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The Effect of Violence Against Police on Policing Behavior

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Abstract

There are rising concerns about the quality of policing in high-violence urban neighborhoods in the United States. Residents of these areas are concerned that police officers are failing to reduce crime, but also that when police officers do engage, their tactics are too severe. This paper examines whether risks to officer safety drive both phenomena. To do so, I exploit variation in unprovoked ambushes on police within and across beats using administrative 911 call data from a large American city. Results show that ambushes lead to an 8 percent decline in arrests, an effect that persists for at least three and a half years after the ambush. In contrast, I find no effect on police severity, as measured by use of force and civilian complaints. This suggests police officers respond to increased risk by de-policing, rather than using more aggressive tactics.

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1 Introduction

Serious violence is very geographically concentrated in the United States. For example, 78 percent of murders occur in only 5 percent of American counties (FBI UCR, 2014). Consequently, policing has the highest expected marginal return in high-violence areas. This is reflected by the fact that 56 percent of residents in low-income, high-violence areas would prefer police to spend more time in their neighborhood (Gallup, 2019). At the same time, civilians in these neighborhoods are the least satisfied with policing. For example, civilians in high-violence neighborhoods are 40 percent less likely to approve of police officers' ability to prevent crime, help victims, and solve problems compared to those who live in low-violence areas (Maxson et al., 2003). Further, movements such as Black Lives Matter and public support for greater surveillance of police officers reflect a growing concern with policing severity (Morin et al., 2017). Low income and minority Americans are also 66 and 76 percent more likely to believe police use lethal force too quickly (Ekins, 2014). This paper examines the extent to which increased risk to officer safety drives both phenomena.

Despite increasing concerns with the effectiveness and severity of policing, there is little evidence on how risks to officer safety could alter policing behavior. An important exception is Legewie (2016). This paper compares similar police stops before and after a civilian shooting of a police officer using a regression discontinuity and matching design. Legewie (2016) finds police officers use force more often during stop and frisk events on black civilians, but not white ones, in the 14 days after a shooting. This paper's approach has several advantages relative to Legewie (2016). First, I can examine long-run effects, thereby testing whether police response is temporary or persistent. Additionally, because I use emergency calls for service, where officers are assigned to incidences by a computer system rather than initiating them, there is no selection in whether an event is recorded. For example, police officers could change their reporting behavior of civilian interactions after a violent event. This means I am able to estimate the effect of increased risk on police arrests without concerns about misreporting. Finally, emergency calls include detailed information on call urgency, severity, and incident descriptions. This is consequential because I can observe important characteristics of police and civilian interactions even when an arrest is not made, or use of force is not recorded.

To estimate the effect of threats to officer safety on police behavior, I use data on ambushes and calls for service from Indianapolis, Indiana. From 2014-2017, Indianapolis experienced seven different police ambushes across four different police beats. Ambushes are defined as a "situation where an officer is assaulted, unexpectedly, as the results of premeditated design by the perpetrator" (FBI LEOKA, 2015). Using police ambushes to measure threats against police officers has multiple benefits. First, police ambushes are perhaps the most severe act of violence an officer can face and are highly salient. In addition, the relative infrequency of the events gives me variation that can be used to identify effects. This contrasts with officer assaults, of which there were nearly 3,500 (averaging 35 per beat per year) over my sample period. Finally, an ambush is an act of violence that, by definition, was not initiated or provoked by officers. As a result, ambushes are the one form of violence against police that can be perceived as only due to the fault of the perpetrator.

Using this variation in ambushes, I compare how police behavior changed in ambushed beats compared to other beats over time. The identifying assumption is that absent an ambush those beats would have experienced similar changes in police behavior. I show empirical support for this assumption, as police officer arrests and use of force change similarly before an ambush. Results show that violence against police, in the case of ambushes, decreases arrest by 8 percent in the three years following an ambush. These results are the strongest in the first six months after an ambush, where I estimate arrests decreased by 10 percent. In contrast, results indicate policing severity – as measured by use of force and civilian complaints – does not increase after an ambush. These results are robust to the inclusion of call level controls, covariate-by-time controls, and beat-specific linear time trends. I also show the results are not driven by changes in the composition of calls coming from ambushed beats after an ambush.

By providing the first evidence that violence against police officers leads to decreases in arrests and no change in use of force or civilian complaints, this paper contributes to related literature on understanding the effects of violence and trauma on decision making. Much of this literature focuses on the relationship between exposure to disruptive events and risk preferences.[1] While this literature forces on measuring changes in risk preferences, I focus on observed changes in high stakes decisions. Second, I provide evidence on how the context surrounding police officers affects behavior. This contrasts with most of the existing literature, which has focused on the potential bias in police decision making (e.g., Fryer, 2016; Weisburst, 2017; West, 2018). Other papers emphasize how characteristics of a police

[1]Some of this literature focuses on civil conflicts (e.g., Callen et al., 2014; Jakiela and Ozier, 2018; Voors et al., 2012), while another branch centers on natural disasters (e.g., Cameron and Shah, 2015; Cassar et al., 2017; Eckel et al., 2009). There is no consistent evidence on whether events like these increase or decrease risk aversion.

force such as, degree of monitoring[2], militarization[3] or composition[4] may alter crime. This paper contributes to this literature by demonstrating whether and how police officers change behavior based on the perceived risks. These results suggest that the same qualities that raise the marginal benefit of police officers – a dangerous, high-crime area – are the qualities that can result in the behavioral response of de-policing. On the other hand, results here provide evidence against the hypothesis that increased personal risk to officers – at least of the type studied here – generates increases in use of force and other controversial tactics.

2 Institutional Details

2.1 Ambushes in Indianapolis

I can estimate the effect of violence against police on police behavior using ambushes. The Indianapolis Police Department defines an ambush according to the Federal Bureau of Investigation's definition from its Law Enforcement Officers Killed and Assaulted data collection guidelines. The Federal Bureau of Investigation defines an ambush as a "situation where an officer is assaulted, unexpectedly, as the result of premeditated design by the perpetrator".[5] Ambushes provide an ideal environment to consider violence against police for multiple reasons. First, police ambushes are one of the most severe acts of violence an officer might face. This concern is reflected by the fact that some police departments adopted programs to provide basic psychological support and design different policing tactics (pairing up police

[2] e.g., Ariel et al. (2015); Ater et al. (2014); Cheng and Long (2018); Heaton et al. (2016)

[3] e.g., Bove and Gavrilova (2017); Harris et al. (2017)

[4] e.g., Miller and Segal (2012, 2018)

[5] The Federal Bureau of Investigation defines an assault as "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault is accompanied by the use of a weapon or by a means likely to produce death or great bodily injury".

officers on calls etc.) after the high profile Dallas ambush in 2016 (Smith, 2019). Further, a survey of officers before and after the Dallas ambush reported that officers were more concerned about their physical safety after the ambush (Morin et al., 2017). Second, the rare nature of ambushes gives me variation in exposure to locations where extreme violence occurred. In contrast, Indianapolis experienced nearly 3,500 officer assaults from 2014-2017. This means, on average, each police beat was treated 35 times an average year, making it difficult to estimate the effect of less severe violence because each police beat is treated multiple times in a small period. Finally, for ambushes, the officers involved not initiate or incite the perpetrators. The responding officer could have provoked police injury in other settings. Therefore, for ambushes only, other officers on the force will perceive the violence to be the perpetrator's fault alone.[6] During the time I consider in this paper (2014-2017), Indianapolis experienced seven police ambushes in four different police beats. I only take the first ambush in each beat for my analysis.

