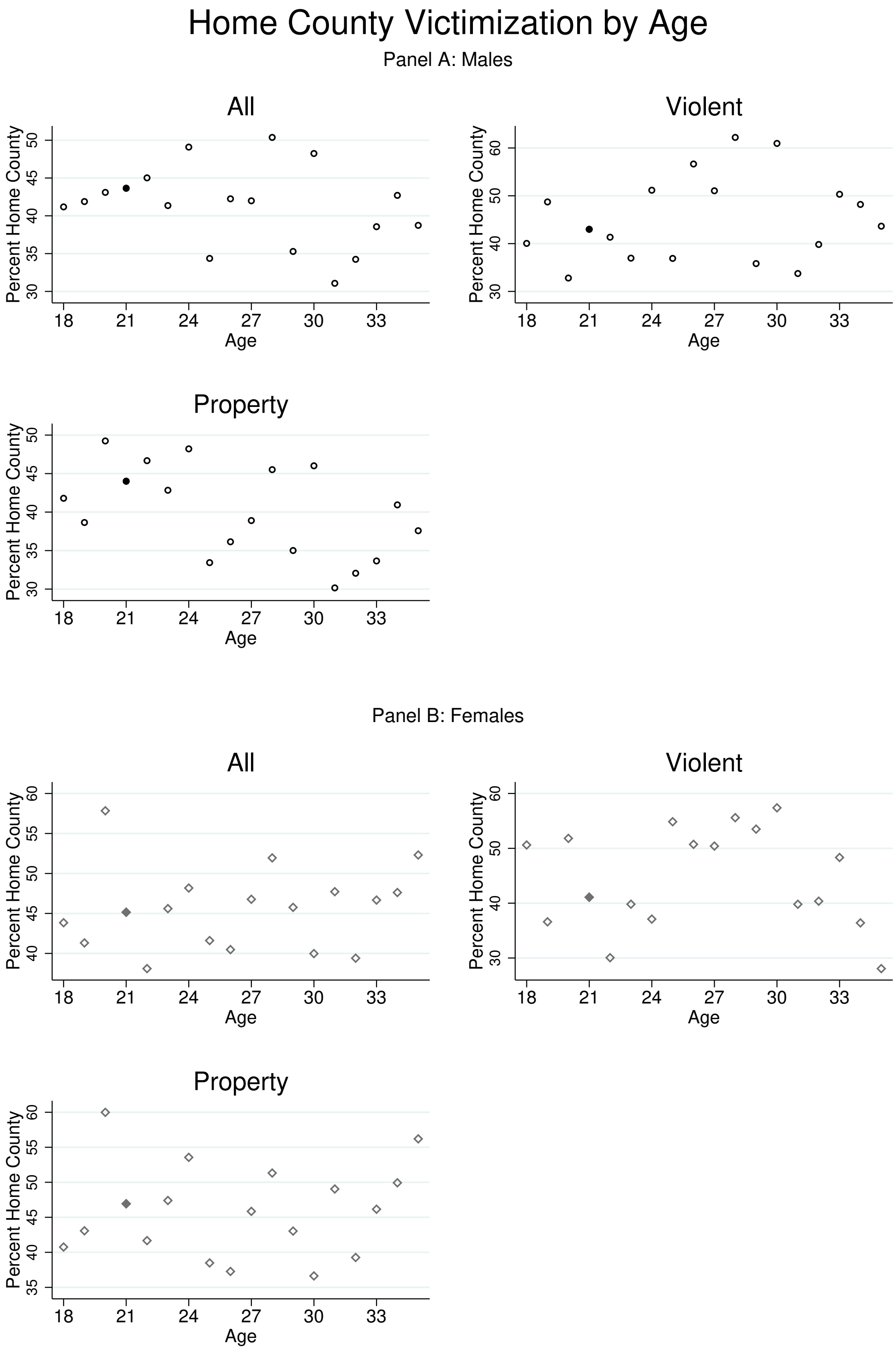
This figure contains local polynomial regressions of age in years at the time of survey on percent of victimizations at that age. Each observation is the percent of all victimizations within crime type and gender that occur at a given age.

