

The Impact of the First Professional Police Forces on Crime*

Anna Bindler
University of Gothenburg

Randi Hjalmarsson
University of Gothenburg and CEPR

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Abstract: This paper evaluates the impact of the introduction of professional police forces on crime using manually transcribed archival records and two natural experiments in history: the formation of the London Metropolitan Police in 1829 (the first professional force worldwide) and the subsequent roll-out of professional forces to the counties of England and Wales from 1839 to 1856. These new professional police were explicitly tasked with deterring crime, which contrasts the mandate of the old, informal ‘police’ to simply apprehend criminals. Estimating pre-post, difference-in-differences as well as event-study specifications, we find evidence that the creation of a professional police force reduces crime overall and across crime categories. Our estimates of the effect of the London Metropolitan police on violent crime range from -26% to -57%, where the former corresponds to charges and the latter to daily incidents. London property crime incidents decreased by 26% while charges increased by 21%. This is consistent with a crime reducing deterrence and/or incapacitation effect on property crime being offset by increased crime reporting and/or clearance rates. A difference-in-differences analysis of county force roll-out finds that high quality police forces, measured by the population to force ratio, reduced overall crime by 18% (and across crime categories), while there was no net crime reducing effect of a force that was not sufficiently large.

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1. Introduction

Do *more* police reduce crime? A large theoretical and empirical economics of crime literature has attempted to answer this question for the last 50 years. Chalfin and McCrary's (2017) recent review of the empirical research concludes that there is at least a "consensus that increases in police manpower reduce crime". The main contribution of our paper is to study a yet unstudied margin of policing: Do *any* police reduce crime? Specifically, we identify the effect of the introduction of a modern day professional police force (i.e. the extensive margin) on crime using two natural experiments in history: the formation of the London Metropolitan Police in 1829 (the first professional police force in the world) and the subsequent roll-out of rural county police forces throughout England and Wales during the following 30 years. In contrast to the temporary shocks to policing that are often studied in the existing literature, these new police institutions were permanently put in place and still exist today – more than 150 years later.

According to Becker's (1968) economic model of crime, the answer to both questions – do more/any police reduce crime – should be yes: if increasing the number of police at any margin increases the (perceived) chance that an offender is caught, then crime should be reduced through deterrence. Crime can further be reduced due to incapacitation if the (additional) police lead to an increase in apprehensions (thereby preventing recidivism). Empirical evidence of the crime reducing effect of police is more elusive, however, due to both a likely simultaneity bias – more police are hired in higher crime locations or times – and potential measurement error in the number of police (Chalfin and McCrary, 2018).¹ A large body of empirical research, dating to Levitt (1997), identifies the causal effect of police on crime using strategies ranging from instrumental variables to natural experiments that locally or temporarily increased police numbers.² Despite consensus that more police reduce crime, it is less clear whether this is driven by deterrence or incapacitation. Reductions of local crime in police hot spots strongly suggest that deterrence plays a role (Chalfin and McCrary, 2017).

Our paper makes four key contributions to the police-crime literature. First, our analysis of a *large* shock to policing at the *extensive margin* – the creation of an entirely new force – contrasts the existing literature evaluating the marginal effect of an additional officer. Second,

¹ Given these difficulties in identification, it is hence not surprising that most of the earliest studies on the topic (see Cameron (1988) for a review) that do not account for this simultaneity bias find either no evidence of deterrence or even a positive effect of police on crime.

² Levitt (1997) used U.S. mayoral and gubernatorial election years to instrument for the number of police, though McCrary (2002) ultimately showed this to be a weak instrument. Alternative instrumental variable designs include police hiring grants (Evans and Owens, 2007). Natural experiments involving the reallocation of police forces following an unexpected shock include the 2005 London terrorist attacks (Draca et al., 2011) and the 1994 attacks on Argentinian Jewish centers (DiTella and Schargrodsky, 2004).

we study a *permanent* shock to policing, and can trace out the long-run impacts of police force formation on crime in our county analysis. This contrasts the literature looking at the temporary reallocation of police officers, for instance, following a terrorist attack (Draca et al., 2011). Third, we study how the ‘quality’ of the new police, measured in part by the population to officer ratio, affects crime. That is, we study not only the extensive margin of introducing police, but also key characteristics of these new forces that may impact their effectiveness. Finally, our study contributes to explaining 19th century trends in crime. Crime rose in the first half of the century but was followed by a decline in the latter half despite the quickly growing population – an ‘English miracle’ (Taylor, 1998). Did the formation of professional police forces contribute to this miracle?^{3,4}

The idea of ‘policing’ certainly already existed prior to the creation of professional forces. In London, less formal institutions included thief-takers and the Bow Street Runners. In counties, local watches were often organized. Why then would one expect the formation of a ‘professional’ force to affect crime and not simply crowd out these pre-existing, informal police (without affecting crime)? One reason is that the primary task of these new forces was deterrence. Metropolitan Police were assigned to walk a beat – a regular route – at a pace of about 2.5 miles per hour; the beat was intentionally small to increase visibility. In contrast, the previous ‘police’ were reactionary and focused on catching criminals (for financial reward) rather than crime prevention (Emsley, 2009). The improved ‘quality’ of the new police may also have impacted their effectiveness; they were held to a higher professional standard and provided better working conditions, including full-time salaries.

Empirically identifying the effect of the new police on crime is not a simple matter. One potential confounder is an increase in the reporting of crimes to the police (even if there was no change in criminal behavior). This would only have happened if there was increased societal trust in ‘police’. Yet, anecdotal evidence suggests that, at least initially, there were anti-police sentiments. This is reflected, for instance, in two newspaper articles published on October 1, 1829 (one day after the formation of the Metropolitan Police): The Morning Journal quotes a magistrate as stating that “a strong feeling existed against the new police” while The Morning Herald quotes a member of a mob as shouting “it is one of Peel’s bloody police; they are all

³ Other papers, which are not focused on policing, have studied the economics of crime in historical England using national time series data. Wong (1995) tries to understand declining crime from 1857 to 1892 as a function of opportunities for legal and illegal gains. Wolpin (1978) looks at the relationship between crime and the clearance rate, conviction rate, imprisonment rate, fine rate, and average sentence from 1894 to 1967.

⁴ Other papers studying crime in a historical context (19th century) include Bignon et al.’s (2017) study of the impact of a negative income shock to French vineyards and Mehlum et al.’s (2006) and Traxler and Burhop’s (2010) studies of increasing poverty in Bavaria and Prussia, respectively.

thieves themselves”.⁵ In addition, more charges may have occurred because of an increased ability to detect crime; that is, even if the number of crimes committed did not change, there could have been an increase in clearance rates. This would have naturally occurred because the new force was substantially larger in size than what existed previously. Of course, this increased detection would in turn be expected to *reduce* crime through incapacitation (over and above deterrence). Since many arrested offenders were held while awaiting trial, this incapacitation effect could occur immediately. To disentangle whether the creation of professional police forces reduced crime (through deterrence and/or incapacitation) from the potential confounders of increases in both crime reporting and clearance rates, we rely on two types of crime measures – crime incidents and charges. The availability of incident level data is especially important: Crime reducing effects could be masked in administrative measures (like charges) if the increased clearance channel dominates. At this point, however, we are limited in our ability to disentangle whether the observed crime reductions are due to deterrence or incapacitation. We can point out, though, that the goal of the new police was to shift regimes from one focused on harsh sanctions and incapacitation to one focused on deterrence and increased police presence.

The main distinction between the new and old ‘police’ was that the new police were explicitly tasked with deterrence by being visibly deployed on the streets. Thus, while there are (to the best of our knowledge) no other studies of the extensive margin effect of creating a force, our study is closely related to papers studying police deployment on the streets.⁶ A number of studies report a crime reduction following temporal variation in (often non-permanent) police deployment, including Draca et al.’s (2011) and Di Tella and Schargrosky’s (2004) studies of post-terrorist attack increases in police deployment in London and Buenos Aires, respectively.⁷ Most recently, however, Blanes I Vidal and Mastrobuoni (2018) do not find a significant relationship between non-terrorist attack related temporary increases in police patrols and crime. Negative effects of a visible police presence on crime have been found in studies of private policing using geographic boundaries (MacDonald et al., 2015; Heaton et al., 2016); these studies aim to understand the *permanent* effect of policing using spatial variation in force

⁵ Sir Robert Peel was the Home Secretary from 1822 to 1830 and regarded as the founder of modern day policing. These articles are part of a collection or ‘scrapbook’ on the Open University website:

<https://www.open.ac.uk/Arts/history-from-police-archives/MphcR1/Scrapbooks/sbIntro.html>

⁶ Studies have considered, however, the extensive margin destruction of a police force. As described by Nagin (2013), Andenaes finds a rise in crime rates, especially street crimes likely robbery, after German soldiers arrested all members of the Danish police force in 1944.

⁷ Negative effects of police on crime are also found by Klick and Tabarrok (2005) following increases in Washington DC terrorist alert levels and Weisburd (2017) using variation in officers leaving their beats unattended.

allocation.^{8,9} Our study advances the literature by estimating the effect of a permanent change in policing on crime, exploiting variation both over time and across space.

Our empirical analysis consists of two parts: the formation of the London Metropolitan Police (the ‘Met’) and the subsequent roll-out of county forces. The Met was created in September 1829; it was initially 1,000 men strong and increased to more than 3,000 by May 1830. The initial catchment area was within an approximate 7-mile radius from Charing Cross, London (with, as we find in our analysis, a higher treatment intensity within a smaller radius) and extended to a 15-mile radius in 1839. Excluded from the initial catchment area, however, were the City of London (established its own force in 1832/1839 that is still distinct today) and, until 1839, the Thames River Police. Because not all of London is ‘treated’ by the formation of the Met, our empirical strategy relies on geocoding historical crime data into ‘treated’ and ‘control’ regions of London for periods before and after the Met was created.

We use two data sources for the London analysis. The first is the Proceedings of the Old Bailey (the Central Criminal Court of London and surrounding county of Middlesex). The Proceedings contain the case details and outcomes for more than 200,000 felony trials between 1700 and 1913 and were published after each monthly court session; these have since been digitized by *The Old Bailey Proceedings Online*. From these reports, we have coded the date, location as well as number and type of police witnesses for the most serious offenses (murder, manslaughter, robbery and burglary) from 1820 to 1850. The Old Bailey data have two advantages. First, it allows us to directly study the implementation of the reform; we see a clear and immediate shift in the type of police witnesses (from ‘old’ to ‘new’) that is much larger in the treated than any other area. Second, we can estimate a difference-in-differences design – intuitively, comparing changes in crime inside the treatment and control area(s). The second data source consists of police reports with daily charges and incidents, including stolen property reports, for the nine existing police offices from January to April of 1828 (one year pre-reform), 1831 (one year post reform) and 1832 (two years post reform). These offices were run by the pre-1829 ‘police’ and continued until 1839. These data allow us to look at the effect of the Met on all offenses, rather than just the felonies at the Old Bailey. A limitation, however, is that all of the offices are within the catchment area of the Met, necessitating simple pre-post designs.

⁸ To the extent that decreased response times implies an increase in police presence, Blanes I Vidal and Kirchmaier (2018) find a relationship between response time and the likelihood of clearing a crime.

⁹ Field experiments in criminology have found evidence of deterrence of increased police patrols in crime hotspots in Minneapolis and Philadelphia (Sherman and Weisburd, 1995; Ratcliffe et al., 2011). MacDonald et al. (2015) study the effects of sustained police deployment using variation from the NYPD’s Operation Impact; they find reductions in robberies and burglaries that may be consistent with a deterrent effect of physically present police.

The analyses of both London data sources provide evidence consistent with a crime-reducing effect, especially for violent crimes (including robbery). A significant and persistent reduction in trials is seen for robbery (46%) in the Old Bailey data and for violent crime incidents (57%) and charges (26%) in the daily police report data. Moreover, in the daily police report data, we also see a reduction in stolen property reports (26%) but an increase in property charges (21%). These results are consistent with deterrence and/or incapacitation dominating the apprehension/reporting channels for violent crimes, but vice versa for property crimes.

Professional police forces in English and Welsh counties were allowed for by an 1839 Act but did not become mandatory until the County and Borough Police Act in 1856. The 1856 Act further introduced a national inspectorate tasked with annually certifying the ‘efficiency’ of these forces. The main measure of efficiency was the number of people per officer – with 1,000 being the recommended (but rarely achieved) guideline. Of the 48 English and Welsh counties in our analysis, 16 created police forces in 1840, 23 in 1857 (when mandatory), and 9 in the intermediate years. We use a difference-in-differences design to identify the effect of the creation of a professional force on crime, overall and for efficient versus inefficient forces. Our main measure of crime (the only one available both before and after forces were created) is the annual number of persons committed to trial for violent, property and other crimes, which we manually transcribed for each county from historical *Judicial Statistics* yearbooks.

We find no overall effect of creating just any professional police force. But, the creation of an ‘efficient’ county force (in terms of people per officer) reduced crime overall (19%) and across crime categories (18% for violent, 14% for property and 24% for other offenses). The creation of an inefficient force, on the other hand, did not have a net crime (trial) reducing effect. An event-study specification demonstrates that the crime-reducing effect of efficient force creation is not immediate (delayed by one to two years) and increases in magnitude over time. The insignificant leads support the plausibility of the parallel trend assumption and a lack of anticipatory effects. Finally, these main results are robust to controlling for spill-over effects of police forces in neighboring counties, although these in fact show local effects on crime: efficient neighbors decrease crime, potentially by cooperating with the local force (such cooperation was indeed a factor used by the inspectors in evaluating efficiency).

The remainder of the paper proceeds as follows. Section 2 provides institutional details related to the creation of the Metropolitan Police in 1829 and the roll-out of county police forces between 1839 and 1856. Section 3 presents the two London data sources and analyses of the Metropolitan Police. Section 4 presents the county data and analysis. Section 5 concludes.

2. *Institutional Background*

2.1. *The Introduction of the London Metropolitan Police in 1829*

Though there was no professional ‘police’ in London until the Metropolitan Police Act of 1829, the idea of policing existed before. Dating back to the Westminster Watch Act of 1735, this was largely in the form of unpaid and part-time local (night) watchmen. The Bow Street Runners of London date to around 1750; there were typically eight Runners at a time, who were sworn constables of Westminster (Emsley, 2009). They primarily located and arrested serious offenders, and they were not meant to deter crime. Indeed, there were so few that they could not have had a large physical presence. In many ways, the Runners were not too different from the thief-takers of the 18th century, i.e. men who earned their livings from private and public rewards upon the convictions of ‘serious’ criminals. By the end of the 1700s, however, the Bow Street Runners were essentially full-time policemen and seen as less corrupt than the thief-takers, and the Bow Street house at which they were located became a centralized collection point of crime incidents for the Runners to follow-up on.¹⁰

The Bow Street office was used as a model for the establishment of seven additional Police Offices in the Middlesex Justices Act of 1792: Queen's Square (Westminster), Great Marlborough Street (Westminster), Worship Street (Shoreditch), Lambeth Street (Whitechapel), Shadwell (closed and replaced by Marylebone High Street by 1816), Union Hall (Southwark) and Hatton Garden. Each office was staffed by three magistrates and up to 12 constables (Emsley, 2009). These were amongst the first salaried police. A Thames River police was established in 1798 in Wapping.¹¹ These Police Offices existed until 1839, i.e. 10 years after founding the Met, and play an essential role in our analysis. During the overlapping period, the original Offices and the new Met co-existed, with a “live-and-let-live arrangement on the streets, even assisting each other when necessary” (Emsley, 2009).

Finally, the Metropolitan Police Act of 1829 (10 Geo.4, c.44) created the London Metropolitan Police (the ‘Met’) on September 29, 1829. This was the first *professional* police force in the world. Initially 1,000 men strong, there were more than 3,000 officers by May 1830. Panel A of Figure 1 documents the weekly number of hires from September 1829 to March 1831, and Panel B the weekly growth of the Met until 1856. Two observations stand out. First, initial hiring happened in two stages. Recruits were first hired for six inner divisions in

¹⁰ This summary is based largely on the London Lives website, accessed February 6, 2018. <https://www.londonlives.org/static/Policing.jsp>

¹¹ For a summary of the Police Offices, see the Open University webpage, <http://www.open.ac.uk/Arts/history-from-police-archives/Met6Kt/MetHistory/mhPolOffices.html> (accessed February 6, 2018).

September 1829 and then five months later in February 1830 for the 11 outer divisions. See column (5) of Appendix Table A1. Second, the Met grew almost constantly in the next 30 years to about 6,000 men in 1856, but the population was growing quickly as well.

The initial catchment area of the Met was within an approximately 7-mile radius from Charing Cross in Central London.¹² This was extended to 15-miles in 1839.¹³ Excluded from the initial catchment area, however, were the City of London (which established its own force in 1832 – expanded in 1839 and still distinct today) and, until 1839, the Thames River Police.¹⁴ Panel A of Figure 2 presents a historical map of the original jurisdiction of the Metropolitan Police. In Panel B, we map the (geocoded) pre-existing police offices and demonstrate that they were all centrally located within the 7-mile radius (and even a smaller, 4-mile radius) and thus ‘treated’ by the creation of the Met. Moreover, Appendix Table A1 shows that the number of police hired into each division is approximately the same, regardless of the geographic size of the division. As the inner divisions are smaller, there is a potentially more intense treatment in a shorter radius around Charing Cross, an idea we will return to in the empirical specification.

If ‘policing’ already existed, why would the creation of the Met affect crime? The first obvious reason is that there were sharply more police. Moreover, the primary task of these new professional police was deterrence. To this end, Metropolitan Police officers were assigned to walk a beat – a regular route – at a pace of 2.5 miles per hour; the beat was intentionally small to increase visibility and the new policemen ‘were supposed to get to know everyone who lived on these beats’.¹⁵ In contrast, the previous ‘police’ were reactionary, and focused on catching criminals, rather than preventing crime (Emsley, 2009). Increased standards and quality may also have increased the effectiveness of the new police. We obtained information on police quality from documents reporting the reason of removal of officers from the force. Panel A of Figure 3 shows the weekly number of leavers among the first recruits (recruited between

¹² While all descriptions of the formation of the Met describe this 7-mile radius, no explicit distance was written in the original act. Rather, there is a list of treated parishes in the appendix of the act; to date, we have not located a complete copy of this list. Our analysis uses the 7-mile radius to define all potentially treated areas, but also breaks this up into a certainly treated inner circle and potentially less intensely treated outer circle.

¹³ Met. Police Act 1839, section II: http://www.legislation.gov.uk/ukpga/1839/47/pdfs/ukpga_18390047_en.pdf. The expanded jurisdiction included all parishes that were partly (entirely) within 12 (15) miles of Charing Cross.

¹⁴ Before 1832, ‘policing’ in City of London was the responsibility of the City’s *Day Patrol* and *Night Patrol*. By 1803, these patrols were 16 men strong and increased to 49 men by 1815. In April 1832, the *City Day Police*, incorporating the previous *Day Patrol* and expanded to 100 men, became fully operational. In November 1838, the *City Day Police* and the *Nightly Watch* (which had replaced the *Night Patrol*), merged into one establishment from which the *City of London Police* was created in August 1839. This information is based on a leaflet, accessed on the London Metropolitan Archives website on May 17, 2018: <https://www.cityoflondon.gov.uk/things-to-do/london-metropolitan-archives/visitor-information/Documents/01-family-history-at-lma.pdf>

¹⁵ While this was possible in the inner divisions in Central London, beats in the outer divisions were often larger and it is plausible that policemen in these divisions were not able to fulfill these tasks (see Emsley, 2009).

September 1829 and March 1831), Panel B the weekly number of removals by broad reason (resignation, dismissal or death) and Panel C the weekly number of dismissals for specific reasons (drunk, neglect or misconduct, criminal behavior or other).¹⁶ These figures demonstrate high turnover of officers especially at the very beginnings of the Met, and that ‘police quality’, in particular alcohol consumption, was taken seriously by the new professional police (one can even observe annual firing spikes for being drunk on duty around Christmas).

Clearly, a relevant question is *why* the Met was created. Was the formation of a professional force a direct response to rising crime? This is indeed possible as the 1829 Metropolitan Police Act itself states:

“[...] offences against property have of late increased in and near the metropolis; and the local establishments of nightly watch and nightly police have been found inadequate to the prevention and detection of crime, by reason of the frequent unfitness of the individuals employed, the insufficiency of their number, the limited sphere of their authority, and their want of connection and co-operation with each other [...].”

But, anecdotal evidence also points towards alternative reasons for forming the Met, including a need for a centralized (non-military) body to maintain order, police provision independent of parish wealth, and a desire for order and tidiness.¹⁷ The first of Sir Robert Peel’s nine Principles of Law Enforcement highlights these alternative reasons: “The basic mission for which police exist is to prevent crime and disorder as an alternative to the repression of crime and disorder by military force and severity of legal punishment.”

2.2. The Roll-out of Professional Police Forces Across England and Wales

Professional forces were subsequently introduced in counties and boroughs throughout England and Wales via three acts: The 1835 Municipal Corporations Act, The County Police Act of 1839 (or 1839 Rural Constabulary Act) and the County and Borough Police Act of 1856.

The 1835 Act required the boroughs (i.e. more urban areas) to appoint both a watch committee and sufficient number of fit men to act as constables, tasked with preserving the peace and preventing crime. There was general resistance, such that only 93 of 171 boroughs even claimed to have established such a force by the end of 1837 (Hart, 1955). Many boroughs admitted to just fulfilling their ‘statutory obligations’ by appointing the same ‘police’ as before (rather than carefully selecting new recruits; Hart, 1955). Given the limited and fuzzy

¹⁶ These figures are based on manually transcribed documents from the National Archives in London that are available after 1833. Panel A is based on the *Register of Recruits into the Metropolitan Police* (MEPO 4/31), Panel B and C on the *Home Office: Police Entry Books* (HO 65/11, 65/12 and 65/13).

¹⁷ See <http://www.open.ac.uk/Arts/history-from-police-archives/Met6Kt/MetHistory/mhFormMetPol.html> (last accessed on May 17, 2018).

implementation of the 1835 Act, we do not attempt to study the effect of borough police forces. Rather, we focus on the rural county forces created by the 1839 and 1856 Acts.

The 1839 Act gave the Quarter Sessions' justices in each county the power to create a police force for all or part of the county *if they chose*. This act also provided guidance regarding the structure of such a force (Stallion and Wall, 1999). Why were the 1835 and 1839 Acts passed? Though some historians argue that they were a response to criminals fleeing already treated areas (London and then the municipalities), Hart (1955) argues that there is no anecdotal or empirical evidence (based on very rough statistics) to support this argument. Rather, she argues that an increased concern about relying on the military and deficiencies in the implementations of earlier acts motivated the 1839, and ultimately, 1856 Acts (Hart, 1956).

The 1856 Act consisted of four main features. First, at the next General or Quarter Sessions after December 1, 1856, a police force had to be established in every borough or county without an existing one. Second, all forces (new and old) had to be 'efficient'. Third, an Inspectorate of Constabulary was created to annually inspect and certify efficiency for all forces, introducing a large measure of centralization to local policing. Fourth, clothing for constables and 25% of wages would be paid by the Treasury upon certification (Hart, 1956).¹⁸

In 1856, three inspection districts – Northern, Midlands, and Southern – were formed, each with an assigned inspector.¹⁹ According to Cowley and Todd (2006), the initial (unofficial) inspections in 1857 found many counties with inefficient or even non-existent forces. The inspectors assessed efficiency according to (i) the size of the force, (ii) the ratio of officers to the population, (iii) the quality of supervision, and (iv) the degree of cooperation with neighboring forces. Stipulated by the 1839 Act, one officer per 1,000 people was taken as the norm by the inspectors (the 1856 Act provided no recommendation). Following unofficial advice given by the inspectors during the preliminary inspections in early 1857, just five districts were declared inefficient in the first official inspection. All but one (Rutland) were declared efficient the following year (Cowley and Todd, 2006).²⁰ Anecdotally, the Inspector's rigid interpretation of a sufficient ratio of police officers per population led to counties complaining about not being certified (Hart, 1956). This discrepancy between local government desires and inspector recommendations is highlighted in an 1883 statement by Sir Vernon Harcourt (Home Secretary from 1880 to 1885) regarding the definition of efficiency: "...the

¹⁸ This increased to 50% of wages after 1874; the government also aided in pension payments after 1890.

¹⁹ The initial inspectors were Woodford for Northern (previously war general and chief constable of Lancashire since 1839), Cartwright for Midlands (no policing but substantial local government experience), and Willis for Southern (chief constable of Manchester city since 1842). See Cowley and Todd (2006).

²⁰ Rutland remained inefficient until the 1861/62 inspection year.

fanciful cast-iron rule of so many [police]men per 1,000 inhabitants. Nothing can be more ridiculous than to apply the same measure to all places alike regardless of circumstances.”

