The Impact of the First Professional Police Forces on Crime*

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This version: November 7, 2018

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 the Met reduced London crime overall and across crime categories. A difference-in-differences analysis of county force roll-out finds that high quality police forces, measured by the population to force ratio, significantly reduced violent, property and other crimes, while there was no visible net crime reducing effect of a force that was not sufficiently large.

JEL Codes: K42, N93, H0
Keywords: police, crime, deterrence, economic history

* This paper would not have been possible without the tremendous effort of our research assistants Srinidhi Srinivasan and Vu Tran, and the financial support of Vetenskapsrådet, The Swedish Research Council, Grants for Distinguished Young Researchers. We thank seminar and workshop participants at the University of Gothenburg, the VATT Institute for Economics Research (Helsinki), the University of Uppsala, the University of Bologna, the NBER Summer Institute (Crime), the Transatlantic Workshop of Crime (Science Po Paris) and the IZA Bonn for helpful comments and discussion. Authors: Randi Hjalmarsson, Department of Economics, University of Gothenburg, Email: randi.hjalmarsson@economics.gu.se (corresponding). Anna Bindler, Department of Economics, University of Gothenburg, Email: anna.bindler@economics.gu.se.

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 concludes that there is at least a "consensus that increases in police manpower reduce crime". The main contribution of our paper is to provide evidence on a yet unstudied margin of policing: Do *any* police reduce crime? Specifically, we identify the effect of the introduction (i.e. the extensive margin) of a modern day professional police force on crime using two natural experiments in history: the formation of the London Metropolitan Police (the first professional force in the world) in 1829 and the subsequent roll-out of rural county police forces throughout England and Wales during the following 30 years. The permanent nature of these new police institutions – they still exist today – provide a stark contrast to the temporary shocks to policing typically studied in the existing literature. Moreover, various cities around the world, and especially in the United States, modelled their own police departments on the London Metropolitan Police (the 'Met'). Most prominently, police forces worldwide adopted the Met's innovative emphasis on crime prevention or deterrence.[1]

Becker's (1968) economic model of crime suggests that the answer to both questions – do more/any police reduce crime – should be yes. If more police (at any margin) increase the (perceived) chance that an offender is caught, then crime should be deterred. Crime can further be reduced by incapacitation if the (additional) police increase apprehensions (thereby preventing recidivism). Empirical evidence of the crime reducing effect of police is more elusive, however, due to both simultaneity bias – more police are hired in higher crime locations or times – and measurement error in the number of police (Chalfin and McCrary, 2018).[2] Empirical researchers, dating to Levitt (1997), causally identify a crime-reducing effect of police with natural experiments that locally or temporarily increased police numbers.[3] Yet, there is less consensus on whether this effect is driven by deterrence or incapacitation; reductions of local crime in police hot spots point towards the former (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 –

[1] U.S. police forces were established in New York City (1845), New Orleans and Cincinnati (1852), Boston and Philadelphia (1854), Chicago and Milwaukee (1855) and Baltimore and Newark (1857). See Uchida (2015) and https://www.britannica.com/topic/police/Early-police-in-the-United-States (viewed October 22, 2018).

[2] It is therefore unsurprising that the earliest studies (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.

[3] See Chalfin and McCarary (2017) for a recent review.

contrasts the existing literature that evaluates the marginal effect of an additional officer. Second, 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. 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?[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 the slow pace of about 2.5 miles per hour (hence the nickname PC Plod); the beat was also 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, including a higher professional standard and better working conditions (e.g. full-time salaries), may also have impacted their effectiveness.

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 Met): 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 shouting "it is one of Peel's bloody police; they are all thieves themselves".[5]

[4] Other papers have used national time series data to study crime in historical England. Wong (1995) emphasizes opportunities for legal and illegal gains from 1857 to 1892. Wolpin (1978) looks at the relationship between crime and the rates of clearance, conviction, imprisonment, and fines, as well as average sentences from 1894 to 1967. Outside England, Bignon et al. (2017) analyze the impact of negative income shocks to French vineyards and Mehlum et al. (2006) and Traxler and Burhop (2010) study increasing poverty in Bavaria and Prussia, respectively.

[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

In addition, an increased ability to detect crime could have led to more charges, even if the number of crimes committed did not change; i.e. there could have been an increase in clearance rates. This would have been expected as the new force was substantially larger than the previously existing informal policing. This increased detection would also be expected to *reduce* crime through incapacitation (over and above deterrence). To disentangle whether the new forces reduced crime (through deterrence and/or incapacitation) from increases in crime reporting and clearance rates (the potential confounders), we rely on two types of crime measures – incidents and charges. Incident level data is especially important as it allows one to abstract from the potential problem of crime reducing effects being masked by an increased clearance rate; this could potentially happen with administrative measures, e.g. charges.

