Police Patrols and Crime*

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Abstract

A small but influential literature has used the aftermath of terrorist attacks to estimate large effects of police street deployment on crime. However, the elasticities obtained in these settings may not easily extrapolate to more ordinary circumstances. This paper exploits a natural experiment that increased police presence by economically-realistic amounts, and under relatively normal circumstances. We find the effect of police patrolling on crime to be a precisely estimated zero.
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1 Introduction

The causal relationship between police street deployment and crime represents one of the key tests of the deterrence mechanism underlying the standard economic model of crime (Becker, 1968)[1]. It is also an important policy parameter in its own right. In England for instance foot patrol is at the core of Robert Peel's pioneering vision of a modern police, and retains enormous contemporary importance (Reiner, 2000). In the US 68% of police officers are assigned to patrol operations (Reaves, 2015). Understanding their effect on crime prevention is critical to an accurate evaluation of whether this lopsided allocation of resources seems warranted.

The most empirically sound economic studies (Di Tella and Schargrodsky 2004; Draca, Machin and Witt 2011) estimate the effects of large and sustained increases in static police deployment following deadly terrorist attacks[2]. These two terrorism-based studies identify large and remarkably similar elasticities of around 35%. Together with the seemingly modest effects of lengthier prison sentences, these findings have led to the demand for visible police deployment to be a central policy lever in terms of crime deterrence (Durlauf and Nagin 2011, Chalfin and McCrary 2014).

The terrorism-based studies above are undoubtedly persuasive in arguing that deterrence is at work in certain specific circumstances. However, from a policy perspective it is unclear how useful these elasticities are. The large, sustained and concentrated deployments in these papers create ideal conditions for police presence to be highly salient to potential offenders. This salience is likely compounded by the fact that the aftermath of a deadly terrorist attack is an occasion when citizens (including potential offenders) are unusually aware of police levels.

In general, round-the-clock protection of a highly sensitised citizenry is economically and politically unfeasible. Instead, police protection typically consists of officers moving around large areas while spending little time in each location, i.e. *patrolling*. Policy-makers

[1]By contrast, police manpower numbers may affect crime through the combination of deterrence and incapacitation effects (Levitt 1997, Evans and Owens 2007, Machin and Marie 2011).

[2]Di Tella and Schargrodsky (2004) analyse the 24/7 police protection that the Argentinian federal government decided to provide to 270 Jewish and Muslim institutions, following a terrorist attack in the main Jewish center of Buenos Aires. Draca, Machin and Witt (2011) study the increase in police deployment following the July 2005 terror attacks in London. This deployment was heavily concentrated around underground stations, which had been the main target of the attacks. Because of this, the allocation of additional officers to each borough was proportional to the number of stations in the borough. An additional often cited study is Klick and Tabarrok (2005), which uses terror alerts in Washington D.C. as a source of (time series) variation in police deployment. Regarding internal validity, a potential drawback of these empirical strategies is that terror events and threats may terrorize the population and lead to changes in victims' and offenders' behavior. Not controlling for such 'correlated shocks' would bias the elasticity away from zero.

must therefore evaluate whether the rather thin cover afforded by these patrolling officers is a worthwhile use of resources. The use of terrorist attacks, while useful in terms of exogenous variation, may hinder the extrapolation of the resulting elasticities to these policy-relevant scenarios.

Estimating the effect of police patrols under more normal circumstances is empirically challenging. In addition to the obvious difficulty of isolating exogenous variation in police presence, an appropriate research strategy must leverage sufficient statistical power to identify the effects of small levels of police presence. This requires an exceptionally rich dataset with both a large number of observations and the measurement of police presence with a high level of granularity.

This Study We estimate the effect of police patrols on crime by exploiting a unique dataset tracking police officers in real time, and a policy experiment that created the type of variation (small, short-lived, unrelated to extraordinary events) in street deployment that closely resembles typical police patrolling. The elasticity that we find differs greatly from those in the aforementioned terrorism-based papers.

In October 2013, Essex Police (UK) introduced a new operation that, over a nineteen month period, targeted a total of 6,000 200 metre-radius areas. Every week a different set of areas was chosen, with each area receiving an average of ten additional minutes of police presence per day. Together, these areas represented the population of locations where crime (specifically, burglary) occurred in Essex, during this period.

The policy had two features that make it uniquely valuable from an econometric perspective. Firstly, the weekly choice of targeted areas was determined by a simple and rigid rule. We can use this rule to identify areas that would have been targeted in the period immediately preceding October 2013, had the policy already been in place. Secondly, the time variation of the increase in policing for the selected areas was discontinuous and driven by inflexible organisational constraints: it started on Friday and lasted exactly seven days. As we detail in the next section, these two features of the policy allow us to convincingly isolate exogenous variation in police presence, and to account flexibly for the possibility of correlated shocks in the crime propensity of an area.

We use these two features to construct an instrumental variables empirical strategy, and find the effect of police presence on crime to be a precisely estimated *zero*. Importantly, our estimates allow us to reject an elasticity *half the size* of those found by Di Tella and Schargrodsky (2004) and Draca, Machin and Witt (2011).

In the presence of such a zero effect, it is particularly important to understand whether our result might be following from deficiencies in the estimating sample or the measurement of the crime and police deployment variables. We argue that this is unlikely, for three reasons. Firstly, compliance with the policy was very high and this permits the construction of unusually strong instruments[3]. Secondly, the large temporal and geographical breadth of the policy provides sufficient statistical power to rule out relatively small effects. To confirm this, we sequentially experiment with a large variety of clustering strategies, and find that the estimated standard errors remain low throughout. Lastly, this paper is unusual in measuring police presence through a GPS-based dataset that records the location of each police officer every 2.22 minutes. We argue that the precise measurement of the police presence variable makes measurement error in the independent variable an unlikely explanation for our findings.

We explore other potential explanations for our main finding, and end up rejecting all of them. Firstly, we find that the additional police deployment created by the policy occurred during the hours of the day when crime typically takes place. This suggests that temporal misallocation of patrolling intensities is not a likely explanation for the zero finding. Secondly, our main finding is robust to varying the type and circumstances of the crime. For instance, we find the same effect when focusing only on crimes cataloged to have taken place in the street (where patrolling should in principle be more effective). The effect is also the same effect regardless of the pre-existing crime or patrolling intensities of an area.

It would be surprising if, even under the most favourable circumstances, police street deployment was completely unable to deter crime. More interesting in our opinion is the question of whether police patrolling *as typically practiced* provides a meaningful contribution to crime prevention. We believe that the natural experiment that we exploit here generates an estimate that is more easily extrapolated to the circumstances and levels of police deployment that are the norm. The fact that this estimate is not statistically different from zero has important implications when allocating resources to combat crime. In this respect, recent findings by Mastrobuoni (2016) and Blanes i Vidal and Kirchmaier (2017) suggest that emphasising the incapacitation effects produced by rapid response policing may be more effective than relying on the deterrence of thinly-spread patrolling officers.

Related Literature A large literature in economics has studied the relation between police manpower and crime, typically at the state or city level (Levitt 1997, McCrary 2002,

[3]It is well-known that the (weak instruments) 2SLS coefficients are biased in the direction of their OLS counterparts (Bound, Jaeger and Baker, 1993). Having a strong instrument is important because, as it is typical in this literature, the OLS estimates of crime on police are *positive* in our sample.

Evans and Owens 2007, Machin and Marie 2011, Chalfin and McCrary 2017). While its findings are important for general budgetary planning, they do not directly address the channels through which manpower may matter, and therefore what the police should actually do in order to decrease crime. For instance, it may be that higher police numbers allow for more thorough investigations and for the incapacitation of a higher number of criminals. Because the implications of the incapacitation and deterrence channels are very different, separating the two effects is important. Levitt (1998) shows that this is very difficult with police manpower data.

Our paper contributes to the small but influential body of work in economics analysing one particularly important practice: visible street police deployment. In addition to the seminal terrorism-based papers, a related study is Weisburd (2017). As a result of the dual role of officers in terms of proactive and reactive policing, she shows that officers leave their patrolling beat unattended when they are asked to respond to incident calls outside it. This is used as a source of exogenous variation in patrolling, and the study greatly benefits from the ability to track the location of police officers as they travel around the city. The instrument is however subject to the caveat of potential geographical correlation in crime patterns[4].

An extensive body of work in criminology has studied the effect of police street deployment on crime. Early influential studies on random patrolling in Kansas City (Kelling et al., 1974) and Newark (Kelling 1981, Pate 1986) found no evidence of an effect but were thoroughly criticised on methodological grounds. Subsequent research exploits field experiments targeting high-crime areas, or 'hotspots' (Braga, 2007). These papers often find statistically significant effects, which is unsurprising given the often large sizes and long durations of the interventions (Nagin, 2010)[5]. We differ from these studies in that we use natural variation to analyse the effects of realistic increases in patrolling in areas with representative levels of crime.

[4]MacDonald, Klick and Grunwald (2015) and Heaton et al. (2016) study the effect of *private* police on crime by using discontinuities in police intensity around the boundaries of university campuses. In their settings, university police increases the number of patrols in an area but may also result in better investigations and more arrests, with the resulting incapacitation effects. It is therefore problematic to interpret their effects as uniquely reflecting deterrence. Consistently with the notion that slow-moving incapacitation may be at work, an expansion of the jurisdictional boundaries of the Chicago campus police in Heaton et al. (2016) was not accompanied by corresponding decreases in crime.

[5]In Ratcliffe et al. (2011), for instance, patrolling was static and close to the 24/7 dosage in Di Tella and Schargrodsky (2004). It would be extraordinary if such a treatment failed to yield an effect on crime. In Sherman and Weisburd (1995) treatment areas received a much lower dosage, and statistical differences between treatment and control areas were limited to vandalism and drunk behaviour. No effects were found on more serious crimes such as assault, theft or burglary.

Plan We describe the institutional setting in Section 2. We outline the empirical strategy in Section 3. In Section 4 we present the data and discuss some descriptive evidence. We present and interpret the results in Section 5. Section 6 concludes.

2 Institutional Setting

In this section, we outline some of the key features of the institutional setting in which our study takes place.

Police Patrols Essex Police employs approximately 2,750 police officers to serve a population of 1.77 million, or 155 per 100,000 inhabitants[6]. The county of Essex includes both rural and suburban areas, as well as five towns of between 100,000 and 200,000 inhabitants (Basildon, Southend-on-sea, Thurrock, Colchester and Chelmsford), stretching an area of 3,600 square km (1,400 square miles). Its level of crime is fairly typical of the United Kingdom. For instance, in 2016 Essex Police was ranked 17th (out of 43 forces in England and Wales) in crimes per capita.

