I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 11393

Police Patrols and Crime

Jordi Blanes i Vidal
Giovanni Mastrobuoni

MARCH 2018

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 11393

Police Patrols and Crime

Jordi Blanes i Vidal
London School of Economics and Center for Economic and Policy Research

Giovanni Mastrobuoni
Collegio Carlo Alberto, University of Essex and IZA

MARCH 2018

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Police Patrols and Crime*

An 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 standard circumstances. This paper exploits a natural experiment that aimed to increase police presence in more than 6,000 well-defined areas, by economically-realistic amounts and under relatively normal circumstances. Using data transmitted by GPS devices worn by police officers, we first document exogenous and discontinuous changes in patrolling intensity. We do not find that these increases in patrolling were accompanied by corresponding decreases in crime. The standard errors are small enough to reject relatively small elasticities. We discuss and empirically evaluate explanations for our findings.

JEL Classification: D29, K40
Keywords: police, crime, natural experiments, deterrence

Corresponding author:
Jordi Blanes i Vidal
London School of Economics
Houghton Street
London WC2A 2AE
United Kingdom
E-mail: j.blanes-i-vidal@lse.ac.uk

* Giovanni Mastrobuoni gratefully acknowledges the support of the British Academy Mid-Career grant. We thank David De Meza, Steven Durlauf, and participants at various seminars and conferences for very useful comments. We thank Essex Police for providing the data and in particular Chief Supt Carl O'Malley, Ted Hampson, Paul Wraight and Deno C. for providing invaluable insight in understanding the data and Operation Insight. The research in this paper should not be interpreted as an evaluation of the impact of Operation Insight. All errors are our own.

1 Introduction

The causal relationship between police street deployment and crime represents one of the most common 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 United States 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 allocating this large resources of patrols is worthwhile, or whether more resources should be allocated to other police operations.

The most influential studies in economics, Di Tella and Schargrodsky (2004) and Draca, Machin and Witt (2011), estimate the effects on crime of large and sustained increases in static police deployment following deadly terrorist attacks.[2] These two terrorism-based studies identify large and 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).

We regard the terrorism-based studies as persuasive in the claims that deterrence is at work in their specific circumstances.

However, from a policy perspective it is unclear whether these results are generalizable to more common policing strategies. 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. Both features imply that the elasticities following terrorist attacks may be larger than in other periods.

In general, round-the-clock protection of a highly sensitised citizenry is economically

[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.

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 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 more common policing strategies.

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 potentially 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 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.[3] Together, these areas represented the population of locations where crime (specifically, private dwelling 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 below, we use these features to construct a "difference-in-difference" instrumental variables empirical strategy. The policy allow us to i) convincingly isolate exogenous variation in police presence, ii) account flexibly for the possibility of correlated shocks in the crime propensity of an area, iii) construct various counterfactuals that are not influenced by

[3]Given the small size of these areas, these ten minutes represent a large relative increase (i.e. approximately 33%) in patrolling time.

the policy.

We fail to find a decrease in crime that corresponds to the increase in police patrolling induced by the policy. Furthermore, our estimates are precise enough to be able to reject an elasticity *half the size* of those found by Di Tella and Schargrodsky (2004) and Draca, Machin and Witt (2011).

In light of these findings, it is particularly important to understand whether our result might be following from deficiencies in the estimating sample or in 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.[4] 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 2.18 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.[5]

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 on 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 from a policy perspective is the question of whether police patrolling *as typically practiced* provides a sizable contribution to crime prevention. We believe that the natural experiment that we exploit here generates evidence that is more easily extrapolated to the circumstances and levels of police deployment that are the norm. The evidence is consistent with the elasticity between police

[4]It is well-known that the (weak instruments) 2SLS coefficients are biased in the direction of their OLS counterparts (Bound, Jaeger and Baker, 1995). 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.

[5]Moreover, there is no measurement error in the timing of the policy-driven increase in policing.

and crime measured at the daily level being relatively small.[6]

Vigorous debates are taking place in many jurisdictions regarding the optimal size of police budgets, as well as the optimal allocation of resources inside departments.[7] Concentrating resources in a small number of locations (Braga, 2007) or incapacitating criminals by responding rapidly to calls for service (Mastrobuoni 2015, Blanes i Vidal and Kirchmaier 2017) have been argued to be effective strategies in combating crime, and may compete in terms of resources with the policy of using general purpose patrolling to thinly spread police presence through large areas. While a cost-benefit analysis of alternative uses of police resources is beyond the scope of this paper, we contribute to this debate by providing estimates of the relation between police and crime that may be more generalisable and therefore more useful to the policy maker than those in the existing literature.

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, 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 police practice: visible street police deployment. In addition to the seminal terrorism-based papers, a related recent study is Weisburd (2017). As a result of the dual role of officers in terms of proactive and reactive policing, she shows that at times officers leave their patrolling beat unattended when asked to respond to incident calls outside it. As long as this decision is unrelated to crime in the abandoned area, the implied

[6] To reiterate, the best interpretation of our evidence is *not* that police street deployment has no effect whatsoever on crime, but instead that this relation may be small in typical circumstances. For a fascinating case study on the effects of completely abolishing the police force in a country, see Andenaes (1974).

[7] In the U.S., many cities have substantially increased the policing share of their budgets, while cutting other services such as mental health services and housing and youth programs. While coinciding with the secular decrease in crime, this increase has been questioned by representatives of minority communities (see for instance Center for Popular Democracy, 2017). In the UK, the decrease in police resources that started in 2010 has particularly affected front line services such as neighbourhood (patrolling) policing. Influential organisations such as the English police regulator have argued that the budget for neighbourhood policing should be exempt from budgetary cuts (HMIC, 2014).

variation can be used to instrument police presence. Benefitting from the ability to track the location of police officers as they travel around the city, she finds that this has a modest effect on crime levels.[8]

Criminologists have long argued that police street deployment can be effective only when it is highly concentrated. Field experiments targeting high-crime areas, or 'hotspots' (Braga, 2007) have often found statistically significant effects.[9] By contrast, it is typically argued by criminologists that moderate (i.e. in their terminology 'random') levels of patrolling have no effect on crime[10]. However, the evidence with respect to random patrolling (Kelling et al. 1974, Kelling et al. 1981) has long been exposed as methodologically flawed (Larson 1975, Sherman 1986). One way to interpret our paper is as providing evidence supporting the claim that relatively low intensity patrolling is not associated with very high elasticities on crime.

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 main results in Section 5. Section 6 discusses potential explanations for these results. Section 7 concludes.

2 Institutional Setting

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

[8]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.

[9]This is unsurprising given the often large sizes and long durations of the interventions (Nagin, 2013b). 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. Despite this, this seminal paper is often quoted as providing strong evidence of decreases in crime.

[10]Banerjee et al. (2017) argue instead that a certain element of randomness can help to prevent displacement of crime from 'hotspots' and into other less targeted areas.

Police Patrols Essex Police employs approximately 2,750 police officers to serve a population of 1.77 million, or 155 per 100,000 inhabitants.[11] 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 separate 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 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.[12] 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

[11]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).

[12]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.

these areas therefore represents, according to the theory, a good use of resources in terms of crime deterrence. By 2013, policing initiatives based on the optimal forager theory had been undertaken in several UK cities including Trafford (Manchester), Birmingham and Leeds.[13]

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 separate PowerPoint presentations and distributed every Friday to the police officers in the 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.[14]

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.

Using Operation Insight to study the Effect on All Crime Essex Police introduced Operation Insight in order to protect the areas around recent burglaries from being burgled again. In this paper we use the variation in patrolling associated with the policy to study the effect of patrolling on *all crime*. Note that, in doing so, we implicitly assume a large degree of equivalence between the additional patrolling induced by Operation Insight and the regular patrolling activities of Essex Police. Such an equivalence would break, for instance,

[13]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.

[14]Police districts covering largely rural areas were not part of Operation Insight. The participating districts were Basildon, Southend, Rochford, Castle Point, Thurrock, Brentwood, Epping, Harlow, Colchester, Chelmsford (town only) and Tendring. The excluded districts were Braintree, Maldon, Stansted Airport, Uttlesford, Rochford and Epping. Our data and study focuses exclusively on the participating districts.

if the Operation Insight-induced visits were by officers in plain clothes, or that are for other reasons 'invisible' to potential offenders. It would also break if potential criminals believed that the additional officers patrolling an area to prevent repeated burglaries would fail to intervene when coming across other types of criminal activity.

We believe that it is legitimate to regard the Operation Insight-induced additional patrolling as akin to 'regular patrolling', at least from the perspective of potential criminals. The police officers visiting the recent area of a burglary were regular officers, wearing their regular uniforms and, when driving, aboard their regular police cars. In fact, Operation Insight simply consisted of a geographical emphasis on certain areas among those officers that were anyway doing their regular patrolling duties.[15] Furthermore, the explicit objective of Operation Insight was to create deterrence, albeit deterrence of the specific crime of repeated dwelling burglaries.