2.3. Other Changes in Victorian England

Criminal justice reforms in the 1800s were of course not isolated to policing. Some of the greatest changes occurred with respect to sanctions, such that the 19th century is characterized by a large decrease in expected punishment. Reforms in the first half of the 1800s gradually abolished capital punishment, which before existed for more than 200 offenses, and replaced it with transportation to Australia. Increasingly perceived as inhumane and not deterrent, the Penal Servitude Acts of 1853 and 1857 replaced transportation for short and long-term sentences, respectively, with penal servitude or imprisonment. Other criminal justice reforms focused on increasing the chance of a fair trial by shifting the burden of proof to the prosecution with the presumption of innocence (1827) and entitling felony defendants to attorneys (1836). Other reforms were procedural. Most relevant for our context is an 1855 Criminal Justice Act that extended powers to judges to deal with certain types of larceny cases summarily, i.e. outside of the courtroom.²¹ This resulted in a national reduction in the number of trials for certain types of property offenses that was, however, not seen for ineligible offenses (e.g. violent offenses). A common feature of all of these criminal justice reforms – one which distinguishes them from the roll-out of mandatory police forces and lends credibility to our causal interpretation – is that they were national and affected all counties at the same time.

More generally, 19th century England was a dynamic period of reform, development and growth. Much can be attributed to the Industrial Revolution, which led to agglomeration, urbanization and population growth. The population of London grew from one to three million in the first 60 years of the 19th century.²² Other population characteristics– many that are commonly associated with crime – were potentially also changing: An increased population share living in urban areas, an increase in population density, an increase in the share of immigrants, and a potentially changing age and gender composition.²³

²¹ Criminal Justice Act, 18 & 19 Vict. c. 126. Specifically, according to the 1856 Judicial Statistics, the 1855 Act extends the right of judges to summarily sentence cases that originated in the Juvenile Offenders Act of 1847, which authorized justices to convict of simple larceny juvenile (younger than 14) offenders. By the “Act of 1855 this power was further extended, with the consent of the accused, to all cases of simple larceny (without distinction of age) where the property stolen does not exceed five shillings in value, to attempts to commit larceny from the person or simple larceny, and to charges to any amount of simple larceny, larceny by servants, and larceny from the person, where the accused, on being asked by the Justices, elects to plead guilty.”

²² See <https://www.oldbaileyonline.org/static/Population-history-of-london.jsp> (accessed on September 14, 2016).

²³ Additional events to keep in mind are the first cholera epidemics; these primarily affected London, however, and did not explicitly coincide with the introduction of the Met (the first was in 1832 and the last in 1866). See Gilbert, Pamela K. “On Cholera in Nineteenth-Century England.” *BRANCH: Britain, Representation and*

3. *The London Metropolitan Police Force (1829) and Crime*

3.1. *London Data Description*

Our London analysis necessitates geocoded historical crime data to identify crimes in the treated and control areas. We use two data sources with respective advantages and limitations.

The first is the *Proceedings of the Old Bailey*. The Old Bailey is the Central Criminal Court of London and the surrounding county of Middlesex, and responsible for all felony trials. The Proceedings were published after each monthly court session and include the records of more than 200,000 trials from 1700 to 1913; these have since been digitized by *The Old Bailey Proceedings Online*. Easily identifiable information includes the case, session date, defendant's name and gender, and detailed offense, verdict (acquit, guilty of original or lesser charge), and sentencing categories (mainly: death, transportation, prison).²⁴ Additional, but less easily identifiable information include the location and the date of the crime as well as the number and type of potential police witnesses. Given the time-consuming nature of recording that additional information and geocoding the address, we focus on the most serious felonies (murder, manslaughter, robbery, burglary), for which we can reasonably assume that their felony classification (and hence their representation at the Old Bailey as opposed to a lesser court) does not change during this period. Focusing on these most serious offenses also limits the potential for a change in crime reporting behavior – i.e. a murder would always be reported.

We have geocoded this data for trials between 1820 and 1850 to identify offenses in the treatment and control areas (within/outside a 7-miles radius from Charing Cross and within/outside the City of London, respectively) before and after the introduction of the Metropolitan Police. To geocode the data, we use the most detailed address available in the Proceedings (e.g. an intersection, parish/district name or end/mid points of a street).²⁵ We map these locations into modern day maps of London to obtain postcodes and geo-coordinates for each location. Further, we have manually recorded the date of the crime (instead of the trial).

Figure 4 shows maps for the period before the introduction of the Met (1820 to

Nineteenth-Century History. Ed. Dino Franco Felluga. Extension of *Romanticism and Victorianism on the Net*. http://www.branchcollective.org/?ps_articles=pamela-k-gilbert-on-cholera-in-nineteenth-century-england (last accessed on February 5, 2018).

²⁴ We have used the Old Bailey data in previous projects looking at (i) the impact of abolishing the death penalty on jury verdicts, (ii) path dependency in jury decisions, and (iii) the gender gap in jury and judge decisions from 1715 to 1900 (see Bindler and Hjalmarsson, forthcoming a, forthcoming b, and 2017).

²⁵ Whenever locations have changed names (e.g. changes in street names), we use historical maps to identify their current address (this is the case for roughly 40% of our regression sample). Further, when the most detailed address is a long street (about 11% of our regression sample), we geocode the endpoint of that street as the relevant crime location (that is, we assign potentially untreated observations to the treatment area). Our results are qualitatively robust to excluding either of those 'fuzzy' locations from our analysis.

September 1829), after the initial introduction but before the expansion of the catchment area (September 1829 until August 1839) and after that expansion (1839-1850). The grey dots represent the location for a defendant-crime observation; those within the City of London are colored in black. The bordered areas indicate modern date postcode areas and the red circles indicate radii of 7 and 15 miles from Charing Cross, respectively.²⁶ Appendix Table A2 provides the number of trials (i.e. crimes) by crime type within a 7-mile radius from Charing Cross, in the City of London, and outside the 7-mile radius for different time windows. Finally, we recorded whether there were any police witnesses at the trial, the type of police for the first five police witnesses, and whether there were any police present at the crime scene. As seen in Appendix Table A2, police witnesses were called constables (a label used before and after the creation of the Met), policeman (a post-Met label), watchman (a pre-Met label) and a handful of other labels that were either predominantly pre or post-Met.²⁷

There are two key advantages of the Old Bailey data. First, using information on police witnesses, we can assess the timing and extent to which the Metropolitan Police Act was implemented in treated versus control areas. Second, we can test for a crime-reducing effect of the Met and demonstrate the robustness of a before-after design to a difference-in-differences specification. The main disadvantage is that it includes only serious felonies that go to trial.

In this respect, our second data source – the *Report or Account of the Proceedings at the several Police Offices* – provides a very good complement. These are reports by the nine police offices that were run by the pre-1829 police and continued through 1839. The reports are publicly available from the London National Archives as pdf documents.²⁸ We manually transcribed the data from January to April of 1828 (the year pre-reform), 1830 (the year post reform) as well as 1831 and 1832. Unfortunately, these daily police reports did not exist before 1828 and those for the second half of 1828 and 1829 are missing.²⁹ For each office and day (except Sundays), a detailed description of ‘charges’, ‘informations’ and ‘property stolen’ are reported. We use these data to create three measures of crime: (i) the daily number of ‘property stolen’ entries, listing the number of property crime incidents, (ii) the daily number of property, violent, and other ‘informations’, containing crime incidents but also general information for

²⁶ Shapefiles for the postcode areas were obtained from Maproom’s UK Postcodes Shapefiles and contain OS, Royal Mail and National Statistics data.

²⁷ Other predominantly pre-Met labels include beadle, conductor, marshalsman, officer, patrol and street keeper. Other predominantly post-Met labels include inspector, sergeant, superintendent, captain and Thames.

²⁸ See Appendix Figure B2 for an example page of data.

²⁹ The files for the second half of 1828 as well as for 1829 have, according to information on the website of the National Archives, been lost. We therefore coded data from the documents corresponding to the months of January until April for the years 1828 (MEPO 4/12), 1830 (MEPO 4/13), 1831 (MEPO 4/15) and 1832 (MEPO 4/17).

the population, e.g. the escape of a prisoner, and (iii) the daily number of charges, which are most comparable to modern day arrest data, by crime category (property, violent, other). The first two measures capture crime incidence, whereas the third incorporates both crime incidence and apprehension by the police.

In contrast to the Old Bailey data, this second source has the advantage of capturing crime incidence (not just trials) and including all crime types, not only (selected) felonies. Moreover, these more minor crimes are more common, increasing precision despite the short time window. Yet, there are two limitations. Since all offices are located within the Met's jurisdiction (see Panel B of Figure 2), we are restricted to a before-after design. Second, we cannot examine pre-trends, as the reports only start one year before the introduction of the Met. We therefore rely on the robustness of the Old Bailey analysis to a difference-in-differences design when making the case for a causal interpretation of these results.

3.2. Analysis of The Old Bailey Proceedings

Evidence of the Introduction of the Metropolitan Police (Old Bailey Data)

We begin by assessing whether there is evidence of the introduction of the Metropolitan Police in the Old Bailey trial reports. That is, do we see an increased number and/or different type of police witnesses at trial after the Met was created? An important caveat is that this analysis conditions on crimes brought to trial: We cannot control for the possibility that the new police affect the number of crimes committed or the likelihood that a case comes to trial. Panel A of Figure 5 plots the annual share of trials that had a police witness of any sort for both the treated area (i.e. within a 7-mile radius but not the City of London) and the potential control area (outside the 7-miles radius or in the City of London). The vertical lines indicate the years 1829 (initial year of the Met formation) and 1839 (expansion to 15 miles). There is no obvious change in the proportion of trials with *any* police witness around these reforms. Panel B demonstrates, however, a clear shift in the type of police. The share of trials with an 'old' labelled police witness (watchman or other) drops sharply with the reform from about 70% to 20% while the share with a 'new' label (policeman or other) increased from close to 0% to almost 50%.³⁰

Table 1 looks at this 'first-stage' more formally by estimating simple pre-post designs for each potential treatment and control area (Panel A) as well as difference-in-differences specifications (Panel B). Two estimation windows are used throughout our Old Bailey analysis.

³⁰ The measure of the type of the police witness refers to whether any of the first five police witnesses is of the respective type. Note that less than 1% of trials in our regression sample have more than five police witnesses. The presence of constables, a label that is not distinctively pre- or post-Met, is excluded from this figure.

The first and larger window (1820-1839) allows for the possibility of controlling for pre-reform trends and lagged implementation effects while the second and shorter window (1828-1832) reduces the possibility of confounders and mimics the estimation window of our second data source (the daily police reports). Given the rarity of burglary, robbery and murder, an advantage of the larger window is that it increases sample size and precision. We divide the potential treatment area into two areas – within 4 miles and 4 to 7 miles from Charing Cross – to allow for a potentially more intense treatment in the inner divisions (as highlighted in Section 2.1). The two control areas include (i) offenses outside the 7-mile area and (ii) the City of London.³¹

The pre-post estimations are simple regressions of each measure of police presence at crime trial i for offense o in area a at date t on a dummy indicating whether the offense occurred after the introduction of the Metropolitan Police ($PostMet$). Offense type dummies are included to allow for differential police involvement across offenses. The difference-in-differences specification is presented in equation (1) below, where the main coefficients of interest are β_1 and β_2 . The former captures the effect of the Met on the main treatment area (within 4 miles) while the latter captures the effect of the Met on the area within the 4-7 miles radius (with uncertain treatment intensity). Year, offense, and area fixed effects are included.

$$(1) \quad Police_{ioat} = \beta_1(Treat * PostMet)_{iat} + \beta_2(Uncertain * PostMet)_{iat} + \alpha_y + \alpha_o + \alpha_a + \varepsilon_{ioat}$$

As seen in Table 1, and consistent with the descriptive figures, there is little evidence that the creation of the Met increased the presence of *any* police at a trial. But, it did significantly change the *type* of police witness: The pre-post specifications (Panel A) show that the likelihood of a trial having a ‘new’ police witness increased by 59 and 45 percentage points in the 4 miles and 4-7 miles radius areas, respectively (using the 1820-39 estimation window in column (3)). In contrast, the presence of ‘old’ police decreased by 49 and 25 percentage points in the respective areas. Thus, the pre-post analysis confirms that there was a treatment, and indeed suggests that it might have been stronger in the inner (4-miles) circle.

The pre-post specification for the control area (more than 7-miles radius) indicates that there was some increase (19 percentage points) in ‘new’ and no change in ‘old’ police. For this area to be a perfectly neat control group, we would ideally find a zero estimate on the presence of new police, as we do for old police. The point estimate for new police is, however, much

³¹ There was little change in the City of London police until April 1832, at which point the City Day Police became fully operational. See Section 2.1 for details.

smaller than for either of the treatment areas. It could arise for a number of reasons: (i) the 7-miles radius is not a perfect boundary and some Met police actually patrol this area,³² (ii) the term ‘police’ is increasingly used at the Old Bailey, regardless of the actual type, (iii) some crimes committed outside the 7-miles radius led to arrests within the seven miles, and (iv) measurement error in our geocoding. Not all of these reasons represent actual spillover effects of the treatment to the control group. But, even if they did and the control group was partially treated, this would lead to an *underestimation* of the treatment effect in the difference-in-differences specification. The above explanations could similarly explain the significant (but smaller) increases in the new and decreases in the old police for the City of London area. These are found even for the smaller window (1828-1832) during which the City is untreated for almost the whole period. While our baseline includes the City as a control group, we conduct robustness checks to this definition.

The difference-in-differences results presented in Panel B of Table 1 show an approximately 25 percentage point increase and 29 percentage point decrease in the likelihood of a new and old type police, respectively, being present as a witness in the 4-mile radius. There is no significant effect for the 4-7 mile area of uncertain treatment intensity.³³

Before turning to the reduced form (crime) results, we examine one more aspect in which the creation of the Met may have affected policing. As the Met officers were constantly walking a short beat, it seems plausible that they become more likely to be present at the crime scene itself, either by witnessing the crime or being close enough to be called upon for assistance, i.e. there could have been a shorter response time. This may depend on the type of crime and be especially relevant for street crimes. Panel A of Figure 6 presents maps of London by modern-day post code areas for 1820-1829, 1829-1839, and 1839-1850. These maps provide suggestive evidence that this may have occurred; darker shaded areas correspond to a higher share of trials with police present at the crime scene. Columns (7) and (8) of Table 1 look at this in the formal regression framework: There is a significant 11 percentage point increase in police presence at a crime scene in the 4-mile radius for the larger sample period. But, it is not seen immediately (in the short window) and is not robust to the difference-in-differences specification.

³² Indeed, the original Act does not actually say a 7-mile radius but provides a list of parishes than can be included. We currently do not have access to this full list.

³³ These estimates are robust (and available upon request) to including annual area-specific time trends, month fixed effects to control for seasonality, (modern-day) postcode area fixed effects to control for unobserved heterogeneity across London, and excluding ‘fuzzy’ locations from the analysis (see Section 3.1). Qualitatively similar results are found by offense (but lack statistical power for homicide). The same pattern is also seen when including the City of London in the treatment group after April 1832 or in the uncertain treatment area.

Main Empirical Specification and Results (Old Bailey Data)

Having established that the creation of the Met did affect ‘policing’ in London, we turn to the question of whether it affected crime. This section estimates the reduced form effect of the formation of the Met on burglary, robbery and homicides. Panel B of Figure 6 maps the total number of trials in each 10-year time period by post code area, where darker shaded areas correspond to more offenses. From 1820-29 to 1830-39, there is a decrease in the number of crimes in the areas overlapping the treatment area (while there is an increase from 1839 to 1850). Panel A of Figure 7 plots the annual number of trials for the treated (inside the 7-miles radius) and control (outside the 7-miles radius/within the City of London) areas. A drop in crime is seen in the treatment area around 1830, with increases beginning in the mid-1830s. A similar pattern is seen for the control area, but - as the figure also highlights - there are many more crimes in the treatment than control area. Panel B therefore plots the annual percentage change in the number of trials for both areas: There is some fluctuation for both groups, but importantly trends appear to follow the same pattern in the treatment as in the control area before the introduction of the Met. This is reassuring with respect to the usual parallel trends assumption.

To study the effect of the introduction of the Met on crime, we have to temporally and geographically aggregate the data. In our baseline, we do so at the month by area level: treated (less than 4 miles from Charing Cross), uncertain (4 to 7 miles from Charing Cross) and control area (more than 7 miles from Charing Cross, as well as the City of London). Table 2 begins with a simple pre-post comparison of the average number of crimes before and after the introduction of the Met, for all crimes and separately by crime type (burglary, robbery and homicide). Panels A and B show means for the 1820-1839 and 1828-1832 windows, respectively.³⁴ There is a significant reduction in the average number of monthly crimes in the treated area overall and for burglary and robbery; the average number of total monthly crimes is reduced by 37% from 6.46 to 4.10 in the larger time window. A significant reduction of a similar magnitude (40%) is seen in the shorter estimation window (1828-1832). In contrast, there is little evidence of a decrease in crime for the (less intensively treated) uncertainty area. We do not see any significant change in total crime for the control area (though there is a reduction in burglary, offset by an increase in robbery). For the City of London, there is a significant but smaller reduction in crime (22%) when looking at the larger estimation window which virtually disappears when narrowing the window in Panel B (hence excluding the time period after 1832 when the City of London may have been partially treated).

³⁴ Significance levels are based on simple pre-post regressions; the results are robust to including month dummies.

To make the case that these post-Met reductions in crime in the treated area have a causal interpretation, we turn to difference-in-differences estimations using the area outside the 7-miles radius and the City of London as the best possible control groups. We again split the potentially treated areas into two groups: a (certainly) treated area within a 4-miles radius and an uncertainty area 4 to 7 miles from Charing Cross. Given the higher treatment intensity in the inner circle and the suggestive evidence from the difference-in-means comparison above, we believe this is the best suited specification.

The outcome variable is the number of trials overall and for offense o in area a during time period t . The baseline analysis aggregates the data at the month (t) and area (a) level, using the four previously defined areas (treatment, uncertain, control and City of London). We later conduct robustness tests to alternative aggregation levels (weeks and circles around Charing Cross). Equation (2) below presents the difference-in-differences specification, including year, month and area fixed effects. The main coefficients of interest are γ_1 and γ_2 ; these capture the effect of the Met on the main treatment area as well as the area of uncertain treatment intensity.

$$(2) \quad Trials_{at}^o = \gamma_1(Treat * PostMet)_{at} + \gamma_2(Uncertain * PostMet)_{at} + \alpha_y + \alpha_m + \alpha_a + \varepsilon_{at}$$

Intuitively, we estimate the change in crime in the treated areas before and after the introduction of the Met compared to the change in crime in the control areas. Compared to the simple pre-post analyses, this allows us to account for general trends in crime that would have occurred independently of the reform. For this to be the case, the usual parallel trend assumption must hold and we must assume that during the estimation window nothing else changed in the treatment but not in the control group (or vice versa) that could have affected crime rates.

Figure 7 gives some indication that pre-trends in the number of trials were relatively parallel in the treatment compared to the control areas. Yet, this comes with the caveat that the number of trials is generally much lower in the control areas which makes a visual inspection more suggestive. We more formally test for pre-reform differences between the treatment and control areas when we move from the difference-in-differences to an event-study design.

Are there potential confounders? We discuss five potential concerns. One obvious candidate is the *City Day Police* which became operational in the City of London in April 1832 (see above). It is possible that the City Day Police introduced a similar treatment to the City of London as the introduction of the Met did to the treatment area. Thus, part of our control group (City of London) was partially treated in 1832 which (if anything) leads to a downwards bias in the estimated treatment effect. Nonetheless, we show that our results are robust to re-

allocating the City of London to the treatment group after April 1832 or the uncertainty group, respectively. A second potential confounder is the first cholera epidemic of 1832, which could have differentially affected regions (though we do not have evidence of this) – both in terms of police and potential criminals. The smaller estimation window mostly avoids this concern, however. Third, other (potentially relevant) criminal justice changes during this period include the abolition of capital punishment for burglary and robbery in 1837; however these would be relevant for both treated and control areas and not a concern in the shorter time window.

A fourth potential concern is whether there were spill-over effects from the treatment to the control areas. There are two potential types – spill-overs of policing and displacement of crime. Our discussion of police witnesses (above) already raised the possibility of the former, i.e. that some of the control area was policed by the Met. Recall, however, that the reported change in the control area was much smaller than in the treatment area and that we cannot rule out that it arises due to measurement error or simply a change in terminology. Either way, if there is a spill-over of policing to the control area, this would attenuate our estimates of a crime reducing effect of police. On the other hand, if there is displacement of crime, i.e. if criminals chose to commit crime in less policed areas than the newly treated Metropolitan Police jurisdiction, then this would bias the difference-in-differences estimates in the direction of a crime reducing effect. However, the pre-post estimations do not suggest any significant change in crime in the control area. Moreover, it is important to keep in mind the historical context – criminals would likely be travelling on foot.³⁵ In that context, the control area with a radius of 7 to 15 miles (about 11 to 24 kilometers) from Charing Cross is not insignificant in size.

Finally, the sample period is characterized by dynamic population growth. Could this bias our estimates, in particular given that we use crime levels rather than rates? Population growth implies, if anything, more potential criminals and an increase in crime. Thus, if population grows in the treated areas, this would counteract a crime-reducing effect in the pre-post analysis. In the difference-in-differences, the associated bias depends on how population growth compares in the treatment and control areas. If it is comparable, then the pre-post bias is in fact eliminated. In contrast, if the population grew faster (slower) in the control areas, this would bias us towards (against) finding a crime-reducing effect of the Met. Unfortunately, we cannot directly measure population growth within the various treatment and control areas. However,

³⁵ Horse drawn stage coaches could be hired, and starting in 1829, the first ‘omnibuses’ were introduced in central London (horse-drawn buses), but these alternatives were expensive. See the Old Bailey Online, last accessed June 19, 2018: <https://www.oldbaileyonline.org/static/Transport.jsp>.

we do not believe this to be a substantive concern given that our main analysis is conducted within a narrow time window before and after 1829, thereby mitigating such concerns.

The results from the difference-in-differences estimation are shown in Table 3. Columns (1) to (3) correspond to the baseline specification described above; Panel A shows the results for total crime, and Panels B to D separately for burglary, robbery and homicide. Using the larger 1820-1839 window in column (1), we find that the introduction of the Met leads to highly significant decreases in trials in the treatment relative to the control area for total crime as well as for burglaries and robberies, but not homicides. The baseline effects are sizeable: Relative to the average number of pre-Met crimes in the treatment group, the point estimates translate into a reduction in total crime by about 34% (33% for burglaries and 46% for robberies). Though at least partially treated, we do not find any effects of the Met on crime in the uncertainty relative to the control area; this could imply that there was no change in crime levels in the uncertainty area or that the crime reduction effect was offset by increased apprehensions. One possible explanation is that there is a smaller deterrence effect, because police were less visible as they walked potentially larger beats. Another possibility is that any deterrence effects are offset by a spill-over of criminals from the inner (more intensively patrolled) circle to the outer circle. That is, the crime displacement discussed above may have happened to the 4-7 mile circle, and not to the control group.

Focusing on the inner 4-mile radius, we note that the difference-in-differences estimates are close to the simple pre-post comparison of means (37% for total crime, see above). Further, moving to a narrower estimation window (allowing for fewer potential confounders), the difference-in-differences specification yields similarly sized effects. Relative to the pre-Met mean in the treatment group, the results in column (3) of Table 3 translate into a 34% decrease in total crime (36% for burglaries and 46% for robberies). Finally, columns (4) to (6) and (7) to (9) of Table 3 show the results when the City of London is alternatively assigned to the treatment and uncertainty groups, respectively, after the introduction of the City Day Police in April 1832. Unsurprisingly (as the treatment is distorted), the former attenuates the point estimates but yields the same pattern as the baseline while the latter results in point estimates only marginally different from the baseline. That is, our main finding of the Met leading to overall significant and sizeable reductions in crime (trials) is robust to alternative estimation strategies and varying estimation windows.

Figure 8 shows the results from event study estimations for total crime (Panel A) and by crime type (Panels B to D), where we estimate a more flexible specification by interacting the treatment indicator with dummies for two-year intervals before and after the introduction of the

Met.³⁶ To account for the mid-year timing of the introduction of the Met, we define a year from September to August (instead of January to December). The purpose of these specifications is twofold – to use the leads to test for the parallel trend assumption and to study the dynamic effects of creating the Met. Were the effects immediate, and did they change over time (officer quality increased with both experience on the beat and in recruiting)? The results are indicative of parallel trends for robbery and homicide: The coefficients are not significantly different from zero in the years leading up to the reform. The results for burglary, however, suggest that there were increasing burglary rates in the treated relative to the control area; i.e. parallel trends are not satisfied. We therefore focus on the findings for robbery and homicide. For homicide, as in the baseline, we see no effect of the creation of the Met, in the short or long-term. For robbery, the effect is immediate and persistent.

Table 4 presents robustness checks to the level of temporal and geographic aggregation. Columns (1) to (3) aggregate the data to the week by area level (i.e. a smaller temporal period) while columns (4) to (6) consider the month by 1-mile distance band level (i.e. smaller geographic areas). Since crime is a rarer event in these smaller units, we adopt an extensive margin measure of crime (*any crime*) for this table. We generally see the same pattern of results. Using the largest time window (1820-1839), the introduction of the Met led to a 12 percentage point reduction in the chance of any crime (murder, robbery, burglary) in a given week and area (column (1)), with similar point estimates for both robbery and burglary. Similar estimates are found when using finer geographic areas (column (4)). These results are robust, and if anything even larger, in a smaller window from 1825-1835. When looking in the 1828-1832 range, however, we see a loss of precision for burglary, but a robust effect for robbery.