2.2 Police Patrol

One advantage of the approach used in this paper is I examine behavior during responses to 911 calls for service. Using calls for service is important because officers are assigned to these events, meaning that there are no civilian and police interactions that are unreported. This is because every 911 call has at least one police officer assigned to it. No call can be unanswered. Leveraging 911 calls is also consequential because I observe important characteristics of police

[6]It could be the case that a police officer is assaulted because they accelerated a situation due to events in their personal life. For example, imagine a scenario where a police officer is going to get a divorce and allows this to spill over into his work life. The potential divorce could cause him to violently engage with a civilian and lead to the officer himself being injured. In this scenario, other officers might perceive the assault to be the fault of the officer, and not a situation that could happen to them.

and civilian interactions even when an arrest is not made or use of force is not recorded. I consider the effects of increased risk to officer safety on policing behavior by using data from Indianapolis, which includes information on the expected severity of each call (call priority) and a detailed, standardized call description. Each patrol officer is assigned to a beat within the city. There are 33 beats in Indianapolis and 1345 patrol officers in my time periods. Beats in the city center, where the population is more concentrated, are smaller than those closer to the suburbs. A map of police beats is shown in Figure 1.

After a call for service comes in, the dispatcher will assign the police officer on duty in a beat or the closest officer geographically to respond to the call. Because officers are sometimes assigned based on their geographic locations, during one shift a police officer may respond to calls in many different beats. This variation allows me to include individual police officer fixed effects in all my specifications, thereby ensuring any response to police ambushes is not due to the sorting of officers across regions or call.

When an officer is dispatched to a call, they are given information on the location of the call, the priority of the call, as well as a call description. The priority of a call, a number between 1 and 4, provides the officer with details on the severity and urgency of a call. Lower numbers correspond to higher severity and urgency. For example, a burglary in progress will be assigned a lower priority number than a report of a missing vehicle. The call description is one of 200 different call types. For example, a police officer could be told that a call is for a traffic accident or a burglary in progress. After the police officer responds to the call for service, they are required to record if a call ended in an arrest using the computer in their police car. A call could also end in a use of force. I connect calls for service to use of force using separate police reports on use of force. The use of force files record all incidences of use of force. The most common type of use of force is hands and feet, but gun uses of force do represent 7 percent of all uses of force. I also consider civilian complaints against police officers as an outcome. Unlike arrests and use of force, complaints are not measured at the call level. This is because I am unable to link civilian complaints to specific 911 calls. If a civilian believes they were treated improperly by a police officer or that an officer violated protocol they can submit a complaint to the Citizens Police Complaint Office. Office staff may follow up on the complaint to secure further details. These complaints may be filed on-line, however, more "serious" complaints must be filed as a formal complaint.[7] Formal complaints must be filed in person within 60 days of the incident (City of Indianapolis, 2019). During my time period 16,380 complaints were filed against Indianapolis police officers.

3 Data

My data comes from the Indianapolis Police Department and include all 911 calls for service from 2014-2017. I link these data to Indianapolis Police Department data on use of force incidences and records of ambushes. Summary statistics for my data are shown in Table 1. Panel A shows summary statistics for the call level analysis. In total I have 3.4 million calls for service. Ten percent of calls come from ambushed beats and 90 percent come from beat in which there was not an ambush. The average call priority (a measure of the severity and urgency of a call) is 1.78, where the most severe and urgent calls are assigned a priority of 1. On average ambushed beats have slightly more severe and urgent calls for service,

[7] The police department does not explicitly define what a more serious complaint is. Rather, the staff reviewing a complaint will make this distinction.

reflected in the lower call priority. My first two outcome variables, arrest and use of force, are measured at the call level. On average 7 percent of calls end with an arrest and 0.6 percent of calls end in use of force. Ambushed beats have slightly higher arrests and use of force per call. I also consider the effect of police ambushes on civilian complaints and number of calls. These outcomes cannot be connected to specific 911 calls, so I perform this analysis at the beat-day-hour level. Summary statistics at the beat-day-hour level are shown in Panel B. Each beat has about 2 civilian complaints and 937 calls for service per week. Ambushed beats are less likely to receive civilian complaints compared to un-ambushed ones and ambushed beats receive more calls for service than un-ambushed ones.

4 Methods

I will estimate the effect of increased risk to officer safety on policing behavior using a difference-in-differences approach. This method will compare ambushed and un-ambushed beats, over time. In particular, I will estimate the following generalized difference-in-differences model to determine the impact of ambushes on arrests and use of force:

$$Outcome_{ctbo} = \theta_b + \beta(AfterAmbush_t * AmbushedBeat_b) + \alpha_o + \omega Year * Month_t + X_c + \epsilon_{ctbo}$$

where $Outcome_{ctbo}$ is an indicator variable for whether a call for service c, in beat b, at time t, attended by officer o, ended in an arrest or use of force. θ_b are beat fixed effects to control for any systematic differences across police beats. For example, some beats may experience higher average arrests or crime than others. $AfterAmbush_t * AmbushedBeat_b$ is the treatment variable that takes of a value of one for ambushed beats, after an ambush. The coefficient on this term, β measures the change in arrests or use of force per call after an ambush, relative to arrests or use of force in un-ambushed beats. Year-by-month fixed effects, ω_t, control for shocks to arresting behavior that are common to all beats in a year-month. X_c can include call-level controls. Specifically, X_c includes controls for call location—latitude and longitude, dispatch time and fixed effects for call description and priority. Finally, I include individual officer fixed effects, α_o, to account for any time-invariant officer characteristics. These individual officer fixed effects also account for any differential sorting of officers after an ambush. In each specification standard errors are clustered the the beat level.

In this approach, the identifying assumption is that absent an ambush, ambushed beats would have experienced changes in arrests and use of force similar to un-ambushed beats. I test this assumption in the following ways. First, I graphically examine whether arrests or use of force began diverging before an ambush. If arrests and use of force were not changing similarly during the time period before an ambush it would suggest that un-ambushed beats are not a valid counterfactual for the change ambushed beats would have experienced absent treatment. Next I include covariate-by-time controls. If my results could be explained by idiosyncratic shocks to areas with specific characteristics, then including these controls would absorb my results. For example, if just areas with very high priority calls received a shock that coincided with treatment, then controlling for priority-by-year-by-month effects could change my estimate. Finally, I also include beat-specific linear time trends, which allow for the possibility that beats followed different trends over time. For instance, if my results can be explained by beats trending differently based on unobserved or observed characteristics over time, then the beat-specific trends should absorb my treatment effect. If my estimates are robust to these specification these tests provide evidence to support the validity of this research design.

I also include an additional test of identification. For this test, I consider whether observed determinants of an arrest exhibit pre-trends or change after treatment. Rather than focus on each covariate separately, I predict arrest for every call using all characteristics about the call except treatment status. This means I predict arrest and use of force using the latitude, longitude, dispatch time, call priority, and call description. Therefore, predicted arrest is a linear combination of all observable characteristics about the call. For example, if call priority is very important for determining if a call ends in an arrest, it will be given more weight in the prediction. If my identifying assumption holds, I would expect no divergence in arrests in the pre-period. Further, if I do not observe a treatment effect for predicted arrests, this provides further evidence that my results can be attributed to ambushes and not other confounding factors.

Another threat to identification is that the number of calls for service could be changing after ambush. For example, it could be the case that after an ambush civilians do not make as many domestic abuse calls and, therefore, officers do not have the opportunity to make arrests or use force on those types of calls. I directly test for threat by estimating Equation 1 using the inverse hyperbolic sine of number of calls as the outcome variable and collapsing my data to the beat-day level. If I find that the number of calls does not change after an ambush, this provides evidence that I am identifying the effect of ambushes and not a change in the number of calls.