It is important, however, to note two additional caveats. Firstly, Operation Insight created (and our paper studies) *local* (i.e. small) changes to the intensity of patrolling. While we believe that the study uncovers policy-relevant elasticities, we want to emphasise that the evidence here is not well-suited to predicting the likely consequences of, say, eliminating the police completely. Secondly, we estimate the *contemporaneous* effect of patrolling on crime (that is, the effect on crime during the days on which the patrolling took place). While in this respect our study does not deviate from the literature, we want to emphasise that we do not attempt to capture potentially delayed effects.

The objective of this paper is not to evaluate the success of Operation Insight. Our study is instead similar to the terrorism-based papers because we also exploit increases in policing that follow specific criminal episodes and have the objective of preventing their re-occurrence (burglaries, terrorist attacks). An advantage of our setting is that the number of crimes triggering the increase in patrolling is very large (around six 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

[15]Note that, due to the way that we have created our sample, the increase in patrolling in the 'treated' areas is *not* at the expense of patrolling intensity in the 'control' areas.

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.[16] 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

[16]We find in Section 5 that there is in fact no empirical support for the existence of correlated shocks following burglaries in Essex County before* Operation Insight.

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. Using pre-policy areas rather than neighboring areas as counterfactuals has the additional advantage that the total stock of resources in the control areas is not changing.

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. Control areas are those around the locations of pre-October 2013 burglaries. 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 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.[17]

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

[17] Figure 8B provides an eyeball test of the parallel trends assumption.

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 identification 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-October 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 participating 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.[18]

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 center. We then follow every area for 28 days (centered 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,

[18] The estimation strategy in (1) and (2) does not directly address the issue of potential spillovers to locations right outside the baseline 200m.-radius areas. Given the findings in Table 3 that police presence did *not* reduce crime in the targeted areas, we believe that there is no rationale for a deep investigation of these geographical spillovers. Nevertheless, Column 4 in Table 7 shows that the baseline findings remain unchanged when increasing by 60% the size of the studied areas. Note also that the increase in patrolling in the treated areas (during the post week) was *not* at the expense of patrolling levels in the control areas. This is because, by construction of the sample and empirical strategy, policing in the control areas occurred *before in time*, relative to the policing in the treated areas. The police resources generating the higher police presence in the treated areas were extracted from the rest of the Essex geography, which is not part of our estimation sample.

which corresponds to a crime approximately every 9 days.[19] 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%. For simplicity, our baseline estimates are 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 2.18 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, for the regular patrol officers in the participating districts. 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 emits a GPS signal from inside an area. To construct this variable, we assume that officers who send signals from within the area entered the area right after emitting their last signal from outside the area and exited the area as soon as signals are emitted again from outside the area. Given the 2.18-minutes frequency of the GPS signals we implicitly assume that once there is a signal, officers remain inside for at least 2.18 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.[20]

[19] This is unsurprising given the small size (approximately .03 squared km.) 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 relatively precisely estimated.

[20] Appendix Figure A1 displays the distribution in the length of the police visits to an area, before and

Studies on the effect of police patrolling on crime often lack direct measures of police deployment. Even in studies 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 daily crimes on daily 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.[21]

5 Main Results

In this section we present and interpret the main results of the paper.

Baseline Estimates Table 3 displays the baseline findings. We find in Column 2 that police patrolling time 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

during Operation Insight. As we can see, Operation Insight lead to an increase in the percentage of visits associated with relatively longer duration. This is confirmed in Appendix Table A1, which displays OLS regressions equivalent to (1) but with the number of distinct visits and average length of visit in the left-hand side.

[21] An additional important conclusion from Figure 7 and Table 2 is that the power of our sample is clearly sufficient to the estimation of statistically and economically strong correlations between police and crime. This confirms our claim that deficiencies in power or in the measurement of the main variables are unlikely in our study.

mean of police time in a standard week, such as 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.[22]

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).[23]

In the next section, 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.[24]

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 (eyeball) test of the identification assumption underlying the differences-in-differences variation of the empirical strategy. Specifically, we find no evidence of a differential trend in the crime variable for treated and control areas in the days prior to the post-Friday (i.e. the 'parallel trends' assumption).

[22] Appendix Table A1 shows that the increase in police presence was the result both of an increase in the number of visits to an area and an increase in the average length of stay in that area. Needless to say, these variables are also not statistically significant predictors of crime when instrumented with the variation in our experiment.

[23] The estimated elasticity in these papers is approximately 35%. An alternative way of evaluating our results is to perform a one-tailed test against -.0010835 which is the estimate that corresponds to a 35% elasticity in our context. We reject the null that the coefficient is -.0010835 against the one-tailed alternative that it is lower (in absolute levels) than that at the .1%.

[24] 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 have been the result of Operation Insight focusing the attention of police officers on the importance of taking burglaries seriously.

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 for the elasticity, and shows that we can always reject relatively small elasticities.

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: (a) the choice of estimation functional form, and (b) the sequential introduction of different sets of control indicators. In addition to providing a check on the robustness of the estimates, this second 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 5 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. The robustness of the estimates to exploiting different sources of variation is reassuring.

We conclude this subsection by examining the robustness of the estimates to changes in the functional form specification. As displayed in Figure 6A, the dependent variable is a count variable with a very large percentage of zeros. The linear models that we adopt throughout the paper have the advantage of simplicity and robustness to distributional assumptions (Angrist and Pischke, 2008). 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 Column 6. 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 mis-specification.

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

$$Crime_{its} = \kappa_i + \sigma_t + \eta_s + \nu_{its} \tag{3}$$

where κ_i and σ_t are area and day relative to post-Friday fixed effects.[25] We display the estimated days-relative-to-burglary-date fixed effects, $\hat{\eta}_s$ in Figure 9. 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.[26]

The main conclusion from Figure 9 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 5.

6 Discussion

In this section, we discuss and evaluate potential explanations for our main finding. We first investigate its robustness when studying different geographical areas, time periods and measures of crime. Regardless of the estimation method, we always fail to find a statistically significant relation between crime and the police presence generated by our experiment.

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

[26] The dependent variable in (3) 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.

We conclude the section outlining two remaining explanations for the lack of deterrence: (a) the difficulty in influencing perceptions of apprehension risk, and (b) criminals' ability to intertemporarily substitute the commission of a crime when police visits to an area are relatively short.

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. For instance, we may find a stronger effect on assaults than on burglaries, given that the latter are the quintessential 'home crime.' Alternatively, it may be that there is an effect on burglaries (particularly, private dwelling burglaries), given that Operation Insight had as a purpose the target of those. However, in Table 6 we display second-stage estimates using the numbers of crimes of specific types as dependent variables and fail to find that an effect of police patrolling, regardless of the crime type.

In the last column of Table 8 we study whether police *street* presence at least had an effect on crimes that were recorded as having occurred 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 estimate is not statistically different from zero for this new variable.[27]

Estimates by Time of Day Another potential 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 10 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

[27]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 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.

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 centered around the 0-3am interval of the post-Friday. We then estimate:

$$Police_{its} = \eta_i + \rho_t + \delta_s + \sum_{k=1}^{8} \phi_k (Treatment_i \times Post_t \times Interval_k) + \omega_{its} \tag{4}$$

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 11A that Operation Insight increased police presence mostly between midnight and 3pm. These are hours that, according to Figure 10, are associated with higher than average crime. We conclude that the temporal mis-allocation 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 11B, 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 the treated areas during the post-week were not associated with decreases in crime at any time of the day.

Estimates by Size of the Area Operation Insight established that patrolling should increase in an area of 200m. radius, centered around the location of a burglary. We now investigate whether the additional police presence may have concentrated, and perhaps have a meaningful impact, in the close vicinity of the burglary. To do this, we replicate the baseline dataset but using instead 50m. and 100m.-radius circles. We indeed find that police presence increased more, in relative terms, as we focus on smaller areas centered around the burglary location. For instance, at the 50m. and 100m.-radius, police presence increased by 40% and 37% respectively, relative to the mean throughout the sample. However, we fail to find any effect of police on crime.

We also replicate the baseline estimation using larger circles of 250m. radius. At the 250m. radius, police presence increased by almost the same number of minutes that at the baseline 200m.-radius, despite the fact that the area size and mean police presence are 60% larger. This suggests that the locations right outside the Operation Insight targeted areas did not benefit from a positive spillover in police presence. Instead, the policy was quite precisely concentrated on the official areas of interest. Again, we do not find a statistically significant effect of police on crime at this larger area level.