Finally, Appendix Table A3 demonstrates the robustness of the baseline results to a series of sensitivity checks, including: (i) baseline area specific time trends, (ii) excluding crimes reported to be ‘somewhere’ on a long street, which could lead to crimes being miss-classified as treated offenses given our geocoding strategy, (iii) including only crimes for which we could identify the coordinates without having to refer to historical maps, and (iv) excluding offenses with missing crime dates (rather than instead assigning trial dates, as in the baseline).³⁷

3.3. Analysis of The Daily Police Reports

Summary Statistics (Daily Police Reports)

³⁶ We similarly interact (but do not show) the indicator for the uncertainty area with these two-year dummies.

³⁷ The results are also generally robust to alternative functional forms, such as Poisson.

The second part of the London analysis uses a simple pre-post design to analyze the daily crime reports described in Section 3.1. The raw data include nine police offices. Though the Thames River Police were not officially included in the jurisdiction of the Met, we do not believe that it is a suitable control group due to potential spill-over effects: The jurisdiction of the Thames River Police (i.e. the Thames River) is bordered on both sides by the Met catchment area. We thus omit the Thames Police Office from our sample.³⁸ Table 5 presents summary statistics for the remaining eight offices for the entire period, the pre-reform period (1828), a one-year post period (1830) and a three-year post period (1830-1832). For the entire sample period, there are on average 0.5 informations, 6.4 charges and 0.4 reports of stolen property per day and station. The largest share of informations and charges is for property crimes, followed by violent and other crime. ‘Other’ informations include non-crime incidents such as escaped prisoners or lost and found reports, while the property and violent categories refer to actual crimes. Looking across years, the number (and chance) of informations and stolen property reports is higher in 1828 than in 1830, while for charges there appears to be a decrease for violent but not for property crime.

Figure 9 illustrates these patterns: Panels A and B show the number of informations and charges, respectively, by crime category, while Panel C shows the number of stolen property reports. For these figures, the data are aggregated to the weekly level – Monday to Saturday. The patterns seen in the figures parallel the summary statistics: There is an overall decrease in informations and stolen property reports and an increase in charges after the introduction of the Met. The figures do not suggest that this is purely due to crime trends over time: Comparing the years after the reform (1830-1832), we do not see continued decreases or increases.³⁹

Main Empirical Specification and Results (Daily Police Reports)

Equation (3) presents the baseline pre-post specification used to estimate the effect of the introduction of the Metropolitan Police on daily crime reported to the different police offices:

$$(3) \quad Y_{ywdi} = \beta PostMet_{ywd} + \alpha_w + \alpha_d + \alpha_i + \varepsilon_{ywdi}$$

The dependent variable, Y , is the daily measure of crime in year y , calendar week w and day of the week d as reported by office i . Our main variable of interest, $PostMet$, equals one in the

³⁸ We also exclude the “Metropolitan Police Office” as this office was created in 1832.

³⁹ To underline that this is actually the case, Appendix Figure A1 shows the number of property stolen incidents separately by office. Again, these figures do not suggest general crime trends.

years following the introduction of the Metropolitan Police (i.e. 1830 to 1832) and zero in the year before (i.e. 1828). Our baseline specification includes police office fixed effects to control for unobserved heterogeneity across different areas in London as well as fixed effects for calendar weeks and day of the week to control for seasonal patterns and variation in crime rates over the days of a week. Given that the lack of a suitable control group necessitates a pre-post design, one may remain concerned about confounding factors, i.e. other things changing at the same time. To alleviate such concerns, we limit the sample period to the year before and after the reform for large parts of this analysis. A second concern is that having only one pre-period of data (January to April of 1828) limits our ability to say anything about pre-existing trends in crime. But, one argument made for the new police was rising crime rates – it would therefore be hard to imagine deterrence being confounded by a downward trend in crime. Moreover, the above analysis of the Old Bailey data suggests that the results are robust to the smaller time window and both pre-post and difference-in-differences designs.

Table 6 presents the results of estimating equation (3) – the baseline pre-post specification using the high-frequency daily crime reports – for each outcome: any informations (Panel A), number of informations (Panel B), any stolen property reports (Panel C), and number of charges (Panel D). Column (1) shows the raw pre-post difference when the sample is restricted to one year before and after the reform only (i.e. 1828 and 1830) including all crime categories. There is a significant reduction in the likelihood of observing *any* informations by 15 percentage points (32% relative to the 1828 mean), the *number* of informations by 0.302 (38%), and the likelihood of any stolen property incidents by 9.8 percentage points (25%). In contrast, there is an increase in the total number of charges by 0.88 (16.6%). We build up to the baseline specification by adding police office fixed effects in column (2), calendar week fixed effects in column (3), and day of the week fixed effects in column (4).⁴⁰ Column (5) includes the daily reports for January to April of two additional post-reform years (1831 and 1832). For all outcomes, the magnitudes of the point estimates increase while the sign and precision remain the same. We will discuss possible reasons for this pattern shortly (in Table 7).

Columns (6) to (8) of Table 6 look separately at property, violent and other crimes for both informations and charges. For informations, we see negative point estimates for all three crime categories, with a reduction of any property and violent informations of 24% and 57%,

⁴⁰ Appendix Table A4 presents a number of robustness checks, including estimates: (i) at the weekly instead of the daily level, (ii) excluding incomplete weeks of data, as occur at the beginning of each year or in weeks with holidays, (iii) excluding one office at a time to rule out that our results are driven by one particular office, and (iv) based on alternative specifications, including logarithms of the dependent variable (where appropriate).

respectively. For charges, there is a more heterogeneous pattern: property crime charges increase by about 21% while violent crime charges decrease by about 26%.

To interpret these results, one must keep in mind the differences between the crime outcomes: Both informations and property stolen are proxies for criminal incidents, comparable to modern day offense data. For both outcomes, we find significant decreases across crime categories that can be interpreted as a crime reducing effect of the Met – either through deterrence and/or incapacitation. In contrast, the effect of the Met on our third outcome, charges, has to be interpreted as the sum of such a crime reducing effect and an increase in apprehensions and/or crime clearances. Finding a positive effect on charges for property crime and a negative effect for violent crime suggests that the apprehension effect dominates deterrence/incapacitation for property but not for violent crime. Why? One reason is that the physical presence of the Met officers walking the streets may have allowed them to apprehend many property offenders, such as shoplifters or pick pocketers, as crimes were being committed. Lastly, we interpret the reduction in crime *incidents* for property crime as evidence of a crime reducing effect. Of course, this could also reflect substitution from uncleared to cleared crimes (consistent with the increase in charges). Yet, seeing evidence for the reduction of violent crimes (for which we see a decrease both in incidents and charges), suggests that at least some of the reduction in property incidents is driven by a true reduction in criminal behavior.

Extensions: Short and Medium Term Dynamics (Daily Police Reports)

This section aims to better understand the dynamic effects of creating the Met. As described in Section 2.1, there were two initial hiring waves, the inner divisions in September 1829 and the outer divisions in February 1830. There is not, however, a one-to-one mapping of pre-existing police offices to the new Met police divisions. Rather, as seen in Appendix Table A1, the catchment area of some offices correspond almost completely to early hiring inner divisions, others just to later hiring outer divisions, and others to a mix of early and late hiring divisions.⁴¹

We thus take advantage of this two-stage initial hiring and estimate a specification that allows for different coefficients on the treatment variable in (i) January 1830 (after the

⁴¹ In particular, the 1832 Daily Crime Reports (MEPO 4/17) include hand written letters next to (almost) each entry that correspond to the (Met) police division. We use that information to match the pre-Met police offices to police divisions based on the share of crimes observed in each office/division. For Queen Square, Marylebone, Marlborough Street, Bow Street and Union Hall, we are able to match each office to the corresponding divisions. For Hatton Garden, Lambeth Street, Worship Street and Thames Office, we cannot uniquely match but instead aggregate these offices and all corresponding divisions to match at that aggregated level. Using that matching between offices and divisions, we tabulate the number of police officers hired before and after 01 February 1830 (using data from MEPO 4/31). The result is shown in Appendix Table A1 and illustrates that there is heterogeneity in the timing of Metropolitan Police hiring across the police offices in our sample.

introduction of the Met and before the second hiring wave), (ii) all other months in 1830 (after the second hiring wave), (iii) 1831 and (iv) 1832. That is, we estimate the baseline specification presented in equation (3), but decompose the treatment into multiple time periods. We can thus study the immediate effect of a large hiring wave in February 1830 (and thereby implicitly allow for heterogeneous effects of the two hiring stages) and whether the impact of the formation of the Met changes over time. Table 7 shows the results for the number of charges in columns (1) to (3), any informations in columns (4) to (6), and stolen property incidents in column (7). There are two key takeaways. First, the point estimates generally increase over time. In light of our discussion of the increasing quality of the police after the initial introduction of the Met, and the continued hiring, this may not be too surprising. Second, while some of the crime reduction effect is immediate (for informations and stolen property), the dominating apprehension effect does not kick in until the second wave. This may mean two things: (i) Visible police (even if low quality) may deter crime even if they do not increase clearance rates, and (ii) pre-existing offices in the areas with initial hiring may have been better at clearances than those more affected by the second hiring wave (i.e. starting from different base levels).

3.4. Summary and Discussion of the London Metropolitan Police Findings

What are the key takeaways of the above analyses of the impact of the London Metropolitan Police on crime? First, we find clear evidence of the immediate implementation of the Metropolitan Police Act in Old Bailey police witness testimony, with the greatest ‘treatment’ seen in the more intensively patrolled 4-mile radius around Charing Cross. Second, using the same Old Bailey data, we find that the introduction of the Met in 1829 significantly reduced trials (homicides, robberies, and burglaries combined) by about 34%, driven by burglary and robbery. The event study analysis suggests that for robbery these results are most persistent and robust to the identification assumptions. Given that the outcome measure is trials (as opposed to crime incidents), this suggests that the crime reducing effects of deterrence and/or incapacitation dominate any apprehension and reporting effects for robbery. Third, our pre-post analysis of the daily police report data found evidence consistent with both reduced criminal activity and increased apprehensions for all crimes (violent, property, and other). However, we find that that the former dominates for violent crimes (here, the coefficient is negative for both the incident related outcomes and charges) while the results are consistent with apprehension/reporting effects dominating for property crimes (here, there is an increase in charges but a reduction in incidents). In summary, a dominating crime-reducing effect for violent crimes (including robbery) is seen in both London analyses.

Are these crime-reducing effects attributable to deterrence or incapacitation? While we are not able to empirically disentangle these channels, one should recall that the goal of the new police force was deterrence - it is hard to imagine that it did not play at least some role.

4. The County Police Forces (1839-1856) and Crime

4.1. County Data

Our evaluation of the roll-out of county police forces uses manually transcribed archival records to measure police force creation and crime. We first collected information concerning the year of force formation as well as its initial size from a book by the Police History Society (Stallion and Wall, 1999). After the mandatory creation of police forces in 1856, there is systematic annual data in the (yearly) *Judicial Statistics* on the number and type of police officers for each police force. Appendix Table A5 lists the dates of police force creation and initial force size for each county. Figure 10 illustrates the year of force formation with a map of all counties in England and Wales.⁴² What is immediately apparent is that (i) there is no obvious clustering in the years of force creation by neighboring counties and (ii) the earliest reformers are not just those closest to London/Middlesex. Figure 11 demonstrates the evolution of the number of county forces in England and Wales over time: 16 counties created police forces in 1840 (immediately after permission was granted), 2 in 1841, 1 each in 1843, 1844, 1848, 1851 and 1852, 2 in 1856, and 23 in 1857 (when mandatory).⁴³

Crime data is limited in its availability before the 1856 Act. Before 1856, the only systematic measure of crime that we can collect from the *Judicial Statistics* is the annual number of persons committed or bailed for *trial*; see Appendix Figure B2 for a sample page of data (in a single county and year).⁴⁴ This measure is available for both the entire sample period and the six main crime categories: class 1 (offenses against persons), class 2 (offenses against property with violence), class 3 (offenses against property without violence), class 4 (malicious offenses against property), class 5 (forgery), and class 6 (other). We combine these crimes into three broad categories: violent (crimes against person and violent property offenses), property (non-violent property), and other (malicious property, forgery and other). The specific offenses included in each category are listed in Appendix Table A6.

⁴² The map's boundary data are based on the 1851 registration districts of England and Wales and was downloaded from https://vision.port.ac.uk/downloads/download_free/boundaries.jsp.

⁴³ As in the main analysis, these figures exclude York, Sussex, Suffolk and Middlesex counties because forces were created for sub-county level areas in different years and crime data are only available for the whole county.

⁴⁴ In 1834, there was a change in the table title from the number of persons committed to the number of persons committed or bailed. We therefore demonstrate the robustness of our results to beginning the sample in 1835.

A potential disadvantage of using trials to measure crime is that it may confound changes in prosecution behavior (in which the police played a potentially significant role at the time) with changes in criminal behavior. However, Appendix Figure A2 demonstrates that all three measures of crime available in the *Judicial Statistics* after 1857 – i.e. trials (our measure of crime), total number of indictable crimes committed and the total number of individuals apprehended for indictable offenses – move in lock-step until the early 1890s. Another potential concern is the impact of the Criminal Justice Act of 1855, which gave judges the ability to summarily deal with larceny cases, on the number of trials. This is reflected in the large decrease in the number of trials, specifically property offenses, in the year before the mandatory creation of the police forces (see Panel A of Figure 12). Given that the 1855 Act is a national shock (comparable figures for each county are available upon request), our difference-in-differences design should mitigate concern about this potential issue. Moreover, we estimate the effect of creating a force for two categories (violent and other) unaffected by that reform and for the early reforming counties using a sample period completely prior to the 1855 Act.

Finally, we use available census records from 1851 and 1861 to generate relevant control variables at the county level: the share male, married, native, in various age groups, unemployed or out of the labor force, and farmers.⁴⁵ We have coded the annual county population from the *Judicial Statistics* after 1857, and use the 1851 and 1841 censuses to estimate the population in earlier years.⁴⁶ We use this population variable to create crime *rates*, but our preferred crime measure is the *number* of crimes to avoid the measurement error associated with the population variable. Yet, we demonstrate the robustness of our results to both measures of the outcome.

4.2. Sample Creation, Treatment Definition and Summary Statistics

We use a difference-in-differences design to identify the extensive margin effect of creating rural county police forces on crime. We restrict our sample to rural county jurisdictions for which we can both cleanly identify the year of force creation and measure crime. The raw data includes 52 counties. We drop Middlesex since we cannot disentangle it from London in the crime data, and at least part of Middlesex was already treated by the Met. We also drop three

⁴⁵ We obtained the census data from *North Atlantic Population Project, UK Censuses*.

<https://www.nappdata.org/napp/>

⁴⁶ For 1851 to 1857, we assign counties the population reported in the 1857 *Judicial Statistics* (which in turn were based on the 1851 Census), and we use the 1841 Census for the years before 1851. Note that the county population in the Census includes the entire county population whereas the county population in the *Judicial Statistics* includes the rural areas of the counties only (i.e. the catchment area of the county police force). To use consistent measures of population, we thus weight the 1841 Census measures with the share of the rural population as in the 1851 Census (i.e. the ratio of the county population in the *Judicial Statistics* compared to the 1851 Census).

counties (York, Sussex, and Suffolk) that represent aggregates of regions with initially separate forces (but with crime data only available at the aggregate level).⁴⁷ Appendix Table A5 lists each of the 48 counties included in the analysis.

We then create the main treatment variable: Did county c have a professional force in year t ? We define year t to be fiscal year t ending on September 29 of that year, as this is how the crime data is reported in the *Judicial Statistics*. Specifically, we identify whether a county had an existing police force for *any* or *all* of the fiscal year; in the former, the first treated year is typically only partially treated whereas in the latter, the first treated year is fully treated. The above-described treatment only captures whether there existed any professional county police force, but nothing about the quality of the force. One important measure of quality is the ‘efficiency’, i.e. the number of people per officer in the county. We can measure this upon force formation, which we will use to characterize the ‘efficiency’ of the new force. Appendix Table A5 lists the initial size and the calendar and fiscal start years of each force.

Finally, our baseline analysis uses a sample window of eight years before and after the earliest and latest reform years, respectively, i.e. 1832 to 1865. We chose eight years such that the earliest treatment year was sufficiently long after the creation of the Metropolitan Police, but will conduct sensitivity checks with respect to the start and end years of the sample.

Table 8 presents summary statistics for all analysis sample counties (N=48) and for those characterized as early (1839 or 1840), mid, and late reformers (after the 1856 Act was passed). The average number of charges per year (for all counties over the entire time period) is 367, which corresponds to 1.79 charges per 1,000 population (1.3 property, 0.3 violent, and 0.1 other, respectively). 75% of the counties are in England and the average county population was close to 200,000 in 1858. It is also clear that the police forces became more efficient over time: the ratio of people to police averaged 2,857 at the time of force formation but was down to 1,700 by 1858. In terms of characterizing early, mid and late reformers, Table 8 shows that early reformers were on average largest in terms of population and acreage, while the mid-reformers were smallest in both of these measures. In addition, the earliest reformers did *not* have the highest crime rate (based on the whole time period): the average crime rate per 1,000 population was 1.9 for early reformers, 2.5 for mid-reformers, and 1.5 for late reformers.

4.3. Empirical Approach: County Police Force Formation

⁴⁷ Forces were created in East and West Sussex in 1840 and 1857; near the end of 1856 and beginning of 1857 for the York sub-parts and 1840 and 1845 for the Suffolk sub-parts.

To identify the causal effect of the formation of a county police force on crime, we estimate the difference-in-differences specification presented in equation (4):

$$(4) \quad \textit{Crime}_{ct} = \beta \textit{Force}_{ct} + \alpha_c + \alpha_t + X_{ct}\theta + \varepsilon_{ct}$$

The dependent variable, *Crime*, is the number of persons committed to trial in county *c* and fiscal year *t*. We look at both the log number of annual county trials and log number of trials per capita, for all crimes and by broad crime category. Because of measurement error in the population variables, our preferred measure is the *number* of trials. The primary variable of interest, *Force*, is an indicator equal to one for county-year combinations for which the county had a professional force for any or all of the fiscal year. The baseline specification includes county (α_c) and year (α_t) fixed effects. The former controls for unobservable but constant differences across counties, including pre-existing crime levels which may be related to the decision not to create a professional force until it was mandated. The year fixed effects capture national shocks that impact all counties, such as other criminal justice reforms (e.g. offense specific abolition of capital punishment, summary judgements for property crimes in 1855, or the 1850s abolition of transportation). Standard errors are clustered at the county level.

For β to represent the causal effect of the creation of a professional force on crime, we make the usual parallel trends assumption – i.e. the change in crime (trial) rates in treated counties would have been the same as in control counties in the absence of the reform. Panel B of Figure 12 visually demonstrates the plausibility of this assumption by presenting the average annual log charges separately for the early, mid and late reformers. Crime rates are remarkably parallel for these three groups. We more formally test the parallel trends assumption in an event study analysis that allows for differential effects in the years leading up to the treatment. Another identifying assumption is that the timing of police force formation is random. Anecdotally, this seems reasonable, at least for the earliest and latest reformers. The earliest reformers created a force immediately after the passage of the 1839 Act, but they did not lobby for this Act and did not know that it was coming. The latest reformers only created a force when they had to – after the 1856 Act; again, (to the best of our knowledge) they did not know it was coming. We test this assumption in Section 4.5.

In analyzing the formation of county police forces, the same potential confounders of increased reporting and/or clearance rates exist as in London. Our outcome measure of trials only allows us to estimate the combined effect of deterrence and incapacitation with these confounders. In contrast to the London analysis, we do not have crime incident data at the

county level. Thus, we can only detect a deterrence and/or incapacitation effect if it is larger than these offsetting channels: a null or increasing effect of police on charges does not rule out the existence of such a crime reducing effect, but does not allow us to identify it.

4.4. The Effect of County Police Force Formation on Crime: Results and Robustness

Table 9 presents the results of estimating the baseline specification for 1832 to 1865. The dependent variable is the log number of trials in columns (1) and (2) and the log number of trials per capita (crime rate) in columns (3) and (4). Panel A considers all charges while panels B to D consider violent, property, and other charges, respectively. The variable of interest, *Force*, is equal to one in any county-year combination in which there exists a police force for at least part of the year (columns (1) and (3)) or all of the year (columns (2) and (4)), and equal to zero otherwise. The first takeaway from Table 9 is that the creation of a police force, on average, does not have a significant effect on either overall crime or violent or property crime. Second, the creation of a force appears to reduce ‘other’ crimes by 10 to 17 percent. Third, the estimates are comparable when using the log number of crimes versus the log crime rate; for the remainder of the analysis, we emphasize the log number of crimes given the measurement error concerns in the denominator of the crime rate. Fourth, a larger reduction in other crimes is seen when defining the first treatment year as having a force for all of the year rather than just part of the year. This is perhaps not surprising as a force cannot be created overnight and there was a need to recruit and train officers.

The results in Table 9 show the effect of creating any police force, regardless of its quality. Yet, some forces may have been in name only or thought to be inefficient by the inspectors. The lack of an overall effect of force formation on crime could be masking differential effects of forces of varying quality. One ‘quality’ measure that we observe upon force formation is efficiency – the number of people per policeman. Are there differential effects of creating ‘efficient’ versus ‘inefficient’ forces? In studying that question in an expanded specification, one must rely on the additional assumption that ‘efficiency’ is conditionally random. The official recommendation in the 1839 Act was to have 1,000 people per policeman. However, few (if any) forces initially achieved that level of efficiency. Some initial evidence regarding the determinants (or lack thereof) of force type can be seen in Table 8. Simply put, it is not just early reformers, i.e. maybe particularly motivated counties, that were efficient (using 1,500 people per officer as a threshold); rather similar proportions of early (20%), mid (33%), and late (17%) reforming counties were efficient at formation. We look at the determinants of efficient force creation more explicitly in the next section.

Table 10 estimates the impact of efficient versus inefficient force formation, using various thresholds in defining efficiency, ranging from 1,500 (column (1)) to 2,500 people per policeman (column (5)). There are 10 and 30 efficient forces under the strictest and weakest thresholds, respectively. There does appear to be a differential impact of creating an efficient rather than an inefficient force: Column (1) of Table 10 shows that creating an efficient force with less than 1,500 people per policeman decreases the overall number of crimes by approximately 19%; this effect is seen across crime categories (18% for violent, 14% for property and 24% for other offenses). In contrast, creating an inefficient force does not significantly affect crime overall; rather, it increases the number of property crimes (albeit insignificantly) and only marginally significantly reduces the number of other crimes. It is the positive effect of inefficient forces on the largest crime category of property offenses that is masking the crime reducing effect of creating an efficient force in the baseline regressions. While the crime reducing effect of an efficient force gets smaller as we relax the definition of efficiency in columns (2) to (5), we still see an overall reduction in crime for efficient forces.

We next consider the dynamics: Is there an immediate effect of creating a police force? Is it persistent? Specifically, we estimate an event-study specification where we interact our treatments (creation of efficient and inefficient forces) with dummies for two-year intervals leading up to and following the reform. The omitted category is the two years immediately prior to the first fully treated (fiscal) year. The results are presented in Figure 13 for all crimes categories combined, and for each offense category separately in Appendix Figure A3. The top and bottom panels of Figure 13 present the estimates for the efficient and inefficient forces, respectively; both the efficient and inefficient force estimates come from the same regression. The following conclusions can be drawn: First, the negative effect of efficient police force formation on crime is not immediate (except for other crimes) but rather kicks in three years after the reform. Second, the negative effects of efficient force creation continue to get larger in magnitude over time. Third, for forces that were inefficient upon creation, no negative effect on crime is seen in any of the eight years after the force is created. These event study specifications also provide tests of our core identifying assumptions of parallel trends and the ‘random’ timing of force creation: There are no significant differences in crime rates in the years leading up to the reform for either efficient or inefficient forces. Additional robustness and identification tests are presented in the next section.

4.5. Sensitivity and Identification Tests for County Police Analysis

Appendix Table A7 presents a sensitivity analysis of our main finding that only the creation of an efficient force reduces crime (using the 1,500 people per officer threshold). Specifically, we show that the results are robust to (i) controlling for population, England and inspection region dummies, national linear and quadratic time trends, and inspector specific and large county (above median acreage) specific time trends, (ii) reducing the sample period by three years on both sides of the window, (iii) breaking the sample into two periods: 1832 to 1849 (identified off early reformers) and 1850 to 1865 (identified off late reformers), and (iv) restricting the sample to the 36 English counties (excluding the 12 Welsh counties).

We next turn to tests of the identifying assumptions of randomness in (i) the timing of force formation and (ii) the efficiency of the created force. Appendix Table A8 assesses the former. Columns (1) to (4) consider whether being an early reformer (reformed in 1840) is affected by lagged crime rates and whether a neighboring county had a police force in the previous year (in 1840, this is equivalent to bordering London/Middlesex). Neither lagged crime (overall or by category) nor lagged neighboring forces predict being an early reformer. Columns (5) to (9) look at the reform timing for all counties by regressing a dummy equal to one in the year a county creates a force and zero in the years prior on lagged crime and neighboring forces. Counties exit the sample once a police force is created, as there is no longer a choice to be made. The sample is restricted to 1840 (the first possible fiscal reform year) to 1857 (the last possible year of adoption).⁴⁸ Again, lagged crime rates overall and by crime category never significantly predict reform adoption. We do see that having a neighboring force (driven by ‘inefficient’ forces with more than 1,500 people per officer) decreases the chance of reform (significant at the 10% level). This raises the question of whether the creation of a force has spill-over effects on nearby counties, which we address shortly.