96% of Essex police officers work in frontline roles, which includes patrolling neighbourhoods, responding to 999 calls, roads policing and protecting vulnerable people (Dempsey, 2016). Police patrolling is the main responsibility of ten community policing teams, each covering a distinct district. Every team includes a combination of police constables (i.e. 'sworn' police officers) and PCSOs (i.e. Police Community Support Officers of a lower rank and lacking in certain legal powers). These neighbourhood officers can operate either by car or on foot, as their remit includes engaging personally with members of the local community. Additional patrolling is also occasionally provided by response police constables, whenever they are not engaged in their core responsibility of attending to 999 calls. These constables typically travel by car, as they must be ready to attend the scene of a perhaps distant incident on short notice.

Predictive Policing The natural experiment that we exploit in this paper originated in the first semester of 2013, when the leadership of Essex Police decided to use 'predictive policing' to assign resources more effectively. The idea behind predictive policing is that the areas where future crimes will occur can be successfully predicted by using high-quality datasets on recent criminal activity (Perry, 2013). Underlying this belief is the academic

[6]Numbers are based on full time equivalent and exclude Police Community Support Officers and Special Constables. They are only slightly below the UK average of 180 (Allen and Dempsey, 2016).

finding that most crimes are suffered by a very small number of victims, and that therefore repeat incidents account for a very large proportion of crimes (Pease, 1998).

A type of predictive policing that has received substantial attention from UK academics, police organisations and popular media is the optimal forager theory[7]. This theory posits that criminals, and in particular burglars, tend to return to the areas where they have successfully committed crimes in the recent past (Bernasco, 2008). Following from this, the theory predicts that an area is most at risk of being targeted again in the immediate aftermath of a burglary occurring in it (Ross and Peace 2007). Assigning intensive patrolling to these areas therefore represents, according to the theory, a good use of resources in terms of crime deterrence.

To the best of our knowledge, no evaluation was undertaken to investigate whether the theory was supported by empirical evidence in the case of Essex[8]. Despite this, the force decided to introduce a programme designed to combat the supposedly higher burglary risk. In the context of the UK, such a programme was not unusual. By 2013, policing initiatives based on the optimal forager theory had been undertaken in several UK cities including Trafford (Manchester), Birmingham and Leeds[9].

Operation Insight In the US, the concept of predictive policing has led to a number of sophisticated software programmes (such as PredPol and Temple's Near Repeat Calculator) that attempt to identify the likely location of future crimes. Essex Police, by contrast, decided to adopt a simple rule in its aim to protect the vicinities of recent burglaries. The initiative was labelled Operation Insight. It was introduced in October 2013, and it worked as follows. A team of analysts identified every Thursday evening the locations where domestic burglaries had occurred during the previous seven-day period. The analysts would next draw circles of 200 metres radius, centered around every one of those locations (see Figure 1 for an example of one such area). The maps displaying these circular areas would then be put together in ten separate PowerPoint presentations and distributed every Friday to the police officers in the ten separate districts into which Essex Police is organised (a reproduction of a map from the Colchester district is provided in Figure 1). Police officers also received the explicit instruction to patrol each area whenever possible, and to stay inside for a minimum of fifteen minutes during every visit.

[7]In the US, the theory is often known as the 'near-repeat' victimisation (Townsley et al. 2003, Johnson and Bowers 2004). For influential UK media coverage, see http://www.bbc.co.uk/guides/zqsg9qt.

[8]We carry out such an investigation in Section 5 and find no supporting evidence.

[9]All three initiatives were proclaimed successful by the police forces introducing them, although, with the partial exception of Trafford (Chainey, 2012), no credible independent evaluations were undertaken to investigate their effects (Longstaff et al., 2015).

The increase in police presence starting each Friday was designed to last exactly until the following Thursday. By then new burglaries would have occurred and new areas would be identified and chosen for intervention. While lacking in the sophistication of other software-based initiatives, the temporal increase in police presence was expected to provide a strong deterrent effect on the anticipated surge in crime predicted by the optimal forager theory.

An Unexceptional Natural Experiment Essex Police introduced Operation Insight in order to protect the areas around recent burglaries from being burgled again. Because a patrol officer may however deter all kinds of criminals, we will be using the variation in policing to study the effects on all types of crimes. Our study is similar in this respect to the terrorism-based papers because we also study the effect of increases in policing that follow specific criminal episodes (burglaries vs. terrorist attacks). An advantage of our setting is that the number of crimes triggering the increase in patrolling is very large (close to nine thousand). As we argue in the next section, this permits the selection of a highly credible control group, as well as the use of idiosyncratic within-treated-area variation in the timing of the additional deployment.

As we mentioned in the introduction, an additional major advantage is that the changes in policing were not triggered by exceptional events. Firstly, Operation Insight only required police officers to spend slightly more of their time in the vicinity of houses that had been burgled during the previous days. Such small variation in the typical patrolling activities of officers contrasts with the static, sustained and highly visible increases in police deployment following the terrorist attacks of London and Buenos Aires. Secondly, the increase in deployment occurred under relatively normal circumstances. With the obvious exception of the burgled household, we would not expect potential victims and offenders to display abnormal levels of sensitivity to the levels of police presence.

3 Empirical Strategy

Figure 2A provides a stylised timeline of police deployment in the 200m.-radius areas, as envisaged by Operation Insight. Note that we define a week in the figure and throughout the paper as the seven-day period starting on a Friday. We can see that for an area hit by a burglary on a particular week (the 'burglary week'), the policy was designed to be activated at the beginning of the following week (the first day of the 'post week', henceforth 'post Friday'), and to be deactivated at its end. In this section we discuss our strategy to estimate the effect of patrolling on crime, placing special emphasis on potential challenges to identification and the measures that we undertake to overcome them.

Differences-in-Differences Variation We first discuss the differences-in-differences variation that we use as part of our empirical strategy. An important concern in using the variation from Figure 2A for identification is that the crime triggering the additional police deployment may have an independent effect on the likelihood of future crimes, as potential victims and/or criminals respond to it by altering their behaviour. A different version of the correlated shocks problem is that the triggering burglary may have been the result of a temporal change in the underlying conditions of an area that makes crime particularly likely (for instance, public holidays in areas where residents are likely to take vacations abroad).

The seminal terrorism-based papers are of course also subject to this issue of correlated shocks, and undertake a battery of additional tests to alleviate it. In our context, this concern is strong because the concept of predictive policing that inspired Operation Insight is predicated upon the existence of correlated shocks, in particular on the assumption that burglars respond to a successful burglary by returning to its vicinity[10]. If unaccounted for, such a correlated shock would generate a positive bias on our estimates and render any claim based on differences-in-differences variation unconvincing.

To successfully exploit differences-in-differences variation, we therefore need a control group consisting of geographical areas that are identical in every respect to the areas treated by Operation Insight (including being the location of a recent burglary), except for the corresponding increases in patrolling. Since burglaries are very common events, we have a natural control group: areas around burglaries that did not receive an increase in police presence as these burglaries occurred immediately prior to the introduction of Operation Insight.

Our dataset, which we describe in more detail in the next section, therefore includes the 200m.-radius areas around all burglaries between January 2013 (ten months before the start of Operation Insight) and April 2015. We label an area as 'treated' if the corresponding burglary occurred during Operation Insight. For every area we then compute police presence and crime levels both for the burglary week and the post week, as well as one week on either side.

The differences-in-differences variation part of our empirical strategy is illustrated in Figures 2A and 2B. The identifying assumption of empirical strategies based on differences-in-differences variation is typically the existence of 'parallel trends' among the treated and

[10]We find in Section 5 that there is in fact very little empirical support for the existence of correlated shocks following burglaries in Essex County during our sample period.

control groups. In our context, the assumption is one of identical correlated shocks following a burglary for the pre-October 2013 and the post-October 2013 periods. Specifically, if the existence of a burglary changed the baseline crime propensity by $X\%$ for the pre-October 2013 areas (for the post week, relative to other weeks), the assumption is that the additional crime propensity following a burglary is also $X\%$ for the post-October 2013 areas (for the post week, relative to other weeks). While we regard this assumption as quite weak, we use the variation in police presence by burglary day of the week to relax it further.

Day of the Week Variation As we discussed in Section 2, the increase in patrolling in an area was designed to be triggered on the Friday following a burglary, regardless of the day of the week when the burglary occurred. Figure 3 illustrates how this rule led to some burglaries (e.g. Thursday burglaries) triggering additional police deployment almost immediately, while other burglaries (e.g. Friday burglaries) led to additional presence only as many as six days into the future. This idiosyncratic variation in the timing of the increase in police presence allows us to introduce a control dummy for every day following and preceding a burglary, without exhausting all the variation in the data.

To understand the effect of these dummies, consider the +1 days (relative to the burglary date) dummy. This dummy perfectly controls for the average underlying likelihood for an area to be the location of a crime on the day immediately following a burglary in that area. The identification of the effect of police on crime then exploits the fact that for some burglaries the +1 day is also associated with an increase in police presence, while for other burglaries the +1 day is not. Controlling for this full set of dummies, we are then able to identify the effect of patrolling on crime under the assumption that any potential correlated shocks following a burglary do not differ depending on the day of the week on which the burglary occurred.

Estimating Equations We have just discussed conceptually two sources of variation in police presence associated with the introduction of Operation Insight, together with the identifying challenges that exploiting these sources allows us to overcome. We now outline an empirical strategy, consisting of a baseline dataset and a set of estimating equations, designed to take advantage of these sources of arguably exogenous variation. We postpone the description of data sources and the details of variable construction until the next section.

We construct our baseline dataset as follows. For every burglary occurring in Essex between January 2013 and April 2015 we identify the 200m-radius area surrounding it. Figure 4 displays a map of Essex County and the location of these 8,662 areas. Every area is then followed for a 28-days period, centered around the 'post-Friday' (i.e. the first day of the 'post week'). This creates a balanced panel, with $t = 1...28$, where $t \in [8, 14]$ during the burglary week and $t \in [15, 21]$ during the post week. An additional time characteristic of every observation that we explicitly account for is its relation s to the burglary date, with $s = 0$ for the burglary day, $s \in [-14, -1]$ for the preceding days and $s \in [1, 19]$ for the following days.