Estimates around the Discontinuity of Policy In the baseline sample, the treatment group comprises of areas around burglaries occurring in the October 2013 to April 2015 period, while the control group includes the January 2013 to September 2013 period. These periods are short enough to provide some confidence in the assumption that burglaries, and the potential correlated shocks around them, are on average identical across treatment and control groups. Nevertheless, in Column 1 of Table 8 we provide additional confidence in this respect by restricting our sample to an eight week period, centered around the introduction of Operation Insight. We find that Operation Insight led to a very large increase in police presence (around 24 minutes) in the first four weeks of its implementation. As a result, even with this drastically smaller sample, we are able to generate a very strong instrument (Kleibergen-Papp F-statistic = 48.24). We find that even after increasing police presence by more than 50% of the overall mean, we fail to find a decrease in crime during the post-weeks of this four week period.

Estimates from Specific Areas Essex county includes some 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 in the 200m.-radius areas makes it difficult to identify how crimes are affected by police presence. Of course, the standard errors in our baseline estimates already account for this sample variability (or potential lack of thereof). Nevertheless, in Column 2 of Table 8 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 Column 2 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 fail to find a statistically significant relation with crime, even in these high crime areas.

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.

Estimates Accounting for Effects on Reported Crime We next investigate whether police officers, by their mere presence, may have caused an increase in the likelihood of *reporting* crime, enough to offset a hypothetical decrease in actual 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.[28] 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.[29] In Column 4 we find that this new dependent variable, which is likely unaffected by this positive 'recording bias', is uncorrelated with police presence.

Patrols and the Expected Probability of Apprehension As Nagin (2013a) argues, the most natural channel through which police presence may affect crime is a change on criminals' perceptions of apprehension risk. The study of the relation between police and crime is therefore the study of the joint hypothesis that: (a) these perceptions can be meaningfully affected by police activity, and (b) potential criminals adjust their behaviour responding to

[28] Note that this positive 'recording bias' is potentially present in *any* study of police and crime. Our paper is unusual in that the richness of 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.

[29] 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.

changes in these perceptions. Unfortunately, the direct measurement of the expectation of being apprehended is next to impossible in field settings and for this reason most of the literature has been restricted to estimating a reduced-form relation.

Our study also adopts a reduced-form approach. A plausible (if untested) explanation of our finding is that small *absolute* increases in police presence have, at least in the short term, a negligible effect on criminal perceptions, even if these increases are large in *relative* terms.[30] In our experiment, visits by the police to the targeted areas were relatively brief (see Appendix Figure A1) and this may have made it difficult to impact criminal expectations about the likelihood of future presence and the resulting probability of apprehension.

Using information on whether crimes were cleared, we can investigate whether the likelihood of being arrested for a crime was in fact higher when the programme was in place. To do this we regress the daily clearance rate on the interactions between the treatment area dummy and each of the days relative to the post Friday dummies, and plot the resulting coefficients in Figure 12. We find no evidence of changes in the clearance rate coinciding with the introduction of Operation Insight, which is consistent with the notion that criminal expectations remaining unchanged. This may be a finding of general validity: throughout the world, patrol officers are typically assigned to large areas, so our results suggest that police presence *as typically deployed* may be too diffuse to affect apprehension probabilities and criminal perceptions significantly. By contrast, the elasticities of more than 30% obtained by Di Tella and Schargrodsky (2004) and Draca, Machin and Witt (2011) may have been the result of settings where criminal perceptions may have been more malleable. The static police presence following terrorist attacks and concentrated around Jewish buildings and underground stations may have affected criminal perceptions with an intensity that cannot be replicated by weaker (in absolute terms) treatments and/or during periods of relative normality.

There is a second reason why small absolute increases in police presence may be associated with low elasticities, relative to large increases: it may be easier for criminals to intertemporarily substitute over the very short-term, relative to over the medium or long-term. For instance, a criminal's motivation and window of opportunity are likely to remain largely unchanged over a ten-minute period. Therefore, doubling police presence from a five-minute to a ten-minute visit may have the only effect of increasing the time that the criminal waits until the police departs from an area. A criminal's ability to intertemporarily substi-

[30] A reminder here that the concept of elasticity accounts for the relative change in the independent variable, in this case police presence. Our argument is therefore that the elasticity may be larger for higher initial levels of police, relative to lower initial levels.

tute may however be much lower if police presence instead doubles from a twelve-hour to a twenty-four hour visit (the same relative increase). In this last case, the criminal may even be forced to completely forgo the commission of the crime. This would also be consistent with the clearance rate remaining constant. Unfortunately, our empirical setting lacks the type of very short-term experimental variation that would allow us to investigate empirically the magnitude of this intertemporal substitution, and we leave this for future research.

7 Conclusion

Despite the widespread use of police patrolling for crime deterrence (Reaves, 2015), little is known about its likely effect during relatively standard circumstances. The main obstacle has been to isolate realistic, exogenous and measurable variation in police street deployment. The setup that we study in this paper contains all these three ingredients. We estimate the deterrent effect of a realistic increase in police patrolling exploiting: (a) the exogenous variation in well-defined locations and time periods generated by Operation Insight, and (b) information on the location and timing of all crimes committed in Essex, together with high-frequency information on the location of police officers provided by their GPS locators. We find no evidence of deterrence and are able to rule out elasticities half the size of those obtained by the most influential studies in economics. The apparent lack of deterrence is present in a variety of samples, time periods, estimation strategies and measures of crime. A potential explanation for our findings is that changes to police presence in the vicinity of current typical levels are not associated with large elasticities.