Appendix Table A9 looks more formally at the determinants of both continuous (people per police) and dichotomous (less than 1,500 people per police) measures of efficiency upon creation. We consider all potential determinants available to us, including fixed geographic variables (acreage, number of parishes and neighboring counties, English versus Welsh counties), variables measured in the 1851 census (share farmers, male, married, native, employed, and the age distribution), as well as crime rates and whether any neighboring counties had efficient or inefficient forces in the year before force formation.⁴⁹ There is little

⁴⁸ This specification is motivated by Buckles et al.’s (2011) and Goldin and Rouse’s (2000) analyses of U.S. state reforms of blood test requirements for marriage and the adoption of screens for orchestra auditions, respectively.

⁴⁹ These regressions are purely descriptive. It should also be noted that there is limited power – we have a cross-section of 48 counties – and that force formation could have occurred prior to the year in which some of these variables (especially the census variables) are measured.

information, including crime in the year before force formation, which consistently predicts the type/size of the police force. (Lagged crime and neighboring forces are also not significantly related to efficiency when excluding the other controls.) Moreover, to the extent that these variables are constant over time, they are captured by county fixed effects.

Finally, Table 11 assesses the robustness of our results to possible spill-over effects of creating a police force in one county on crime in neighboring counties. Specifically, we estimate the effect of having an efficient or inefficient county force (using the 1,500 threshold) while controlling for (i) whether a neighboring county in year t (i.e. a border-sharing county) had any force or (ii) whether there were any neighboring efficient and inefficient forces. Controlling for neighboring county police forces has no impact on the baseline estimates: having an efficient force still decreases all crime by almost 19%, violent crime by 18%, property crime by 14%, and other crime by 24%. One's prior expectation is perhaps that having a force in a neighboring county increases crime locally, as criminals flee the neighboring county to commit crime elsewhere. This is, in fact, what we see when we look at the effect of having any neighboring forces itself: there is a significant increase in local crime of 19%, which is driven by property crimes. However, when looking at the effects of having efficient and inefficient neighboring forces, we again see very different effects. Having an efficient neighbor actually significantly decreases crime, while having an inefficient neighbor significantly increases crime in a county. One potential explanation for this is that the inspectors classified forces as efficient and inefficient using multiple criteria, not just per capita police. Another important criterion was the degree of cooperation between neighboring forces. If a neighbor that is efficient in terms of size is also efficient in terms of cooperation, then this could further decrease crime.

4.6. Discussion of County Police Force Formation Results

To summarize, the above analysis of the roll-out of professional county police forces has four key findings. First, the creation of 'efficient' county forces reduces trials overall and across crime categories. Second, the formation of 'inefficient' forces does not have an observable crime reducing effect (trial data). Third, the effect of creating an efficient force is not immediate and increases in magnitude over time. Fourth, there are spill-over effects of neighboring forces, with an inefficient neighbor increasing and an efficient neighbor decreasing 'local' crime.

What do these findings tell us about the ways in which the creation of a county force decreases crime? On the one hand, there are two main channels through which crime can be reduced: deterrence and incapacitation (i.e. preventing criminals from recidivating). On the other hand, creating a police force might increase measures of 'crime' through increased

reporting of crime incidents and apprehensions. The net negative effect for efficient forces suggests that deterrence and incapacitation outweigh reporting and apprehension channels. However, while (anecdotally) the aim of the new forces was deterrence, we cannot empirically disentangle it from incapacitation. Finally, an interesting takeaway is the increase in the magnitude of the crime-reducing effect over time. This highlights the importance of force quality. The police forces clearly improved in ‘quality’ over time: people per officer ratios continued to decrease, supervisors were increasingly hired, and experience was gained.

What can we conclude about the impact of creating an ‘inefficient’ police force? While there is no negative net effect on the number of charges brought to trial, we cannot rule out the possibility of deterrence and/or incapacitation. We simply cannot disentangle whether there is a null effect because a force had no effect at all or because the positive and negative channels off-set each other.

5. Conclusion

This paper addresses a yet unstudied question in the literature on police and crime: Do *any* police reduce crime? To identify the extensive margin effect of police on crime, we exploit variation from two natural experiments in history: the introduction of the London Metropolitan Police in 1829 and the subsequent roll-out of professional county police forces throughout England and Wales. In London, we find evidence consistent with deterrence and/or incapacitation for both violent and property crimes (i.e. a reduction in crime incidence). For violent crimes, the fact that we see a reduction in both incidents and charges, i.e. in all available measures of crime, suggests that the crime reducing channels dominate increased apprehensions and/or reporting. For property crimes in London, however, the results are consistent with the reverse: There is an increase in property crime charges despite the reduction in crime incidence. Our county analysis finds that creating ‘efficient’ police forces in terms of the population per police ratio reduced crime overall and across crime categories, while creating ‘inefficient’ forces did not have a net crime reducing effect. We also find that the effect of ‘efficient’ police on crime is not immediate upon force creation but rather increases over time, potentially with increases in force quality.

How do these findings compare to the existing literature? Given the lack of estimates of the extensive margin effect of police on crime, the most comparable estimates come from studies of police deployment as well as those of additional, private police. Using terror-related shocks to deployment, Draca et al. (2011) and DiTella and Schargrodsy (2004) find elasticities of crime with respect to police of around -0.3, i.e. a decrease in crime of approximately 0.3%

with a 1% increase in police. MacDonald et al. (2016) study the effect of private police patrols within defined geographic boundaries using a geographic regression discontinuity design. They find a 45-85% increase in the number of crimes outside of the boundaries of the private police catchment area, which they convert to an elasticity of -0.33 (-0.2 for property crime and -0.7 for violent crime). Our findings for both the London and county analyses are generally in line with these results. Efficient county forces decreased rural crime by 19%, while the London Metropolitan Police decreased urban crime by 24-57% depending on crime category.

Finally, the above county results refer to the effect of creating an ‘efficient’ force, where efficiency is defined as having fewer than 1,500 people per officer. How does that compare to today’s police force sizes? Data from the UCR’s *Crime in the United States* suggest that overall there were 3.5 law enforcement officers per 1,500 population in the U.S. in 2016 (3.9 for metropolitan areas and 4.5 for non-metropolitan areas, respectively).⁵⁰ That is, using a ratio of one officer per 1,500 people as a threshold for efficiency is conservative in today’s terms.

Lastly, despite the historical setting, our results may have important policy implications today, especially with respect to institution building in developing countries. We found the impact of creating an institution (the professional police force) depended on the intensity of treatment in the London analysis and the quality of that institution in the county analysis. That is, we only found a net crime reducing effect of the creation of relatively high quality (efficient) forces, as we measured by the police to population ratio and (indirectly) the degree of cooperation with neighboring forces. Other measures of institutional quality may be particularly relevant today, including for instance, police corruption. This was, after all, one of the justifications for the formation of the London Metropolitan Police.

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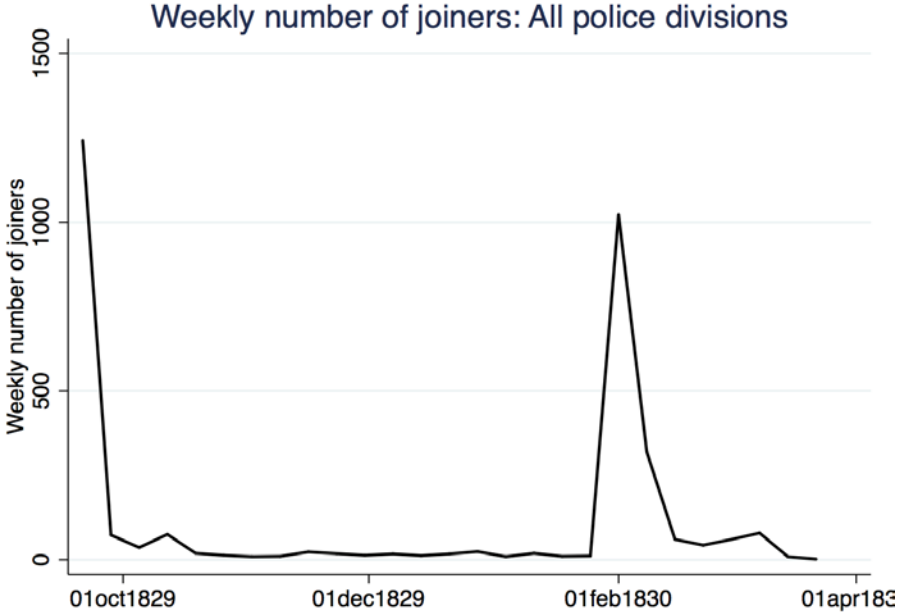
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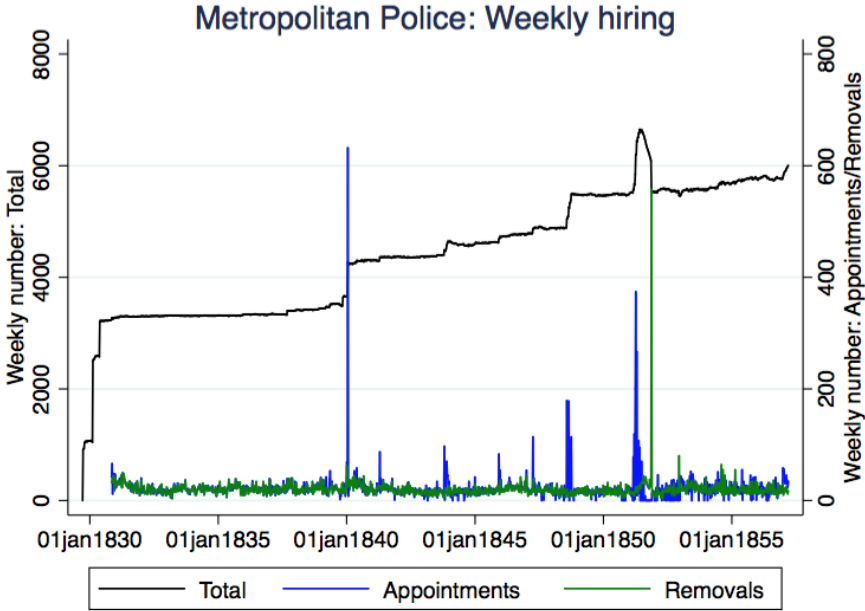
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Figure 1. London Metropolitan Police – Weekly Hires

Panel A. 1829-1831



Panel B. 1830-1856



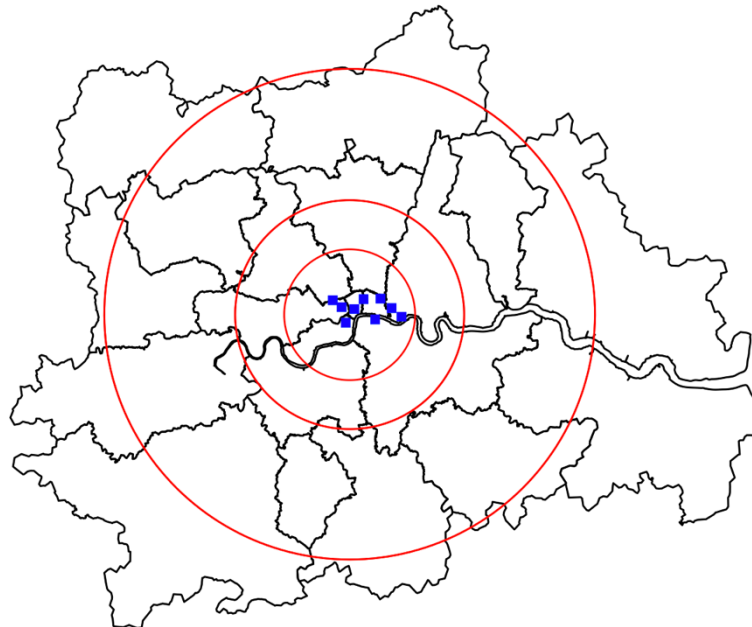
NOTES – Panel A shows the weekly number of police joining the Metropolitan Police across all police divisions between September 1829 and March 1830. The data underlying this figure were manually transcribed from the *Register of recruits into the Metropolitan Police* available at the London National Archives (MEPO 4/31). Panel B shows the weekly number of total police, appointments as well as removals from the Metropolitan Police between 1829 (1830 for appointments and removals) and 1857. This figure is based in manually transcribed data from the *Weekly State of the Metropolitan Police 1829-1857* available at the London National Archives (MEPO 4/1).

Figure 2. The London Metropolitan Police Jurisdiction (1829)

Panel A. Original Map of the London Metropolitan Police District in 1829



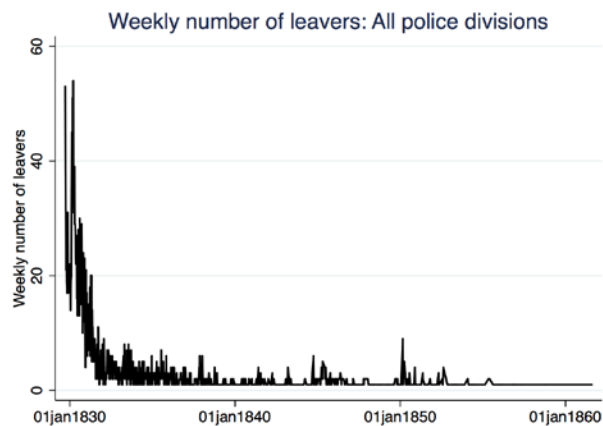
Panel B. Police Stations (Existing Before the Metropolitan Police and until 1839)



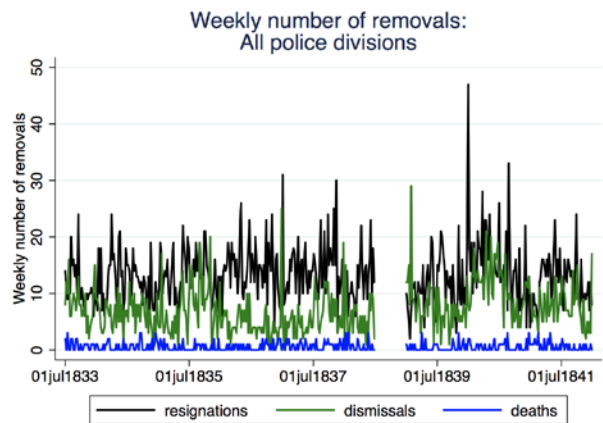
NOTES – Panel A presents a map of the original London Metropolitan Police District. Shaded in red is the City of London Police area, outside of the Met’s jurisdiction. The large letters indicate the various districts of the Metropolitan Police. The map is available from the British Library’s online map collection: <http://www.bl.uk/onlinegallery/onlineex/craxe/j/007000000000019u00055000.html>. Panel B shows a map of London centered on Charing Cross, with the pre-existing police offices indicated by blue squares and 4-, 7- and 15-miles radii around Charing Cross in red. The borders represent modern day postcode areas; the shapefiles were obtained from Maproom’s UK Postcodes Shapefiles and contain OS, Royal Mail and National Statistics data.

Figure 3. London Metropolitan Police – Turnover and Quality

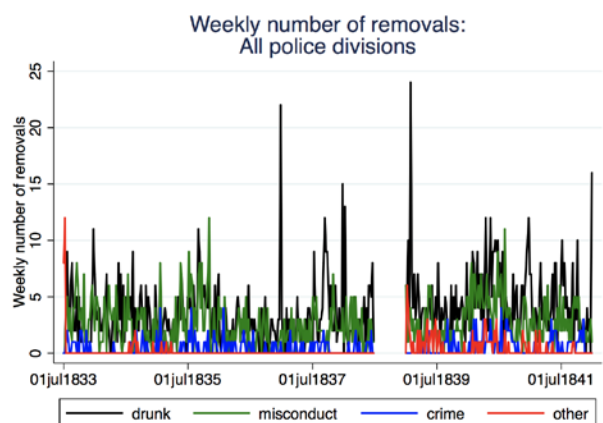
Panel A. Weekly Number of Leavers



Panel B. Weekly Number of Removals, by Reason

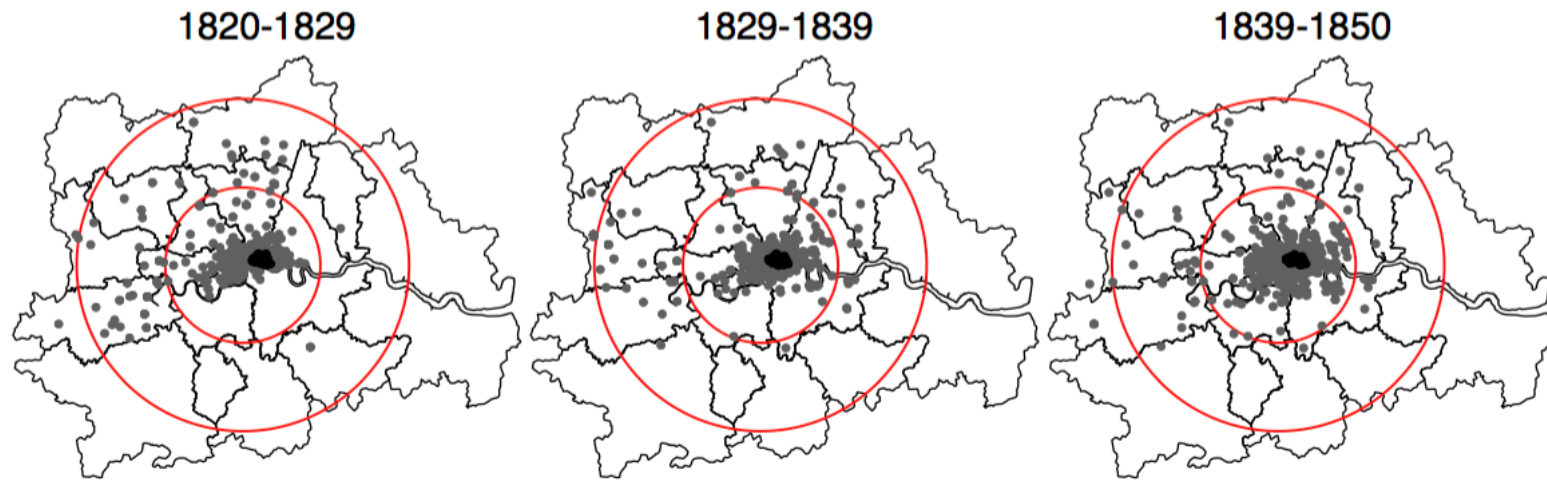


Panel C. Weekly Number of Dismissals, by Reason



NOTES – Panel A shows the weekly number of leavers from the London Metropolitan Police among those officers who were recruited between September 1829 and March 1831. The figure is based on manually transcribed data from the *Register of recruits into the Metropolitan Police* sourced from the London National Archives (MEPO 4/31). Panel B presents the weekly number of removals from the London Metropolitan Police by broad reason (resignation, dismissal, death), Panel C shows the weekly number of dismissals further split up by more detailed reason (drunkenness, neglect or misconduct, criminal behavior, other). These figures are based on manually transcribed data from the *Home Office: Police Entry Books, Series I. Metropolitan Police* sourced from the London National Archives (HO 65/11, 65/12 and 65/13).

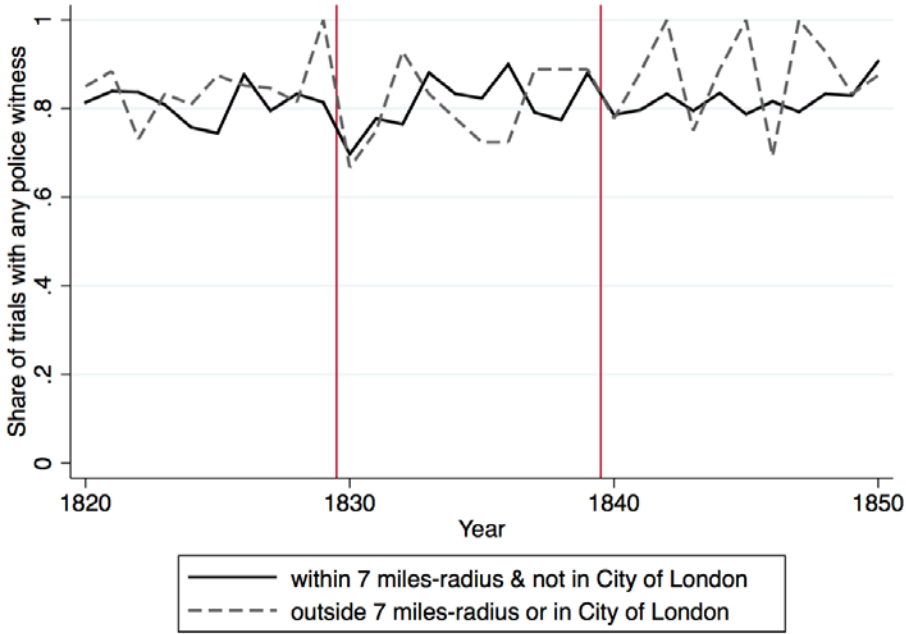
Figure 4. Geocoded Data from the Old Bailey Proceedings



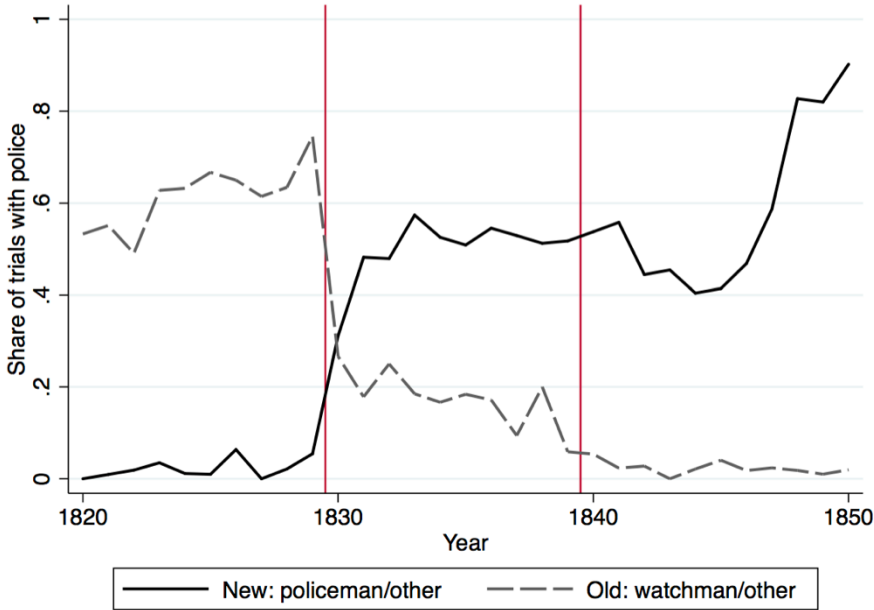
NOTES – The figure plots geocoded crime locations of murders, manslaughters, robberies and burglaries trialed at the Old Bailey between 1820 and 1850. Each dot represents a trial-defendant observation; the black dots represent crime locations inside the City of London. The two red circle mark a 7- and 15-mile radius from Charing Cross, respectively. The borders represent modern day postcode areas; the respective shapefiles were obtained from Maproom’s UK Postcodes Shapefiles and contain OS, Royal Mail and National Statistics data.