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 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 post-terrorist attack increases in police deployment in London (Draca et al., 2011) and Buenos Aires (Di Tella and Schargrosky, 2004).[7] Most recently, however, Blanes I Vidal and Mastrobuoni (2018) do not find a significant effect of non-terrorist attack related temporary increases in patrols. Negative effects of a visible police presence on crime have also been found in studies of private policing using geographic boundaries (MacDonald et al., 2015; Heaton et al., 2016); these studies use spatial variation in force allocation to understand the permanent effect of policing.[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 and the subsequent roll-out of county forces. Created in September 1829, the Met was

[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. A handful of other papers have studied the effects on crime of police strikes (Pfuhl, 1983) and police slowdowns (Cann Chandrasekher, 2016), though the latter of course differs from the extensive margin. The permanent nature of the introduction of police forces distinguishes our study from those of police reduction experiments, i.e. temporary shocks.

[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.

[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.

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 of Charing Cross, London and extended to a 15-mile radius in 1839. Excluded from the initial catchment area, however, was the City of London (it is still not under the Met jurisdiction today). Because not all of London is initially 'treated' by the formation of the Met, our analysis 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 (Central Criminal Court of London and County of Middlesex), which contain the details of more than 200,000 felony trials from 1700 to 1913. These have been digitized by *The Old Bailey Proceedings Online*. Our analysis relies on the most serious offenses of burglary, manslaughter, murder, and robbery, for which we manually coded the following details: offense date and location as well as number and type of police witnesses. The advantages of the Old Bailey data are two-fold. First, we can directly observe reform implementation: there is an instant shift in the type of police witnesses ('old' to 'new') that is by far largest in the treated area. Second, we can estimate a difference-in-differences design comparing changes in crime in treated and control area(s). The second data source consists of daily police reports for the nine police offices run by the pre-1829 police (and continued until 1839); these include both crime incidents and charges as well as stolen property reports. These data include all offenses, rather than just the selected felonies at the Old Bailey. However, as all offices are within the Met's catchment area, this necessitates simple pre-post designs.

Both London analyses 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, the daily report analysis finds a reduction in stolen property reports/incidents (26%) but an increase in administrative 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 in 1839 but did not become mandatory until the County and Borough Police Act of 1856, which also introduced a national inspectorate to annually certify force 'efficiency'. The main 'efficiency' measure was the number of people per officer, 1,000 being the advised (but rarely achieved) guideline. Of the 48 counties in our analysis, 16 created forces in 1840, 23 in 1857, and 9 in the intermediate years. We use a difference-in-differences design to identify the effect of creating a professional force on crime, overall and for efficient or inefficient forces. Our main crime

measure (the only one available both pre- and post-reform) is the annual number of persons committed to trial by crime type (transcribed from historical *Judicial Statistics* yearbooks).

We find no overall effect of creating just any professional police force. But, creating an 'efficient' county force (in terms of people per officer) reduced crime overall (19%) and across categories (18% for violent, 14% for property, 24% for other offenses). Creating an inefficient force, however, did not have a net crime reducing effect (i.e. observable in administrative data). Event-study specifications show that the crime-reducing effect of efficient forces is not immediate (delayed by one to two years) and increases over time. Insignificant leads support the plausibility of the parallel trend assumption and a lack of anticipatory effects. Finally, our main results are robust to controlling for spill-over effects of neighboring county forces.

The remainder of the paper proceeds as follows. Section 2 provides institutional details related to the 1829 creation of the Met and the subsequent roll-out of county police forces. Sections 3 and 4 present data and analysis for the Met and county roll-out, respectively. Section 5 concludes with a discussion of the external validity of these historical experiments to today, highlighting three contexts in which we can provide contemporary insights: the extensive margin collapse of modern day forces, the (re-)invention of forces in developing countries, and in the interpretation 2nd Amendment of the U.S. Constitution.