FIGURES

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

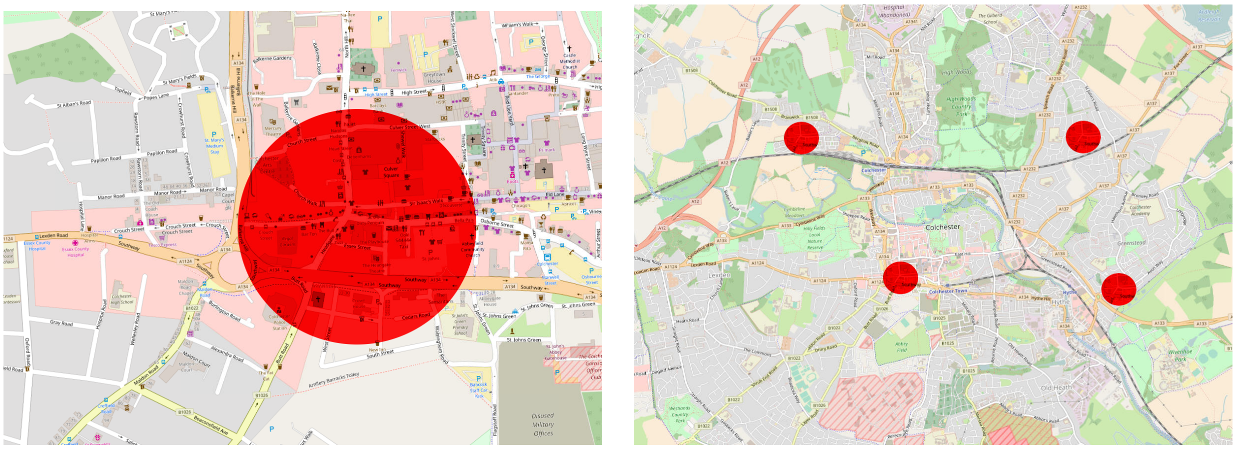


FIGURE 2: Differences-in-Differences Variation

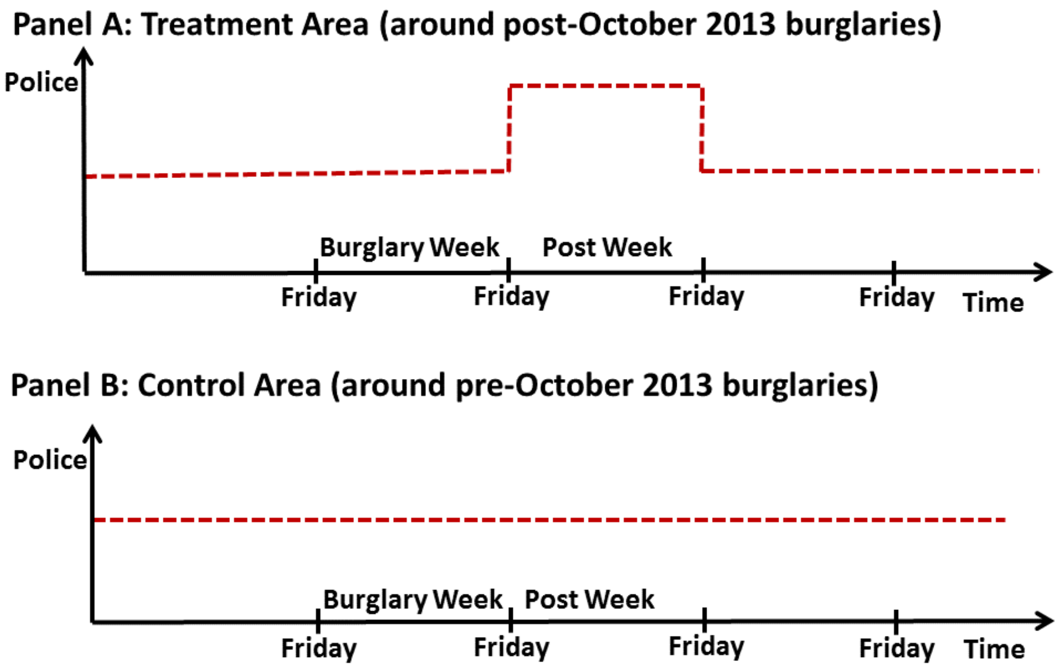


FIGURE 3: Daily Variation by Burglary Day of the Week

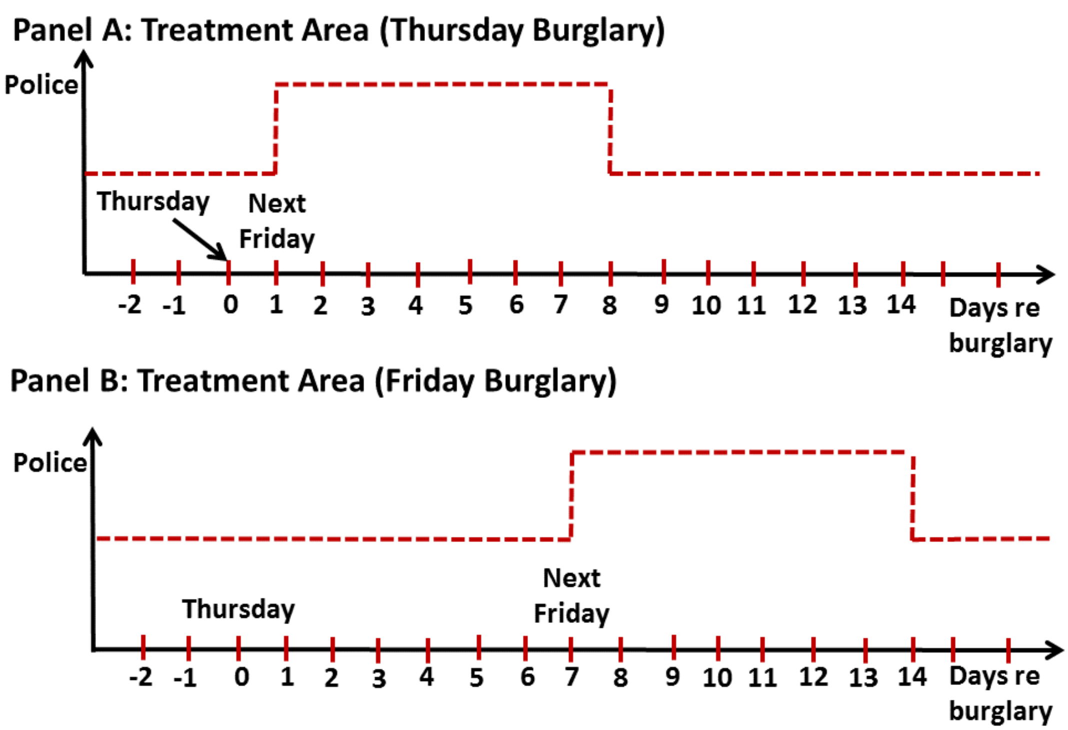


FIGURE 4: Location of the 8662 Burglaries

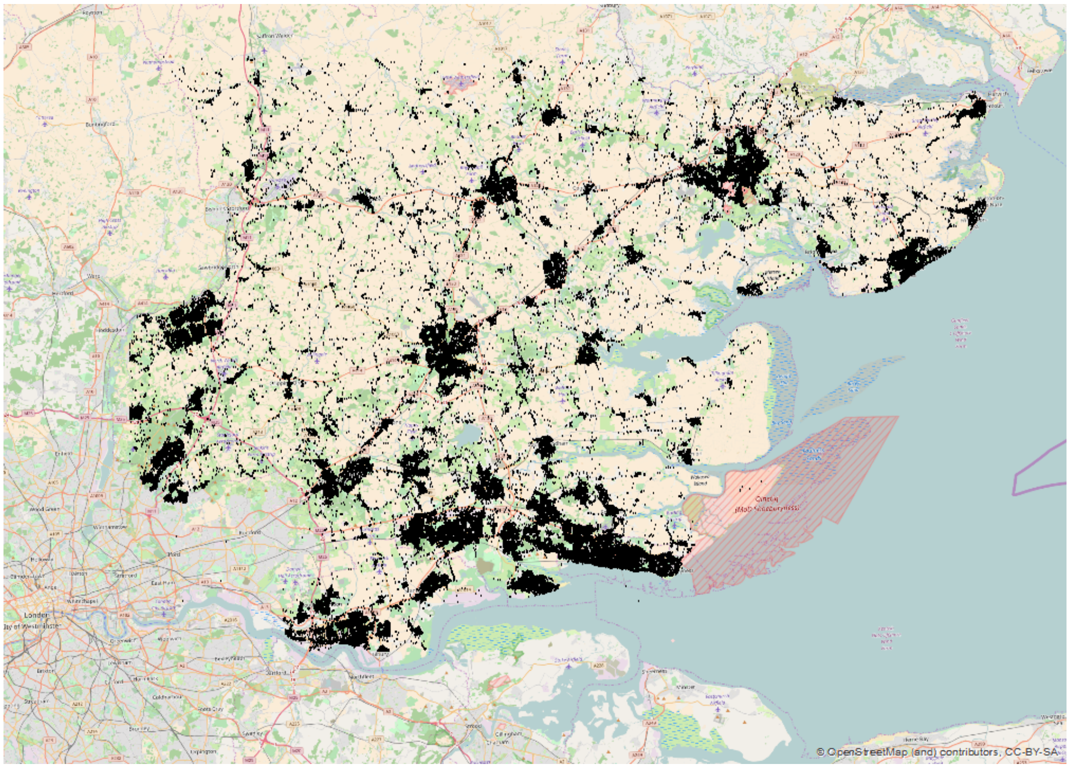
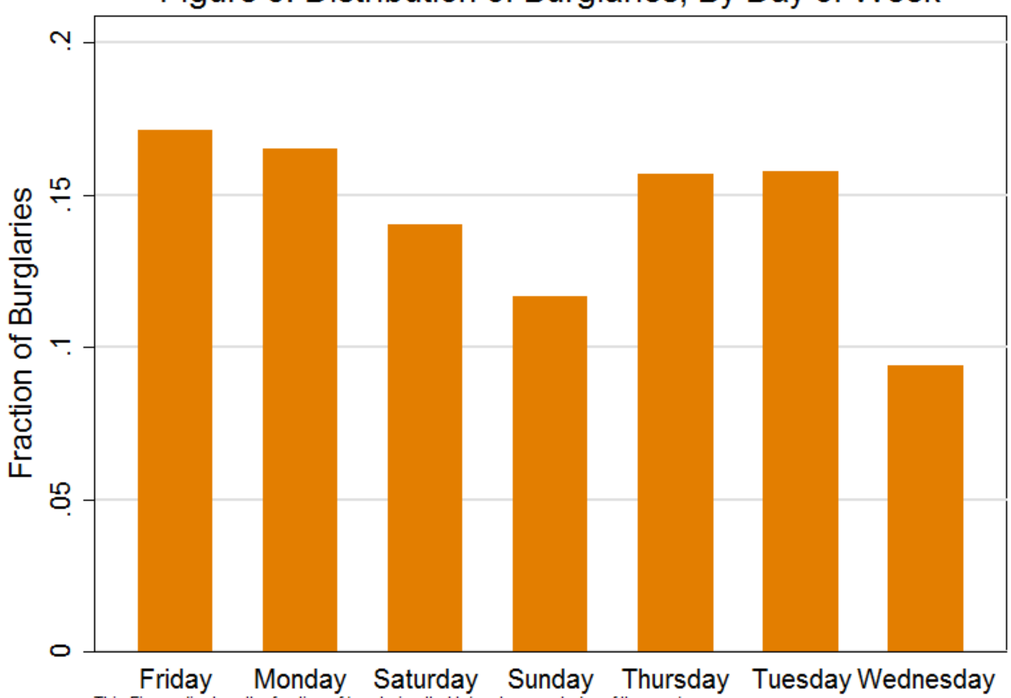