5 Results

5.1 The Effect of Ambushes on Police Behavior

I first consider the effect of ambushes on predicted arrests to determine if important characteristics of calls for service are changing before or after treatment. Arrests are predicted using all call characteristics (latitude, longitude, dispatch time, call priority, and call description) aside from treatment status. To determine if ambushed and un-ambushed beats began diverging before treatment, I graph the estimated divergence, over time, between ambushed and un-ambushed beats relative to thirteen or more months before the ambush. Explicitly, the Figures in this section graph a dynamic version of Equation 1, where $AfterAmbushed_t$ is replaced by indicators variables for months before/after an ambush. Formally, I estimate the following equation:

$$Outcome_{ctbo} = \theta_b + \Sigma_{t=-19}^{42+}\beta(MonthsAfterAmbush_t * AmbushedBeat_b) + \alpha_o + \omega Year * Month_t + X_c + \epsilon_{ctbo}$$

where I combine $MonthsAfterAmbush$ into three month bins. In Figure 2, I plot dynamic difference-in-differences estimates for predicted arrest. If my identifying assumption holds, I would expect no divergence in predicted arrests before an ambush and no treatment effect. Figure 2 shows no divergence in predicted arrest in the pre-period and no evidence of a strong treatment effect after an ambush. This further supports that my research method is identifying the effect of an ambush, rather than any other confounding factor.

Next, I consider the effect of ambushes on arrests. To do so, I compare the changes in arrests in ambushed beats to un-ambushed beats, before and after an ambush. Figure

3 shows results for observed arrests. Importantly, both ambushed and un-ambushed beats appear to track each other in terms of arrest per call in the periods before an ambush. This suggests that in the absence of an ambush, ambushed and un-ambushed beats would have continued to track each other. Therefore, no evidence of pre-trends in arrests supports the validity of this research design. There is also a sharp decrease in arrests in the six months immediately after an ambush, further supporting that ambushes caused this divergence. The decrease in arrests persists for in the following months as well, although the decrease is not as dramatic.

Estimation results of Equation 1 are shown in Table 2. Each column reports the effect of an ambush on arrests per call. In Column 1, where only beat, year-by-month, and officer fixed effects are included, the estimated effect of police ambushes is a decrease in arrests per call by 8% (0.6 percentage points). This represents a substantial decrease in arrests and is statistically significant at the one percent level. In Column 2, I include call-level controls. Specifically, I control for the latitude, longitude, and dispatch time of a call. I also add call priority and call type fixed effects. The estimate in this specification is -0.00594 (8%) and is statistically significant at the one percent level. In Column 3 I include covariate-by-time (each characteristic from Column 2 interacted with year-x-month) controls. If my results could be explained by idiosyncratic shocks to areas with specific characteristics, then including these controls would absorb my results. For example, if just areas with very high priority calls received a shock that coincided with treatment, then controlling for priority-by-year-by-month effects could change my estimate. But, my estimate in Column 3 remains very similar in magnitude and significance. In Column 4, I add beat-specific linear time trends, which allow each beat to follow a different trend over time. These trends allow for both observable and unobservable beat characteristics to change linearly over time. If results are being driven by ambushed beats being on a different path than un-ambushed beats, then adding a beat-specific linear time trend should absorb the treatment effect. However, this estimate is similar in magnitude and significance to Columns 1, 2, and 3. Finally, given the more dramatic drop in arrests just after an ambush, I separately consider treatment effects for the first six months after an ambush and all the months afterward. In both periods, I estimate statistically significant decreases. The decrease in the first six months after an ambush corresponds to a 10 % decrease in arrests compared to a 8% decrease in later months. Taken together, these results indicate that violence against police (ambushes) can lead to a significant and sustained decreases in arrests for at least three and a half years.

Even though I estimate a decrease in arrests, the severity of policing could increase on each call. For example, now officers engage with civilians less to make arrests, but when they do choose to engage, more force is used. I answer this question directly by estimating Equation 2 but replacing use of force as my outcome variable. As before, I first show predicted values in Figure 4. Again there is no clear trend in use of force leading up to the ambush, supporting my identifying assumption. There is also no break in use of force behavior after the ambush for predicted use of force. This indicates identifying the effects of an ambush on use of force will not be confounded by other factors.

I present my main results for use of force in Figure 5. Here there is no trend in use of force leading up to an ambush, further validating my research design. After an ambush, there is also no clear change in use of force behavior. Estimation results from Equation 1 are in Table 3. Table 3 uses the same specifications as Table 2. Column 1 shows the baseline specifications, Column 2 adds controls, Column 3 allows controls to vary with time, and Column 4 includes a beat-specific linear trend. Across the first four columns, my estimates remain statistically insignificant and close to zero. The magnitude of the coefficients suggests that use of force may have decreased by as much as 1% (Column 3) or increased by up to 2.5% (Column 4). Finally, Column 5 shows results for the first six months after an ambush separately from longer-term effects. The coefficient for the first six months after an ambush suggests a slight decline in use of force after an ambush, but is not statistically significant at conventional levels. Together, these estimate show no evidence of an increase in use of force or policing severity, after an ambush. In fact, I am able to rule out any increases in use of force above 4.5%.[8]

Now, I consider another measure of policing severity: civilian complaints. I investigate civilian complaints because they are not reported by police officers, as use of force is, but rather by the civilians that interact with police officers. Civilian complaints are not measured at the call level because when civilians make complaints it is not possible to link them back to the 911 call that caused the complaint. To consider the effect of ambushes on complaints directly, I estimate Equation 2 using the inverse hyperbolic sine of the number of complaints in a beat-day-hour as my outcome variable. Results for civilian complaints are shown in Figure 6. Again, there is no trend in complaints leading up to an ambush, further validating my research design. After an ambush, there is also no clear change in the number of complaints. Estimation results from Equation 2 are in Table 4. Column 1 includes beat

[8]The top of the 95% confidence interval from the estimate in Column 1 represents a 4.5% increase in use of force.

and year-by-month fixed effects. The coefficient is not significant at conventional levels and is close to zero. This estimate rules out increases in complaints of 1/100 of a percent, or one complaint per beat-year.[9] In Column 2, I estimate the show and longer-term effects of ambushes on complaints. The coefficient for the first six months after an ambush is close to zero and is not statistically significant at conventional levels. The coefficient for longer-term effects suggests a slight decline in the number of arrests and is statistically at the 10 percent level. Jointly, these use of force and civilian complaint results show no strong evidence of increases in policing severity in response to heightened officer risk.

5.2 The Effect of Ambushes on Calls for Service

A potential threat to identifying the effects of an ambush is that the number of calls for service from an ambushed beat may change after an ambush. This is problematic if the decrease in calls comes from the type of calls where arrests are often made or force is typically used. For example, if the number of serious calls decreases in ambushed beats then police officers may not have the opportunity to make arrests and use force, as they could in the calls before the ambush. To address this potential threat directly, I estimate Equation 2 using the inverse hyperbolic sine of the number of calls as my outcome variable. I also separately consider calls where arrest or use of force are very likely. I define "likely" by only keeping calls types that are in the top quartile of ending in a use of force or arrest.[10]

Results for all calls presented in Figure 7a. First, it is important to note the there is no divergence between ambushed and un-ambushed beats before treatment. Second, there is

[9] The top of the 95% confidence interval for column 1 is 0.0001695.

[10] I also present results for arrests and use of force for arrest likely and use of force likely calls in Figures A1 and A2 as well as Tables A1,A2, A3, and A4.

no strong evidence of a change in the number of calls following an ambush. Corresponding estimates are presented in Table 5 Panel A. Column 1 presents the estimate for the entire period after an ambush and Column 2 considers the short and long term effects separately. None of the estimates are statistically significant and are close to zero. My estimate in Column 1 Panel A corresponds to a 2 percent decrease in calls, although it is not statistically significant at conventional levels. Further, in the first six months after an ambush, where I estimate the most dramatic drop in arrests, my estimate represents a 0.04 percent decrease in calls.

Only considering entire sample results may mask heterogeneity in calls that are more likely to experience an arrest or use of force. Therefore, I also consider the effect of an ambush on the number arrest likely calls in Figure 7b and use of force likely calls in Figure 7c. In both figures, there is no evidence of a pre-trend in the number of calls or a change in the number of calls after an ambush. Estimates in Table 5, Panel B, and C, again show no significant change in calls after an ambush. Column 1 presents results for the entire period after an ambush. For both Panel B and C the coefficients are close to zero. For example, in Column 1 Panel B the coefficient suggests a 0.06 percent decrease in arrest likely calls. Column 2 presents results separately for short and longer-term effects. Again, each coefficient is not statistically significant.