We use a 2SLS strategy to estimate the effect of police presence on crime. The first stage equation predicts police presence in area i on day-relative-to-post-Friday t, which is also day-relative-to-burglary-date s, as follows:

$$Police_{its} = \gamma_i + \lambda_t + \theta_s + \beta_1(Treatment_i \times Post_t) + \epsilon_{its} \tag{1}$$

where γ_i are area fixed effects, λ_t are day-relative-to-post-Friday fixed effects, and θ_s are day-relative-to-burglary-date fixed effects. $Treatment_i = 1$ for areas created around post-October 2013 burglaries, and $Post_t = 1$ if $t \in [15, 21]$ (i.e. the 'post week').
The second stage equation estimates the effect on crime of the arguably exogenous variation in police presence isolated by (1):

$$Crime_{its} = \alpha_i + \mu_t + \pi_s + \beta_2 \widehat{Police}_{its} + \upsilon_{its} \tag{2}$$

where β_2 is the coefficient of interest. As argued earlier, the first stage equation isolates variation in police presence that is unrelated to unobserved determinants of crime as long as: (a) any potential correlated shocks following a burglary do not differ across the day of the week in which the burglary occurs; *or*, (b) that these correlated shocks are not different in the post-Otober 2013 period, relative to the pre-October 2013 period. The identifying assumption of our empirical strategy is that either (a) or (b) (or both) are true.

As we mentioned in Section 2, the allocation of police presence was assigned every week separately to each of the teams responsible for the ten districts. Crime patterns may be correlated for areas within a district as a result of geographical proximity but also of the fact that there is a single team in charge of policing all areas within a district. We therefore cluster the standard errors at the district/week level. We show in Table 4 that alternative clustering strategies generate very similar standard errors.

4 Data and Descriptive Evidence

In this section we describe the data sources and the construction of and variation in the main variables of our study. We conclude the section by displaying naive OLS regressions of crime on police presence.

Data on Crime We use two main datasets in this paper, both made available by Essex Police under strict confidentiality agreements. First, we employ a standard dataset on the population of crimes occurring in Essex county between January 2013 and April 2015. For every crime we observe the crime type, time and date and, importantly, its precise geocoded location. Figure 5 displays the distribution of burglary crimes by day of the week, and highlights that the proportions are roughly similar across different days.

We use information on the timing and location of the 8,662 domestic burglaries in the dataset to select the 200m-radius areas in the baseline sample, where every one of these areas is the result of a burglary having occurred at its centre. We then follow every area for 28 days (centred around the post-Friday) and measure the number of crimes occurring in each area and day. Table 1 shows that the average number of crimes per day is .112, which corresponds to a crime approximately every 9 days[11]. As we display in Figure 6A, the variable is in fact almost discrete. In particular, the percentage of area/day observations with more than one crime is only slightly above 1%. Our baseline estimates are therefore based on linear models, although we also present Poisson-IV estimates in Section 5.

Table 1 also reveals that assaults, burglaries, thefts and criminal damage crimes are represented in roughly similar proportions. Approximately two thirds of the sample areas are treated, which follows directly from the fact that Operation Insight was in place during 19 out of the 28 months in our sample period. By construction of the sample, observations in the 'post week' comprise exactly one fourth of the total. Also by construction, the median distance to the 'post Friday' is 0.

Data on Police Presence Our second dataset records police presence across the geography of Essex County. Like many forces, Essex Police has provided every officer with a GPS tracking device which documents their location with very high frequency (in the case of Essex every 2.22 minutes). This information is used by the force control room to monitor officers and to optimise their allocation to incidents. Essex Police provided us with the dataset on the population of signals emitted by these GPS tracking devices during our sample period. In this dataset an observation is a signal from an officer at a particular moment in time and the information of interest is the location from which that signal was emitted.

We use this dataset to construct a measure of patrolling intensity in our sample areas, and at a daily level. The measure is the number of minutes during which at least one officer

[11]This is unsurprising given the small size (approximately 350 squared metres) of these areas. The relatively small variation in the dependent variable might be a problem for inference if we did not benefit from such a large sample size. We find in Section 5 that the coefficients of interest are in fact quite precisely estimated.

emits a GPS signal from inside an area. To construct this variable, we assume that officers enter the area on the second when the first signal from inside is emitted and that they remain inside at least during the following 2.22 minutes, until a new signal is emitted. We find in Table 1 that the number of minutes that the police patrols an average area/day is 38.896, approximately twice the median of 19.661. Figure 6B shows that the distribution of this variable is right-skewed, with 25% of observations with less than five minutes of police patrolling and a small number of observations being patrolled very intensely.
Studies on the effect of police patrolling on crime often lack direct measures of police deployment. Even in papers that are able to use such measures, it is often the case that police patrolling intensity is measured at a very high level of aggregation. An advantage of our study is that we are able to observe the intensity at which different areas are being patrolled at a highly precise level. Given our findings below of a zero effect of police patrolling on crime, we regard the ability to rule out substantial measurement error in the independent variable of interest as being an important advantage of our study.

OLS Estimates Figure 7 displays a kernel regression of the number of crimes on police patrolling time. In addition to suggesting a positive relation, note that the shape of the relation is approximately linear. As it is well-known in this literature, the positive correlation is clearly due to causality from crime to police. In our setting, this reverse causality likely follows from the fact that police officers are asked to attend the areas where crimes have recently occurred.

In Table 2 we confirm this 'naive' positive relation with a set of OLS models. In Column 1 we replicate in parametric form the kernel regression of Figure 7. In Column 2 we add date fixed effects, to control for the fact that both crime and police presence may be higher or lower on specific days. In Column 3 we add area fixed effects, as some areas may be more heavily patrolled if police officers perceive them to be associated with a higher propensity for crime. Even in this third relatively stringent specification the correlation between police and crime is positive and highly statistically significant.

5 Results

In this section we present and interpret the evidence using quasi-experimental variation in police presence at the daily level.

Baseline Estimates Table 3 displays the baseline findings of the quasi-experimental variation in patrolling at the daily level. We find in Column 2 that police patrolling was significantly higher for the treatment areas during the post week. Specifically, these areas received an average of 10.15 more minutes of police time. These additional minutes are highly statistically significant, and represent around one third of the mean of police time in the week prior to the burglary (i.e. 33.5). However, we see in Column 3 that this extra police time did not translate into a decrease in the average number of crimes. Column 3 also indicates that the instrument is very strong (Kleibergen-Papp F = 302.52). Column 1 confirms the absence of a reduced form relation between the instrument and crime.

Our baseline estimates are in direct contradiction with the most influential and credible estimates from the economics literature. Using the lower bound of the confidence interval implied by Column 3 (i.e. $.00014 - 1.96 \times .00037 = -.00058$), and the average crime and police presence during the week prior to the burglary (.11 and 33.5, respectively) we are able to reject an elasticity as low as -17.5%. This is approximately half the value of the elasticities estimated by Di Tella and Schargrodsky (2004) and Draca, Machin and Witt (2011).

Below, we explore potential explanations for this result. We note at this point that lack of statistical power does not appear a likely explanation, given that (a) the first stage estimation is strong both statistically and in terms of the magnitude, (b) the fairly conservative standard errors are quite small, allowing us to reject relatively small elasticities.

A stark visualisation of the increase in police presence having no corresponding effect on crime is provided by Figures 8A and 8B. In these figures we plot the estimates of the interactions between the treatment area dummy and each of the days relative to the post Friday dummies, after controlling for the other indicators from Table 3. We see in Figure 8A a sharp increase in police time that starts on the post Friday and lasts for exactly seven days, for the treated areas relative to the control areas[12].

We find however in Figure 8B that this large increase in police deployment had no impact at all on crime. Figure 8B also provides a (partial) test of the identification assumption underlying the differences-in-differences variation of the empirical strategy. Specifically, we find no evidence of a differential trend for treated and control areas in the days prior to the post-Friday (i.e. the 'parallel trends' assumption).

[12]It seems as if there was an increase in police presence also during the burglary week, for the treated areas relative to the control areas. This increase may be the result of Operation Insight focusing the attention of police officers on the importance of responding to and preventing burglaries.

Alternative Clustering In the baseline empirical strategy the standard errors are clustered at the district/week level. In Table 4 we report the second stage standard errors when these are clustered differently. In Columns (2) and (3) the clustering is only at the area and week level, respectively. In Column (4) we expand the time dimension of the cluster from the week level to the month level. In Column (5) two-way clustering (Cameron, Gelbach and Miller, 2011) is applied, at the district and week level. In Column (6) we allow for flexible spatial correlation between nearby areas, together with serial correlation within an area (Conley, 1999). The estimated standard errors remain low throughout. The bottom row in the table displays the lower bound of the estimated confidence interval, and shows that we can always reject relatively small elasticities.

Estimates by Time of Day We now start the investigation of potential explanations for the finding of a zero relation between patrolling and crime. The first explanation is that the additional police patrolling may have taken place during the 'wrong hours' of the day. Imagine, in an extreme scenario, that all the additional police patrolling occurs at daytime, while all crime occurs at nighttime. In that case it would be natural not to find an effect of the former on the latter.

Fortunately our dataset allows us to observe, at a very high level of detail, the timing of criminal and police activity within a day. Figure 9 shows that, in our sample, the allocation of patrolling effort throughout the day during our sample period was not highly misdirected. We compute in this figure the average number of crimes and police presence in the eight three-hour intervals of the day, relative to the daily totals (.112 for crimes and 38.896 for police time). We find in this figure, for instance, that 16% of crime takes place between 0am and 3am, and that police time during these hours is around 15% of the total daily police presence. Overall, there is no time interval with a very high discrepancy between police presence and crime levels, indicating that on average patrolling intensity is on average well allocated.

Nevertheless, it may be that the *additional* police presence from Operation Insight was misdirected, even if the *average* presence was largely not. We investigate this question by creating a new dataset of 8662 areas and $28 \times 8 = 224$ three-hour intervals, now centred around the 0-3am interval of the post-Friday. We then estimate:

$$Police_{its} = \eta_i + \rho_t + \delta_s + \sum_{k=1}^{8} \phi_k (Treatment_i \times Post_t \times Interval_k) + \omega_{its} \tag{3}$$

where η_i are area fixed effects, ρ_t are interval-relative-to-post-Friday fixed effects, and δ_s are day-relative-to-burglary-date fixed effects. $Interval_k$ are dummy variables for each of the eight three-hour intervals of the day. The estimates $\hat{\phi}_k$ provide evidence on the hours of the day on which the additional deployment from Operation Insight was concentrated.

We find in Figure 10A that Operation Insight increased police presence mostly between midnight and 3pm. These are hours that, according to Figure 9, are associated with higher than average crime. We conclude that the temporal misallocation of patrolling intensity is not a likely explanation of our baseline findings, since Operation Insight increased police presence during the hours of the day in which it should have had the highest effect.