Figure 5: Distribution of Burglaries, By Day of Week



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

Figure 6A: Distribution of Number of Crimes

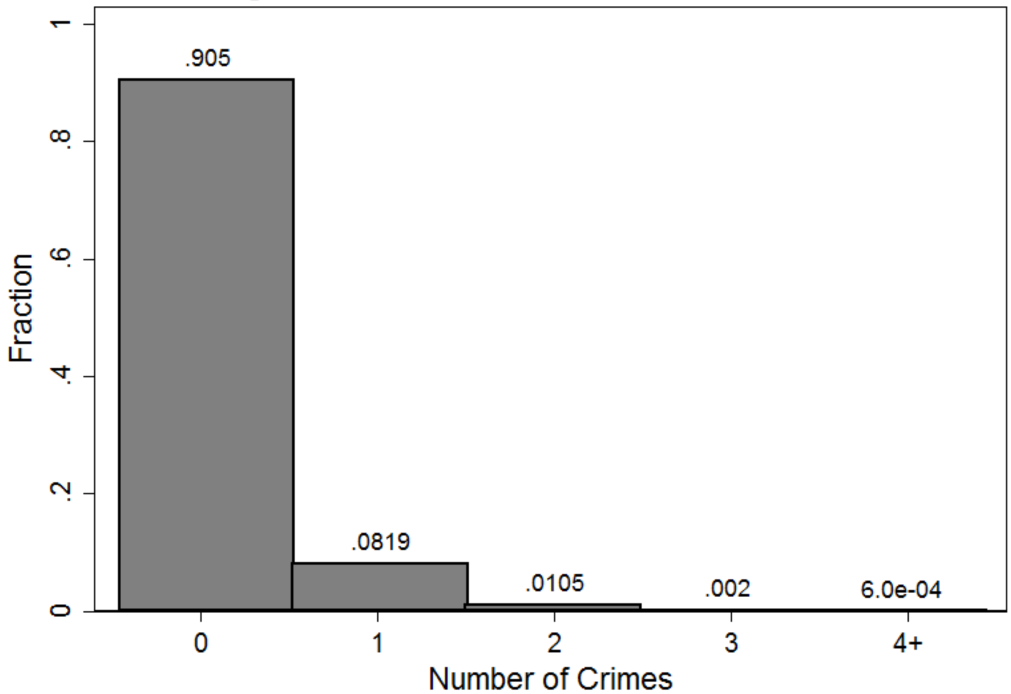
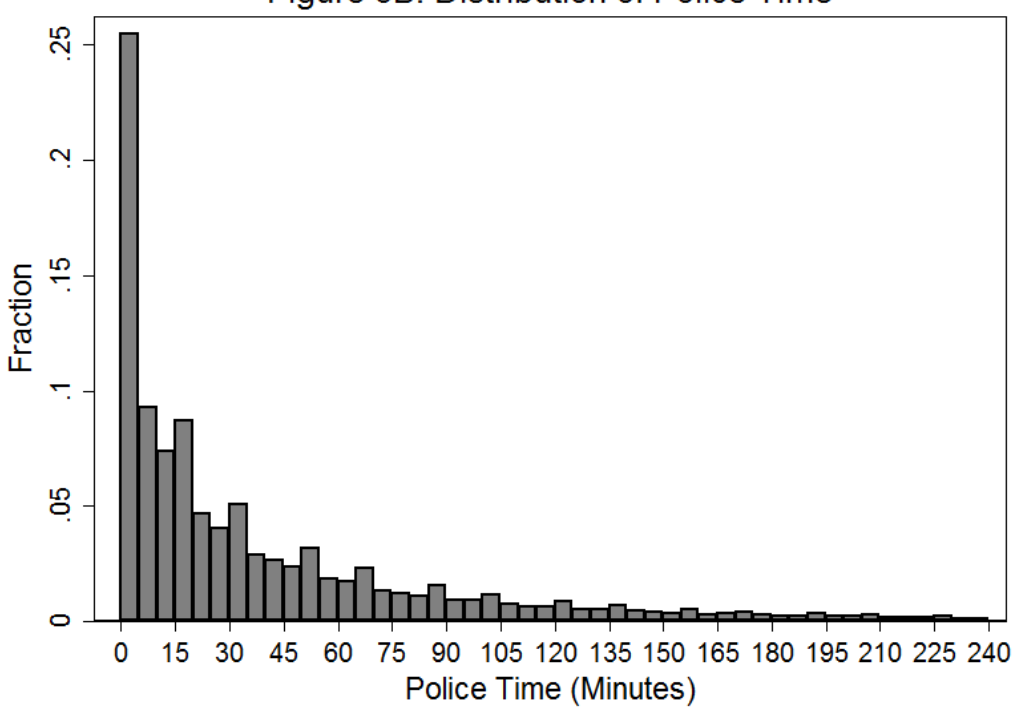


Figure 6B: Distribution of Police Time



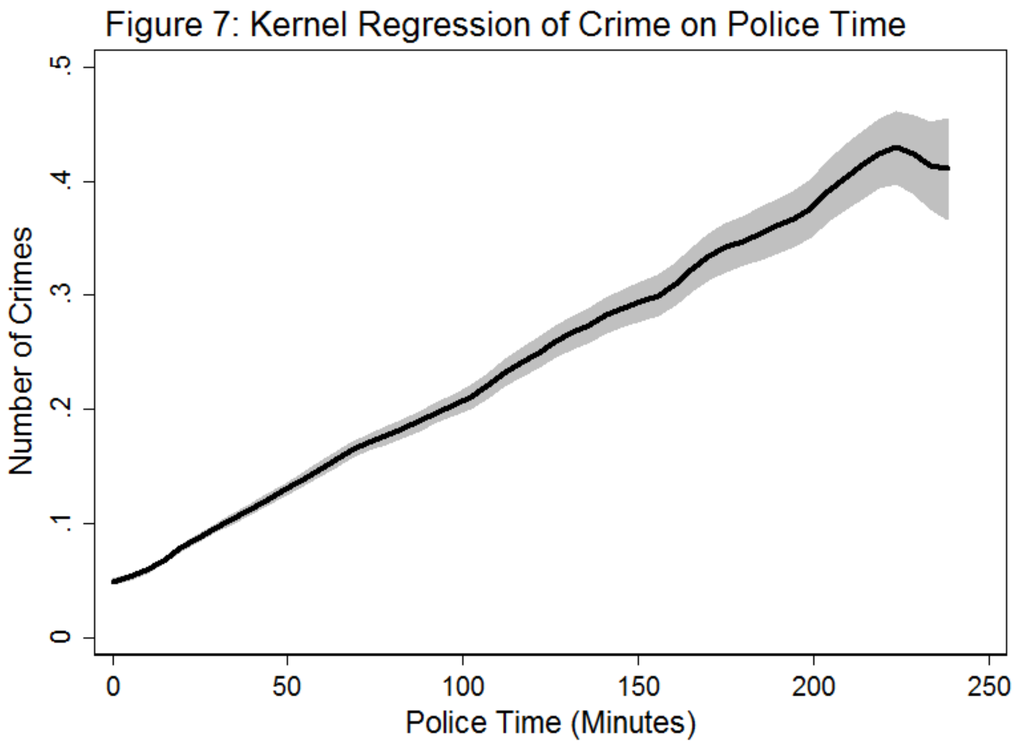
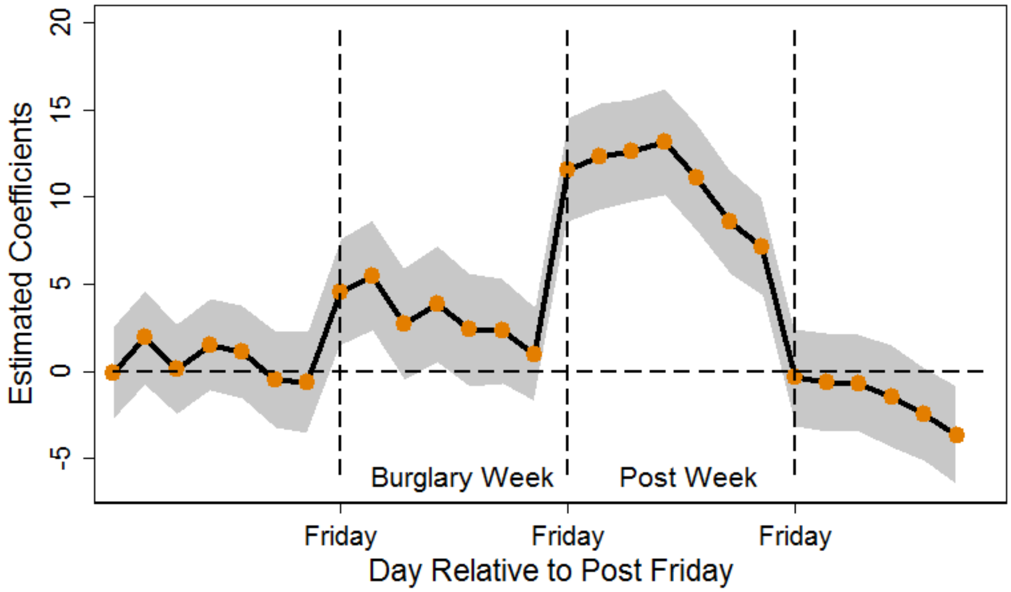


Figure 7: Kernel Regression of Crime on Police Time
Number of Crimes
0
.1
.2
.3
.4
.5
0
50
100
150
200
250
Police Time (Minutes)