These number of call results indicate that there is no meaningful change in the number of calls following an ambush. This suggests that I am identifying the effect of police ambushes rather than the effect of a decline in certain types of calls for service.

6 Conclusion

The fundamental question addressed in this paper is whether increased risk to police officers alters police behavior. As police ambushes only occur in some beats and are some of the most severe threats faced by officers, ambushes provide a unique setting to estimate the effect of violence on policing. Using this variation in ambushes, I can compare how police behavior changed in ambushed beats compared to un-ambushed beats, over time. I find that police ambushes lead to an 8 percent decline in arrests. This fall in arrests persists for at least three and a half years after an ambush. I also show that police severity, measured as use of force and civilian complaints, does not increase after an ambush. This is consistent with police officers de-policing after violent incidences.

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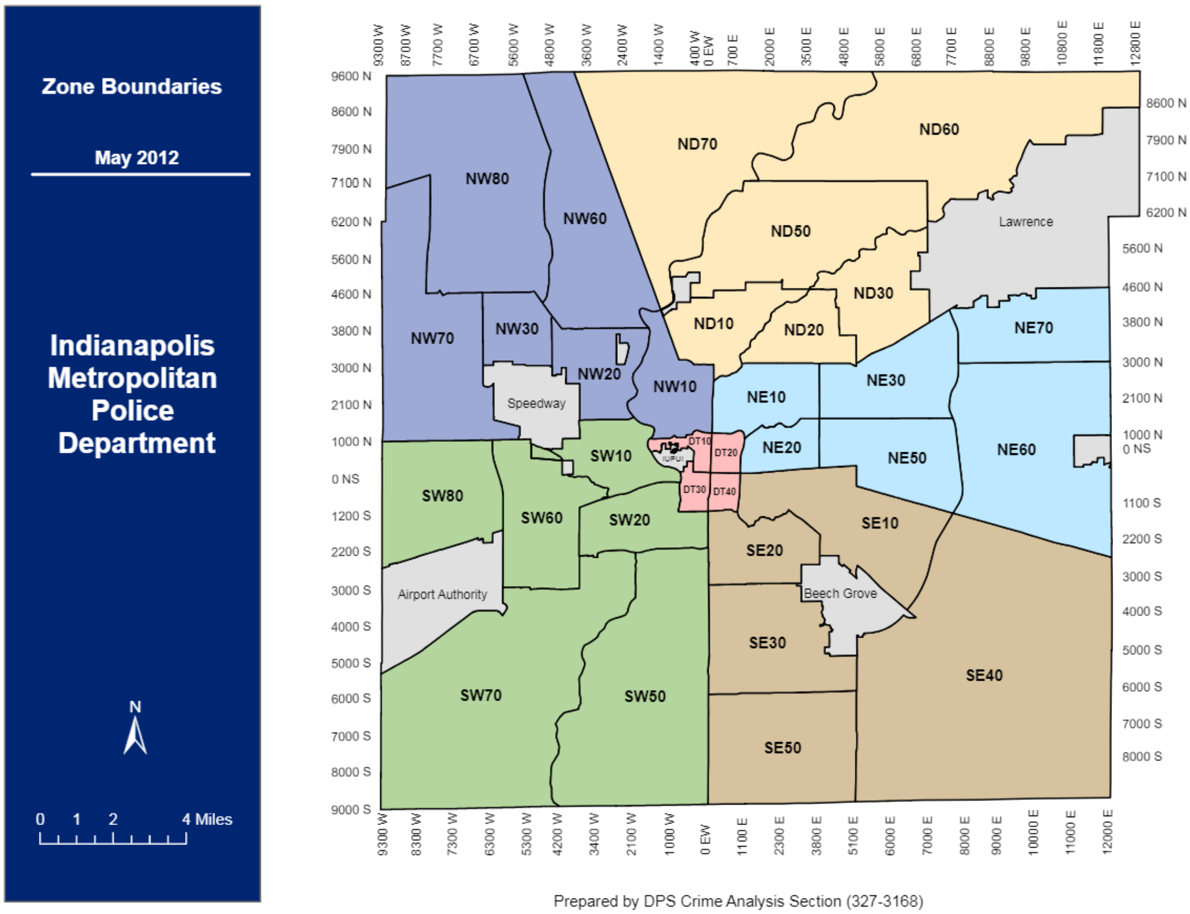
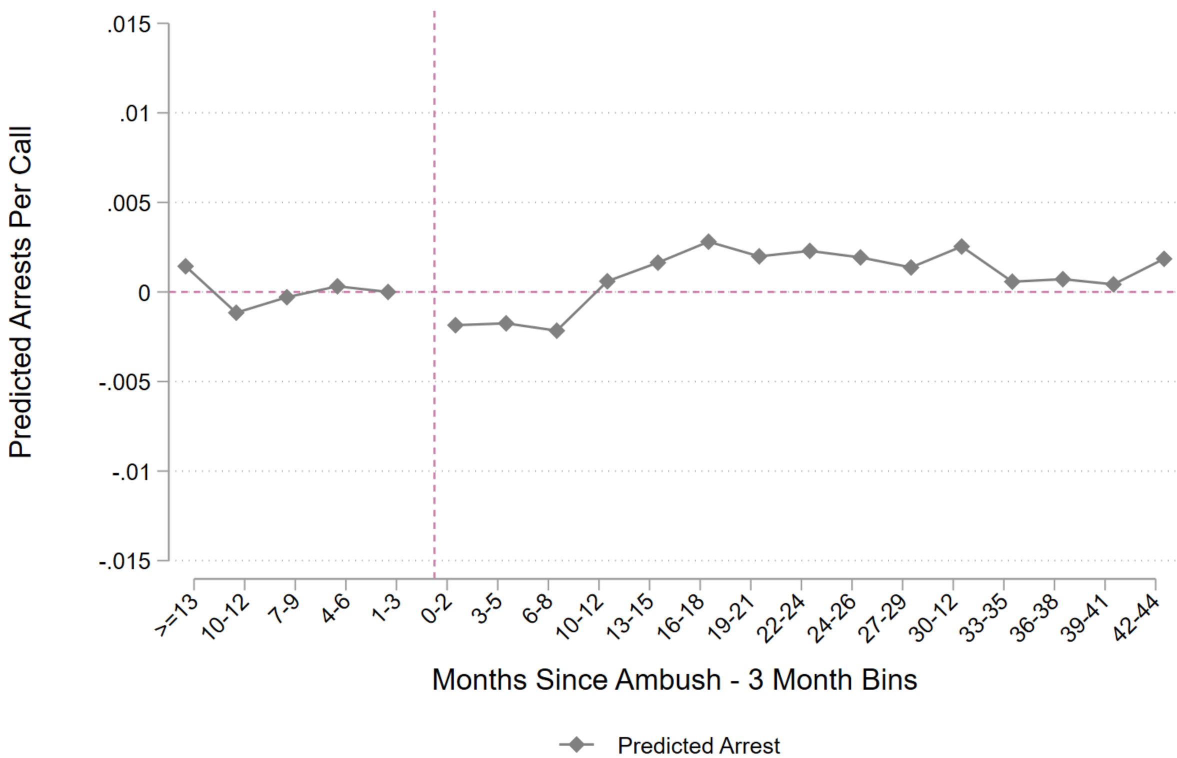
7 Figures and Tables