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 to the Westminster Watch Act of 1735, this was largely in the form of unpaid and part-time local (night) watchmen. London's Bow Street Runners, who were sworn constables of Westminster, date to around 1750 (Emsley, 2009). As there were only eight at a time, they did not have a physical presence and were not meant to deter crime, but rather they primarily located and arrested serious offenders. These early Runners were not too different from the 18th century thief-takers, 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

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

Police Offices in the Middlesex Justices Act of 1792: Queen's Square, Great Marlborough Street, Worship Street, Lambeth Street, Shadwell (closed and replaced by Marylebone High Street by 1816), Union Hall 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 (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 Met's weekly growth until 1856. Two observations stand out. First, initial hiring happened in two stages. Recruits were first hired for six inner divisions in September 1829 and then five months later in February 1830 for the 11 outer divisions (see column (5) of Appendix Table A1).[12] Second, the Met grew almost constantly in the next 30 years to about 6,000 men in 1856. Yet, the population was growing quickly, too.

The initial catchment area of the Met was within an approximately 7-mile radius from Charing Cross in Central London and 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

[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).

[12] Was the Met simply the old police but with a different name? We provide suggestive evidence that this is unlikely. First, the size of the 'old police' amounts to only less than 3% of the size of the Met (3,000 by May 1830). Second, we can compare registers of the first 3,000 officers hired by the Met (Source: MEPO 4/31, National Archives London) to those hired into the Bow Street Foot Parole in the years leading up to the formation of the Met (Source: MEPO 4/508, National Archives London). Only 156 men were hired into Bow Street between 1823 and the creation of the Met in 1829. Since high turnover is observed is the early years, this 156 only corresponds to *hires* and not the existing level of pre-Met 'police'. Though discharges are not reported in later years, i.e. we cannot see who left the Bow Street Runners around 1829, we can observe (using their names) that a number of them were hired by the Met in the initial hiring wave. For instance, 24 of the last 34 Bow Street hires pre-Met subsequently joined the Met, but 9 of them were already dismissed by May 1830 and a number of others soon after. The Met, however, maintained a steady force size in these months, quickly replacing dismissed officers.

[13] While all descriptions of the formation of the Met describe this 7-mile radius, no explicit distance was written in the original act. Rather, the Act includes a "List of the parishes, townships, precincts, and places constituting 'The Metropolitan Police District'". That list includes 88 parishes or places for which we geocoded the main point of interest (e.g. the church of the parish). Of these 88 locations, 85 lie within a 7-mile radius from Charing Cross with the other three are within an 8-mile radius. Moreover, 75% of all 88 locations lie within a 4-mile radius. Our main analysis therefore uses the 7-mile radius to define all potentially treated areas, but also breaks this up into a treated inner circle and potentially less intensely treated outer circle (i.e. where the patrols are less visible due to the larger geographic area). We further test the robustness of our results to using a radius of 8 rather than 7miles. The expanded jurisdiction included all parishes that were partly (entirely) within 12 (15) miles of Charing Cross. Met. Police Act 1839, section II: http://www.legislation.gov.uk/ukpga/1839/47/pdfs/ukpga_18390047_en.pdf.

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. They indeed were all centrally located within the 7-mile radius (and even a smaller 4-mile radius) and thus 'treated' by the creating 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, here the Met Police were likely to be more visible walking on their beats. As such 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: There was a sharp and large increase in the sheer number of 'police'. Moreover, the primary task of the 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] The new police were paid a wage comparable to that of an unskilled agricultural worker, in an effort to recruit men who did not resemble gentlemen and who could gain the trust of the everyday man.[16] In contrast to the Met, 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 before March 1831). After 1833, we can also look at the weekly number of removals by broad reason (resignation, dismissal or death) and dismissals by reason (drunk, neglect or misconduct, criminal behavior or other); see Panels B and C respectively. 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

[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).

[16] See https://www.npcc.police.uk/Publication/History%20of%20Police%20Office%20Pay%20Framework.pdf, last accessed October 22, 2018.

even observe annual firing spikes for being drunk on duty around Christmas).

Clearly, a relevant question is *why* the Met was created. Was it 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. Despite being free to set wages, there was general resistance, such that by the end of 1837 only 93 of 171 boroughs even claimed to have established such a force (Hart, 1955). Many admitted to just fulfilling 'statutory obligations' by re-appointing previous 'police' (rather than selecting new recruits; Hart, 1955). Given the limited and fuzzy implementation of the 1835 Act, we do not attempt to study the effect of borough police. 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), including a pay scale set by the Home Office.[18] Why were the 1835 and 1839 Acts passed? Hart (1955) argues that there is no

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

[18] A county constable should be paid somewhat more than an agricultural worker. Last accessed October 22, 2018, see https://www.npcc.police.uk/Publication/History%20of%20Police%20Office%20Pay%20Framework.pdf,.

anecdotal or empirical evidence (based on crude statistics) that these acts were a response to criminals fleeing already treated areas (London and then the municipalities). 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).[19]

In 1856, three inspection districts – Northern, Midlands, and Southern – were formed, each with an assigned inspector. 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 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 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 (offense by offense) and replaced it with transportation to Australia. Increasingly

[19] This increased to 50% of wages after 1874; the government also aided in pension payments after 1890.
[20] Rutland remained inefficient until the 1861/62 inspection year.