Figure 8A: Coefficients of Day X Treatment Area

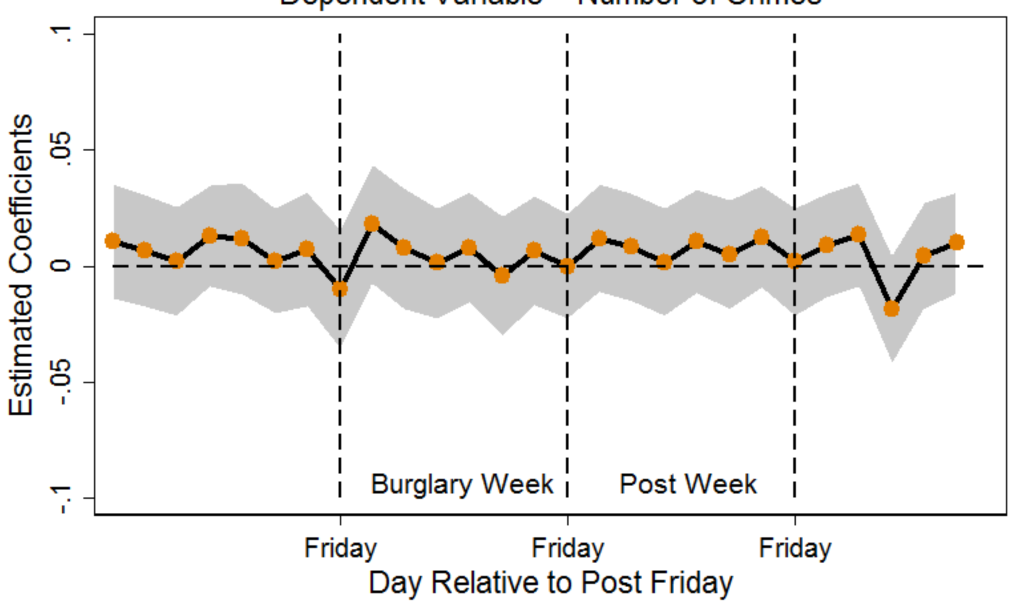
Dependent Variable = Police Patrolling Time



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.

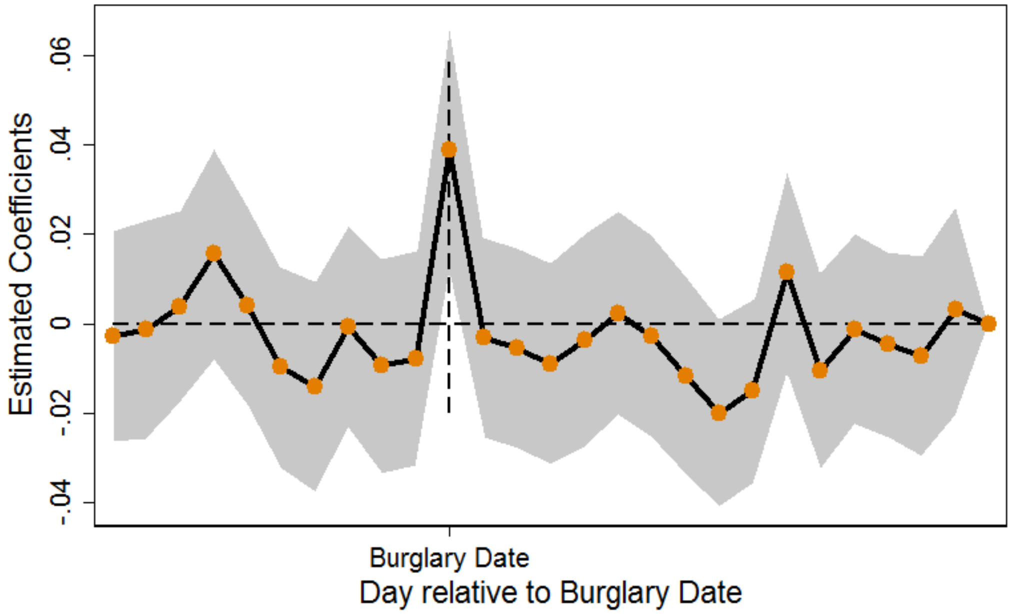
Figure 8B: Coefficients of Day X Treatment Area

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: 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.

Figure 10: Crime Levels and Police Presence
By Hours of the Day

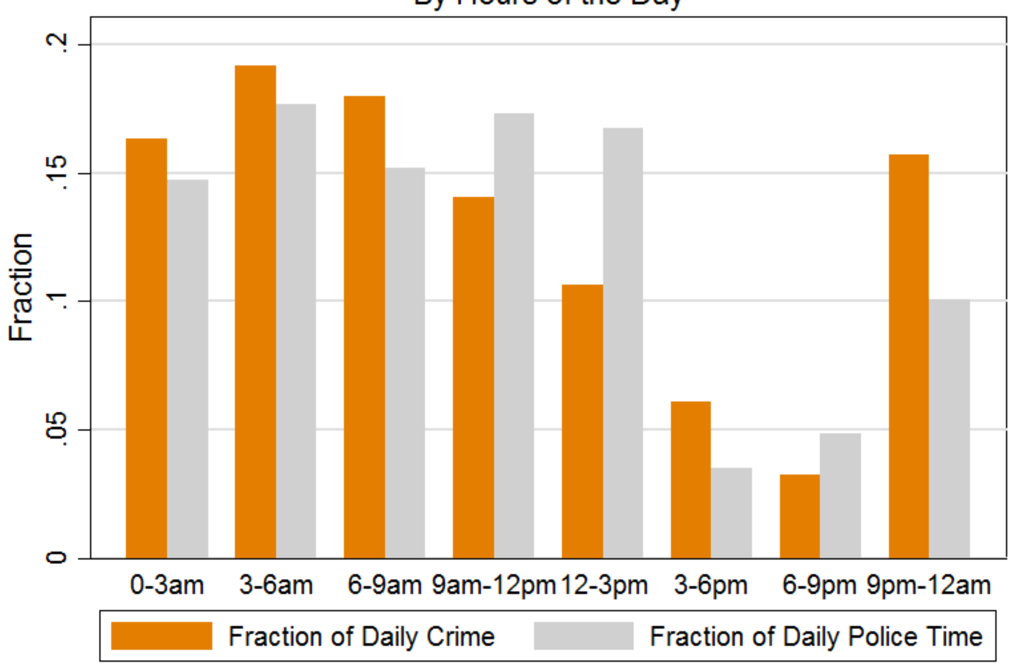
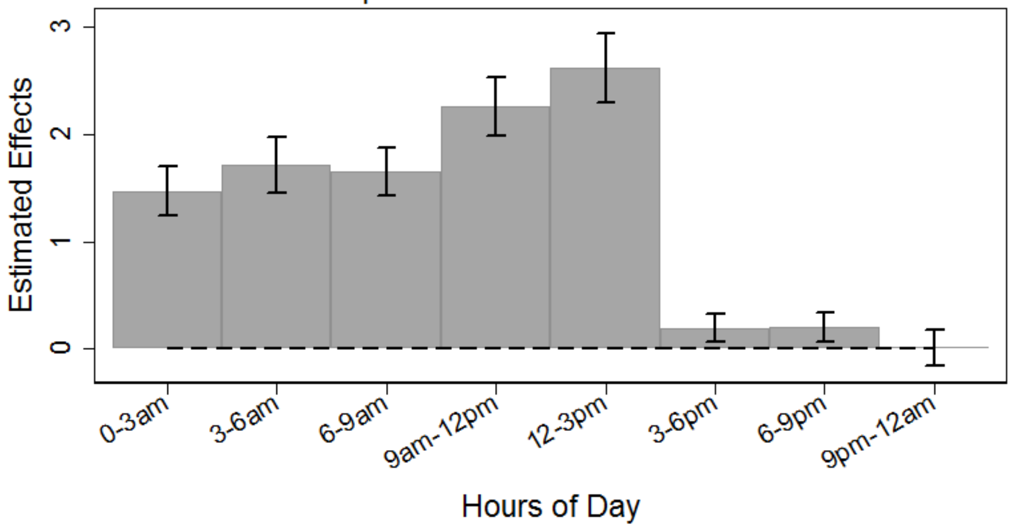
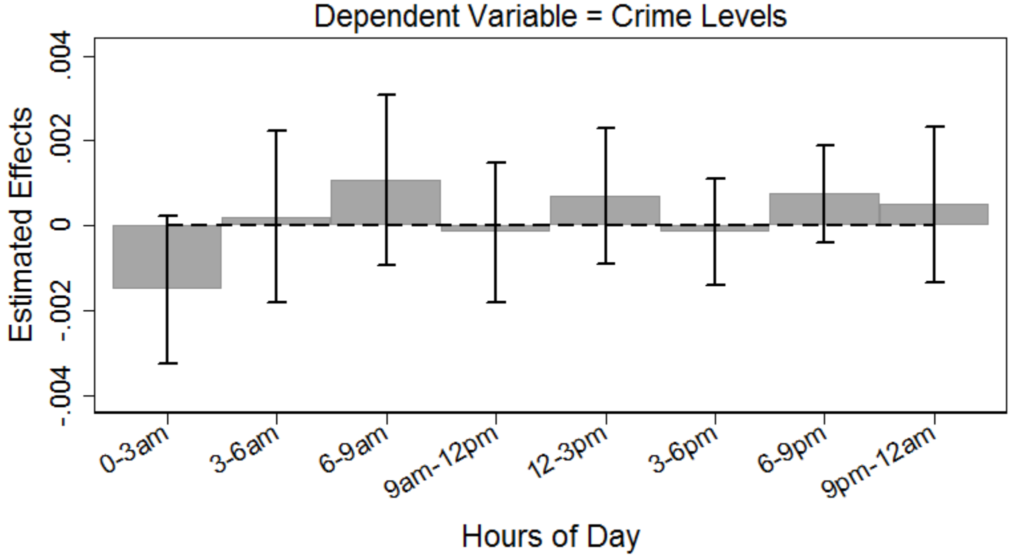


Figure 11A: Coefficients of
Treatment X Post X Hours of the Day
Dependent Variable = Police Time



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.