Figure 1: Indianapolis Police Beats



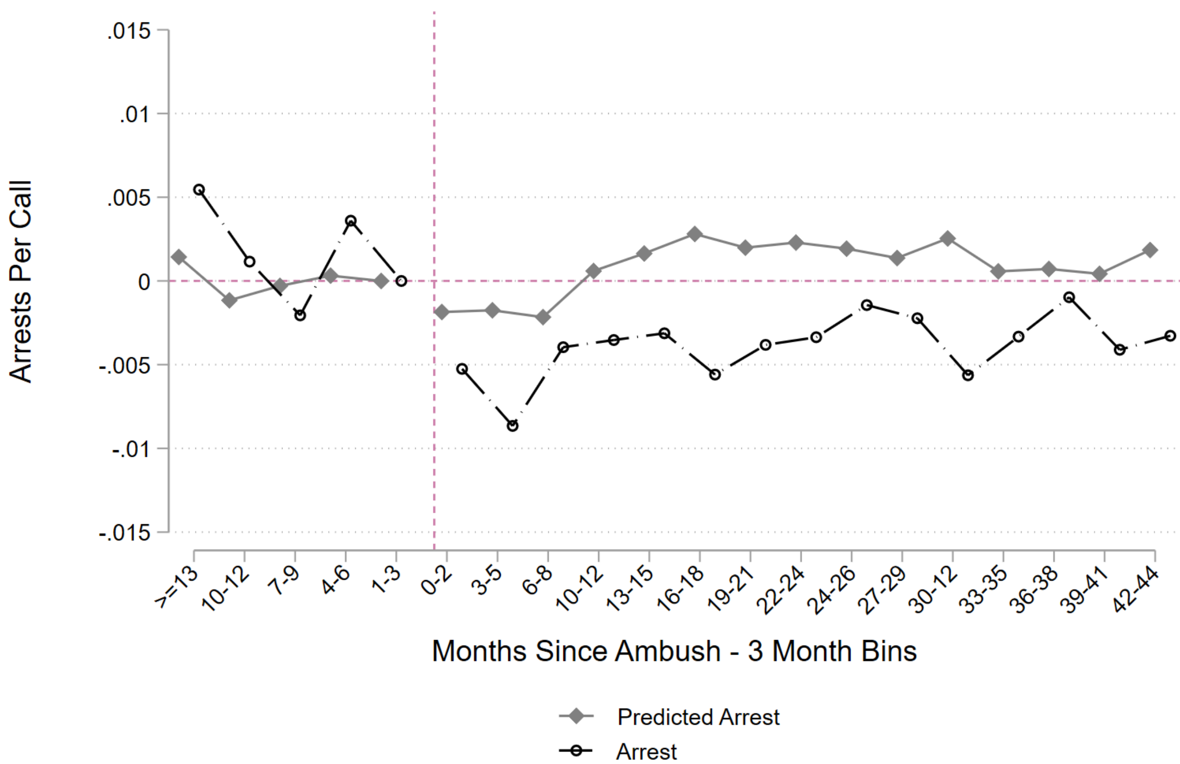
Notes: This figure shows a map of police beats (zones) in Indianapolis and was created by the Indianapolis Police Department.

Figure 2: The Effect of Ambushes on Predicted Arrests



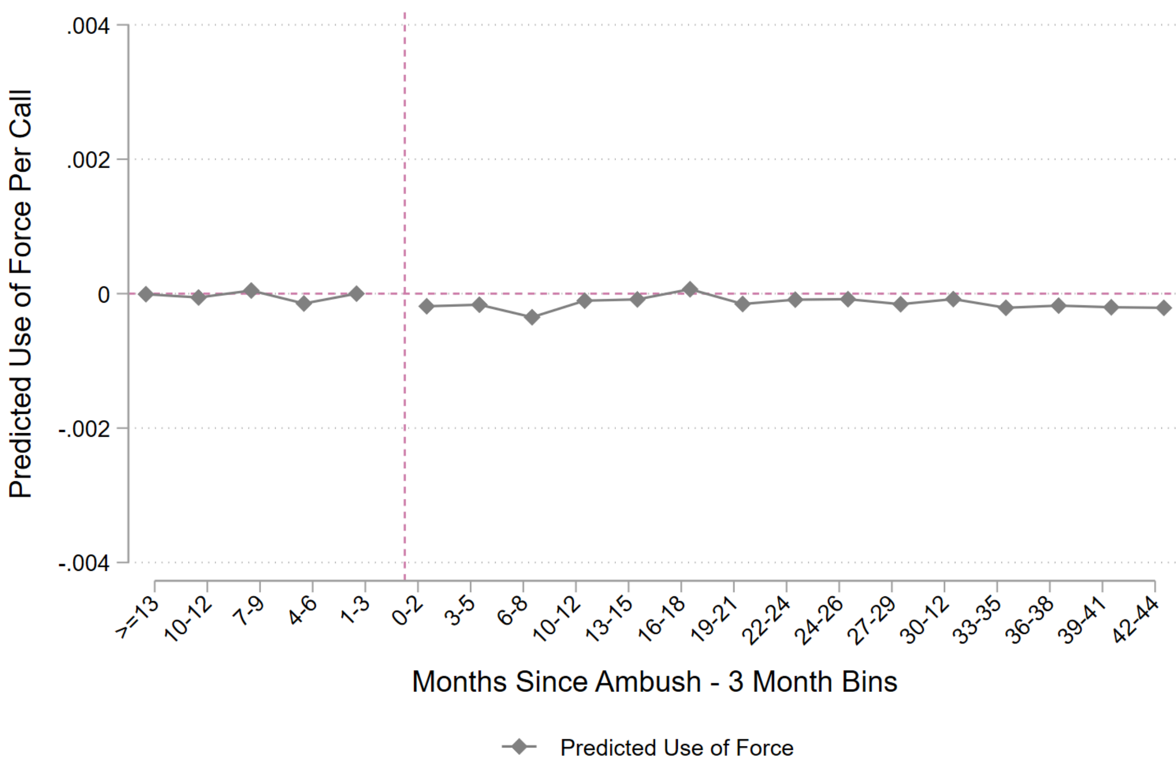
Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes individual police officer, year-x-month, and beat fixed effects. Arrest is predicted using observable call characteristics (latitude, longitude, time dispatched, call priority and call descriptions). Predicted arrest is measured at the call level.

Figure 3: The Effect of Ambushes on Predicted and Real Arrests



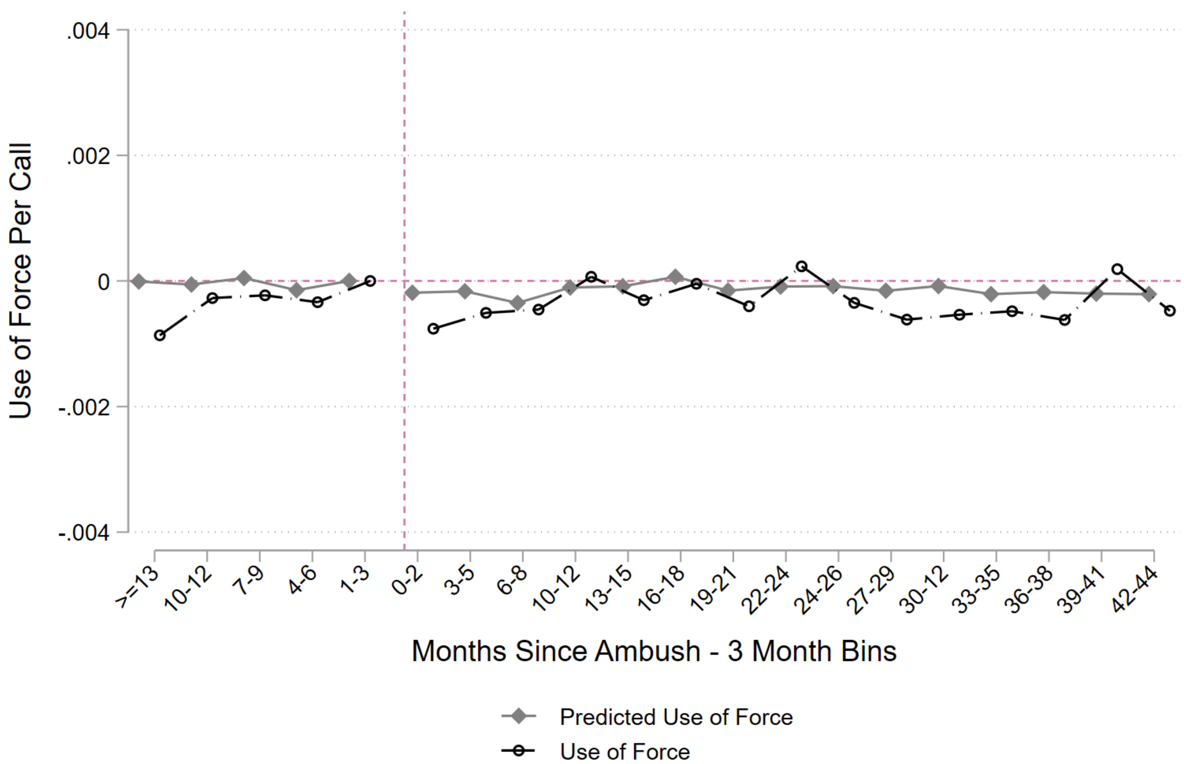
Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes police officer, year-x-month, and beat fixed effects. Results for predicted arrest and observed arrest are shown. Arrest and predicted arrest are measured at the call level.