Figure B7: Reporting Rates



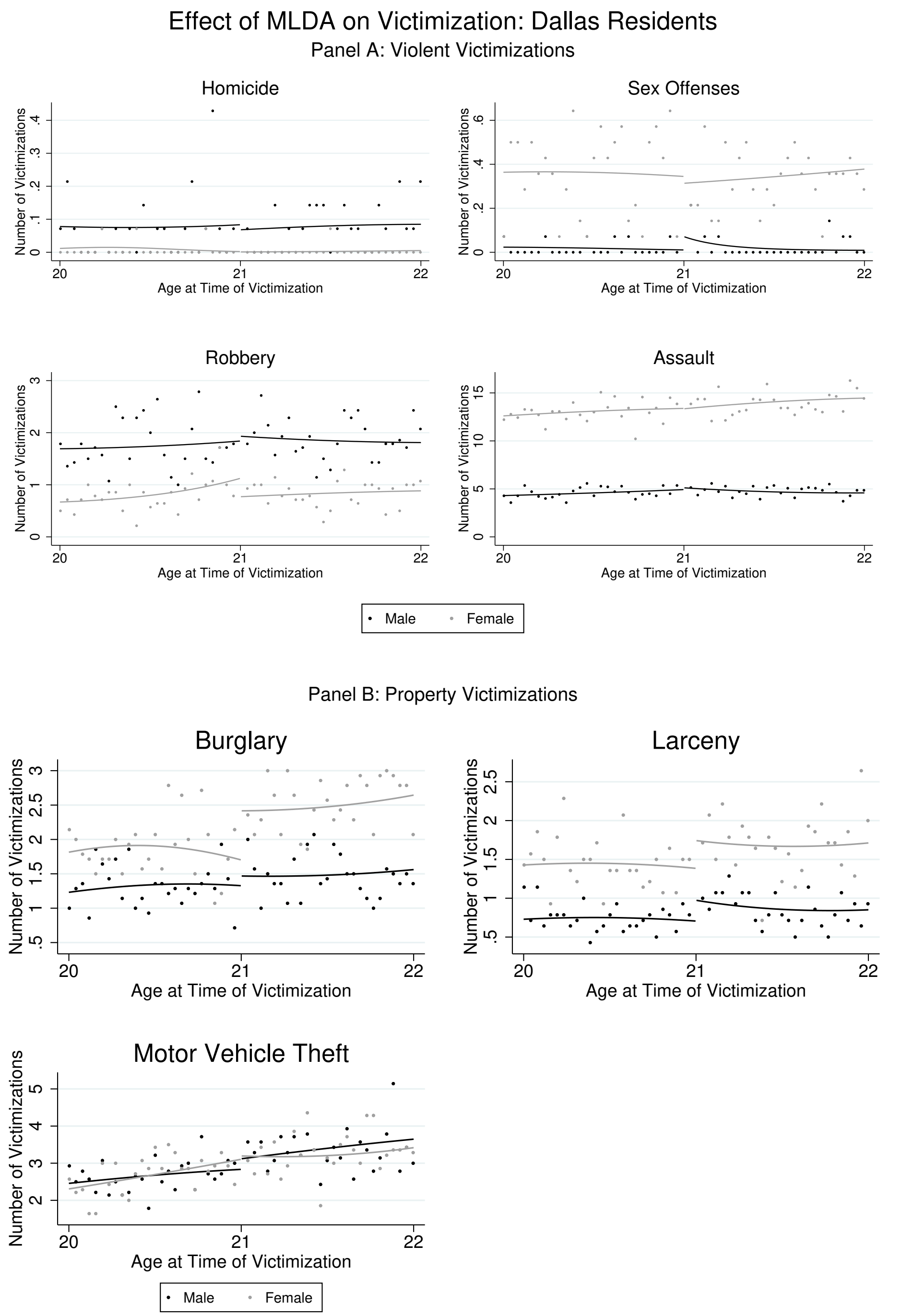
This figure contains average rates of reporting various victimizations to the police for respondents to the 2006-2016 waves of the NCVS. Age is recorded in years at time of survey.