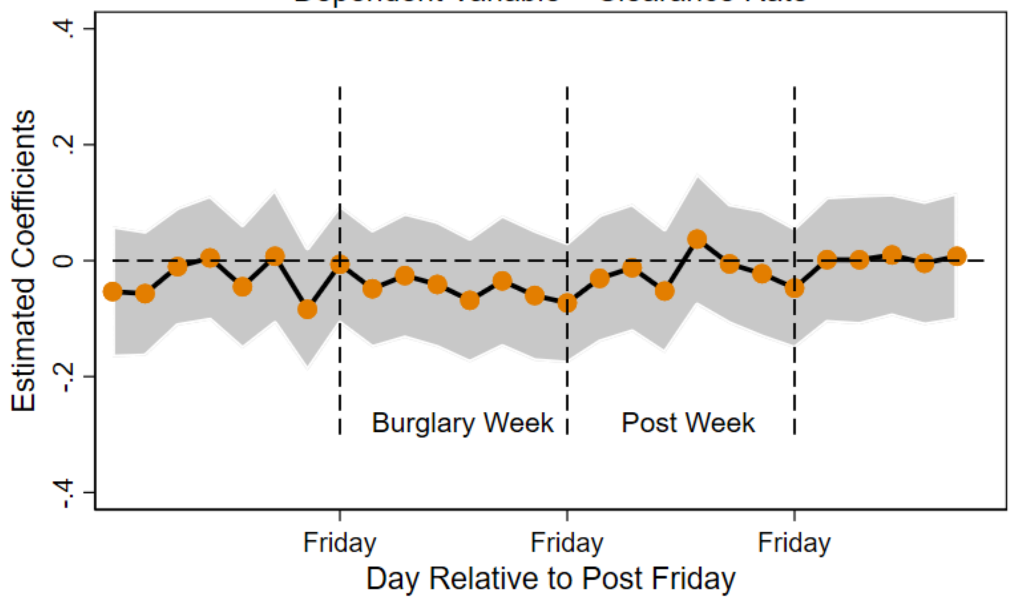
Figure 11B: Coefficients of
Treatment X Post X Hours of the Day
Dependent Variable = Crime Levels



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 12: Coefficients of Day X Treatment Area

Dependent Variable = Clearance Rate



This figure plots the estimated coefficients of the day-relative-to-post-Friday dummies interacted with the treatment area dummy. The dependent variable is the clearance rate. 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 clearance rate in the sample is .211. The number of observations in the regression is 20372.

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	10.15408***	
	(.00373)	(.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	.00014	.00014	.00014	.00014	.00014
	(.00037)	(.00036)	(.00037)	(.00041)	(.00034)	(.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: EVALUATING THE EMPIRICAL STRATEGY

MODEL	(1) Basic DiD	(2) +Area	(3) +Friday	(4) No DiD	(5) Baseline	(6) Poisson
Reduced Form Estimate	-.00185	.00142	.00143	.00116	.00146	.01218
	(.00419)	(.00372)	(.00372)	(.00307)	(.00373)	(.03706)
First Stage Estimate	7.88906***	10.11667***	10.11982***	8.73102***	10.15408***	10.15408***
	(1.11748)	(.58528)	(.58369)	(.55364)	(.5838)	(.5838)
Second Stage Estimate	-.00023	.00014	.00014	.00013	.00014	.00125
	(.00055)	(.00037)	(.00037)	(.00035)	(.00037)	(.00376)
Area F.E.	No	Yes	Yes	Yes	Yes	Yes
Day r.e. Post Week F.E.	No	No	Yes	No	Yes	Yes
Day re Burglary F.E.	No	No	No	Yes	Yes	Yes
Areas in Sample	All	All	All	Treated	All	All
Kleibergen-Papp F	49.84	298.8	300.6	248.7	302.52	.

Every cell displays the estimate of a separate regression. The controls differ across columns, as indicated. 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)-(6) 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)-(6) is 233620. The number of observations in Column (4) is 156864.

TABLE 6: 2SLS ESTIMATES, BY CRIME TYPE

DEP. VARIABLE	(1) Assaults	(2) Thefts	(3) Criminal Damage	(4) Robberies	(5) All Burglaries	(6) Dwelling Burglaries	(7) Commercial Burglaries
Police Time	-.00017	-.00004	.00025*	.00004	-.0001	-.00002	-.00008
	(.00014)	(.00016)	(.00013)	(.00004)	(.00015)	(.00013)	(.00008)
Mean Dep. Variable	.018	.024	.017	.001	.023	.014	.009

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: ROBUSTNESS TO THE AREA SIZE

AREA RADIUS	(1) 50m.	(2) 100m.	(3) 200m.	(4) 250m.
Reduced Form	.00052	-.00093	.00146	-.00414
	(.00107)	(.00193)	(.00373)	(.00446)
First Stage	.9289***	3.62923***	10.15408***	11.29906***
	(.06059)	(.21151)	(.5838)	(.79601)
Second Stage	.00056	-.00026	.00014	-.00037
	(.00114)	(.00053)	(.00037)	(.0004)
Kleibergen-Papp	235.04	294.42	302.52	201.49
Observations	230776	232532	233620	232375
Mean Crimes	.011	.033	.112	.166
Mean Police Time	2.374	9.695	38.896	59.233
Circle Area (sq. km.)	.002	.008	.031	.049

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 vary the size of the area around the burglary. In the baseline estimates of Table 3 we use 200m. radius areas, following the strategy designed by Essex Police. 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 8: OTHER ROBUSTNESS TESTS

MODEL	(1) 8 Weeks Only	(2) High Crime Areas	(3) Low Police Areas	(4) Not 'Caused' By Police	(5) Street Crime
Reduced Form	.0167	.01207	-.00442	.0002	.00012
	(.01251)	(.01023)	(.00452)	(.00323)	(.00343)
First Stage	23.56879***	9.51114***	10.29655***	10.15408***	10.15408***
	(3.39347)	(1.04106)	(.75994)	(.5838)	(.5838)
Second Stage	.00071	.00127	-.00043	.00002	.00001
	(.00052)	(.00107)	(.00044)	(.00032)	(.00034)
Kleibergen-Papp	48.24	83.47	183.58	302.52	302.52
Observations	13944	60335	58620	233620	233620
Mean Crimes	.096	.256	.04	.087	.098
Mean Police Time	41.366	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) uses only the eight-week period centred around the introduction of Operation Insight in October 2013. 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.

REFERENCES

Allen, G. and Dempsey, N. (2016), "Police Service Strength", *House of Commons Library*, Briefing Paper 00634.

Andenaes, J. (1974), "Punishment and Deterrence", *University of Michigan Press*, Ann Arbor.

Angrist, J. D., and Pischke, J. S. (2008), "Mostly Harmless Econometrics: An Empiricist's Companion", *Princeton University Press*, Princeton.

Banerjee, A., Chattopadhya, R., Duflo, E., Keniston, D., and Singh, N. (2017), "The Efficient Deployment of Police Resources: Theory and New Evidence from a Randomized Drunk Driving Crackdown in India", *Working paper*.

Becker, G. S. (1968), "Crime and Punishment: An Economic Approach; in The Economic Dimensions of Crime", *Palgrave Macmillan*, United Kingdom.

Bernasco, W. (2008), "Them Again? Same-Offender Involvement in Repeat and Near Repeat Burglaries European", *Journal of Criminology*, 5(4), 411-431.

Blanes i Vidal, J. and Kirchmaier, T. (2017), "The Effect of Police Response Time on Crime Clearance Rates", *Review of Economic Studies*, forthcoming.