Figure 5. Evidence of the Introduction of the Met Police in the Old Bailey Proceedings

Panel A. Presence of Any Police Witnesses at Trial



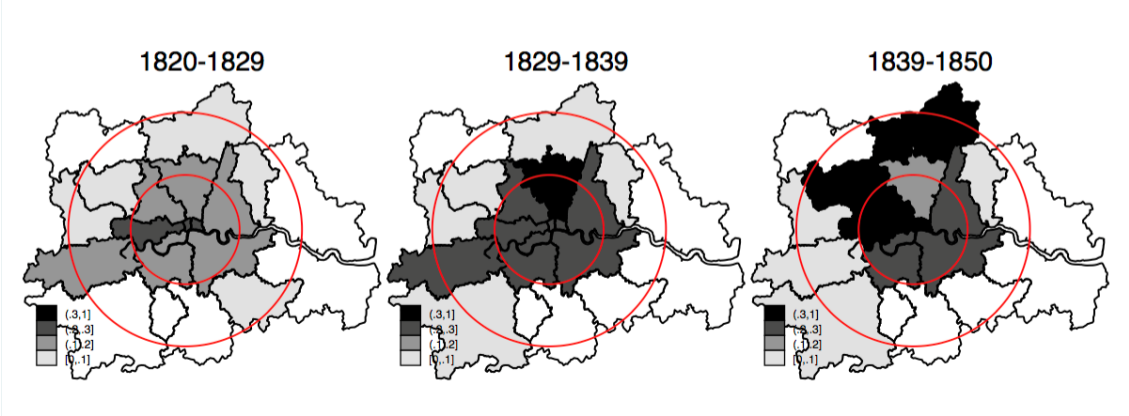
Panel B. Change in Type of Police Witnesses at Trial



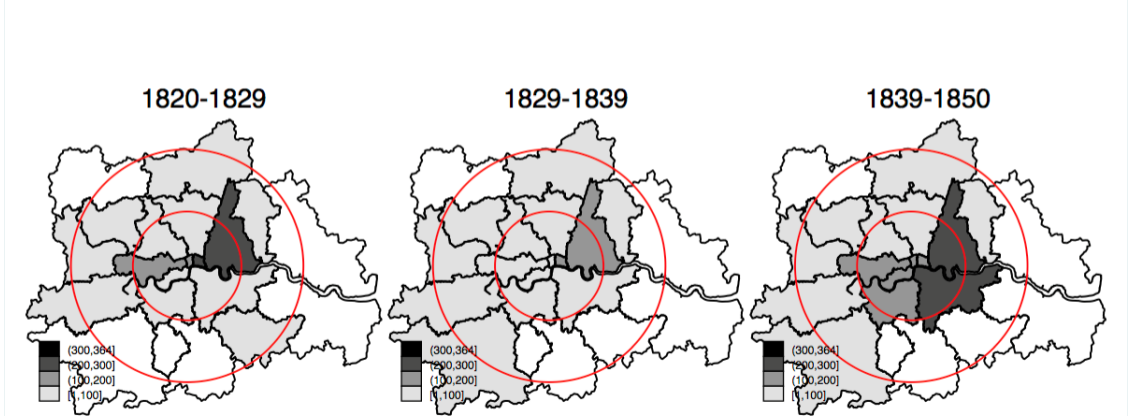
NOTES – Panel A shows the annual share of homicide, robbery, and burglary trials at the Old Bailey from 1820 to 1850 with at least one police present as a witness. The black solid line represents trials for crimes located in the treatment group (within 7 miles from Charing Cross), the grey dashed line trials for crimes located in the control group (more than 7 miles from Charing Cross or in the City of London). Panel B shows the annual share of trials that, among the first five witnesses present at the trial, had at least one of either the new type (black solid line) or the old type (grey dashed line) of police. See the text for details on the types of police. The red vertical lines in both panels represent the timing of the initial introduction of the Metropolitan Police in 1829 and its expansion in 1839, respectively. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

Figure 6. Mapping the Treatment and Reduced Form from the Old Bailey Proceedings

Panel A. Share Trials with Police Present at Crime Scene



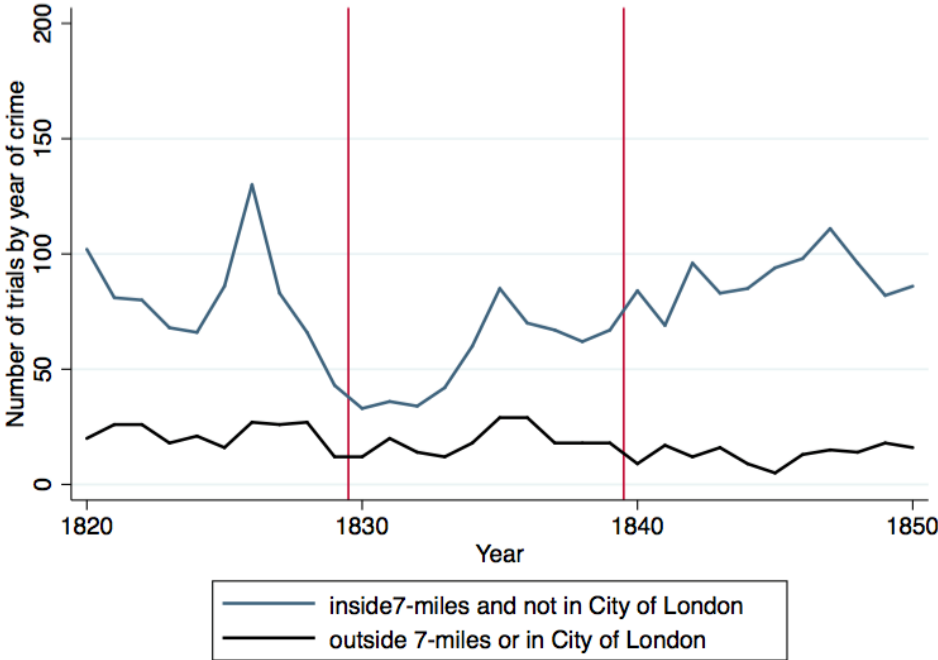
Panel B. Reduced Form Effect of the Metropolitan Police on the Number of Trials



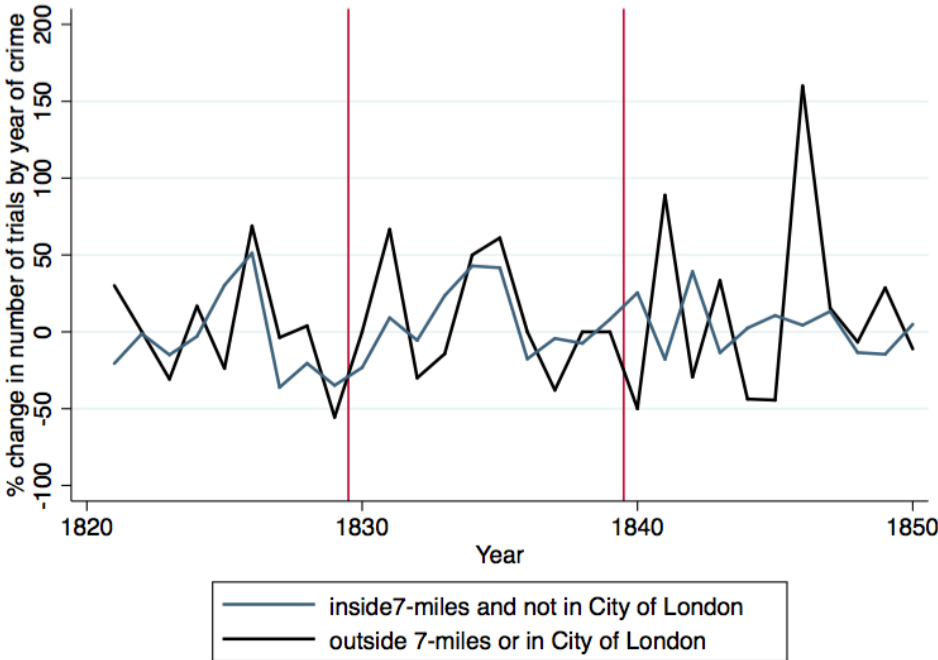
NOTES – Panel A shows maps of London with the share of trials at the Old Bailey from 1820 to 1829 (left), 1829 to 1839 (middle) and 1839 to 1850 (right) with police present at the crime scene, each by (modern-day) postcode area. Darker shaded areas correspond to higher shares of trials with police at the crime scene. Panel B shows maps of London with the number of crimes by postcode area trialed at the Old Bailey from 1820 to 1829 (left), 1829 – 1839 (middle) and 1839 to 1850 (right). Crimes include burglaries, robberies and homicides (see data description in the text). Darker shaded areas correspond to higher number of trials in the respective postcode area. The two red circles mark a 7- and 15-mile radius from Charing Cross, respectively. The borders represent modern-day postcode areas; the respective shapefiles were obtained from Maproom’s UK Postcodes Shapefiles and contain OS, Royal Mail and National Statistics data. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

Figure 7. Reduced Form Effect of the Met on Trials in the Old Bailey Proceedings

Panel A. Number of Trials



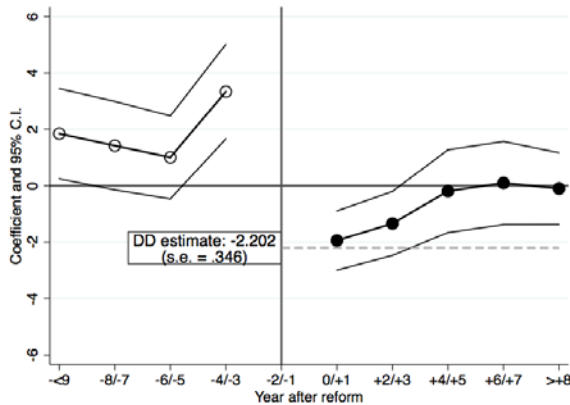
Panel B. Percentage Change in Number of Trials



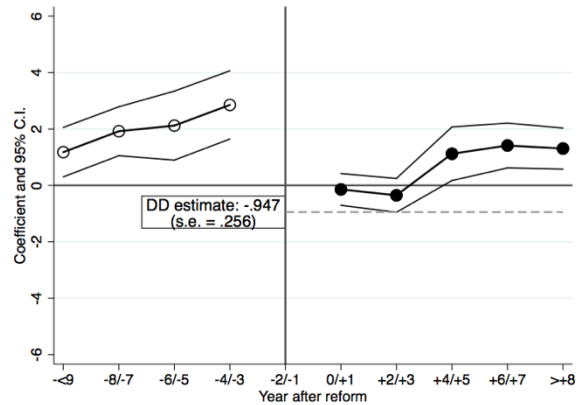
NOTES – The figure shows the annual number of trials for robbery, burglary and homicide at the Old Bailey from 1820 to 1850 (see text for more detail on the data). The blue solid line represents trials for crimes located in the treatment group (within 7 miles from Charing Cross), the black solid line trials for crimes located in the control group (more than 7 miles from Charing Cross or in the City of London). The year refers to the year of the crime, not the year of the trial. The red vertical lines in both panels represent the timing of the initial introduction of the Metropolitan Police in 1829 and its expansion in 1839, respectively. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

Figure 8. Event-Study for Reduced Form from the Old Bailey Proceedings

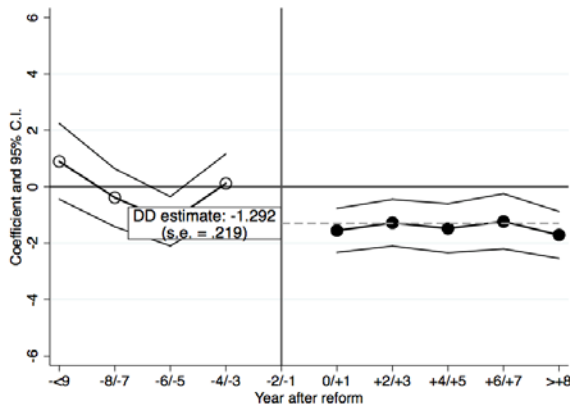
Panel A. Total Crime



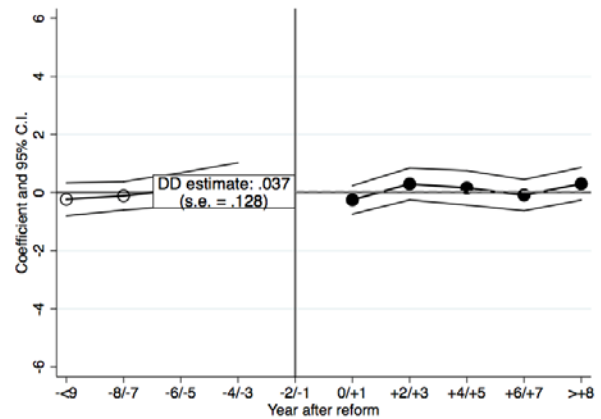
Panel B. Burglary



Panel C. Robbery



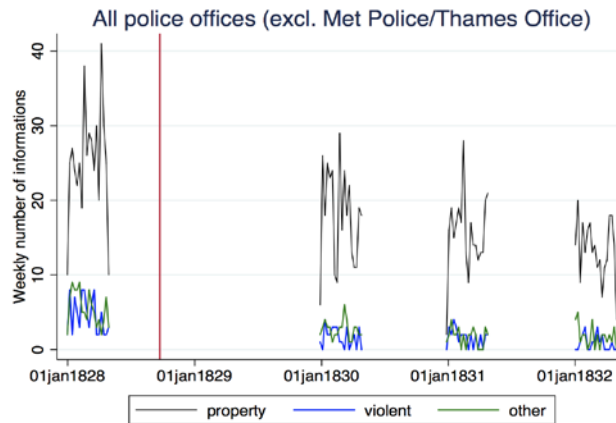
Panel D. Homicide



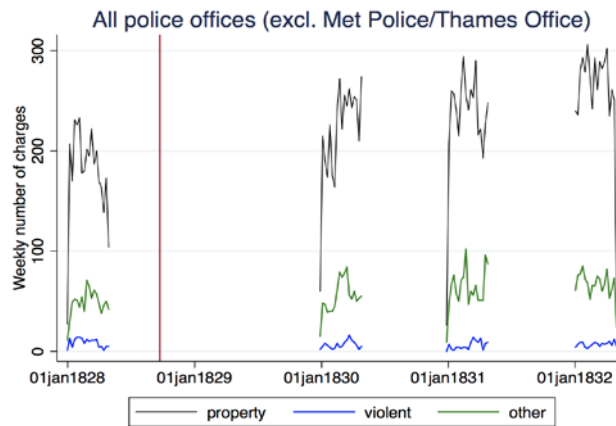
NOTES – The figures show the estimated coefficients and 95% confidence intervals corresponding to the event-study specifications described in Section 3.2. Panel A shows the results for all trials (pooled), Panel B to D for burglary, robbery and homicide trials, respectively. A year is defined as September to August. The vertical line represents the two years before the introduction of the Metropolitan Police (September 1829) which is the omitted category. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

Figure 9. Daily Crime Reports – Weekly Aggregated Crime

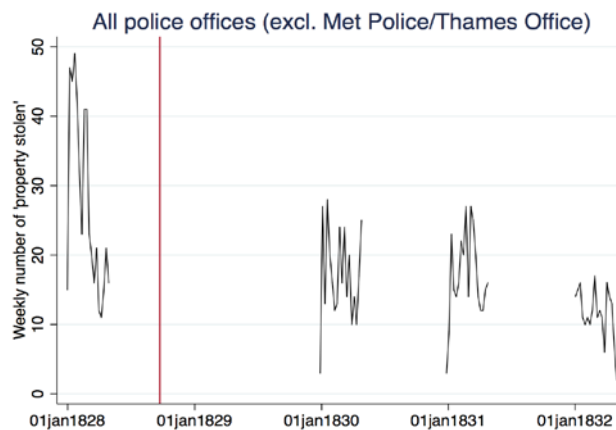
Panel A. Weekly Number of Informations



Panel B. Weekly Number of Charges

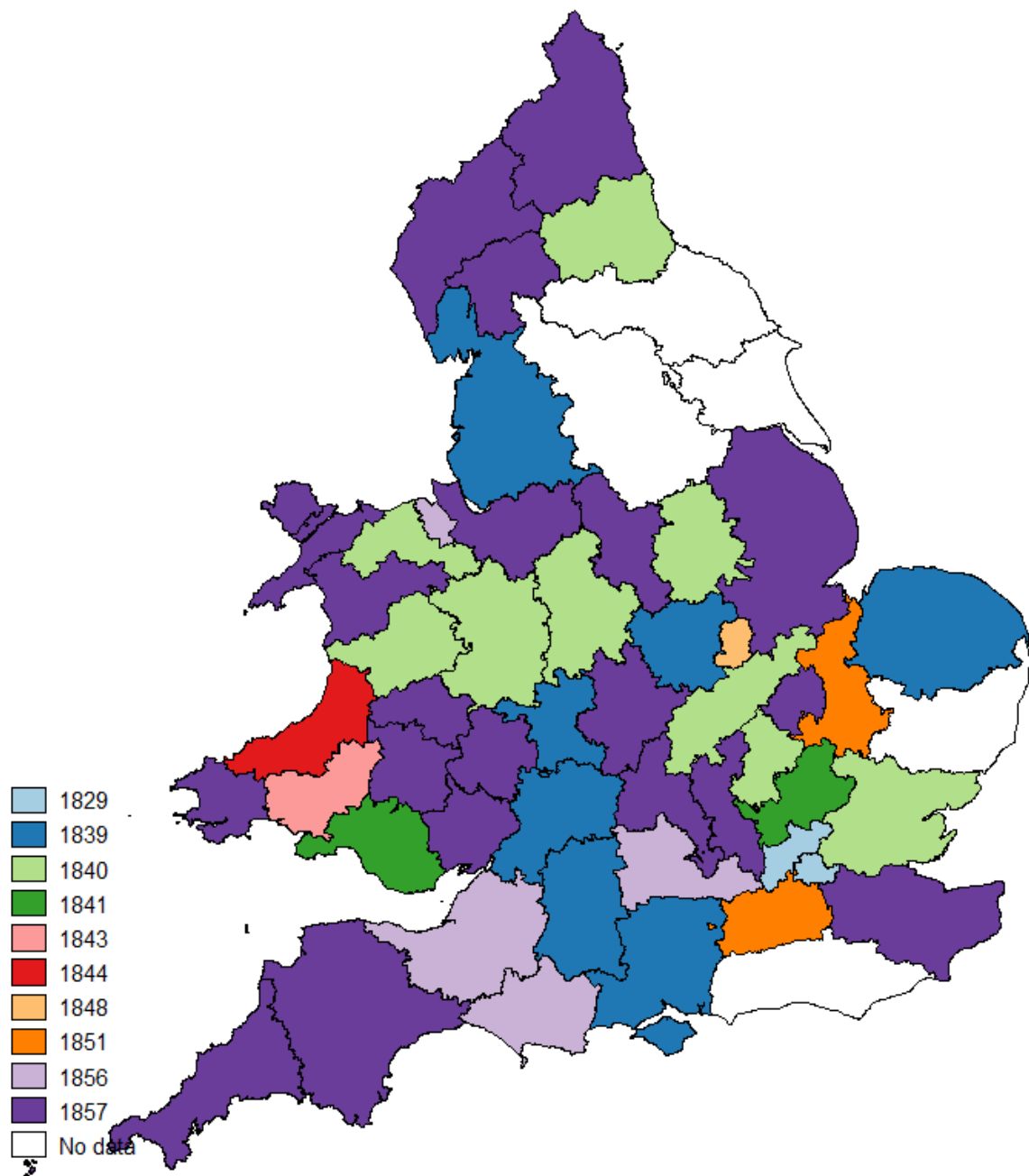


Panel C. Weekly Number of Property Stolen Incidents



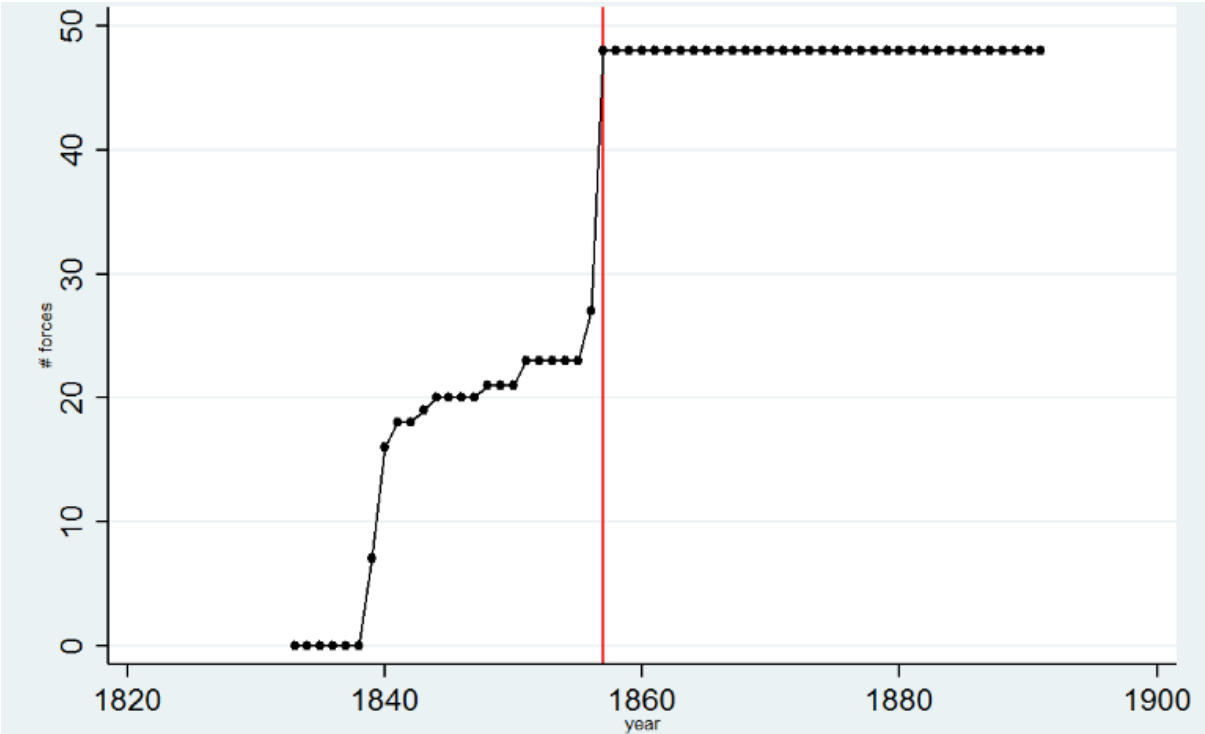
NOTES – Panel A shows the weekly number of informations for property (black line), violent (blue line) and other (green line) incidents for all London police offices (except Thames and the Metropolitan Police, see Section 3.1 for details). Panel B shows the weekly number of charges, again for property, violent and other crime. Panel C shows the weekly number of property stolen incidents. In each panel, the red horizontal line represents the date of the introduction of the Metropolitan Police. The figures are based on manual transcribed data from the *Report or Account of the Proceedings of the several Police Offices* sourced from the National Archives (MEPO 4/12, 4/13, 4/15 and 4/17).

Figure 10. Map of Police Force Start Years for English and Welsh Counties



NOTES – The map illustrates the different start years of police forces across counties in England and Wales. Each color represents a different start year. The counties of York, Sussex, and Suffolk are excluded (left blank) because of multiple start dates for the same county. This map is based on 1851 county registration districts, from Great Britain Historical GIS Project (2012) 'Great Britain Historical GIS'. See the text for details on the police force start years.

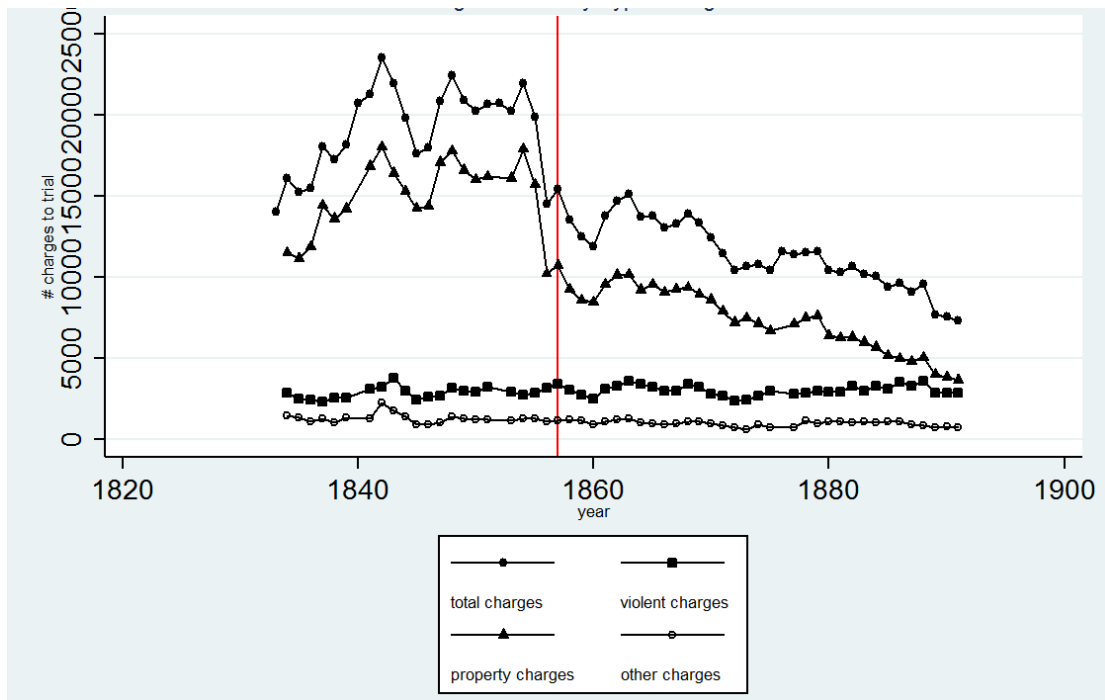
Figure 11. Number of Professional County Police Forces in England and Wales



NOTES – This figure shows the number of county police forces in each year for our analysis sample of 48 counties, i.e. excluding Middlesex, York, Suffolk, and Sussex. The red vertical line marks 1857, the year when the creation of a county police force became mandatory. See Section 4.1 and 4.2 for details on the data and the sample.