Figure 4: The Effect of Ambushes on Predicted Use of Force



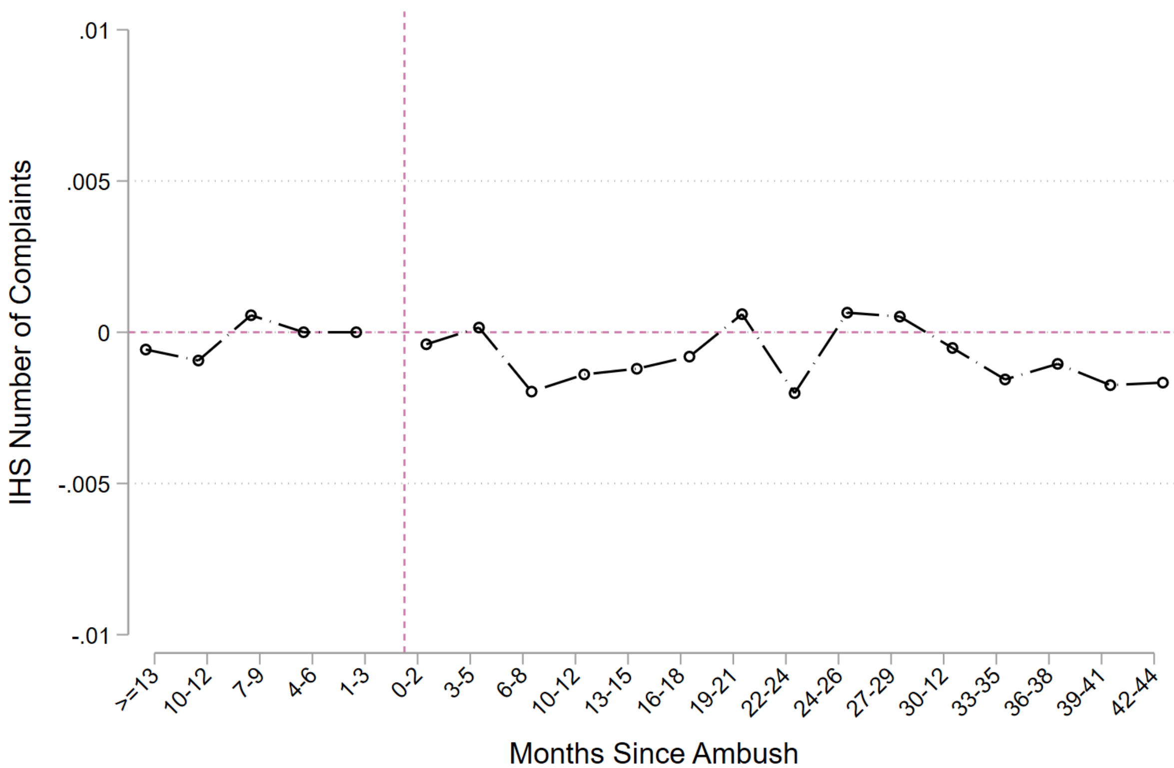
Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes individual, year-x-month, and beat fixed effects. Use of Force is predicted using observable call characteristics (latitude, longitude, time dispatched, call priority and call descriptions). Predicted use of force is measured at the call level.

Figure 5: The Effect of Ambushes on Predicted and Real Use of Force



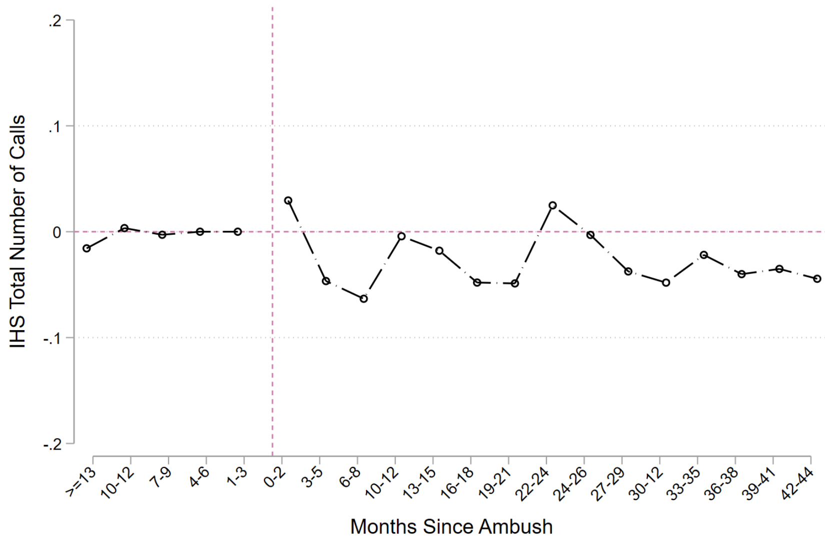
Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes police officer, year-x-month, and beat fixed effects. Results for predicted use of force and observed use of force are shown. Use of Force and predicted use of force are measured at the call level.

Figure 6: The Effect of Ambushes on Civilian Complaints

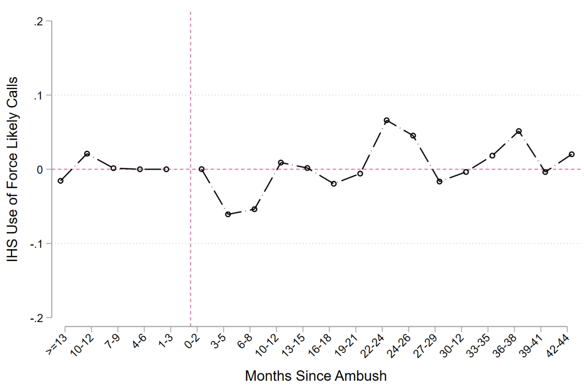


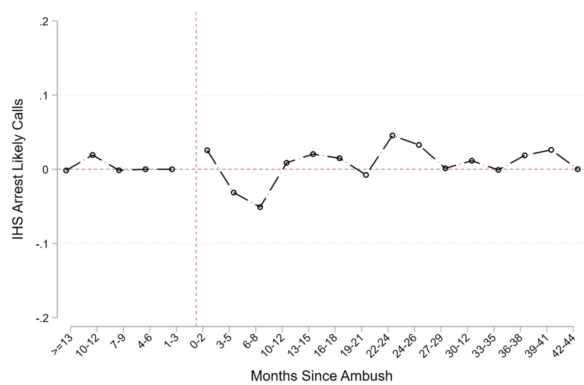
Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes year-x-month and beat fixed effects. The inverse hyperbolic sine of the number of civilian complaints is measured at the beat-day-hour level. The average number of complaints per beat-day-hour is 0.01 (or 2.3 complaints per beat per week).

Figure 7: The Effect of Ambushes on Number of Calls



(a) Total Number of Calls





(b) Arrest Likely Calls

(c) Use of Force Likely Calls

Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes year-x-month and beat fixed effects. The inverse hyperbolic sine of the number of calls is measured at the beat-day-hour level. The average number of calls per beat-day-hour is 6 (or 978 calls per beat per week). Arrest or Use of Force likely calls are calls with calls types that are in the top quartile of arrest or use of force likelihood, respectively.