Figure B8: Home County Victimization



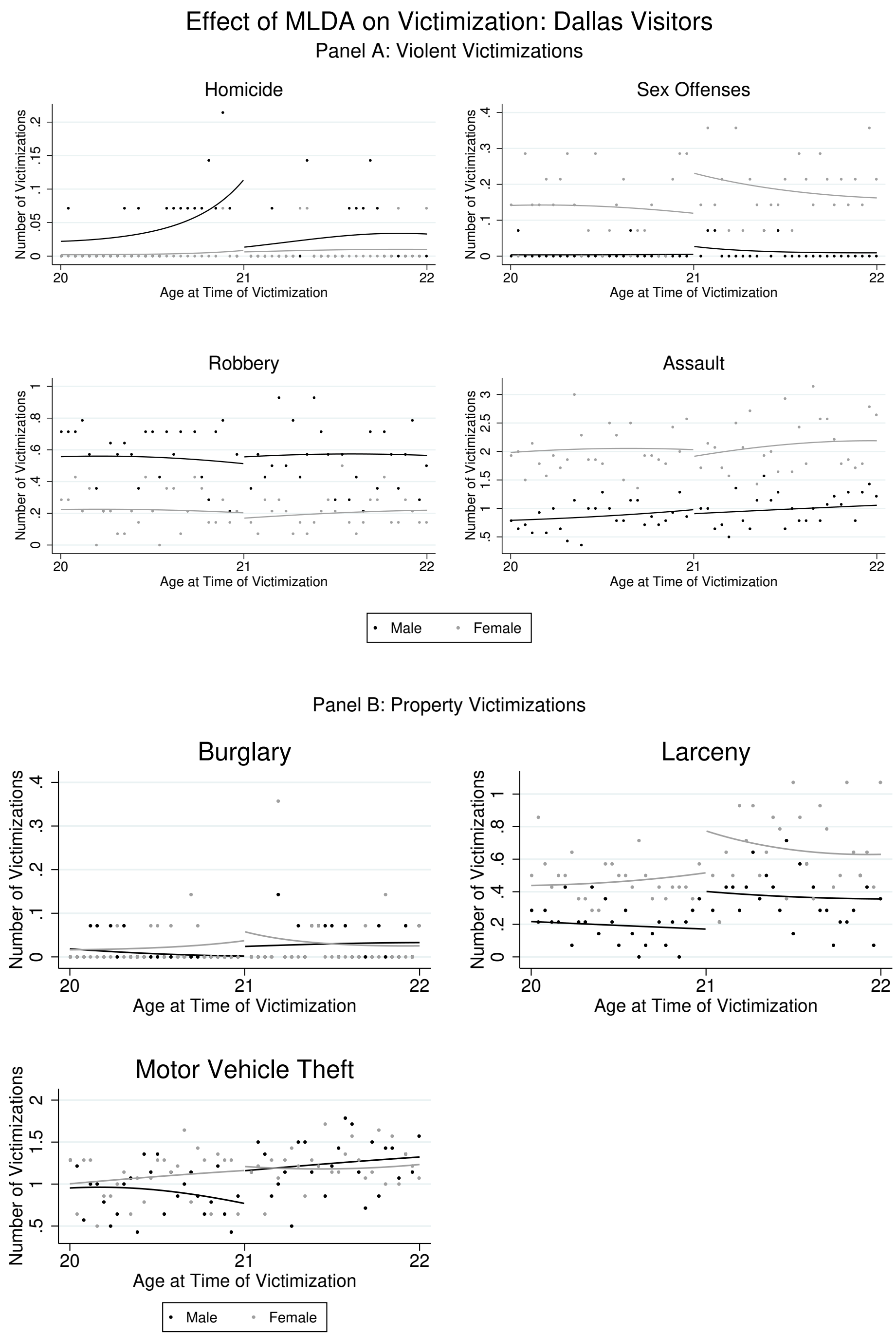
This figure presents the share of victimizations that occured in the victim's home county among respondents to the 2006-2016 waves of the National Crime Victimization Survey. Age is recorded in years at time of survey.

Figure B9: Dallas Residents



This figure contains fitted Poisson estimates and average victimization rates in 14 day bins for victimizations of Dallas residents. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

Figure B10: Dallas Visitors



This figure contains fitted Poisson estimates and average victimization counts in 14 day bins for victimizations of Dallas visitors. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

Sex Offenses, by Police Department

Charlotte-Mecklenburg: forcible rape, forcible fondling, forcible sodomy, sexual assault with object

Dallas: rape, sex offenses and indecent conduct

Denver: harassment - sexual in nature, sex aslt - fondle adult victim, sex aslt - fondle child, sex aslt - fondle-child by pot, sex aslt - non-rape, sex aslt - non-rape pot, sex aslt rape, sex aslt - rape pot, sex aslt w/ object, sex off incest, sexual exploitation of child

Houston: other sex, rape, sex offenses

Kansas city, mo: forcible fondling, forcible rape, forcible sodomy, sexual assault with an object

Milwaukee: ejaculation, forcible fondling, forcible rape, forcible sodomy, sexual assault with object