Figure 12. County-Level Data on Charges Brought to Trial

Panel A. Number of Annual Charges By Crime Type in England and Wales



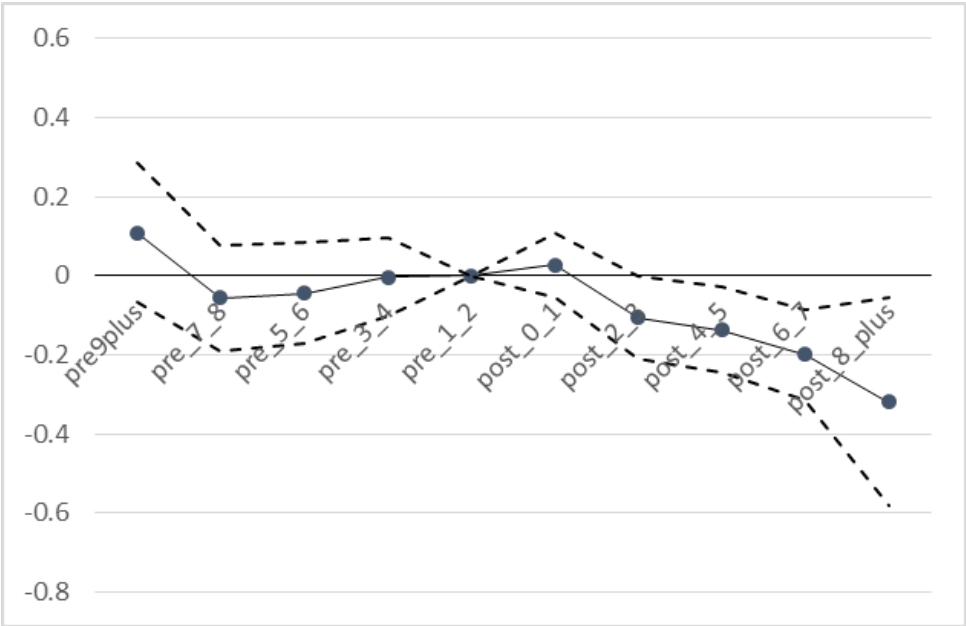
Panel B. Average Log Charges for Early, Mid and Late Reforming Counties



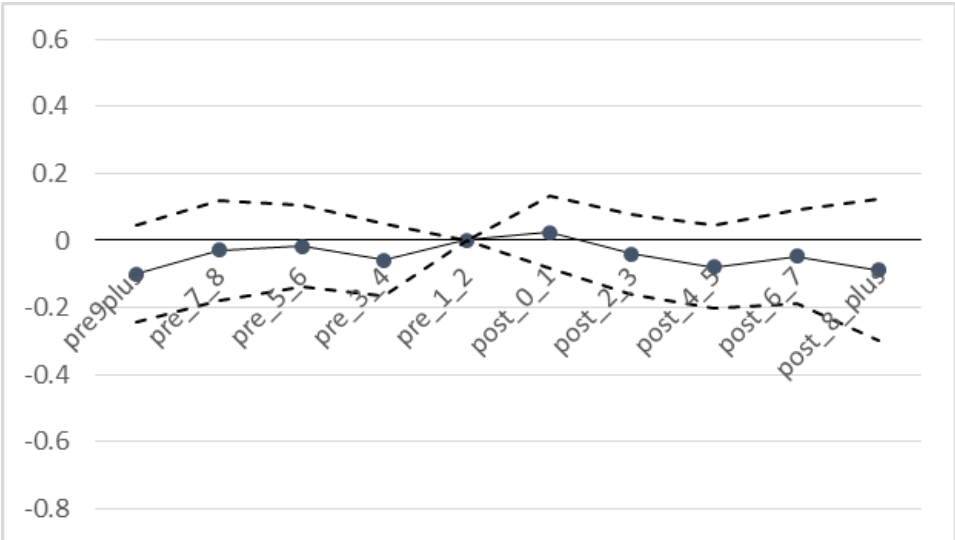
NOTES – Panel A shows the annual number of charges brought to trial in England and Wales, overall and by crime type and for all counties included in the analysis sample, i.e. excluding Middlesex, York, Suffolk, and Sussex. The red vertical line marks 1857, the year when the creation of a county police force became mandatory. Panel B shows the annual average log charges separately for early, mid and late reformers, again excluding the counties of Middlesex, Sussex, York, and Suffolk. The red vertical lines correspond to the earliest and latest years of reform implementation (1841 and 1858). The figures are based on data from the *Judicial Statistics*, see Section 4.1 and 4.2 for details.

Figure 13. Event-Study of Efficient and Inefficient County Police Forces on Crime

Panel A: Efficient Police Forces, Log-Level Specification, All Charges



Panel B: Inefficient Police Forces, Log-Level Specification, All Charges



NOTES – The above event-study figures are based on log-level regressions of offenses on efficient (ratio<1,500) and inefficient (ratio>1,500) force dummies that are interacted with two-year intervals. All years eight or more years after police force formation and nine or more years before police force formation are combined, respectively. The omitted category is the period 1-2 years before the police force is created, where the first year (0) is defined as the first full fiscal year following the creation of a police force. The above figures show the estimated coefficients and 95% confidence intervals for the baseline specification with county and year fixed effects. The dots/solid line correspond to the point estimates, while the dashed lines represent the 95% confidence intervals.

Table 1. Evidence of the Introduction of the Metropolitan Police: Police Witnesses at the Old Bailey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Any police witness		Any "new" police witness		Any "old" police witness		Police at crime scene	
Sample:	1820-1839	1828-1832	1820-1839	1828-1832	1820-1839	1828-1832	1820-1839	1828-1832
<i>Panel A. Pre-Post Analysis</i>								
<i>Treated (<4 miles)</i>								
Post Met	0.059*** (0.020)	-0.009 (0.052)	0.586*** (0.023)	0.491*** (0.052)	-0.493*** (0.023)	-0.528*** (0.061)	0.111*** (0.025)	-0.017 (0.065)
Observations	1,247	209	1,247	209	1,247	209	1,247	209
<i>Uncertain (4-7 miles)</i>								
Post Met	0.036 (0.074)	-	0.446*** (0.070)	-	-0.245** (0.095)	-	0.034 (0.070)	-
Observations	99	10	99	10	99	10	99	10
<i>Control (>7 miles)</i>								
Post Met	0.031 (0.061)	-	0.191*** (0.046)	-	-0.069 (0.078)	-	0.012 (0.045)	-
Observations	168	35	168	35	168	35	168	35
<i>City of London</i>								
Post Met	0.000 (0.042)	-0.025 (0.098)	0.356*** (0.053)	0.240** (0.119)	-0.247*** (0.065)	-0.185 (0.150)	0.013 (0.063)	-0.138 (0.147)
Observations	239	50	239	50	239	50	239	50
<i>Panel B. Difference-in-Differences Analysis</i>								
Post Met x Treatment Area	0.032 (0.041)	0.026 (0.078)	0.253*** (0.040)	0.279*** (0.072)	-0.293*** (0.051)	-0.357*** (0.101)	0.064 (0.044)	0.039 (0.096)
Post Met x Uncertainty Area	0.002 (0.083)	-0.187 (0.315)	0.111 (0.073)	-0.129 (0.080)	-0.045 (0.105)	-0.027 (0.313)	0.011 (0.079)	-0.261 (0.220)
Observations	1,753	297	1,753	297	1,753	297	1,753	297

NOTES - The table shows regression results for the first stage outcomes (dummy variables for any police witness at the trial, any "new" police witness, any "old" police witness, and whether police was at the crime scene). Panel A shows pre-post specifications that include offense fixed effects; Panel B shows difference-in-differences specifications that include year, area and offense fixed effects. The regressions are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations; the sample includes trials for robbery, burglary and homicide. See Section 3.2 for details. Robust standard errors are shown in parentheses below the coefficient. * p<0.1, ** p<0.05, *** p<0.01.

Table 2. Differences in Means in the Old Bailey Proceedings

	Total				Burglary				Robbery				Homicide			
	Before	After	Δ		Before	After	Δ		Before	After	Δ		Before	After	Δ	
<i>Panel A. 1820-1839, Y = Number of crimes per month/area</i>																
Treated	6.46	4.10	-2.36	***	2.84	1.75	-1.09	***	2.82	1.41	-1.41	***	0.80	0.94	0.14	
Uncertain	0.38	0.46	0.08		0.20	0.23	0.03		0.12	0.13	0.00		0.06	0.11	0.05	
Control	0.71	0.72	0.01		0.43	0.20	-0.23	***	0.17	0.34	0.17	**	0.10	0.17	0.07	
City of London	1.13	0.88	-0.25	**	0.45	0.45	0.00		0.59	0.22	-0.37	***	0.09	0.21	0.11	**
<i>Panel B. 1828-1832, Y = Number of crimes per month/area</i>																
Treated	4.60	2.75	-1.85	***	1.10	0.58	-0.53	*	2.80	1.33	-1.48	***	0.70	0.85	0.15	
Uncertain	0.25	0.13	-0.13		0.15	0.00	-0.15	*	0.05	0.10	0.05		0.05	0.03	-0.03	
Control	0.80	0.47	-0.33		0.25	0.13	-0.13		0.30	0.28	-0.03		0.25	0.08	-0.17	
City of London	0.85	0.82	-0.03		0.20	0.18	-0.03		0.50	0.35	-0.15		0.15	0.30	0.15	

NOTES – The table shows the average number of monthly trials for crimes that took place before and after the introduction of the Metropolitan Police (and their difference), for all as well as each offense separately, as well as by area. The treated area includes trials for crimes located within 4 miles from Charing Cross, the uncertain area those located between 4 and 7 miles from Charing Cross, the control area those located more than 7 miles from Charing Cross and City of London those located in the City of London. Panel A shows the results for 1820-1839, Panel B for 1828-1832. The numbers are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations; the sample includes trials for robbery, burglary and homicide. See the text for details. Statistical significance of the difference is based on corresponding before-after regressions. * p<0.1, ** p<0.05, *** p<0.01.

Table 3. Difference-in-Differences: Effect of Metropolitan Police on Crime in the Old Bailey Proceedings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample:	1820-1839	1825-1835	1828-1832	1820-1839	1825-1835	1828-1832	1820-1839	1825-1835	1828-1832
Radius:	<i>Four miles from Charing Cross</i>			<i>Four miles from Charing Cross</i>			<i>Four miles from Charing Cross</i>		
Specification:	City of London = Control			City of London = Treated from 02 April 1832			City of London = Uncertain		
<i>Panel A. Total crime (by month/area)</i>									
Post Met x Treatment Area	-2.202*** (0.346)	-2.766*** (0.491)	-1.574*** (0.537)	-1.483*** (0.209)	-1.637*** (0.289)	-0.984** (0.382)	-2.286*** (0.355)	-2.732*** (0.500)	-1.431*** (0.531)
Post Met x Uncertainty Area	0.234* (0.134)	0.228 (0.185)	0.151 (0.239)	0.099 (0.154)	0.251 (0.208)	0.233 (0.247)	-0.016 (0.152)	0.171 (0.210)	0.344 (0.246)
<i>Panel B. Burglary (by month/area)</i>									
Post Met x Treatment Area	-0.947*** (0.256)	-1.220*** (0.376)	-0.397 (0.267)	-0.447*** (0.150)	-0.595*** (0.205)	-0.224 (0.190)	-0.827*** (0.260)	-1.142*** (0.376)	-0.338 (0.272)
Post Met x Uncertainty Area	0.166 (0.103)	0.082 (0.132)	-0.022 (0.104)	0.214* (0.112)	0.155 (0.145)	0.007 (0.104)	0.273*** (0.106)	0.171 (0.133)	0.100 (0.109)
<i>Panel C. Robbery (by month/area)</i>									
Post Met x Treatment Area	-1.292*** (0.219)	-1.345*** (0.284)	-1.297*** (0.428)	-0.978*** (0.129)	-0.885*** (0.165)	-0.832*** (0.299)	-1.504*** (0.224)	-1.439*** (0.290)	-1.281*** (0.433)
Post Met x Uncertainty Area	0.123 (0.083)	0.190 (0.119)	0.228 (0.162)	-0.017 (0.093)	0.157 (0.127)	0.288* (0.170)	-0.276*** (0.096)	-0.061 (0.132)	0.144 (0.189)
<i>Panel D. Homicide (by month/area)</i>									
Post Met x Treatment Area	0.037 (0.128)	-0.200 (0.186)	0.120 (0.251)	-0.058 (0.085)	-0.157 (0.116)	0.072 (0.181)	0.046 (0.133)	-0.151 (0.191)	0.188 (0.262)
Post Met x Uncertainty Area	-0.055 (0.054)	-0.043 (0.074)	-0.055 (0.115)	-0.099 (0.065)	-0.061 (0.083)	-0.062 (0.119)	-0.013 (0.061)	0.061 (0.082)	0.100 (0.133)
Observations	944	528	240	944	528	240	944	528	240
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to equation (2). Panel A shows the results for all offenses, Panel B for burglary, Panel C for robbery and Panel D for homicide. The dependent variable is the number of crimes (that are brought to trial) per month and area. In columns (1) to (3), the treated area includes crimes located within 4 miles from Charing Cross, the uncertain area those located between 4 and 7 miles from Charing Cross, the control area those located more than 7 miles from Charing Cross and City of London those located in the City of London. In columns (4) to (6), the City of London is alternatively assigned to the treatment group after establishing their own police (1832) and in columns (7) to (9) to the uncertainty group. Regressions are based on manually geocoded data from the *Old Bailey Proceedings Online* and own transcriptions/calculations; see the text for details. Robust standard errors are shown in parentheses below the coefficient. * p<0.1, ** p<0.05, *** p<0.01.

Table 4. Robustness Checks – Alternative Aggregation Levels

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	1820-1839	1825-1835	1828-1832	1820-1839	1825-1835	1828-1832
Radius:	<i>Four miles from Charing Cross</i>			<i>Four miles from Charing Cross</i>		
Specification:	City of London = Control			City of London = Control		
	Extensive margin (1/0): By Week/area			Extensive margin (1/0): By Month/distance bands		
<i>Panel A. Total crime</i>						
Post Met x Treatment Area	-0.121*** (0.032)	-0.166*** (0.043)	-0.099 (0.070)	-0.110*** (0.033)	-0.145*** (0.044)	-0.087 (0.071)
Post Met x Uncertainty Area	0.037 (0.024)	0.017 (0.032)	-0.030 (0.043)	0.035 (0.027)	-0.000 (0.037)	-0.018 (0.047)
<i>Panel B. Burglary</i>						
Post Met x Treatment Area	-0.125*** (0.031)	-0.149*** (0.039)	-0.087* (0.051)	-0.103*** (0.032)	-0.116*** (0.040)	-0.063 (0.054)
Post Met x Uncertainty Area	0.028 (0.017)	0.005 (0.022)	-0.017 (0.025)	0.026 (0.020)	-0.014 (0.025)	-0.032 (0.030)
<i>Panel C. Robbery</i>						
Post Met x Treatment Area	-0.136*** (0.031)	-0.159*** (0.042)	-0.127* (0.065)	-0.134*** (0.032)	-0.161*** (0.043)	-0.168** (0.069)
Post Met x Uncertainty Area	0.018 (0.015)	0.029 (0.022)	-0.002 (0.031)	0.013 (0.017)	0.022 (0.025)	0.027 (0.030)
<i>Panel D. Homicide</i>						
Post Met x Treatment Area	0.028 (0.024)	0.000 (0.034)	0.064 (0.047)	0.013 (0.026)	-0.040 (0.036)	0.044 (0.051)
Post Met x Uncertainty Area	-0.008 (0.011)	-0.005 (0.016)	-0.007 (0.024)	0.001 (0.014)	-0.004 (0.019)	-0.008 (0.024)
Observations	4,164	2,332	1,060	4,248	2,376	1,080
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	No	No	No	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	No	No	No
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows robustness tests for the difference-in-differences specifications of Table 3 with alternative aggregation levels. In columns (1) to (3), the dependent variable is a dummy variable indicating whether there is any crime in given *week* and area. In columns (4) to (6), the dependent variable is a dummy variable indicating whether there is any crime in a given month and *distance band* from Charing Cross. Distance bands are circles around Charing Cross: less than 1 mile, 1-2 miles, 2-3 miles, ... , 13-14 miles and more than 14 miles. See Table 3 for further details on specification and data. Robust standard errors are shown in parentheses below the coefficient. * p<0.1, ** p<0.05, *** p<0.01.

Table 5. Summary Statistics – Daily Crime Reports

Variable	All			Before: 1828			After: 1830			After: 1830-1832		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
<i>Informations</i>												
Number of informations: All	3,232	0.513	0.982	800	0.791	1.154	816	0.489	0.875	2,432	0.421	0.899
Number of informations: Property	3,230	0.401	0.806	800	0.566	0.922	814	0.396	0.743	2,430	0.346	0.757
Number of informations: Violent	3,230	0.0477	0.242	800	0.101	0.362	814	0.0356	0.192	2,430	0.0300	0.182
Number of informations: Other	3,230	0.0647	0.282	800	0.124	0.386	814	0.0590	0.274	2,430	0.0453	0.236
Any informations: All	3,232	0.311	0.463	800	0.465	0.499	816	0.316	0.465	2,432	0.260	0.439
Any informations: Property	3,232	0.269	0.444	800	0.371	0.483	816	0.279	0.449	2,432	0.236	0.424
Any informations: Violent	3,232	0.0433	0.204	800	0.0862	0.281	816	0.0368	0.188	2,432	0.0292	0.168
Any informations: Other	3,232	0.0569	0.232	800	0.107	0.310	816	0.0527	0.224	2,432	0.0403	0.197
<i>Charges</i>												
Number of charges: All	3,232	6.382	3.590	800	5.281	3.154	816	6.161	3.419	2,432	6.744	3.651
Number of charges: Property	3,230	4.946	3.064	800	4.010	2.746	814	4.834	2.878	2,430	5.254	3.101
Number of charges: Violent	3,230	0.155	0.421	800	0.194	0.479	814	0.143	0.402	2,430	0.143	0.399
Number of charges: Other	3,230	1.284	1.355	800	1.077	1.271	814	1.199	1.306	2,430	1.352	1.375
Any charges: All	3,232	0.991	0.0943	800	0.983	0.131	816	0.990	0.0986	2,432	0.994	0.0783
Any charges: Property	3,232	0.976	0.153	800	0.949	0.221	816	0.979	0.143	2,432	0.985	0.121
Any charges: Violent	3,232	0.136	0.343	800	0.164	0.370	816	0.127	0.334	2,432	0.127	0.333
Any charges: Other	3,232	0.660	0.474	800	0.598	0.491	816	0.627	0.484	2,432	0.681	0.466
<i>Property stolen</i>												
Number of incidents	3,230	0.405	0.750	800	0.613	0.951	814	0.376	0.655	2,430	0.337	0.656
Any incident	3,232	0.292	0.455	800	0.394	0.489	816	0.295	0.456	2,432	0.258	0.438

NOTES– The table shows summary statistics for the analysis sample based on the daily crime reports described in more detail in Section 3.1. The first three columns show the number of observations, the mean and standard deviations for the different crime measures for the complete sample, the remaining columns separately for 1828 (one year pre-reform), 1830 (one year post-reform) and the years 1830-1832 (three years post-reform). The data was manually transcribed from the *Report or Account of the Proceedings of the several Police Offices* sourced from the National Archives (MEPO 4/12, 4/13, 4/15 and 4/17).

Table 6. Daily Crime Reports – Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	1828-1830	1828-1830	1828-1830	1828-1830	1828-1832	1828-1830	1828-1830	1828-1830
Crime type:	total	total	total	total	total	prop	viol	other
<i>Panel A. Any informations per day/station</i>								
Post Met Police	-0.149*** (0.024)	-0.149*** (0.022)	-0.148*** (0.022)	-0.148*** (0.022)	-0.206*** (0.019)	-0.090*** (0.022)	-0.049*** (0.012)	-0.055*** (0.013)
<i>Panel B. Number of informations per day/station</i>								
Post Met Police	-0.302*** (0.051)	-0.302*** (0.046)	-0.302*** (0.046)	-0.301*** (0.046)	-0.371*** (0.041)	-0.170*** (0.039)	-0.064*** (0.014)	-0.065*** (0.016)
<i>Panel C. Any 'stolen property' per day/station</i>								
Post Met Police	-0.098*** (0.024)	-0.098*** (0.023)	-0.099*** (0.023)	-0.099*** (0.023)	-0.137*** (0.019)	na	na	na
<i>Panel D. Number of charges per day/station</i>								
Post Met Police	0.879*** (0.164)	0.879*** (0.146)	0.881*** (0.144)	0.890*** (0.140)	1.471*** (0.120)	0.827*** (0.121)	-0.050** (0.022)	0.126** (0.061)
Observations	1,616	1,616	1,616	1,616	3,232	1,616	1,616	1,616
Office FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar week FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Day of week FE	No	No	No	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to equation (3). For a description of the underlying data, see Section 3.1. The dependent variable in Panel A is a dummy variable indicating whether there are *any* informations, in Panel B the number of informations, in Panel C a dummy variable indicating whether there are *any* stolen property reports and in Panel D the number of charges. The top of each column indicates the years included in the sample and where appropriate the crime category. Robust standard errors are shown in parentheses below the estimated coefficients. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Daily Crime Reports – Different Stages of Police Hiring

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample:	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832
Y:	<i>Number of charges</i>			<i>Any informations</i>			<i>Any 'stolen property'</i>
Crime type:	total	prop	viol	total	prop	viol	total
Post Met: 1830, January	0.030 (0.246)	0.214 (0.209)	-0.056 (0.037)	-0.114*** (0.038)	-0.045 (0.038)	-0.055*** (0.019)	-0.114*** (0.040)
Post Met: 1830, > January	1.177*** (0.163)	1.040*** (0.140)	-0.049** (0.023)	-0.160*** (0.024)	-0.106*** (0.023)	-0.046*** (0.012)	-0.094*** (0.025)
Post Met: 1831	1.382*** (0.151)	1.146*** (0.129)	-0.065*** (0.021)	-0.220*** (0.021)	-0.141*** (0.021)	-0.054*** (0.011)	-0.125*** (0.023)
Post Met: 1832	2.157*** (0.160)	1.783*** (0.137)	-0.034 (0.022)	-0.250*** (0.021)	-0.174*** (0.021)	-0.068*** (0.011)	-0.187*** (0.022)
<i>p-value</i>	<i>0.000</i>	<i>0.000</i>	<i>0.483</i>	<i>0.000</i>	<i>0.000</i>	<i>0.079</i>	<i>0.000</i>
Observations	3,232	3,232	3,232	3,232	3,232	3,232	3,232
Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to equation (3) but allowing for separate coefficients by time after the introduction of the Met (note that the second wave of hiring, mainly in the outer divisions, occurred in February 1830). For a description of the underlying data, see Section 3.1. The dependent variable in columns (1) to (3) is the number of charges, in columns (4) to (6) a dummy variable indicating whether there are any informations, and in column (7) a dummy variable indicating whether there are any stolen property. The top of each column indicates the years included in the sample and the crime category. The p-value corresponds to the test of equality of all four shown coefficients. Robust standard errors are shown in parentheses below the estimated coefficients. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Summary Statistics for County-Level Analysis

	All Counties: 1832-1865			Early Reformers: N= 16			Mid-Reformers: N= 9			Late Reformers: N = 23		
	N	mean	SD	N	mean	SD	N	mean	SD	N	mean	SD
Fiscal Start Year	1,632	1850	8	544	1840	0	306	1848	6	782	1857	0
Force existence all year	1,632	0.45	0.50	544	0.74	0.44	306	0.50	0.50	782	0.24	0.42
Charges	1,632	367	505	544	569	731	306	304	321	782	251	276
Violent charges	1,440	62	86	480	94	128	270	51	53	690	43	45
Property charges	1,440	279	392	480	434	567	270	228	247	690	190	218
Other charges	1,440	26	41	480	40	61	270	23	27	690	18	20
Charge rate (per 1000)	1,632	1.79	1.66	544	1.88	0.96	306	2.49	2.95	782	1.45	1.19
Violent charge rate (per 1000)	1,440	0.30	0.26	480	0.31	0.14	270	0.41	0.45	690	0.26	0.19
Property charge rate (per 1000)	1,440	1.33	1.30	480	1.41	0.79	270	1.85	2.28	690	1.07	0.94
Other charge rate (per 1000)	1,440	0.13	0.14	480	0.14	0.10	270	0.19	0.24	690	0.11	0.09
<i>Snap Shot Variables</i>												
England	48	0.75	0.44	16	0.88	0.34	9	0.67	0.50	23	0.70	0.47
Wales	48	0.25	0.44	16	0.13	0.34	9	0.33	0.50	23	0.30	0.47
Number parishes	48	190	156	16	236	176	9	154	127	23	173	151
Acres	48	642642	347403	16	733137	277302	9	507433	244367	23	632598	412494
Population (1858 Jud.Stats.)	48	191492	153919	16	272118	200542	9	132879	93997	23	158340	112680
People per police (initial)	47	2857	2493	15	3098	1974	9	3074	3223	23	2615	2572
Share efficient (<1500) at creation	47	0.21	0.41	15	0.20	0.41	9	0.33	0.50	23	0.17	0.39
People per police (1858)	48	1700	632	16	1554	377	9	1850	1215	23	1742	440
<i>1851 Census Variables</i>												
Farmer (share)	48	0.15	0.09	16	0.12	0.08	9	0.16	0.10	23	0.18	0.09
Male (share)	48	0.48	0.01	16	0.48	0.01	9	0.48	0.02	23	0.49	0.01
Married (share)	48	0.33	0.01	16	0.34	0.01	9	0.33	0.01	23	0.33	0.02
Native (share)	48	0.98	0.02	16	0.98	0.02	9	0.99	0.01	23	0.98	0.02
Employed (share)	48	0.67	0.03	16	0.69	0.03	9	0.67	0.02	23	0.67	0.03
Out of labor force (share)	48	0.33	0.03	16	0.31	0.03	9	0.33	0.02	23	0.33	0.03
Age 0-15 (share)	48	0.38	0.01	16	0.38	0.01	9	0.38	0.01	23	0.38	0.01
Age 16-25 (share)	48	0.18	0.01	16	0.18	0.01	9	0.18	0.01	23	0.18	0.01
Age 26-35 (share)	48	0.14	0.01	16	0.14	0.01	9	0.14	0.01	23	0.14	0.01
Age 36-45 (share)	48	0.11	0.00	16	0.11	0.00	9	0.11	0.01	23	0.11	0.00
Age 46-55 (share)	48	0.08	0.00	16	0.08	0.00	9	0.08	0.01	23	0.08	0.00
Age 56-65 (share)	48	0.06	0.01	16	0.06	0.01	9	0.06	0.01	23	0.06	0.01
Age 66 plus (share)	48	0.05	0.01	16	0.05	0.01	9	0.05	0.01	23	0.05	0.01

NOTES – The table shows summary statistics for the analysis sample of counties for the county force roll-out analysis from 1832 - 1865. Charges by crime type were unavailable for 1832, 1833, 1840, 1852. See Sections 4.1 and 4.2 for details.