Table 1: Summary Statistics

	Full Sample	Ambushed Beats	Un-ambushed Beats
Panel A: Call Level			
Arrest	0.0707	0.0760	0.0697
Use of Force	0.0056	0.0070	0.0053
X-Coordinate	365.3054	-86.0840	452.1991
Y-Coordinate	3900.7045	39.8056	4643.9372
Priority	1.7777	1.6385	1.8045
Observations	3435382	554500	2880882
Panel B: Beat-by-Day-by-Hour Level			
Civilian Complaints	0.0133	0.0090	0.0140
Calls	5.8483	7.1335	5.6581
Observations	1092870	140847	952023

Notes: Data are from Indianapolis calls for service from 2014-2017.

Table 2: The Effect of Ambushes on Arrests

	Arrest	Arrest	Arrest	Arrest	Arrest
After Ambush	-0.00587***	-0.00594***	-0.00601***	-0.00604***	
	(0.00123)	(0.00108)	(0.00107)	(0.00209)	
0-5 Months After Ambush					-0.00710***
					(0.00174)
>5 Months After Ambush					-0.00567***
					(0.00104)
Observations	3415078	3415078	3415078	3415078	3415078
Outcome Mean	0.0707	0.0707	0.0707	0.0707	0.0707
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of *Arrest* on beat specific indicators and an indicator treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level.

Table 3: The Effect of Ambushes on Use of Force

	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
After Ambush	-0.0000544	0.0000454	-0.0000626	0.000138	
	(0.000155)	(0.000158)	(0.000137)	(0.000316)	
0-5 Months After Ambush					-0.000284
					(0.000247)
>5 Months After Ambush					0.000124
					(0.000154)
Observations	3415078	3415078	3415078	3415078	3415078
Outcome Mean	0.00559	0.00559	0.00559	0.00559	0.00559
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of $Use\ of\ Force$ on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level.

Table 4: The Effect of Ambushes on Civilian Complaints

	IHS Number of Complaints	IHS Number of Complaints
After Ambush	-0.000541	
	(0.000362)	
0-5 Months After Ambush		0.0000896
		(0.000737)
>5 Months After Ambush		-0.000676*
		(0.000355)
Observations	1073724	1073724

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of the inverse hyperbolic sine of the number of complaints on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes year-x-month fixed effects. Column 2 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. The average number of complaints per beat-day-hour is 0.01 (or 2.3 complaints per beat per week).

Table 5: The Effect of Ambushes on Number of Calls

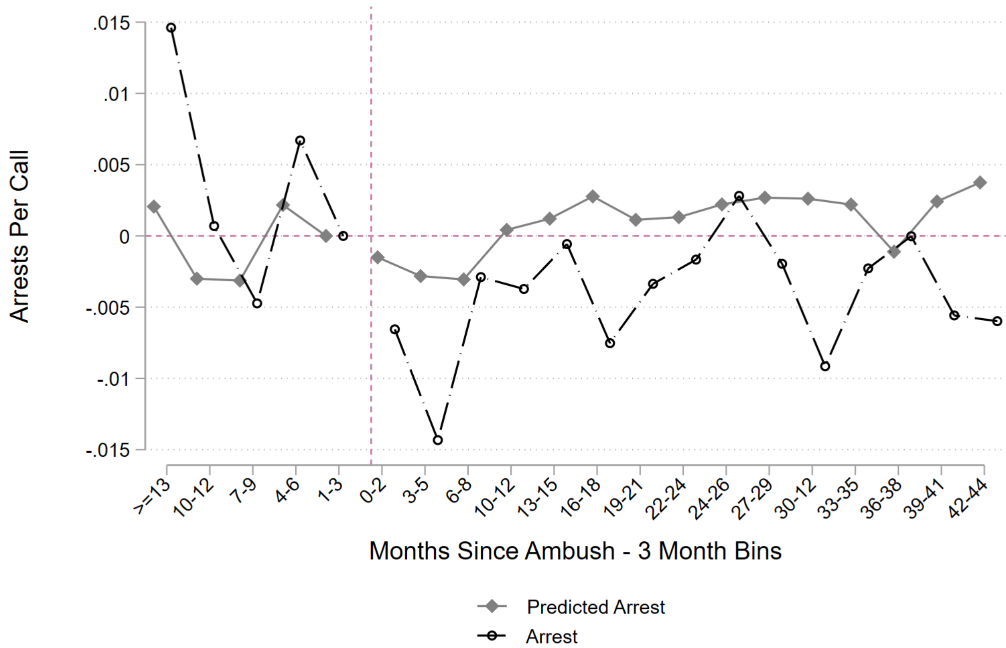
	IHS Number of Calls	IHS Number of Calls
Panel A: All Calls		
After Ambush	-0.0208	
	(0.0200)	
0-5 Months After Ambush		-0.00353
		(0.0199)
>5 Months After Ambush		-0.0245
		(0.0215)
Observations	1073724	1073724
Panel B: Arrest Likely Calls		
After Ambush	-0.000660	
	(0.0185)	
0-5 Months After Ambush		-0.0303
		(0.0189)
>5 Months After Ambush		0.00572
		(0.0233)
Observations	1073724	1073724
Panel C: Use of Force Likely Calls		
After Ambush	0.00376	
	(0.0181)	
0-5 Months After Ambush		-0.00541
		(0.0175)
>5 Months After Ambush		0.00574
		(0.0201)
Observations	1073724	1073724
Beat FE, Year-x-Month FE	Y	Y

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

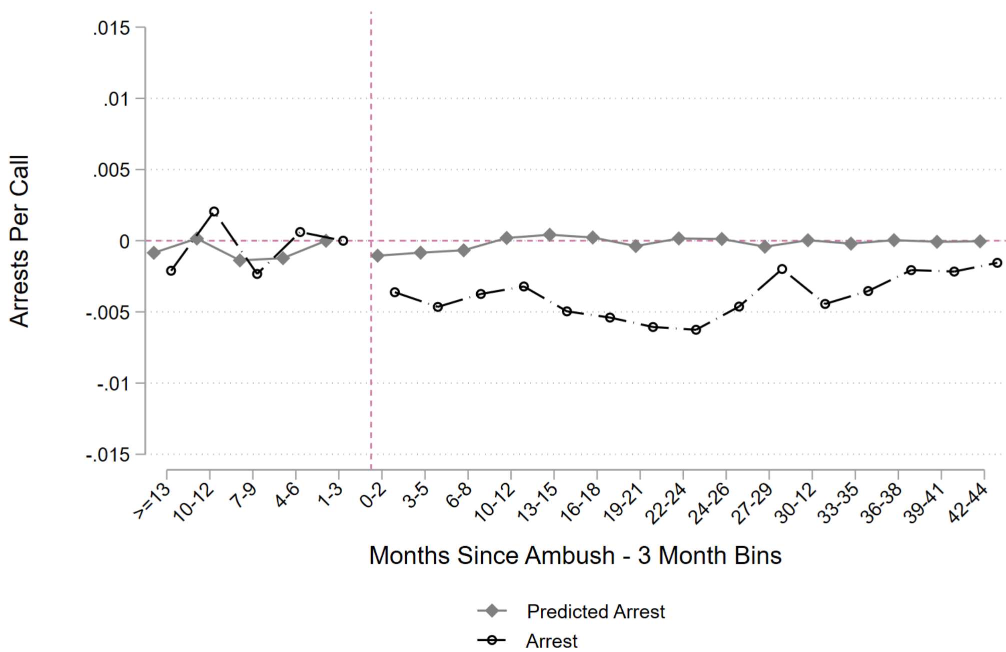
Notes: This table presents results from the regression of the inverse hyperbolic sine of the number of calls on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes year-x-month fixed effects. Column 2 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. The average number of calls per beat-day-hour is 6 (or 978 calls per beat per week).