San Diego: act in concert to commit rape w/foreign object, aggravated sexual assault of a minor with a foreign object, aggravated sexual assault:minor under 14 and 10+ yrs younger, assault w/intent to commit rape/other sex acts, assault with intent to rape, assault with intent to rape in commission of 459, attempted rape, burglary/unspecified, continuous sexual abuse of child, crime against nature/sodomy not specified, oral cop:victim unconscious or asleep, oral copulation, oral copulation / victim unconscious of the nature of the act, oral copulation by force or fear, oral copulation in concert: victim incapable of giving consent, oral copulation w/person under 16, oral copulation w/person under 18 years, oral copulation: victim intoxicated/etc, oral copulation:minor under 14 & 10+ years younger, oral copulation:victim unaware act occurred, oral copulation:victim under 10 years of age, rape, rape by fear or force, rape by threat of retaliation, rape by threats to use authority of public official, rape of drugged victim, rape of spouse by force/fear/threat, rape of spouse unable to resist: under controlled sub/etc, rape of spouse under controlled sub/etc, unable to resist, rape of spouse unable to resist: under controlled sub/etc, rape spouse by force/fear/etc, rape where victim is incapable of giving consent, rape/etc in concert with, orce/violence, rape/etc in concert with force/violence:minor 14 yrs or older, rape: force/fear/etc., rape: spouse unconscious of nature of act, rape: victim believed person is spouse, rape: victim believes person is spouse, rape: victim drugged, rape: victim incapable of consent, rape: victim unconscious of nature of act, rape:victim unconscious of the nature of the act, sex penetration:foreign obj/etc victim unaware:nature of, sex penetration:foreign obj/etc:victim unconscious/asleep, sex penetration:victim unaware act occurred, sexual battery, sexual battery as defined in this section, sexual battery involving restrained/institutionalized person, sexual battery of restrained or incapacitated person (f), sexual battery of restrained or incapacitated person (m), sexual battery on institutionalized person, sexual penetration by threat of retaliation victim/etc, sexual penetration w/ foreign object w/ force, sexual penetration w/force/etc 14 years or older, sexual penetration w/force/etc under 14 years old, sexual penetration w/foreign object w/victim under 18 yrs, sexual penetration w/foreign object w/intoxicated victim, sexual penetration w/foreign object w/victim under 16 yrs, sexual penetration w/foreign object w/victim under 18 yrs, sexual penetration w/foreign object: vic believes is spouse, sexual penetration w/foreign object:threat by auth to arrest, sexual penetration w/foreign object; victim incapable & confined, sexual penetration w/foreign object; victim incapable of consent, sodomy by force or fear, sodomy by force/violence/fear, sodomy by force/violent/fear victim 14 yrs of age or older (f), sodomy w/person under 18 yrs, sodomy/concert/force, sodomy/victim unconscious of the nature of act, sodomy:minor under 14 & 10+ years younger, sodomy:victim under 10 years of age, sodomy:victim under influence anesthetic/etc/any control s, sodomyw/o consent: drugged victim & defendant in mental fa, touch person intimately against will for sexual arousal/e, unlawful sexual intercorse w/minor: 3 yrs old or younger, unlawful sexual intercourse / victim under 18, unlawful sexual intercourse w / minor 18, unlawful sexual intercourse w/minor: more than 3 years old, unlawful sexual intercourse w/minor: perp 21+ victim -16

St. Louis: forcible fondling, forcible rape, forcible sodomy, human trafficking - commercial sex acts, human trafficking, commercial sex acts, sex offenses - forcible fondling, sex offenses - forcible sodomy, sex offenses incest, sex offenses - statutory rape

Sample Period, by Police Department

We obtained data from the following municipal law enforcement agencies for each of the following time periods:

- Charlotte-Mecklenburg, NC: 1/1/2008 - 12/31/2017
- Dallas, TX: 1/1/2007-12/31/2017
- Denver, CO: 1/1/2008-12/31/2017
- Houston, TX: 1/1/2007 - 12/31/2015
- Kansas City, MO: 1/1/2007-4/26/2018
- Milwaukee, WI: 1/1/2007-12/31/2017
- San Diego, CA: 1/1/2008-12/31/2017
- St. Louis, MO: 1/1/2007 - 12/31/2017

Residential Locations, by Police Department

- Dallas: apartment complex/building, apt, condomin, foster home, mobile home, single family, residential property
- Denver: residence/home
- Houston: home/apartment, home/residence
- Milwaukee: offender residence, offender temporary, other residence, other temporary, victim residence, victim temporary
- St. Louis: apartment/condo, housing shelter, other residence, public housing, residence/home

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