In Figure 10B, we provide estimates of the model above, but substituting $Police_{its}$ by $Crime_{its}$ as the dependent variable. Consistently with our baseline findings we find that Operation Insight did not lead to meaningful decreases in crime at any time of the day.

Other Potential Explanations Table 5 investigates five additional potential explanations for our baseline results. We first study whether the potentially misspecified linear models used throughout the paper might be the reason for the finding of a zero effect of police on crime. Figure 6A shows that the number of crimes per day can be reasonably regarded as discrete, given the very small percentage of days when more than one crime occurs in an area. Nevertheless, it may be that count data models represent a better approximation to the data-generating process underlying the crime variable. We therefore estimate Poisson-IV models and display the corresponding coefficients in Table 6 Column 1. Whether in the reduced form regression or in the second stage, we find no evidence of an effect of police on crime. It therefore seems as if our baseline findings are not the result of functional form misspecification.

Essex county includes many rural and suburban areas where crime is not very common. While the areas in our sample are associated with at least one crime (i.e. the burglary around which the area is defined), it may be that the relatively rare nature of criminal events makes it difficult to identify how they are affected by police presence. Of course, the standard errors in our baseline estimates already account for this sample variability. Nevertheless, in Column 2 we restrict our estimating sample to areas that, during the week prior to the burglary week, were associated with high levels of crime (i.e. in the top quartile). The average number of crimes per day in these areas is .256, which represents a crime every four days rather than, as in the baseline sample, a crime every nine days. While the sample size in Panel B is much smaller, the instrument is still very strong (Kleibergen-Papp F-statistic = 83.47). We find that the first stage instrument is 9.51, suggesting that the police increase in patrolling time during Operation Insight was, in these areas, very similar to the increase in the average area. We also find that, even in these high crime areas, the increase in police presence did not have any meaningful effect on crime.

In Column 3 we perform a complementary exercise by focusing on areas that, during the week prior to the burglary week, were relatively less patrolled (i.e. in the bottom quartile). We focus on these areas because the additional patrolling associated with Operation Insight represents there a larger proportion of the pre-existing patrolling intensity. It may be that police patrolling had an effect on crime in these areas, even if it did not in the average area. We find that the increase in police minutes induced by Operation Insight was 10.29, which almost doubles the average, for these areas, of 13.56 minutes (Kleibergen-Papp F-statistic = 183.58). We find that the reduced form and second stage estimates are very small and not statistically different from zero. Therefore, decreasing returns to patrolling intensity does not appear to be a likely explanation for our baseline findings.

We next investigate whether police officers, by their mere presence, may have caused an increase in *recorded* crime. While certain crimes (e.g. homicide) are reported with a probability close to one, others (e.g. drug use) may sometimes only be recorded if a nearby officer happens to spot the crime in progress. As a result, police patrolling in an area may increase the number of recorded crimes, conditional on an underlying level of criminal activity. In principle this positive relation between police and crime may have counteracted a hypothetical deterrence effect, thereby explaining the zero coefficients that we find[13]. To study this notion, we create a new dependent variable that includes only the crimes that *did not* occur contemporaneously to the presence of a police officer inside an area[14]. In Column 4 we find that this new dependent variable, which is likely unaffected by this positive 'recording bias', is uncorrelated with police presence.

Lastly, we study whether police *street* presence at least has an effect on crimes that occur in the *street*. Given that street-based potential offenders should be able to notice the presence of a nearby officer very well (relative to their indoor-based counterparts), one may expect the effect to be higher for a new street-crime dependent variable. In Column 5 we find, however, that the effect is still zero for this new variable[15].

[13] Note that this positive 'recording bias' is potentially present in any study of police and crime. Our paper is unusual in that the richness to the dataset allows us to investigate empirically its likely importance. Blanes i Vidal and Kirchmaier (2017) find that in nearby Greater Manchester the percentage of crimes reported directly by a police officer rather than by a member of the public is actually very low. As a result, we would not expect this issue to be very important in practice.

[14] Specifically, we eliminate all crimes occurring in an area within one minute of a police officer being present in that area. We regard these activities as being potentially unrecorded as crimes in the absence of a nearby officer. The new dependent variable therefore has a mean of .087, which is lower than the mean of the baseline dependent variable.

[15] Note however that we are only able to measure the micro-location where a crime occurred with some error. Essex police classify crimes as having occurred: (1) inside an address, (2) at the rear of a building, (3) in the garden of a building (in the UK, most gardens are located at the rear of the property), (4) at an

Estimates by Type of Crime One potential explanation of our baseline findings is that police patrolling impacts specific types of crime, even if it is difficult to identify its effect on the average crime. In addition to being an interesting exercise per se, this heterogeneity analysis complements the street-crimes analysis above. For instance, we find a stronger effect on assaults than on burglaries, given that the latter are the quintessential 'home crime'. In Table 6 we display second-stage estimates using the numbers of crimes of specific types as dependent variables and find that police patrolling has no effect on any crime type.

Evaluation of the Empirical Strategy The empirical strategy that we describe in Section 3 is designed to overcome the confounding effect of potential correlated shocks in the immediate aftermath of a burglary occurring in an area. We now evaluate the sensitiveness of the estimates to the sequential introduction of different sets of control indicators. In addition to providing a check on the robustness of the estimates, this exercise allows us to evaluate whether the two different sources of exogenous variation in police presence that we use in this paper are in fact essential to the credibility of the empirical strategy.

We start in Column 1 of Table 7 by displaying estimates from a basic differences-in-differences model that simply includes $Treatment_i$, $Post_t$ and the interaction between the two. In Columns 2 and 3 we sequentially add individual (area) fixed effects and time (days relative to the post Friday) fixed effects. Therefore, Columns 1-3 exclusively exploit differences-in-differences variation, although with increasingly extensive sets of controls. In Column 4, on the other hand, we exploit only variation arising from the day of the week in which the burglary occurred. To do this, we restrict the sample to include only treated areas, and use the $Post_t$ variable as an instrument after controlling for the days relative to the burglary fixed effects. In Column 5 we replicate our baseline specification.

We find that the coefficients are remarkably stable across all the specifications. The first stage estimates are all positive and oscillate between 8 and 10 minutes. The reduced form and second stage estimates are always zero and in fact virtually identical across columns 2-5.

Correlated Shocks The consistency of the coefficients in the presence or absence of alternative strategies to eliminate the confounding effect of correlated shocks begs the question of whether these shocks are in fact at all present in our setting. To evaluate this empirically, we use the pre-October 2013 sample period and study whether the occurrence of a burglary

address, (5) in front of an address. We regard (1)-(3) as occurring away from the street view. However, (4) may also include crimes occurring away from street view.

in an area represents a good prediction of past and future criminal events. Specifically, we estimate the following model:

$$Crime_{its} = \kappa_i + \sigma_t + \eta_s + \nu_{its} \tag{4}$$

where κ_i and σ_t are area and day relative to post-Friday fixed effects[16]. We display the estimated days-relative-to-burglary-date fixed effects, $\hat{\eta}_s$ in Figure 11. We find that the estimates on the days following the burglary date are very similar to the estimates on the preceding days. Only the coefficient on the burglary date is positive and statistically significant at conventional levels, although its magnitude is relatively small[17].

The main conclusion from Figure 11 is that the occurrence of a recent burglary does not increase or decrease the underlying likelihood of an area being the location of a crime. The fact that correlated shocks do not appear to be present in our setting likely explains the remarkable consistency of the estimates in Table 7. It also suggests that the increase in police patrolling implemented by Essex Police was designed to tackle a problem, the 'optimal foraging problem' that did not in fact exist, at least in Essex county during our sample period.

[16]The results are very similar if we exclude σ_t from the set of controls.

[17]The dependent variable in (4) naturally excludes the burglary around which the area and time period are defined. The positive coefficient on the burglary date may reflect the existence of more than one crime per incident. For instance, a burglar may be discovered and assault in response the property owner.