Bound, J., Jaeger, D.A. and Baker R.M. (1995), "Problems with Instrumental Variables Estimation when the Correlation between the Instruments and the Endogenous Explanatory Variable is Weak", *Journal of the American Statistical Association*, 90(430), 443-450.

Braga, A.A. (2007), "Policing Crime Hot Spots; in Preventing Crime", *Springer*, New York, 179-192.

Centre for Popular Democracy (2017), "Freedom to Thrive: Reimagining Safety & Security in our Communities", available at https://populardemocracy.org/news/publications/freedom-thrive-reimagining-safety-security-our-communities.

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2011), "Robust Inference with Multiway Clustering", *Journal of Business & Economic Statistics*, 29(2), 238-249.

Chalfin, A., and McCrary, J. (2017), "Criminal Deterrence: A Review of the Literature", *Journal of Economic Literature*, 55(1), 5-48.

Chalfin, A. and McCrary, J. (2017) "Are U.S. Cities Underpoliced? Theory and Evidence", *The Review of Economics and Statistics*, forthcoming.

Chainey, S. (2012), "JDI Briefs: Predictive mapping (predictive policing) (JDI Briefs)", *UCL Jill Dando Institute of Security and Crime Science*", *University College London* London, United Kingdom.

Conley, T. G. (1999), "GMM Estimation with Cross Sectional Dependence", *Journal of Econometrics*, 92(1), 1-45.

Di Tella, R., and Schargrodsky, E. (2004), "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces After a Terrorist Attack", *American Economic Review*, 94(1), 115-133.

Draca, M., Machin, S. and Witt, R. (2011), "Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks", *American Economic Review*, 101, 2157-2181.

Durlauf, S. N., and Nagin, D. S. (2011), "Imprisonment and Crime", *Criminology & Public Policy*, 10(1), 13-54.

Evans, W. N., and Owens, E. G. (2007), "COPS and Crime", *Journal of Public Economics*, 91(1), 181-201.

Heaton, P., Hunt, P., MacDonald, J., and Saunders, J. (2016), "The Short-and Long-Run Effects of Private Law Enforcement: Evidence from University Police", *Journal of Law and Economics*, 59(4), 889-912.

HMIC (2014), "Policing in Austerity: Meeting the Challenge", available at https://www.justiceinspectorates.gov.uk/hmicfrs/wp-content/uploads/policing-in-austerity-meeting-the-challenge.pdf.

Johnson, S. D., and Bowers, K. J. (2004), "The Burglary as a Clue to the Future: The Beginnings of Prospective Hot-Spotting", *European Journal of Criminology*, 1, 237-255.

Kelling, G. L., Pate, T., Dieckman, D., and Brown, C. E. (1974), "The Kansas City Preventive Patrol Experiment", *Washington, DC: Police Foundation*.

Kelling, G. L., Pate, A., Ferrara, A., Utne, M., and Brown, C. E. (1981), "The Newark Foot Patrol Experiment", *Washington, DC: Police Foundation*.

Klick, J., and Tabarrok, A. (2005), "Using Terror Alert Levels to Estimate the Effect of Police on Crime", *Journal of Law and Economics*, 48(1), 267-279.

Larson, R. C. (1975), "What Happened to Patrol Operations in Kansas City? A Review of the Kansas City Preventive Patrol Experiment", *Journal of Criminal Justice*, 3, 267-297.

Levitt, S. D. (1997), "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime", *The American Economic Review*, 87(3), 270-290.

Levitt, S. D. (1998), "Why do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?", *Economic Inquiry*, 36(3), 353-372.

MacDonald, J. M., Klick, J., and Grunwald, B. (2016), "The Effect of Private Police on Crime: Evidence from a Geographic Regression Discontinuity Design", *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 179(3), 831-846.

Machin, S., and Marie, O. (2011), "Crime and Police Resources: The Street Crime Initiative", *Journal of the European Economic Association*, 9(4), 678-701.

Mastrobuoni, G. (2015), "Police Disruption and Performance: Evidence from Recurrent Redeployments within a City", *Working paper*.

Nagin, D. S. (1998), "Criminal Deterrence Research at the Outset of the Twenty-first Century", *Crime and Justice*, 23, 1-42.

Nagin, D. S. (2013a), "Deterrence in the Twenty-First Century", *Crime and Justice*, 42(1), 199-263.

Nagin, D. S. (2013b), "Deterrence: A review of the Evidence by a Criminologist for Economists", *Annual Review of Economics*, 5(1), 83-105.

Ratcliffe, J. H., Taniguchi, T., Groff, E. R., and Wood, J. D. (2011), "The Philadelphia Foot Patrol Experiment: A Randomized Controlled Trial of Police Patrol Effectiveness in Violent Crime Hotspots", *Criminology*, 49(3), 795-831.

Reaves, B. A. (2015), "Local Police Departments, 2013: Personnel, Policies, and Practices", US Department of Justice, Office of Justice Programs, Bureau of Justice Assistance, Washington, DC, US Department of Justice, Office of Justice Programs, Bureau of Justice Assistance, Washington, DC.

Reiner, R. (2010), "The Politics of the Police", *Oxford University Press*, Oxford.

Ross N., and Pease K. (2007), “Community Policing and Prediction”, In Williamson T. (Ed.), *Knowledge-Based Policing*, Chichester: Wiley.

Townsley, M., Homel, R., and Chaseling, J. (2003), “Infectious burglaries. A test of the near repeat hypothesis”, *British Journal of Criminology*, 43(3), 615-633.

Perry, W. L. (2013), “Predictive Policing: The role of Crime Forecasting in law Enforcement Operations”, *Rand Corporation.*

Pease, K. (1998), “Repeat victimisation: Taking Stock”, *Home Office Police Research Group*, London.

Sherman, L. W. (1986), “Policing Communities: What Works; in Communities and Crime”, *University of Chicago Press*, Chicago.

Weisburd, S. (2017), “Police Presence, Rapid Response Rates, and Crime Prevention”, *Working paper.*

Online Appendix

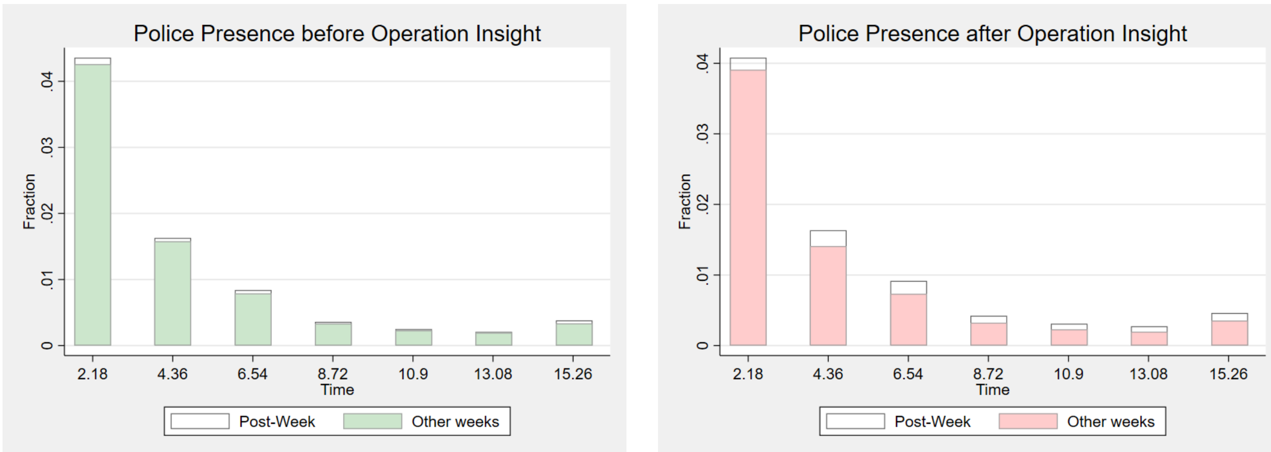


Figure A1: High Frequency Evidence on Police Presence

Notes: We construct the figures aggregating police presence inside our 200m-radius circles in 15-minutes intervals. To better understand the treatment intensity of individual officers we only include 5-minute periods where police presence is at most 5-minute long (thus excluding places where several police officers contribute to the total police time). The left panel shows the distribution of total police presence in 15-minute intervals before October 2013, for both, the post-week and the other weeks.

TABLE A1: EFFECTS ON NUMBER OF VISITS AND AVERAGE LENGTH OF VISIT

DEP. VARIABLE	(1) Visits	(2) Average Length
Treatment X Post	.38141***	2.14664***
	(.04734)	(.15685)
Area F.E.	Yes	Yes
Day re Burglary F.E.	Yes	Yes
Day re Post-Friday F.E.	Yes	Yes

This table displays OLS regressions of Treatment X Post on the number of police visits to an area and the average length of stay on that area. 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. Standard errors are clustered at the Week X District level. The mean number of visits in the sample is 5.553. The mean of average length of stay is 7.404 minutes.