A Appendix

Figure A1: The Effect of Ambushes on Predicted and Real Arrests



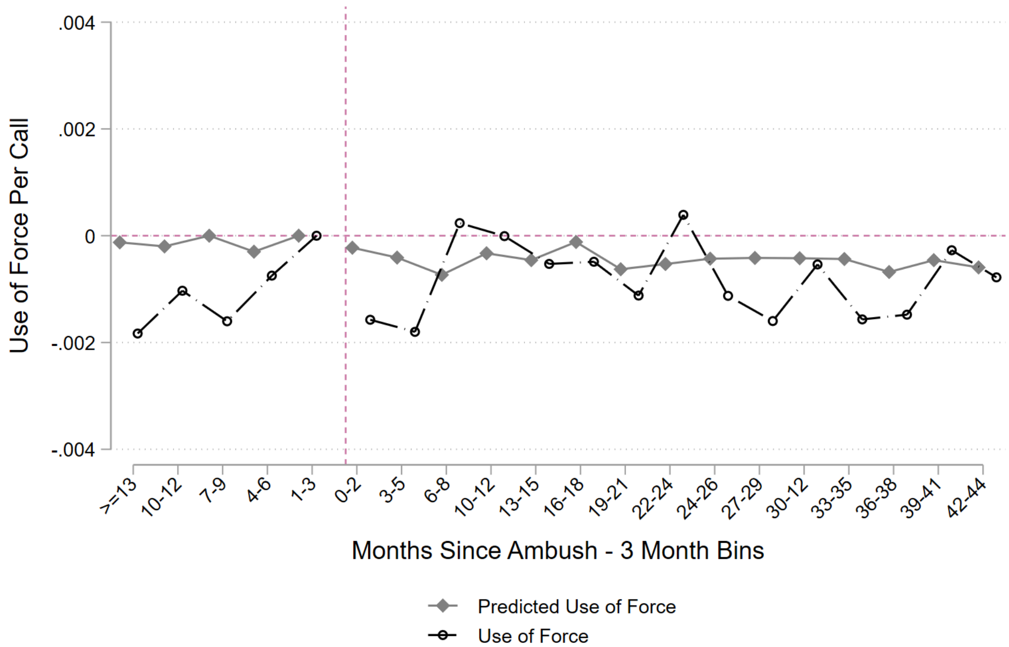
(a) Arrest Likely Calls



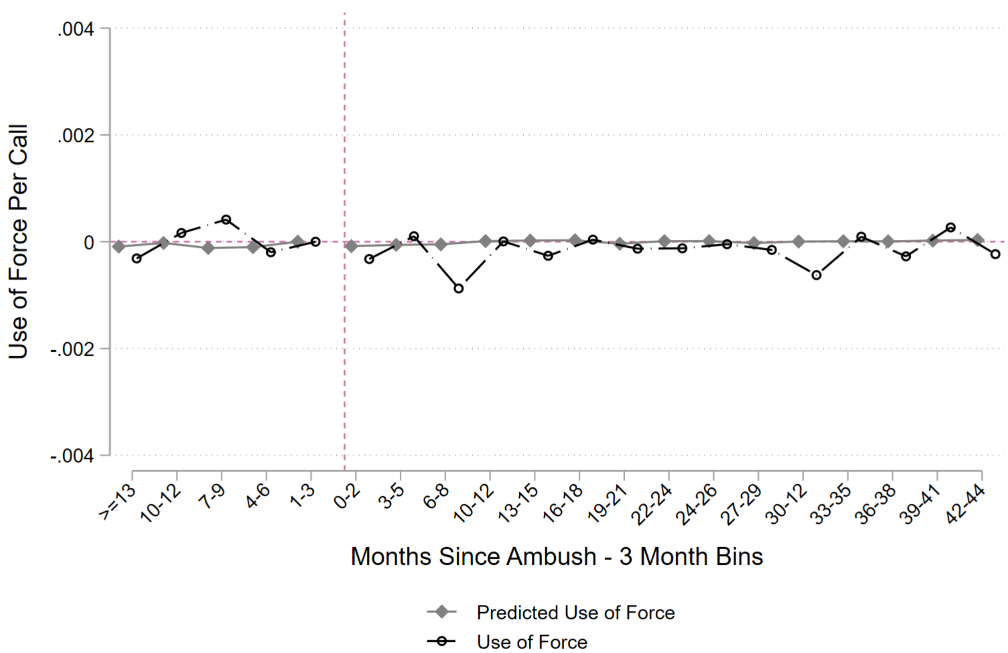
(b) Arrest Unlikely Calls

Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes police officer, year-x-month, and beat fixed effects. Results for predicted arrest and observed arrest are shown. Arrest and predicted arrest are measured at the call level. Arrest likely calls are calls with calls types that are in the top quartile of arrest likelihood.

Figure A2: The Effect of Ambushes on Predicted and Real Use of Force



Use of Force Likely Calls



Use of Force Unlikely Calls

Notes: This figure shows dynamic difference-in-difference estimates from Equation 2 and includes police officer, year-x-month, and beat fixed effects. Results for predicted use of force and observed use of force are shown. Use of Force and predicted use of force are measured at the call level. Use of Force likely calls are calls with calls types that are in the top quartile of use of force likelihood

Table A1: The Effect of Ambushes on Arrests for Arrest Likely Calls

	Arrest	Arrest	Arrest	Arrest	Arrest
After Ambush	-0.00806***	-0.00775***	-0.00813***	-0.00781***	
	(0.00246)	(0.00211)	(0.00220)	(0.00282)	
0-5 Months After Ambush					-0.0119***
					(0.00263)
>5 Months After Ambush					-0.00676***
					(0.00236)
Observations	1310451	1310451	1310451	1310451	1310451
Outcome Mean	0.133	0.133	0.133	0.133	0.133
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of *Arrest* on beat specific indicators and an indicator treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Arrest likely calls are calls with calls types that are in the top quartile of arrest likelihood.

Table A2: The Effect of Ambushes on Arrests for Arrest Unlikely Calls

	Arrest	Arrest	Arrest	Arrest	Arrest
After Ambush	-0.00395***	-0.00416***	-0.00414***	-0.00418*	
	(0.000804)	(0.000692)	(0.000584)	(0.00216)	
0-5 Months After Ambush					-0.00366**
					(0.00139)
>5 Months After Ambush					-0.00428***
					(0.000588)
Observations	2104627	2104627	2104627	2104627	2104627
Outcome Mean	0.0320	0.0320	0.0320	0.0320	0.0320
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of *Arrest* on beat specific indicators and an indicator treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Arrest unlikely calls are calls with calls types that are not in the top quartile of arrest likelihood.

Table A3: The Effect of Ambushes on Use of Force for Use of Force Likely Calls

	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
After Ambush	0.0000123	0.000267	0.000238	0.000207	
	(0.000328)	(0.000333)	(0.000267)	(0.000584)	
0-5 Months After Ambush					-0.000738
					(0.000686)
>5 Months After Ambush					0.000514
					(0.000335)
Observations	1162941	1162941	1162941	1162941	1162941
Outcome Mean	0.0113	0.0113	0.0113	0.0113	0.0113
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of *Use of Force* on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Use of Force likely calls are calls with calls types that are in the top quartile of use of force likelihood.

Table A4: The Effect of Ambushes on Use of Force for Use of Force Unlikely Calls

	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
After Ambush	-0.000154	-0.000166*	-0.000190*	-0.0000297	
	(0.0000965)	(0.0000960)	(0.000104)	(0.000219)	
0-5 Months After Ambush					-0.0000957
					(0.000231)
>5 Months After Ambush					-0.000182
					(0.000126)
Observations	2252137	2252137	2252137	2252137	2252137
Outcome Mean	0.00267	0.00267	0.00267	0.00267	0.00267
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents results from the regression of $Use\ of\ Force$ on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Use of Force unlikely calls are calls with calls types that are not in the top quartile of use of force likelihood.