Table 9. Baseline Effect of Creating Any County Police Force on Crime

	(1)	(2)	(3)	(4)
	Dep. Variable: Log (Number of Charges)		Dep. Variable: Log (Charge Rate)	
	First Treated Year Defined as Police Force Existed for:			
	Any of year	All of year	Any of year	All of year
<i>Panel A: All Charges</i>				
Force	0.00764 [0.037]	-0.02413 [0.033]	0.01528 [0.038]	-0.01772 [0.034]
Observations	1,632	1,632	1,632	1,632
R-squared	0.959	0.959	0.891	0.891
<i>Panel B: Violent Charges</i>				
Force	-0.01776 [0.053]	-0.03078 [0.050]	-0.00558 [0.054]	-0.01994 [0.051]
Observations	1,431	1,431	1,431	1,431
R-squared	0.894	0.894	0.712	0.712
<i>Panel C: Property Charges</i>				
Force	0.02926 [0.044]	0.01707 [0.042]	0.04109 [0.043]	0.02737 [0.042]
Observations	1,440	1,440	1,440	1,440
R-squared	0.958	0.958	0.896	0.896
<i>Panel D: Other Charges</i>				
Force	-0.10892 [0.086]	-0.17869** [0.081]	-0.09496 [0.088]	-0.16599* [0.083]
Observations	1,356	1,356	1,356	1,356
R-squared	0.771	0.772	0.509	0.511

NOTES – The table presents the results of the baseline difference-in-differences specification (see equation (4)), where the variable of interest *Force* is equal to one for a county *c* in any year *t* after which a county police force has been created. The year of police force formation is defined as the first year with a police force for any of the year in columns (1) and (3) and a police force for all of the year in columns (2) and (4). All specifications include county and year fixed effects. The baseline sample includes 48 counties for the years 1832-1865. Standard errors are clustered by county and shown in brackets below the coefficient. *** p<0.01, ** p<0.05, * p<0.1

Table 10. Police Force in Name Only? Heterogeneity by Police Force Efficiency

	(1)	(2)	(3)	(4)	(5)
	Dep. Variable: Log (Number of Charges)				
	Police Force Efficiency Defined According to the Below Thresholds in the Number of <i>People Per Policeman</i> (upon police force creation)				
	1,500	1,750	2,000	2,250	2,500
<i>Panel A: All Charges</i>					
Force Efficient	-0.18991*** [0.062]	-0.12698** [0.058]	-0.11820** [0.055]	-0.10872** [0.049]	-0.07901 [0.048]
Force Inefficient	0.02208 [0.043]	0.03969 [0.051]	0.05297 [0.057]	0.08827 [0.068]	0.06619 [0.075]
<i>Panel B: Violent Charges</i>					
Force Efficient	-0.18284* [0.104]	-0.13354 [0.080]	-0.12870* [0.070]	-0.13043** [0.062]	-0.09254 [0.062]
Force Inefficient	-0.00170 [0.058]	0.02032 [0.068]	0.03413 [0.078]	0.08146 [0.094]	0.04846 [0.104]
<i>Panel C: Property Charges</i>					
Force Efficient	-0.14252** [0.065]	-0.07332 [0.069]	-0.06334 [0.066]	-0.05007 [0.058]	-0.02824 [0.057]
Force Inefficient	0.06412 [0.050]	0.08034 [0.055]	0.08960 [0.060]	0.11347 [0.073]	0.10135 [0.083]
<i>Panel D: Other Charges</i>					
Force Efficient	-0.24291** [0.112]	-0.14565 [0.099]	-0.12481 [0.091]	-0.15664* [0.092]	-0.13200 [0.088]
Force Inefficient	-0.15115* [0.090]	-0.19248* [0.102]	-0.21896** [0.109]	-0.19753* [0.116]	-0.25308* [0.127]

NOTES – This table presents the results of the baseline difference-in-differences specification (see Table 9), where the variables of interest - Force Efficient and Force Inefficient - are equal to one for a county c in any year t after which an efficient or inefficient police force has been created. Efficiency is defined according to the number of people per officer, and varies as indicated at the top of each column. The year of police force creation is defined as the first year with a police force for all of the fiscal year. All specifications include county and year fixed effects. The baseline sample includes 48 counties for the years 1832-1865. Standard errors are clustered by county and shown in brackets below the coefficient. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

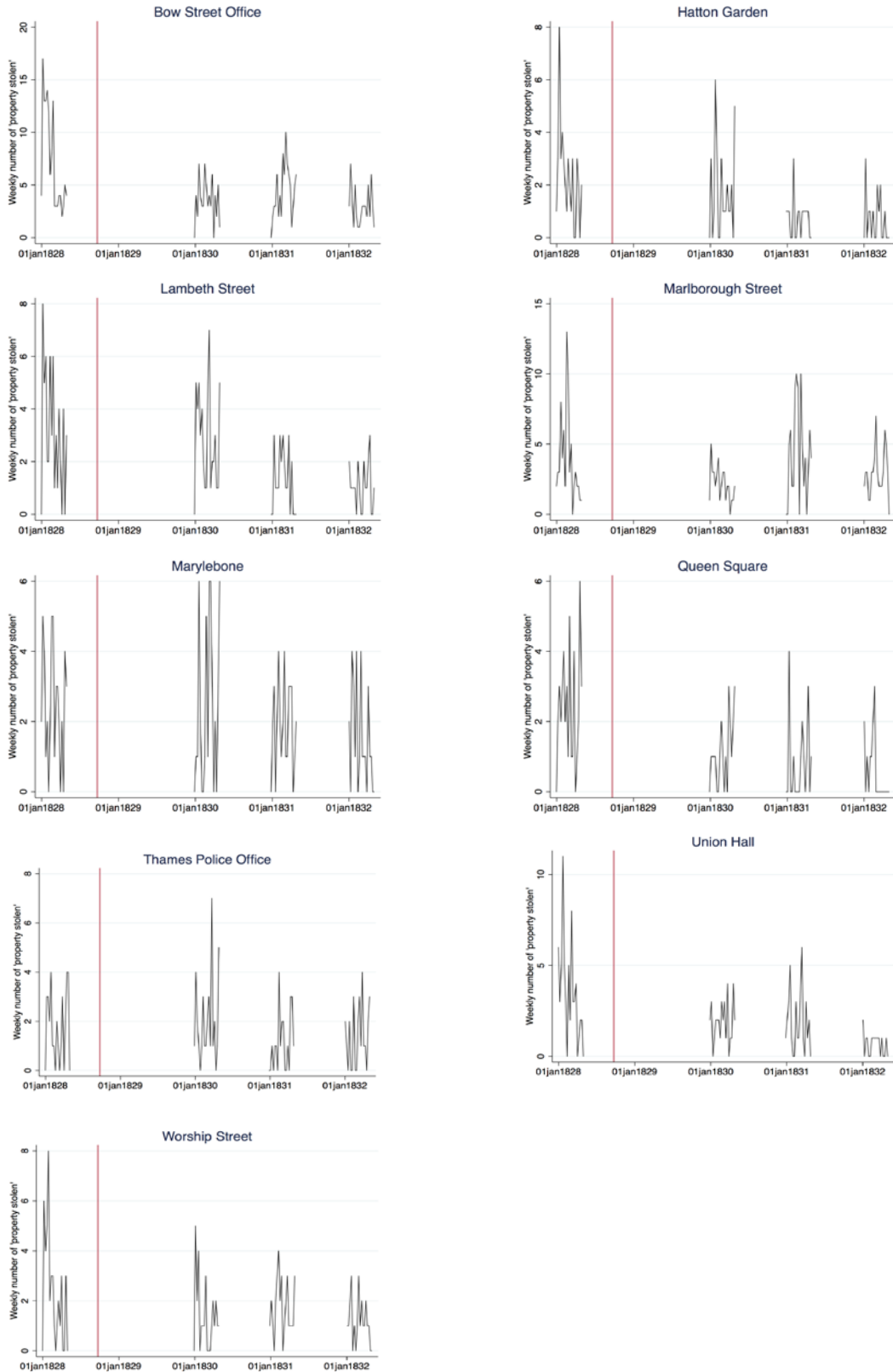
Table 11. Spillover Effects of Neighboring Police Forces

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Log (Number charges)			Log (Number violent charges)			Log (Number property charges)			Log (Number other charges)		
Own Force Efficient	-0.190*** [0.062]	-0.190*** [0.069]	-0.184*** [0.060]	-0.183* [0.104]	-0.183* [0.108]	-0.182* [0.101]	-0.143** [0.065]	-0.143** [0.068]	-0.140** [0.063]	-0.243** [0.112]	-0.243** [0.111]	-0.244** [0.109]
Own Force Inefficient	0.022 [0.043]	0.021 [0.044]	-0.006 [0.045]	-0.002 [0.058]	-0.003 [0.059]	-0.030 [0.059]	0.064 [0.050]	0.062 [0.049]	0.040 [0.052]	-0.151* [0.090]	-0.151* [0.088]	-0.166* [0.089]
Any Neighboring Force		0.189*** [0.059]			0.109 [0.112]			0.253*** [0.055]			-0.106 [0.096]	
Any Neighboring Efficient Force			-0.158** [0.060]			-0.192** [0.074]			-0.149** [0.068]			-0.106 [0.086]
Any Neighboring Inefficient Force			0.131** [0.055]			0.064 [0.137]			0.199*** [0.054]			0.013 [0.122]
Observations	1,632	1,632	1,632	1,431	1,431	1,431	1,440	1,440	1,440	1,356	1,356	1,356
R-squared	0.960	0.961	0.961	0.894	0.895	0.896	0.959	0.960	0.960	0.772	0.772	0.773

NOTES – The table shows the regression results when estimating the effects of having a police force (at all or one that is efficient or inefficient) in a neighboring county. An efficient force (whether it is a county’s own or a neighbor’s police force) is defined as a police force with less than 1,500 people per officer. Middlesex, though excluded from the analysis sample, is classified as an efficient neighbor for those sharing a border after 1829. The year of police force formation is defined as the first year with a police force for all of the year. All specifications include county and year fixed effects. The baseline sample includes 48 counties for the years 1832-1865. Standard errors are clustered by county and shown in brackets below the coefficient. *** p<0.01, ** p<0.05, * p<0.1

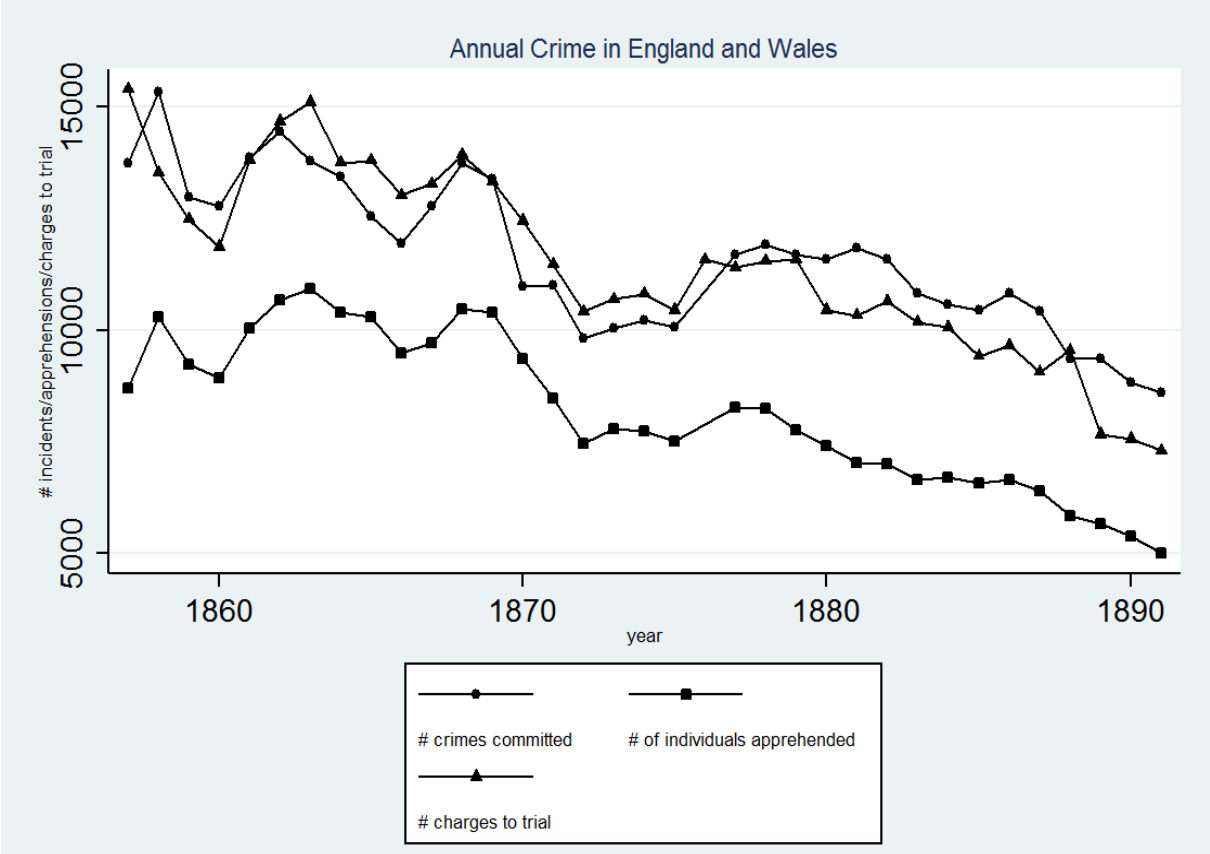
Appendix A: Additional Figures and Tables

Appendix Figure A1. Weekly 'Property Stolen' Reports by Police Office



NOTES – The figures show the weekly number of property stolen incidents for each Police Office. In each panel, the red horizontal line represents the date of the introduction of the Metropolitan Police. The figures are based on manual transcribed data from the *Report or Account of the Proceedings of the several Police Offices* sourced from the National Archives (MEPO 4/12, 4/13, 4/15 and 4/17).

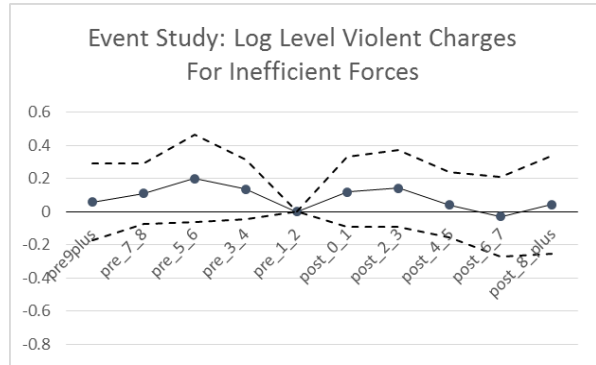
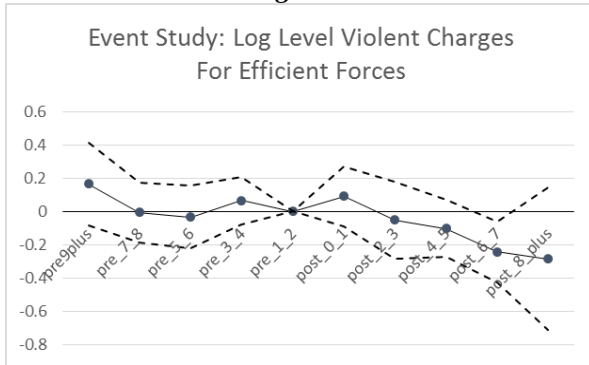
Appendix Figure A2. Charges Brought to Trial as a Crime Proxy (County Analysis)



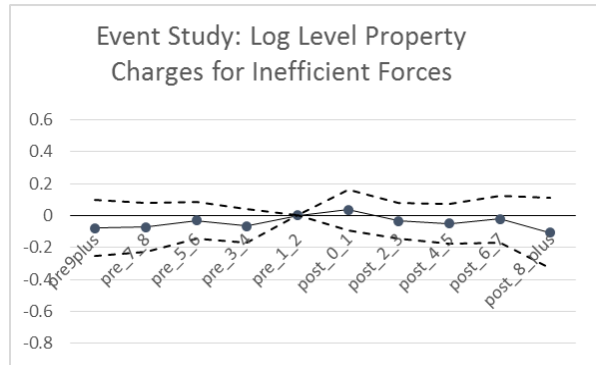
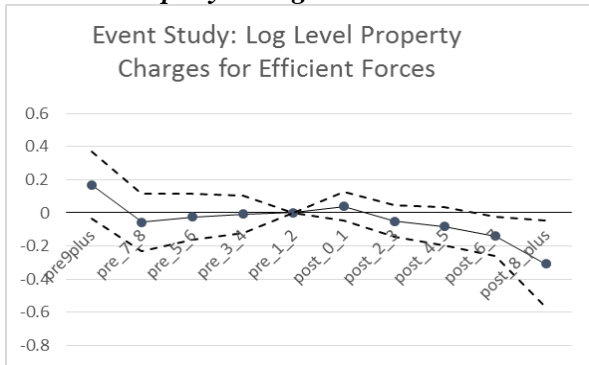
NOTES – The figure presents the national annual number of crimes committed, charges brought to trial, and individuals apprehended in all England and Wales counties, excluding Middlesex, York, Suffolk, and Sussex from 1857 to 1891. The number of charges to trial is the main outcome variable used in the county-level analysis, as it is the only measure available prior to 1857. This figure demonstrates that it is a potentially good proxy for crime. See Section 4.1 for details and data sources.

Appendix Figure A3. Event-Study of Efficient/Inefficient County Police Forces, By Crime Type

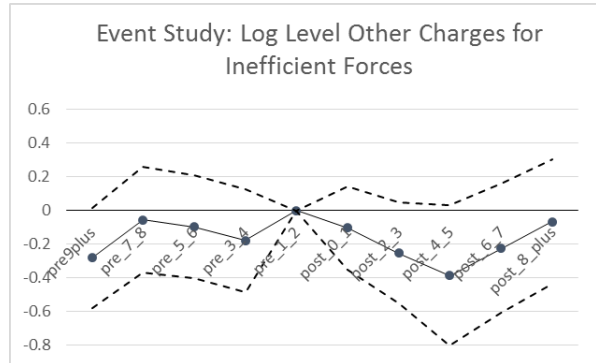
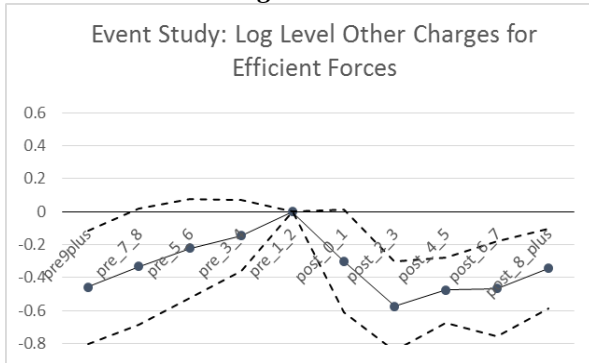
Panel A: Violent Charges



Panel B: Property Charges



Panel C: Other Charges



NOTES – The figures shows the results from event-study specifications for the county-level analysis separately by crime type. See Figure 13 for details.

Appendix Table A1. Metropolitan Police – Initial Hiring in Two Stages

Police division(s):	Police office(s):	(1)	(2)	(3)	(4)	(5)
Metropolitan Police	Pre-existing Police	All hires: 21 Sep 1829 - 27 Mar 1830 all	All hires: 21 Sep 1829 - 27 Mar 1830 service length >= 250 days	Early hires: 21 Sep 1829 - 31 Jan 1830	Late hires: 01 Feb 1830 - 27 Mar 1830	Category Early/late
<i>Panel A. Separately by division</i>						
A	Queen Square	125	60	49	11	early
B	Queen Square	261	136	111	25	early
C	Marlborough Street	300	146	132	14	early
D	Marylebone	280	150	126	24	early
E	Hatton Garden	263	155	139	16	early
F	Bow Street	289	137	118	19	early
G	Hatton Garden, Worship Street	226	156	24	132	late
H	Lambeth Street, Worship Street	221	125	18	107	late
K	Lambeth Street, Thames	210	164	19	145	late
L	Queen Square	202	154	20	134	late
M	Union Hall	207	146	16	130	late
N	Hatton Garden, Worship Street	95	51	0	51	late
P	Union Hall	231	140	12	128	late
R	Union Hall	39	30	0	30	late
S	Marylebone	260	158	40	118	late
T	Queen Square	9	6	0	6	late
V	Queen Square	9	6	0	6	late
<i>Panel B. Aggregated by division-office</i>						
C	Marlborough Street	300	146	132	14	early
F	Bow Street	289	137	118	19	early
EGHKN	Hatton Garden, Lambeth Street, Worship Street, (Thames)	1015	651	200	451	mixed
ABLTV	Queen Square	606	362	180	182	mixed
DS	Marylebone	540	308	166	142	mixed
MPR	Union Hall	477	316	28	288	late

NOTES - Panel A shows the number of hires by the Metropolitan police separately for each police division (of the Metropolitan Police), Panel B for aggregated police divisions by police office (of the pre-existing police). Matching of police divisions to police offices is based on the 1832 Daily Crime Reports listing the division letter next to each entry (National Archives, MEPO 4/17). The number of police officers who joined the Met is based on data from the first 3000 police warrant numbers from the *Register of recruits into the Metropolitan Police* (National Archives, MEPO 4/31). Before February 1830 includes the time period from 21 September 1829 until 31 January 1830; After February 1830 includes the time period from 01 February 1830 until 27 March 1830. The column 'category' presents our own assessment of the timing of the initial hiring by office.

Appendix Table A2. Descriptive Statistics - Old Bailey Proceedings Data

Variable	within 7-miles radius				City of London				outside 7-miles radius			
	1820-1829	1829-1839	1825-1835	1828-1832	1820-1829	1829-1839	1825-1835	1828-1832	1820-1829	1829-1839	1825-1835	1828-1832
<i>Crime</i>												
Number of crime incidents	801	545	698	212	135	104	133	50	82	86	80	35
Burglary	353	236	273	48	53	54	58	11	50	24	31	10
Manslaughter	37	89	76	30	8	18	20	11	3	11	6	5
Murder	64	37	61	20	4	6	5	4	9	10	10	3
Robbery	347	183	288	114	70	26	50	24	20	41	33	17
Distance to Charing Cross (miles)	1.876	2.149	2.045	1.912	1.686	1.673	1.645	1.647	9.973	10.391	10.208	10.59
Distance to Charing Cross (in km)	3.019	3.458	3.291	3.076	2.713	2.691	2.647	2.65	16.046	16.719	16.425	17.039
Number of co-defendants	1.446	1.255	1.4	1.377	1.274	1.231	1.286	1.34	1.598	1.337	1.6	1.771
Days crime to session start	33.842	28.017	32.133	31.705	62.597	24.548	57.174	35.143	64.756	89.419	51.300	34.143
<i>Police</i>												
Any police witness (1/0)	0.815	0.822	0.814	0.788	0.859	0.808	0.85	0.86	0.805	0.802	0.762	0.771
# of police first 5 witnesses who are:	1.446	1.437	1.473	1.547	1.504	1.442	1.519	1.48	1.293	1.256	1.262	1.4
Constable	0.31	0.372	0.288	0.283	0.289	0.25	0.308	0.26	0.537	0.581	0.613	0.829
Policeman	0.001	0.745	0.322	0.297	0.000	0.365	0.135	0.26	0.000	0.163	0.062	0.000
Watchman	0.408	0.033	0.297	0.406	0.407	0.308	0.391	0.28	0.11	0.163	0.138	0.086
Other (pre-Met type)	0.544	0.114	0.401	0.396	0.711	0.298	0.534	0.58	0.476	0.256	0.338	0.371
Other (post-Met type)	0.021	0.141	0.076	0.113	0.03	0.173	0.083	0.1	0.000	0.081	0.025	0.000
Missing	3.715	3.594	3.616	3.505	3.563	3.606	3.549	3.52	3.878	3.756	3.825	3.714
Police at crime scene (1/0)	0.197	0.277	0.259	0.321	0.296	0.279	0.263	0.34	0.073	0.093	0.1	0.114

NOTES - The table shows descriptive statistics for the geocoded crime data from the Old Bailey Proceedings Online (see Section 3.1 for details). One observation is one crime incident (trial). Except for the number of crime incidents, the table reports means for each respective sample. The sample restrictions for each column are indicated at the top of the column.