FIGURES

FIGURE 1: Reproduction of Maps Distributed to Patrols in Colchester, Essex

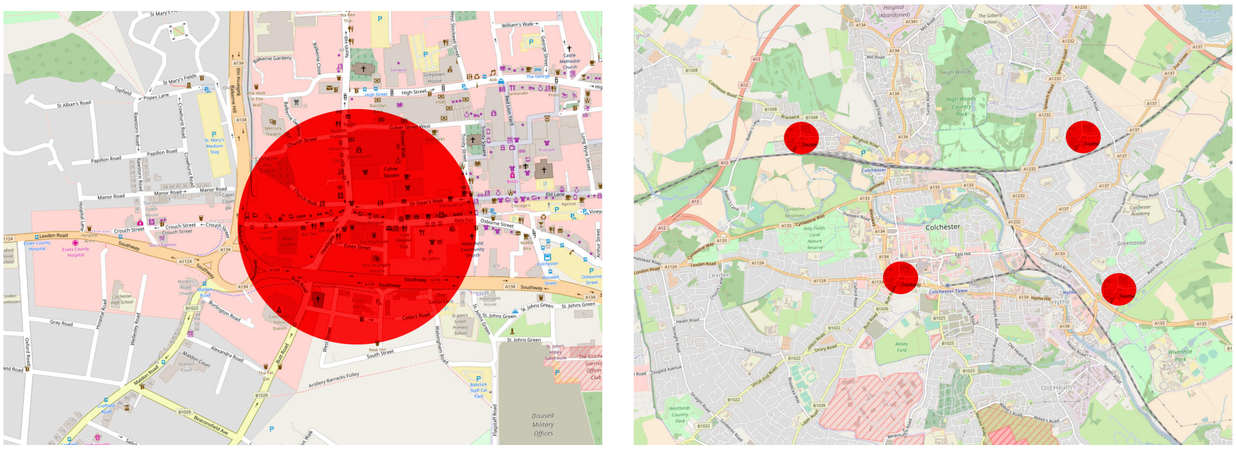


FIGURE 2: Differences-in-Differences Variation

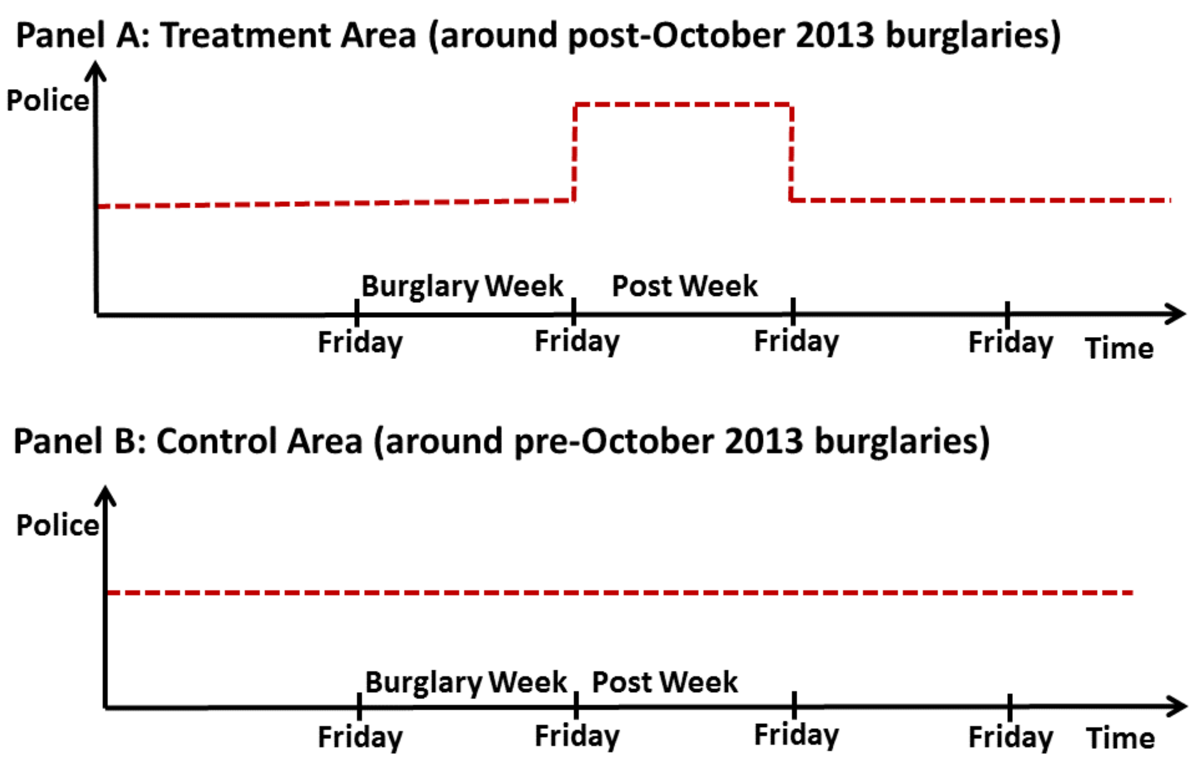


FIGURE 3: Daily Variation by Burglary Day of the Week

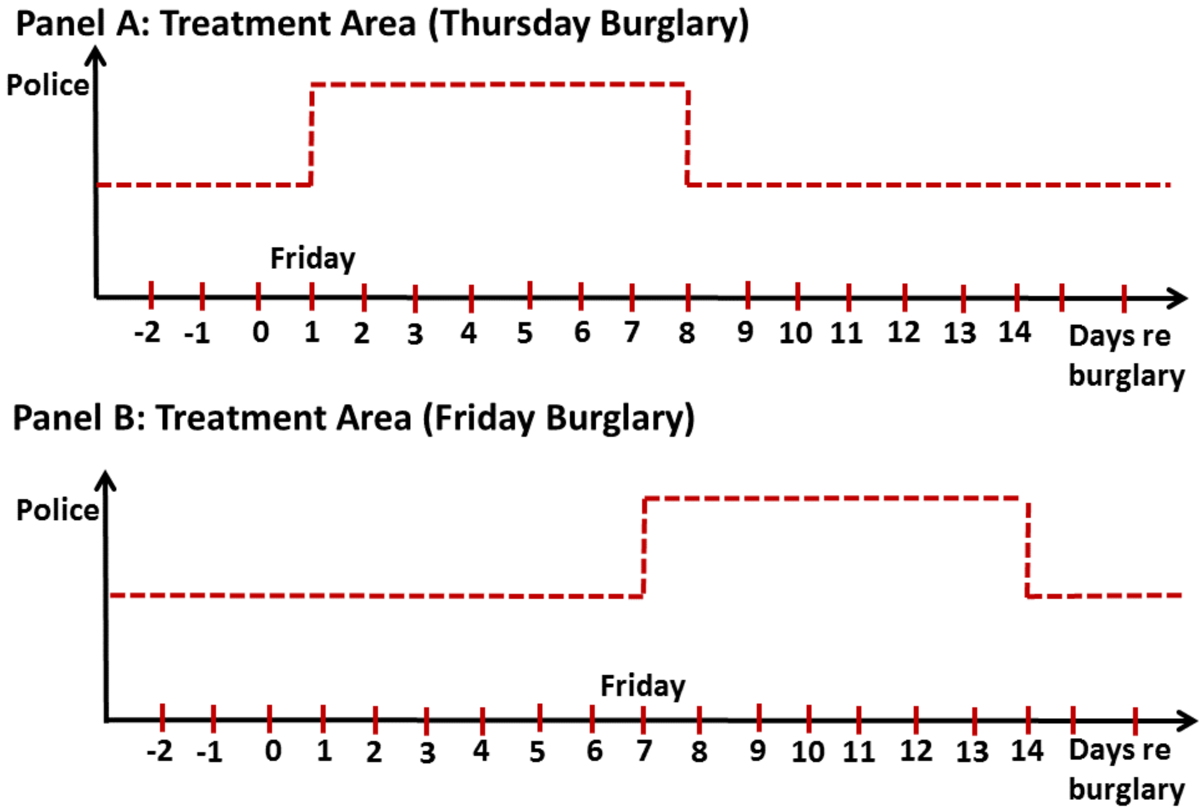


FIGURE 4: Location of the 8662 Burglaries

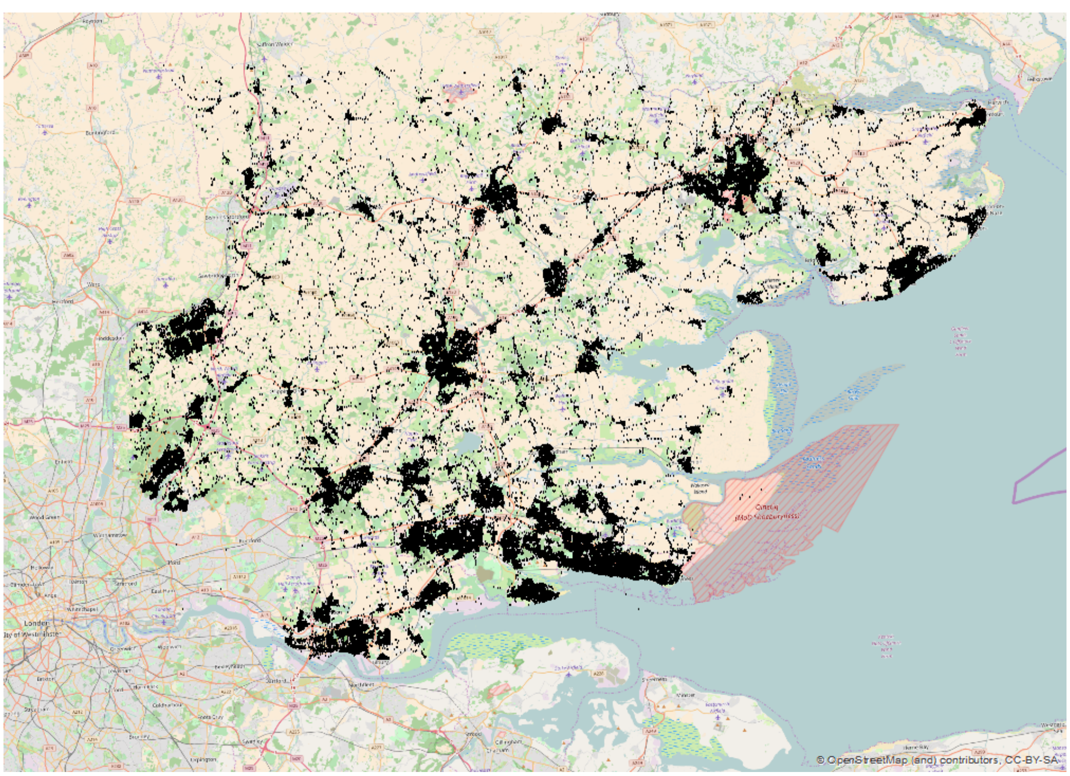
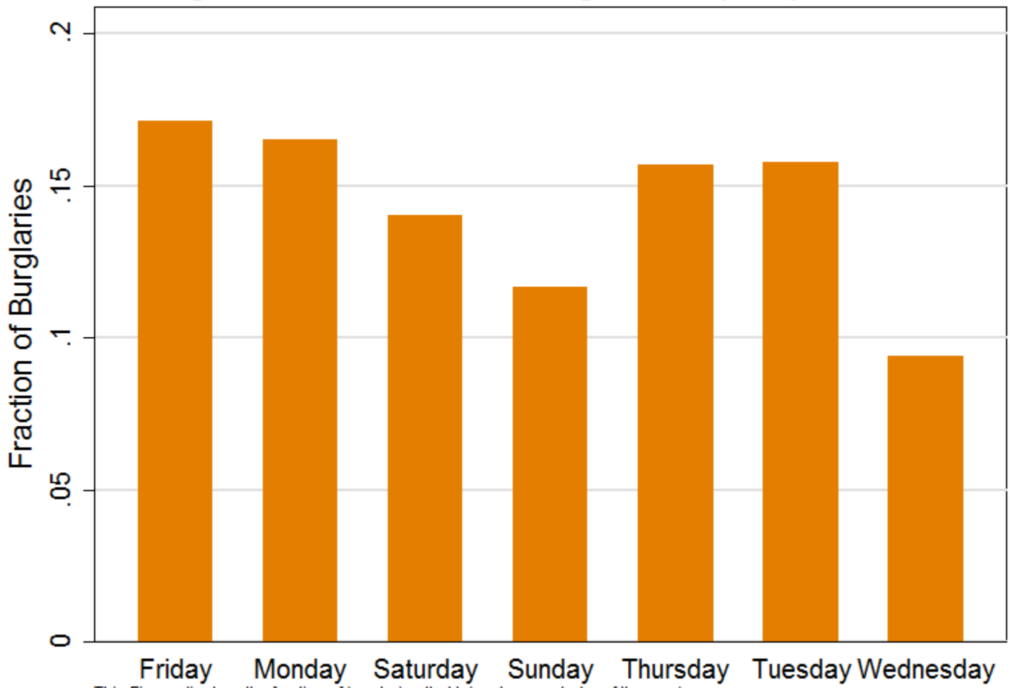


Figure 5: Distribution of Burglaries, By Day of Week



This Figure displays the fraction of burglaries that take place each day of the week.