Appendix Table A3. Sensitivity Analysis for Difference-in-Differences Estimation (Old Bailey Data)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	1820-1839	1828-1832	1820-1839	1828-1832	1820-1839	1828-1832	1820-1839	1828-1832
Specification:	Area specific trend		Exclude "long streets" locations		Include only "no issue" locations		Exclude missing crime dates	
<i>Panel A. Total crime</i>								
Post Met x Treatment Area	-2.959*** (0.612)	-1.602*** (0.584)	-2.033*** (0.300)	-1.583*** (0.491)	-0.874*** (0.252)	-0.669* (0.365)	-2.227*** (0.346)	-1.561*** (0.521)
Post Met x Uncertainty Area	0.279 (0.267)	0.467 (0.403)	0.218* (0.126)	0.092 (0.221)	0.055 (0.109)	0.031 (0.188)	0.233* (0.134)	0.139 (0.238)
<i>Panel B. Burglary</i>								
Post Met x Treatment Area	-1.719*** (0.444)	0.066 (0.370)	-0.978*** (0.239)	-0.358 (0.267)	-0.314* (0.179)	-0.340 (0.217)	-0.947*** (0.256)	-0.397 (0.267)
Post Met x Uncertainty Area	0.119 (0.181)	-0.114 (0.121)	0.170* (0.098)	0.017 (0.104)	0.132* (0.079)	-0.040 (0.105)	0.166 (0.103)	-0.022 (0.104)
<i>Panel C. Robbery</i>								
Post Met x Treatment Area	-0.935** (0.391)	-1.449*** (0.476)	-1.020*** (0.188)	-1.222*** (0.385)	-0.690*** (0.152)	-0.521* (0.304)	-1.292*** (0.219)	-1.297*** (0.428)
Post Met x Uncertainty Area	0.227 (0.169)	0.565* (0.300)	0.087 (0.076)	0.128 (0.143)	-0.018 (0.065)	0.129 (0.114)	0.123 (0.083)	0.228 (0.162)
<i>Panel D. Homicide</i>								
Post Met x Treatment Area	-0.306 (0.251)	-0.219 (0.373)	-0.036 (0.119)	-0.003 (0.227)	0.131 (0.095)	0.192 (0.181)	0.011 (0.126)	0.132 (0.254)
Post Met x Uncertainty Area	-0.067 (0.098)	0.017 (0.157)	-0.039 (0.053)	-0.053 (0.113)	-0.059 (0.043)	-0.058 (0.100)	-0.056 (0.054)	-0.068 (0.114)
Observations	944	240	944	240	944	240	944	240
Year, month, and area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows sensitivity analyses of the difference-in-differences estimation shown in columns (1) to (3) of Table 3 (see notes in that table for details on the baseline specification). The estimation windows are shown at the top of each column. Columns (1) to (2) add an area-specific annual trend; columns (3) and (4) exclude locations that were identified as “long streets” only (and potentially misclassified as treated); columns (5) and (6) exclude locations for which we had to refer to historical maps; columns (7) and (8) exclude observations for which the date of the actual crime is missing in the data and proxied by the session start date instead in the baseline estimation. Robust standard errors are shown in parentheses below the coefficient. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A4. Robustness Checks for Pre-Post Estimation (Daily Crime Reports)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Sample:	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832
Crime type:	total	total	total	total	total	total	total	total	total	total	total
Specification:				<u>Drop one office at the time:</u>						<u>Weekly</u>	
	Bow Street	Hatton Garden	Lambeth Street	Marlye-bone	Marlborough Street	Queen Square	Union Hall	Worship Street	All weeks	Complete weeks	Log outcome
<i>Panel A. Any informations</i>											
Post Met Police	-0.18***	-0.15***	-0.14***	-0.13***	-0.15***	-0.15***	-0.12***	-0.17***	-0.22***	-0.13***	-
	(0.024)	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)	(0.034)	(0.047)	
<i>Panel B. Any 'stolen property'</i>											
Post Met Police	-0.09***	-0.10***	-0.12***	-0.11***	-0.1***	-0.09***	-0.09***	-0.10***	-0.73***	-0.71***	-
	(0.024)	(0.025)	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.119)	(0.165)	
<i>Panel C. Number of charges</i>											
Post Met Police	0.72***	0.87***	0.89***	0.91***	1.14***	0.76***	0.88***	0.96***	8.63***	3.98***	0.28***
	(0.150)	(0.148)	(0.152)	(0.153)	(0.147)	(0.152)	(0.150)	(0.142)	(1.003)	(1.333)	(0.045)
Observations	1,414	1,414	1,414	1,414	1,414	1,414	1,414	1,414	576	240	576
Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar week											
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows robustness checks for the pre-post estimation from the Daily Crime Reports presented in Table 6. Columns (1) to (8) drop one office at the time from the regression sample; the excluded office is indicated at the top of each column. Columns (9) to (11) present the results when the data is aggregated at the weekly instead of the daily level for all weeks, complete weeks only and for all weeks but using the log instead of the level number of charges. Robust standard errors are shown in parentheses below the coefficient. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A5. Dates of County Police Force Formation and Size of Initial Force

County Name	Start Month	Start Year	Fiscal Start Year	First Whole (fiscal) Year	Initial Force Size	Initial People per Police
Gloucester	11	1839	1840	1841	250	1089
Norfolk	11	1839	1840	1841	133	2137
Wilts	11	1839	1840	1841	201	1140
Lancaster	12	1839	1840	1841	500	1184
Leicester	12	1839	1840	1841	25	5807
Southampton	12	1839	1840	1841	106	2024
Worcester	12	1839	1840	1841	41	4159
Northampton	1	1840	1840	1841	29	4694
Essex	2	1840	1840	1841	116	2144
Bedford	3	1840	1840	1841	47	1837
Durham	3	1840	1840	1841	66	2523
Salop	3	1840	1840	1841	23	8198
Nottingham	4	1840	1840	1841	42	3988
Denbigh	5	1840	1840	1841	28	2986
Montgomery	7	1840	1840	1841	26	2557
Stafford	.	1840	1840	1841	.	.
Hertford	4	1841	1841	1842	71	1819
Glamorgan	.	1841	1841	1842	39	3665
Carmarthen	7	1843	1843	1844	57	1694
Cardigan	3	1844	1844	1845	18	3821
Rutland	6	1848	1848	1849	2	11248
Surrey	1	1851	1851	1852	71	1532
Cambridge	11	1851	1852	1853	70	1252
Berks	2	1856	1856	1857	94	1315
Somerset	5	1856	1856	1857	267	1316
Flint	11	1856	1857	1858	26	2494
Dorset	12	1856	1857	1858	110	1398
Brecon	1	1857	1857	1858	29	1903
Cornwall	1	1857	1857	1858	179	1687
Cumberland	1	1857	1857	1858	60	2819
Devon	1	1857	1857	1858	300	1421
Hereford	1	1857	1857	1858	45	2195
Kent	1	1857	1857	1858	231	1355
Lincoln	1	1857	1857	1858	207	1651
Radnor	1	1857	1857	1858	10	2464
Westmoreland	1	1857	1857	1858	14	3422
Bucks	2	1857	1857	1858	102	1531
Warwick	2	1857	1857	1858	133	1373
Derby	3	1857	1857	1858	154	1662
Monmouth	3	1857	1857	1858	49	2352
Oxford	3	1857	1857	1858	10	14062
Anglesey	4	1857	1857	1858	16	3420
Carnarvon	4	1857	1857	1858	37	2558
Chester	4	1857	1857	1858	173	1703
Huntingdon	4	1857	1857	1858	41	1572
Northumberland	4	1857	1857	1858	61	2811
Pembroke	6	1857	1857	1858	33	2242
Merioneth	9	1857	1857	1858	19	2046
Middlesex						Excluded since London cannot be separated.
York						
Sussex						Excluded since rural counties included multiple jurisdictions with different force start dates;
Suffolk						but crime data was not available for same sub-jurisdiction.

NOTES – The table shows the date of police force formation by county, the first fiscal year with an existent police force, the initial size of the police force as well as the initial people-per-police ratio. For two counties, Stafford and Glamorgan, the *month* of police force formation is missing in our data. We treat this as January of that year. See Sections 4.1 and 4.2 for more detail on the data.

Appendix Table A6. Offence Categories in the Judicial Statistics

Classification in this paper	Judicial Statistics: Broad Category	Judicial Statistics: Specific Offenses
Violent	Offences Against Person	Murder, Attempted Murder, Shooting/Stabbing/Wounding to Maim, Manslaughter, Attempts to Procure Miscarriage, Concealing Birth of Infant, Sodomy, Assaults to Commit Sodomy, Rape, Carnal Abuse, Assault with Intent to Carnally Abuse, Abduction, Bigamy, Child Stealing, Assaults (and Inflicting Bodily Harm), Assaults (Common), Assaults of Peace Officers.
	Offences Against Property, with Violence	Sacrilege, Burglary, Burglary (attended with Violence to Persons), Housebreaking, Breaking into Shops/Warehouses and Stealing, Breaking within Curtilage of Dwelling Houses and Stealing, Robbery, Robbery and Attempted Robbery by Persons Armed in Company, Robbery (Attended with Wounding and Cutting), Obtaining Property by THreats to Accuse of Unnatural Crimes, Assaults to Rob and Demand Property with Menace, Stealing in Dwelling Houses such that Persons Therein Are Put in Fear, Sending Menacing Letters to Extort Money, Piracy
Property	Offences Against Property, without Violence	Cattle Stealing, Horse Stealing, Sheep Stealing, Larceny to Value of £5 in Dwelling Houses, Larceny from Person, Larceny by Servants, Simple Larceny, Stealing from Vessels, Stealing Goods in the Process of Manufacture, Stealing Fixtures/Trees/Shrubs, Misdemeanors with intent to steal, Embezzlement, Stealing and Receiving Letters Stolen from the Post Office by Servants, Receiving Stolen Goods, Frauds and Attempts to Defraud
Other	Malicious Offences Against Property	Setting Fire to a Dwelling or Shop (Persons therein), Setting Fire to a House/Warehouse/Cornstack, Setting Fire to Crops/Plantations/Heath, Attempted Arson, Riot and Feloniously Demolishing Buildings/Machinery, Destroying Silk/Woolen Goods in Manufacturing Process, Destroying Hop-binds/Trees/Shrubs, Killing and Maiming Cattle, Sending Threatening Letters to Commit Arson, Other Malicious Offences
	Forgery and Offences Against the Currency	Forging and Uttering Forged Bank of England Notes, Forging and Uttering Other Forged Instruments, Having in Possession Forged Bank of England Notes, Counterfeiting Current Gold and Silver Coins, Having in Possession Implements for Coining, Buying and Putting Off Counterfeit Gold and Silver Coin, Uttering and Having in Possession Counterfeit Gold and Silver Coin
	Offences not Included in the Above Classes	High Treason and Feloniously Compassing to Levy War, Assembling Armed to Aid Smugglers, Assaulting Officers Employed to Prevent Smuggling, Deer Stealing and Feloniously Wounding Deer Keepers, Being Out Armed/Taking Game/And Assaulting Game Keepers, Taking and Destroying Fish in Enclosed Water, Being at Large Under Sentence of Transportation, Prison Breaking, Harboursing and Aiding the Escape of Felons, Riot, Sedition, Breach of the Peace, Refusing to Aid Peace Officers, Keeping Disorderly Houses, Indecently Exposing the Person, Felonies Not Included Above, Misdemeanors Not Included Above

NOTES – The table lists the offense categories as in the Judicial Statistics and as classified by us to define the outcome variables for the county level analysis. See Sections 4.1 and 4.2 for details.

Appendix Table A7. Sensitivity Analysis for Efficient and Inefficient County Police Force Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Sample Years:	1832-1865	1832-1865	1832-1865	1832-1865	1832-1865	1832-1865	1832-1865	1835-1862	1832-1849	1850-1865	1832-1865
Sample Counties	all	all	all	all	all	all	all	all	all	all	English
<i>Panel A. Dependent Variable = Log Total Charges</i>											
Force Efficient	-0.190***	-0.207***	-0.202***	-0.190***	-0.190***	-0.207***	-0.167**	-0.153**	-0.088**	-0.074	-0.099*
	[0.062]	[0.062]	[0.062]	[0.062]	[0.062]	[0.070]	[0.063]	[0.062]	[0.035]	[0.072]	[0.056]
Force Inefficient	0.022	0.043	0.051	0.022	0.022	0.034	0.009	0.020	0.008	0.005	-0.024
	[0.043]	[0.046]	[0.046]	[0.043]	[0.043]	[0.043]	[0.039]	[0.044]	[0.061]	[0.073]	[0.046]
<i>Panel B. Dependent Variable = Log Violent Charges</i>											
Force Efficient	-0.183*	-0.199**	-0.184**	-0.183*	-0.183*	-0.220**	-0.180*	-0.134	-0.049	-0.094	-0.114
	[0.104]	[0.086]	[0.086]	[0.104]	[0.104]	[0.103]	[0.103]	[0.119]	[0.194]	[0.101]	[0.104]
Force Inefficient	-0.002	0.040	0.055	-0.002	-0.002	0.013	-0.003	0.005	0.068	-0.103	-0.058
	[0.058]	[0.058]	[0.056]	[0.058]	[0.058]	[0.058]	[0.055]	[0.060]	[0.089]	[0.108]	[0.054]
<i>Panel C. Dependent Variable = Log Property Charges</i>											
Force Efficient	-0.143**	-0.152**	-0.148**	-0.143**	-0.143**	-0.147*	-0.122*	-0.124**	-0.025	-0.026	-0.046
	[0.065]	[0.067]	[0.069]	[0.065]	[0.065]	[0.077]	[0.064]	[0.061]	[0.064]	[0.069]	[0.054]
Force Inefficient	0.06412	0.087*	0.095*	0.064	0.06412	0.071	0.049	0.05852	0.050	0.038	0.008
	[0.050]	[0.050]	[0.051]	[0.050]	[0.050]	[0.049]	[0.048]	[0.050]	[0.069]	[0.071]	[0.055]
<i>Panel D. Dependent Variable = Log Other Charges</i>											
Force Efficient	-0.243**	-0.245**	-0.240**	-0.243**	-0.243**	-0.253**	-0.221*	-0.285**	-0.316	-0.212**	-0.224*
	[0.112]	[0.110]	[0.111]	[0.112]	[0.112]	[0.114]	[0.115]	[0.118]	[0.249]	[0.101]	[0.115]
Force Inefficient	-0.151*	-0.143	-0.141	-0.151*	-0.151*	-0.148	-0.169*	-0.171*	-0.144	-0.141	-0.103
	[0.090]	[0.091]	[0.093]	[0.090]	[0.090]	[0.092]	[0.085]	[0.085]	[0.111]	[0.123]	[0.092]
+ population	no	yes	yes	no	no	no	no	no	no	no	no
+ Eng. and region dummies	no	no	yes	no	no	no	no	no	no	no	no
+ national linear trend	no	no	no	yes	yes	no	no	no	no	no	no
+ national quad. Trend	no	no	no	no	yes	no	no	no	no	no	no
+ region specific trend	no	no	no	no	no	yes	no	no	no	no	no
+ > median acre trend	no	no	no	no	no	no	yes	no	no	no	no

NOTES – This table presents sensitivity analyses of the baseline difference-in-differences specification (see Table 10), where the variables of interest Force Efficient and Force Inefficient are equal to one for a county c in any year t after which an efficient or inefficient force has been created. A force is efficient if it has less than 1,500 people per officer. The year of force creation is defined as the first year with a force for all of the year. All specifications include county and year fixed effects. The baseline sample includes 48 counties for the years 1832-1865. The different specifications are indicated at the top and the bottom of the table, respectively. Standard errors are clustered by county and shown in brackets below the estimated coefficients. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A8. Determinants of the Timing of County Police Force Formation

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Early adopter? 1840 Cross-section				All Counties: 1840 (earliest possible) -1857 (last possible) adoption				
	Dependent Variable = adoption (1 in year of adoption)								
Lag1: Charge rate	0.0098 [0.032]	-0.0123 [0.016]	-0.0053 [0.022]		-0.0047 [0.004]	-0.0061 [0.005]	-0.0035 [0.005]	-0.0042 [0.005]	
Lag1: Any neighboring force			-0.1063 [0.197]	-0.1051 [0.239]			-0.0703* [0.040]		
Lag1: Any efficient neighboring force								-0.0030 [0.025]	-0.0074 [0.027]
Lag1: Any inefficient neighboring force								-0.0671* [0.036]	-0.0891* [0.045]
Lag1: Violent crime charge rate				-1.2521 [0.993]					0.0798 [0.132]
Lag1: Property crime charge rate				0.1538 [0.164]					-0.0364 [0.028]
Lag1: Other crime charge rate				0.6643 [0.540]					0.2601 [0.186]
Population		0.0000*** [0.000]	0.0000*** [0.000]	0.0000** [0.000]		0.0000 [0.000]	0.0000 [0.000]	0.0000 [0.000]	0.0000 [0.000]
Observations	48	48	48	48	511	511	511	511	454
R-squared	0.002	0.151	0.155	0.212	0.001	0.005	0.015	0.016	0.030

NOTES – The table shows regression results testing for determinants of the timing of county police force formation. The outcome variable in columns (1) to (4) is a dummy variable indicating whether a county adopted a force in 1840 (i.e. an early adopter); the explanatory variables are lagged measures of crime and dummy variables for whether the neighboring county already had a police force (which in the case of early adoption implies being a neighboring county to Middlesex). The dependent variable in columns (5) to (9) is a dummy variable for all counties that is equal to zero until the year of police force formation and one in the year of police force formation. Standard errors (clustered by county in columns (5) to (9)) are shown in brackets below the estimated coefficient. *** p<0.01, ** p<0.05, * p<0.1

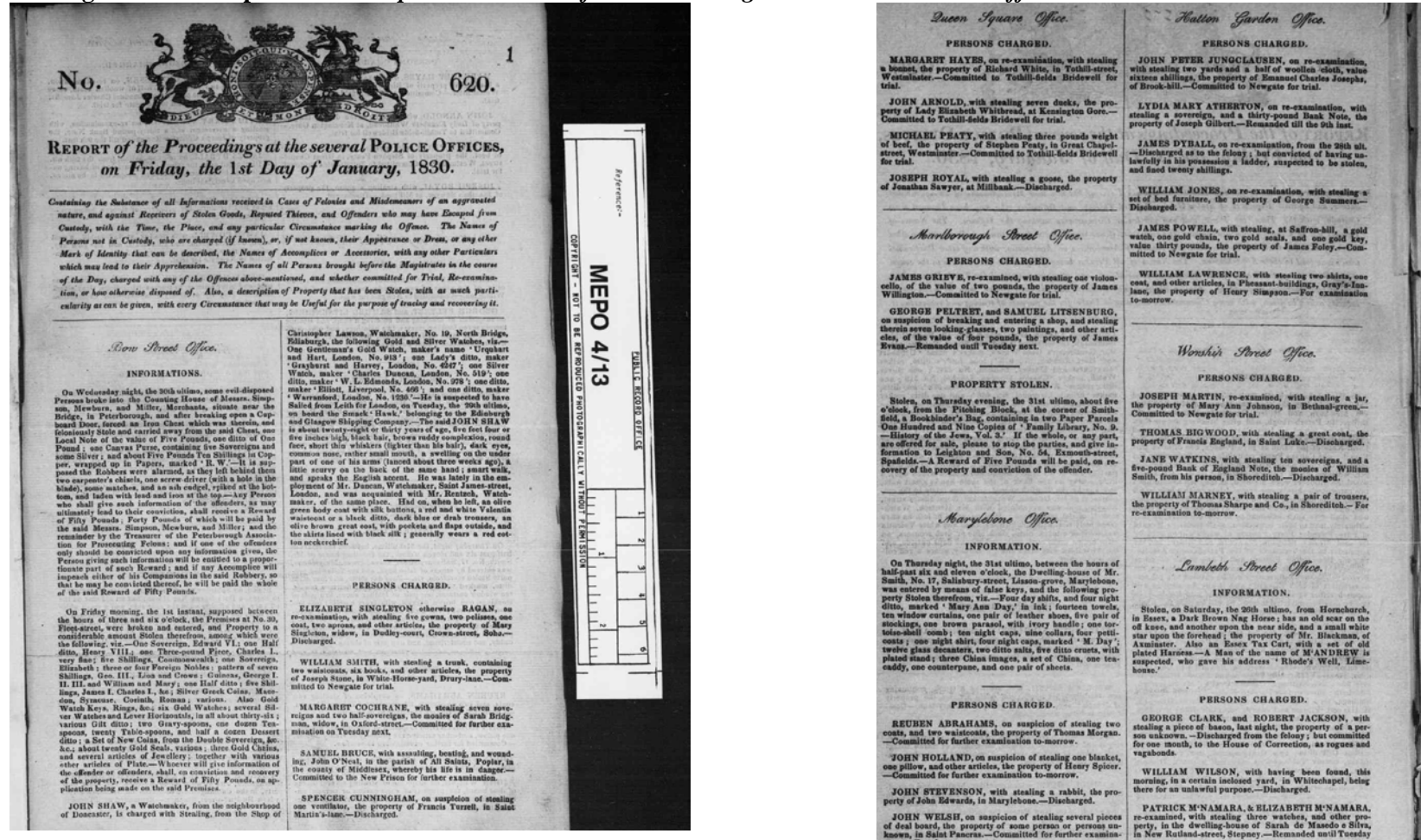
Appendix Table A9. Determinants of Initial Force Efficiency

	Dependent Variable (measured at force creation):			
	(1)	(2)	(3)	(4)
	Efficient? People/Police < 1500?	People/Police	Efficient? People/Police < 1500?	People/Police
<i>Fixed Geographic Variables</i>				
acres	0.000 [0.000]	-0.003 [0.002]	0.000 [0.000]	-0.004* [0.002]
num_parishes	0.000 [0.001]	0.499 [3.844]	-0.000 [0.002]	2.186 [4.554]
number_neighbors	0.029 [0.048]	-35.477 [239.006]	0.030 [0.049]	33.447 [245.052]
england	-0.153 [0.179]	888.542 [1,435.166]	-0.064 [0.296]	1,915.566 [2,424.436]
<i>Variables measured in the 1851 census</i>				
farmer	-0.017 [0.017]	-8.729 [112.116]	-0.021 [0.022]	41.065 [152.019]
male	-0.104** [0.048]	522.620 [388.323]	-0.114* [0.063]	379.598 [398.124]
married	0.050 [0.073]	267.177 [427.595]	0.052 [0.099]	60.880 [524.287]
native	-0.049 [0.088]	-102.064 [325.609]	-0.045 [0.108]	-70.625 [450.446]
employed	-0.007 [0.031]	-246.094 [176.390]	0.005 [0.036]	-285.889 [192.385]
age_0_15	-0.131 [0.127]	-536.135 [589.462]	-0.164 [0.174]	-146.516 [748.652]
age_16_25	0.010 [0.162]	1,363.391 [1,019.616]	-0.071 [0.192]	1,318.013 [1,411.150]
age_26_35	-0.219 [0.209]	23.037 [1,314.718]	-0.183 [0.277]	178.901 [1,319.783]
age_36_45	0.005 [0.457]	-1,844.465 [1,780.253]	-0.135 [0.620]	34.450 [2,241.751]
age_46_55	-0.263 [0.326]	3,415.117** [1,645.359]	-0.340 [0.380]	3,518.238* [1,794.485]
<i>Variables measured in the year before force adoption</i>				
Violent crime rate			-0.201 [1.024]	1,419.548 [4,230.184]
Property crime rate			0.014 [0.170]	-633.048 [661.370]
Other crime rate			0.378 [0.621]	776.423 [3,286.349]
Any neighbors with eff. Force?			-0.078 [0.252]	-739.059 [1,187.424]
Any neighbors with Ineff. Force?			0.225 [0.211]	-705.270 [1,011.393]
Observations	47	47	45	45
R-squared	0.349	0.370	0.378	0.429

Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1 The dependent variable measures police force efficiency at the time of force formation. Census controls are measured in 1851; the omitted age category is older than 55. All census variables are measured as the share of the county population with characteristic X (value of 0-100%). Pre-formation variables are measured one year prior to county police force formation (i.e. using a different year for different counties); pre-formation crime variables are the lagged crime rates (per 1000 population), while the neighboring force variables are indicators for whether any neighbors had an efficient or inefficient force in the year prior to adoption. All regressions have a single observation per county.

Appendix B: Data Sources

Appendix Figure B1. Excerpt from the Report or Account of the Proceedings at the Several Police Offices



NOTES – The above scanned pages highlight the three different measures of crime (informations, charges, and property stolen) which we coded for each office (seen in italics) and each date. The date is clearly indicated at the top of the page. Note that the files for the second half of 1828 as well as for 1829 have, according to information on the website of the National Archives, been lost. We therefore coded data from the documents corresponding to the months of January until April for the years 1828 (MEPO 4/12), 1830 (MEPO 4/13), 1831 (MEPO 4/15) and 1832 (MEPO 4/17).