Figure 6A: Distribution of Number of Crimes

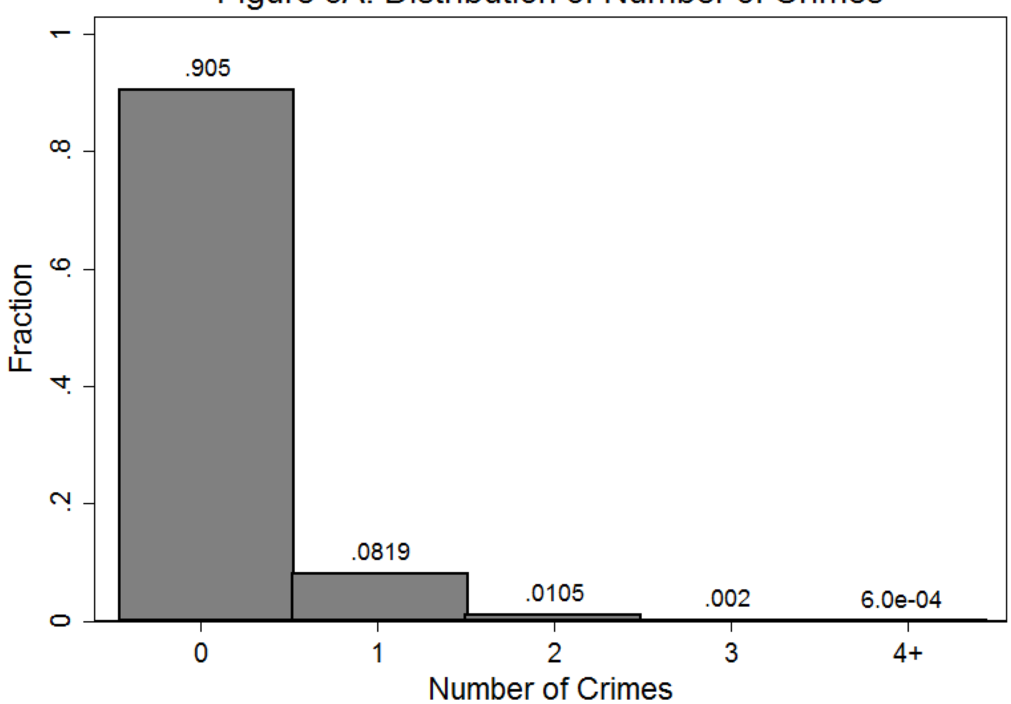


Figure 6B: Distribution of Police Time

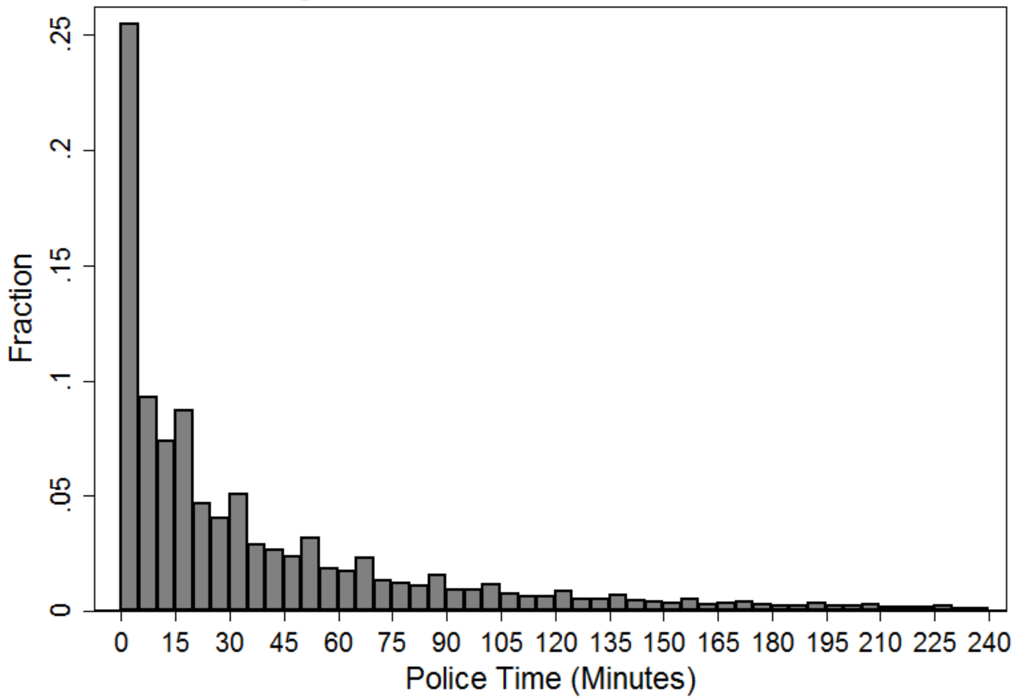


Figure 7: Kernel Regression of Crime on Police Time

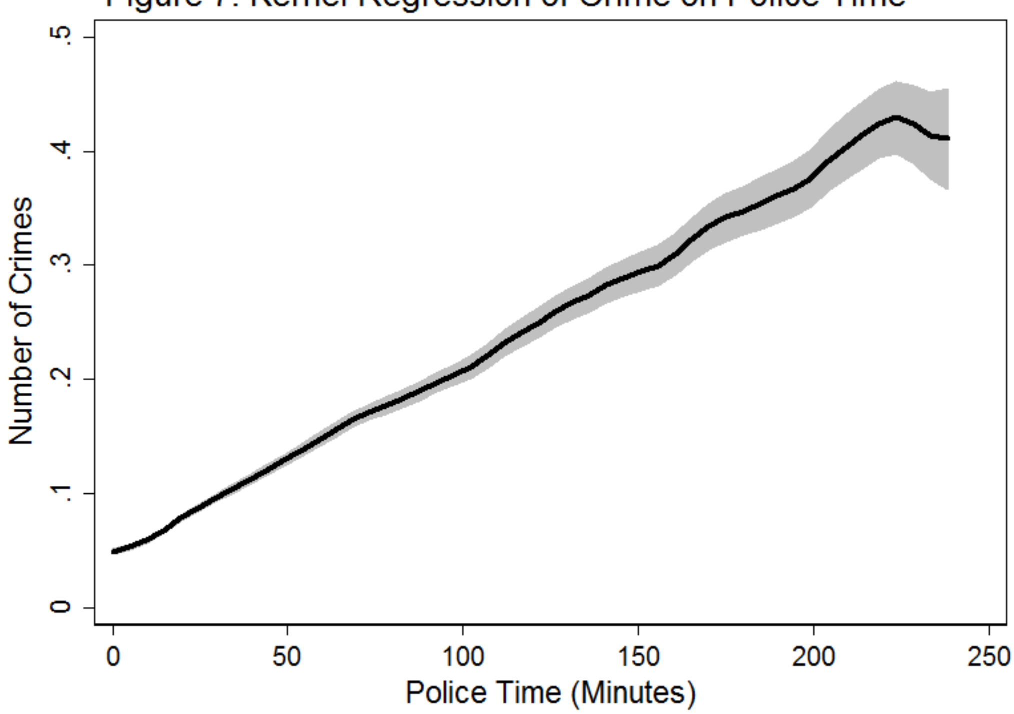
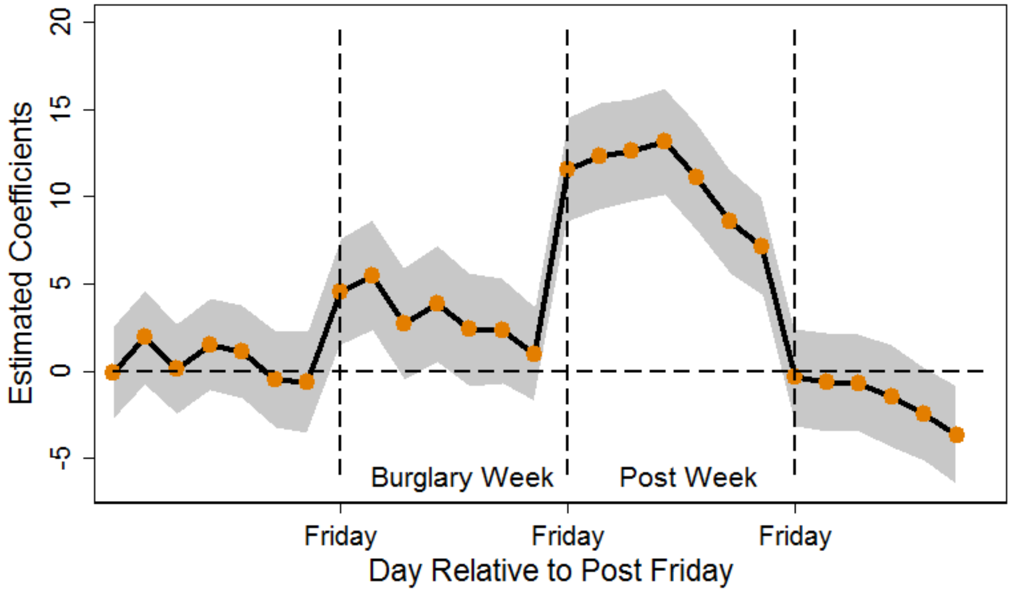
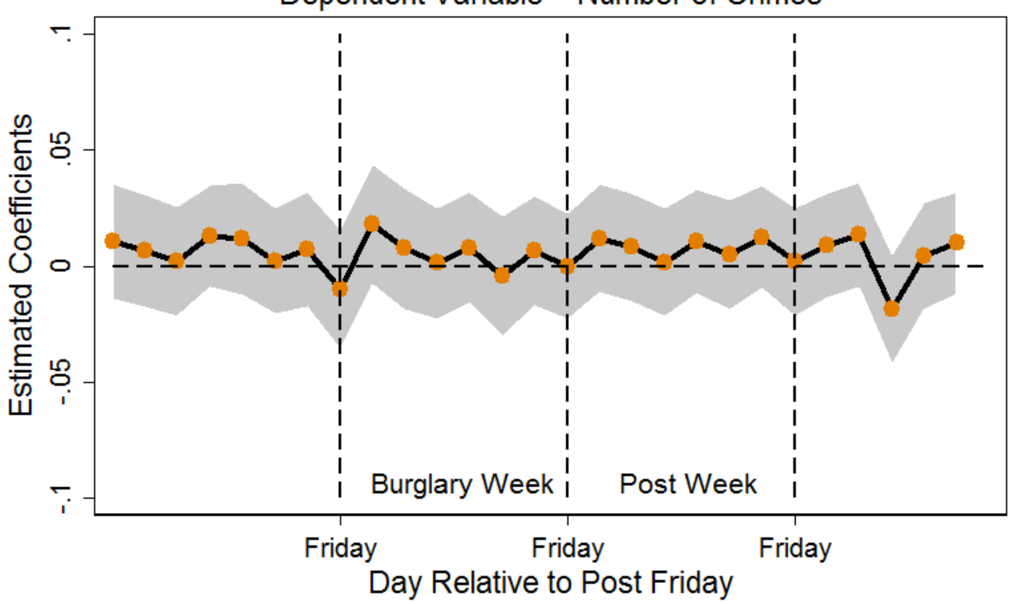


Figure 8A: Coefficients of Day X Treatment Area
Dependent Variable = Police Patrolling Time



This figure plots the estimated coefficients of the day-relative-to-post-Friday dummies interacted with the treatment area dummy.
The dependent variable is police time. The regression includes area fixed effects, day-relative-to-post-Friday fixed effects
and day-relative-to-burglary-date fixed effects. Standard errors are clustered at the Week X District level.
The mean number of crimes in the sample is 38.896. The number of observations in the regression is 233620.

Figure 8B: Coefficients of Day X Treatment Area
Dependent Variable = Number of Crimes



This figure plots the estimated coefficients of the day-relative-to-post-Friday dummies interacted with the treatment area dummy.
The dependent variable is number of crimes. The regression includes area fixed effects, day-relative-to-post-Friday fixed effects
and day-relative-to-burglary-date fixed effects. Standard errors are clustered at the Week X District level.
The mean number of crimes in the sample is .112. The number of observations in the regression is 233620.

Figure 9: Crime Levels and Police Presence
By Hours of the Day

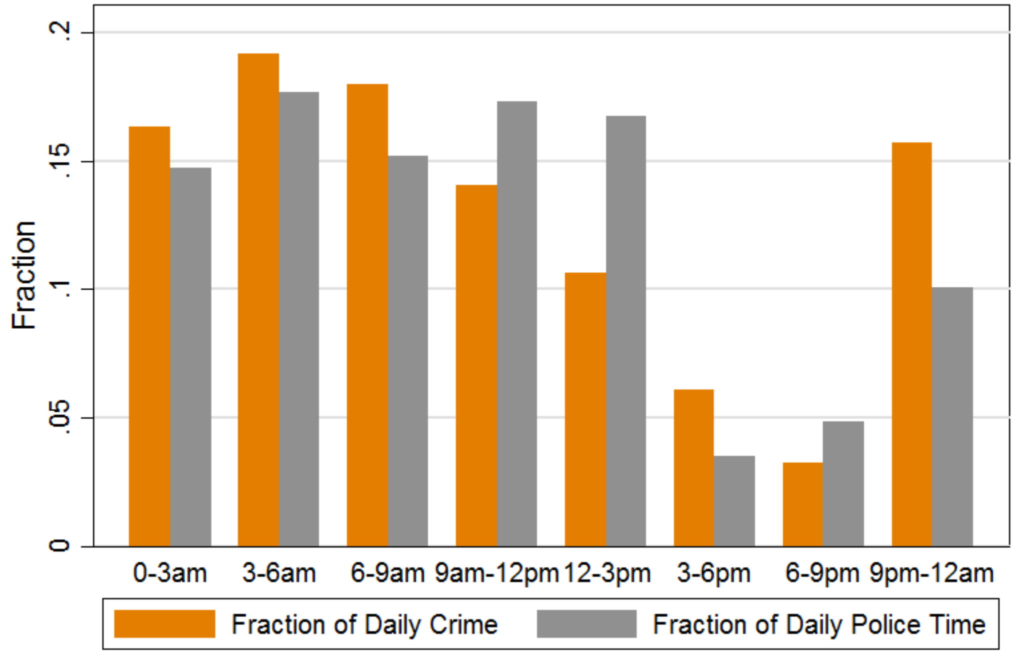
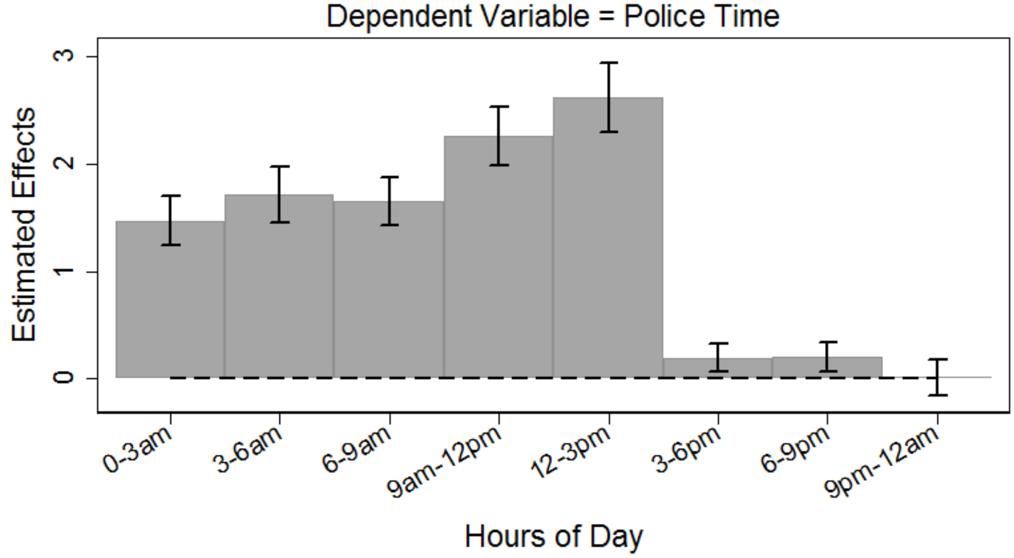


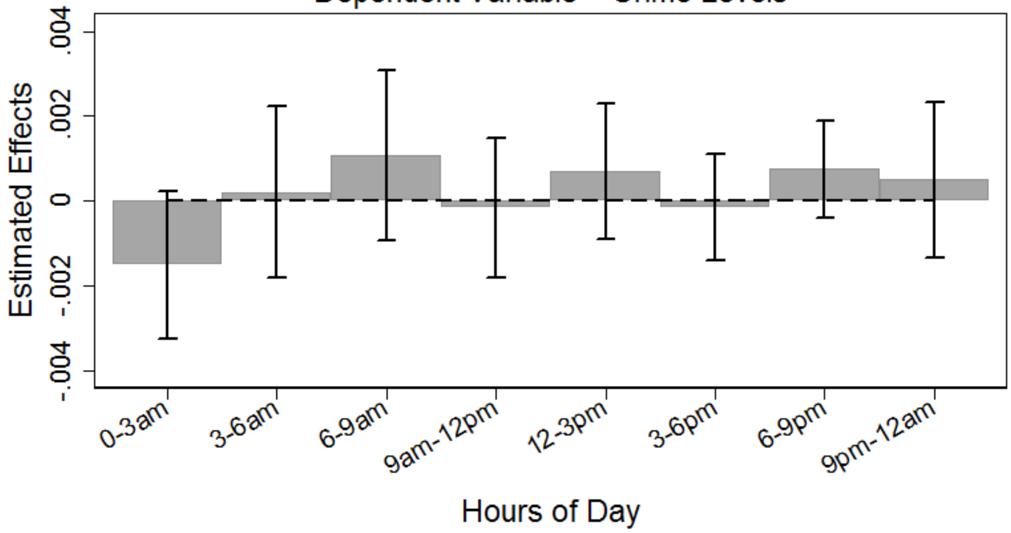
Figure 10A: Coefficients of
Treatment X Post X Hours of the Day
Dependent Variable = Police Time



This figure displays the estimates of a regression of police time on the interaction of the treatment areas, the post weeks and the eight three-hour intervals of the day. An observation is an area and three-hour interval. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 X 8 = 224 three-hour intervals, centred around the 0-3am interval of the post-Friday (the first Friday following the burglary date). The regression includes area fixed effects, three-hour interval relative to post-Friday fixed effects and day relative to burglary date fixed effects. Standard errors are clustered at the Year X Month X District level. The mean number of crimes in the sample is 38.96. The number of observations in the regression is 1928592.

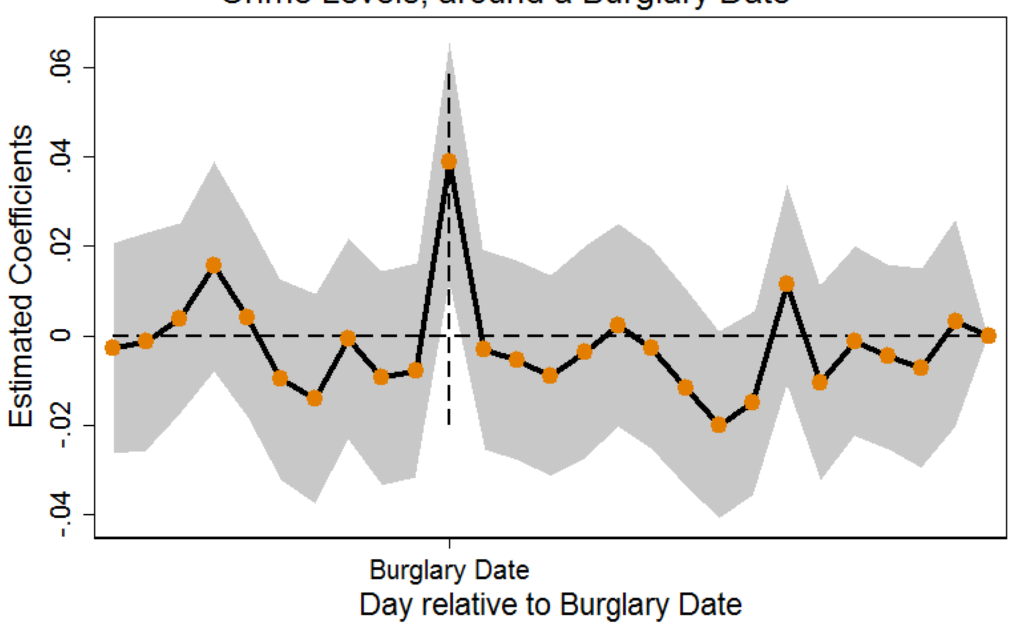
Figure 10B: Coefficients of
Treatment X Post X Hours of the Day

Dependent Variable = Crime Levels



This figure displays the estimates of a regression of crime levels on the interaction of the treatment areas, the post weeks and the eight three-hour intervals of the day. An observation is an area and three-hour interval. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 X 8 = 224 three-hour intervals, centred around the 0-3am interval of the post-Friday (the first Friday following the burglary date). The regression includes area fixed effects, three-hour interval relative to post-Friday fixed effects and day relative to burglary date fixed effects. Standard errors are clustered at the Year X Month X District level. The mean number of crimes in the sample is .11. The number of observations in the regression is 1928592.

Figure 11: Assessing Correlated Shocks
Crime Levels, around a Burglary Date



This figure plots the estimated coefficients of a regression of the number of crimes on the days relative to the burgary date dummies (N=71856). The dependent variable is the number of crimes (mean=.114). The controls are the area fixed effects and day relative to post Friday fixed effects. Standard errors are clustered at the Week X District level.

TABLES

TABLE 1: SUMMARY STATISTICS

	Mean	Median	SD	Min	Max
Days relative to post Friday	-.49	0	8.095	-14	13
Days relative to burglary date	2.586	3	8.358	-14	19
Treatment	.671	1	.47	0	1
Post	.25	0	.433	0	1
Crimes	.112	0	.378	0	17
Assaults	.018	0	.146	0	4
Burglaries	.023	0	.16	0	5
Thefts	.024	0	.166	0	12
Criminal Damage	.017	0	.144	0	17
Robberies	.001	0	.04	0	3
Police Time (Minutes)	38.896	19.661	47.859	0	238.114

This Table reports summary statistics for the baseline sample (N=233640). An observation is an area and day. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 days, centered around the 'post Friday', the first Friday following the burglary date. By construction, the days relative to the post Friday variable distributes between -14 and 13, and has a median of 0. The days relative to the burglary date variable takes a value of zero on the burglary date and it distributes, also by construction, between -14 and 19. The variable Treatment takes value 1 for areas around burglaries that occurred after October 2013, and 0 otherwise. The variable Post takes value 1 during the post week, the week starting on the first Friday after the burglary date. Crimes is the number of crimes on an area/day. Assaults, burglaries, thefts, criminal damage crimes and robberies are defined similarly. Police presence is the number of minutes that at least one GPS signal is emitted from inside the area on that day.

TABLE 2: OLS ESTIMATES

	(1)	**(2)**	**(3)**
DEP. VARIABLE	**Crimes**	**Crimes**	**Crimes**
Police Time	.00169***	.0017***	.00114***
	(.00003)	(.00003)	(.00003)
Year X Month X Day F.E.	No	Yes	Yes
Area F.E.	No	No	Yes

This table displays OLS regressions of police patrolling time on the number of crimes. An observation is an area and day. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 days, centered around the 'post Friday', the first Friday following the burglary date. Columns (2) and (3) control for the Year X Month X Day. Column (3) controls for the Area. Standard errors are clustered at the Week X District level. The mean number of crimes in the sample is .112. The mean of police time is 38.896 minutes. The number of observations in all regressions is 233620.

TABLE 3: 2SLS ESTIMATES

MODEL DEP. VARIABLE	(1) Reduced Form Crimes	(2) First Stage Police Time	(3) Second Stage Crimes
Treatment X Post	.00146 (.00373)	10.15408*** (.5838)	
Police Time			.00014 (.00037)
Area F.E.	Yes	Yes	Yes
Day re Burglary	Yes	Yes	Yes
Day re Post-Friday F.E.	Yes	Yes	Yes
Kleibergen-Papp F			302.52

This table displays 2SLS regressions of police time on the number of crimes, using the interaction of the treatment areas and the post weeks as an instrument for police patrolling time. An observation is an area and day. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 days, centered around the 'post Friday', the first Friday following the burglary date. The variable Treatment takes value 1 for areas around burglaries that occurred after October 2013, and 0 otherwise. The variable Post takes value 1 during the post week, the week starting on the first Friday after the burglary date. Columns (1), (2) and (3) display the reduced form, first stage and second stage estimates, respectively. Standard errors are clustered at the Week X District level. The mean number of crimes in the sample is .112. The mean of police time is 38.896 minutes. The number of observations in all regressions is 233620.

TABLE 4: ROBUSTNESS TO ALTERNATIVE CLUSTERING

DEP. VARIABLE	(1) Crime	(2) Crime	(3) Crime	(4) Crime	(5) Crime	(6) Crime
Police Time	.00014 (.00037)	.00014 (.00036)	.00014 (.00037)	.00014 (.00041)	.00014 (.00034)	.00014 (.00032)
Clusters	Week X District	Area	Week	Month X District	2-Way	Spatially Correlated
Number of Clusters	1044	8662	121	233	121/9	8662
Kleibergen-Papp F	302.52	501.24	226.19	120.63	20.05	302.52
Lower bound	-17.5	-17.1	-17.6	-20.1	-15.9	-14.6

This table displays 2SLS regressions of police time on the number of crimes, using the interaction of the treatment areas and the post weeks as an instrument for police patrolling time. Only the second stage estimates are displayed. The sample, treatment area dummy and post week dummy are defined in Table 3. All regressions include area fixed effects, day-relative-to-post-Friday fixed effects, and day-relative-to-burglary-date fixed effects. Every column clusters the standard errors at a different level, as indicated. Column (5) clusters both at the week and at the district level. Column (6) displays the Conley (1999) standard errors, the cross-sectional units are the cells (cutoff = 10km.) and the time units are the days (one lag). The lower bound of the confidence interval for the implied elasticity is displayed. The mean number of crimes in the sample is .11156. The mean of police time is 38.89627. The number of observations in all regressions is 233620.

TABLE 5: OTHER ROBUSTNESS TESTS

MODEL	(1) Poisson	(2) High Crime Areas	(3) Low Police Areas	(4) Not 'Caused' By Police	(5) Street Crime
Reduced Form	.01218	.01207	-.00442	.0002	.00012
	(.03706)	(.01023)	(.00452)	(.00323)	(.00343)
First Stage	10.15408***	9.51114***	10.29655***	10.15408***	10.15408***
	(.5838)	(1.04106)	(.75994)	(.5838)	(.5838)
Second Stage	.00125	.00127	-.00043	.00002	.00001
	(.00376)	(.00107)	(.00044)	(.00032)	(.00034)
Kleibergen-Papp	302.52	83.47	183.58	302.52	302.52
Observations	233640	60335	58620	233620	233620
Mean Crimes	.112	.256	.04	.087	.098
Mean Police Time	38.896	60.763	13.562	38.896	38.896

This table displays 2SLS regressions of police time on the number of crimes, using the interaction of the treatment areas and the post weeks as an instrument for police patrolling time. An observation is an area and day. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 days, centered around the 'post Friday', the first Friday following the burglary date. The variable Treatment takes value 1 for areas around burglaries that occurred after October 2013, and 0 otherwise. The variable Post takes value 1 during the post week, the week starting on the first Friday after the burglary date. Every cell displays the estimate of a separate regression. In every Column we add a different set of controls. For every Column we display the reduced form, first stage and second stage estimates of police time on the number of crimes. Column (1) estimates a Poisson IV model, while Columns (2)-(5) estimate 2SLS models. Column (1) uses the baseline sample. Column (2) uses only areas where the crime rates during the first two sample weeks are in the top quartile. Column (3) uses only areas where the police time during the first two sample weeks are in the bottom quartile. Column (4) uses as the dependent variable only crimes that did not occur in the same five minute period during which an officer was inside a particular area. Column (5) uses as a dependent variable only crimes that are reported to have occured on the street, rather that inside a building. All regressions include area fixed effects, day-relative-to-post-Friday fixed effects, and day-relative-to-burglary-date fixed effects. Standard errors are clustered at the Week X District level.

TABLE 6: 2SLS ESTIMATES, BY CRIME TYPE

DEP. VARIABLE	(1) Assaults	(2) Burglaries	(3) Thefts	(4) Criminal Damage	(5) Robberies
Police Time	-.00017	-.0001	-.00004	.00025*	.00004
	(.00014)	(.00015)	(.00016)	(.00013)	(.00004)
Mean Dep. Variable	.018	.023	.024	.017	.001

This table displays 2SLS regressions of police patrolling time on the number of crimes of a certain type, using the interaction of the treatment areas and the post weeks as an instrument for police patrolling time. The sample, treatment area dummy and post week dummy are defined in Table 3. Every column has a different crime type as dependent variable. All regressions include area fixed effects, day-relative-to-post-Friday fixed effects, and day-relative-to-burglary-date fixed effects. Standard errors are clustered at the Week X District level. The Kleibergen-Papp F-statistic in all regressions is 302.52. The number of observations in all regressions is 233620.

TABLE 7: EVALUATING THE EMPIRICAL STRATEGY

MODEL	(1) Basic DiD	(2) +Area	(3) +Friday	(4) No DiD	(5) All
Reduced Form Estimate	-.00185	.00142	.00143	.00116	.00146
	(.00419)	(.00372)	(.00372)	(.00307)	(.00373)
First Stage Estimate	7.88906***	10.11667***	10.11982***	8.73102***	10.15408***
	(1.11748)	(.58528)	(.58369)	(.55364)	(.5838)
Second Stage Estimate	-.00023	.00014	.00014	.00013	.00014
	(.00055)	(.00037)	(.00037)	(.00035)	(.00037)
Area F.E.	No	Yes	Yes	Yes	Yes
Day r.e. Post Week F.E.	No	No	Yes	No	Yes
Day re Burglary F.E.	No	No	No	Yes	Yes
Areas in Sample	All	All	All	Treated	All
Kleibergen-Papp F	49.84	298.8	300.6	248.7	302.52

Every cell displays the estimate of a separate regression. In every Column we add a different set of controls. For every Column we display the reduced form, first stage and second stage estimates of police time on the number of crimes. In Columns (1)-(3) and (5) the sample is the baseline sample including both treatment and control areas. In these columns, we use the interaction of the treatment areas and the post weeks as an instrument for police time. In Column (4) the sample only includes treatment areas and the instrument is the post weeks. The variable Treatment takes value 1 for areas around burglaries that occurred after October 2013, and 0 otherwise. The variable Post takes value 1 during the post week, the week starting on the first Friday after the burglary date. An observation is an area and day. The areas are the 200m.-radius areas around the location of a burglary. For every burglary, the corresponding area is measured for 28 days, centered around the 'post Friday', the first Friday following the burglary date. Standard errors are clustered at the Year X Month X District level. The number of observations in Columns (1)-(3) and (5) is 233620. The number of observations in Column (4) is 156864.

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