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 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 – that distinguishes them from the roll-out of 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]

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.

[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 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).

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*. Though many variables (including offense type, verdict, and sentence) are tagged and easily identifiable, the Proceedings also include additional information that can be manually coded – namely the location and the date of the crime as well as the characteristics of police witnesses (number, type, and presence at the crime scene).[24] We will use the latter to assess the timing and extent to which the Metropolitan Police Act was implemented in treated versus control areas. Given the time-intensive nature of the transcription and historical geocoding, we focus on the most serious felonies (murder, manslaughter, robbery, burglary) from 1820 to 1850. For these, we can assume that their felony status (and hence representation at the Old Bailey versus a lesser court) does not change during this period and that changes in crime reporting are of minimal concern – i.e. a murder would always be reported.

Using the geocoded location and date of each offense, we classify offenses as in the treatment and control areas (within/outside a 7-mile radius of Charing Cross and within/outside the City of London, respectively) before and after the introduction of the Met. Thus, using the Old Bailey data, we can implement both a simple before-after design and a difference-in-differences specification. To geocode the data, we use the most detailed address available in the Proceedings (e.g. an intersection, parish/district name or street end/mid points) and map these locations into modern day London maps to obtain postcodes and geo-coordinates.[25]

Figure 4 shows maps of each crime for 1820 – September 1829 (pre-Met), September 1829 – August 1839 (post-Met but pre expansion), and 1839-1850 (post expansion). Each dot represents the location for a defendant-crime observation; treated locations are blue, while control locations (the City of London or outside the 7-mile radius) are green. The borders 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 by crime type as well as details regarding police witnesses within and outside a 7-mile radius of Charing Cross and in the City of London for different time windows. Police witnesses were called constables (both before and after the creation of the Met), policeman (a post-Met label), watchman (a pre-

[24] We have previously used the Old Bailey data in projects studying (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, 2017, 2018 and forthcoming).

[25] Whenever locations have changed names (e.g. street names), we identify the current address using historical maps (roughly 40% of our regression sample). When the most detailed address is a long street (about 11% of our sample), we geocode the nearest street endpoint as the location (i.e., we assign potentially untreated observations to the treatment area). Our results are qualitatively robust to excluding either of those 'fuzzy' locations.

[26] Shapefiles for the postcode areas were obtained from Maproom's UK Postcodes Shapefiles.

Met label) and a handful of other labels that were either predominantly pre or post-Met.[27]

The main disadvantage of the Old Bailey data is that it only includes serious felonies that go to trial. This limitation, however, is addressed by our second data source – the *Report or Account of the Proceedings at the several Police Offices*. These are reports by the nine police offices that were run by the pre-1829 police and continued until 1839.[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 two measures of crime incidence: (i) the daily number of 'property stolen' entries and (ii) the daily number of property, violent, and other 'informations'. Most comparable to modern day arrest data, we also create a third administrative crime measure that is the daily number of charges by crime category (property, violent, other). Again, this incorporates both crime incidence and apprehension by the police.

Thus, this second source has the advantage of capturing crime incidence (not just trials) and including all crime types, not only (selected) felonies. As these more minor crimes are more common, precision increases despite the short time window. Yet, there are two limitations. As all offices are located within the Met's jurisdiction, 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 Met in the Old Bailey trial reports. 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

[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, which are publicly available from the National Archives.
[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).

annual share of trials with a police witness of any sort for both the treated (i.e. within 7-miles but not the City of London) and potential control area (outside 7-miles or in the City of London). The vertical lines indicate 1829 (year of 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. But, Panel B demonstrates a clear shift in the type of police. The share of trials with an 'old' labelled police witness (watchman or other) drops sharply from about 70% to 20% while the share with a 'new' label (policeman or other) increased from 0% to almost 50%.[30]

Table 1 looks at this 'first-stage' more formally by estimating 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. 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 (daily police reports). An advantage of the larger window is that, given the rarity of burglary, robbery and murder, it increases sample size and precision. We divide the treatment area into two areas (within 4 miles and 4-7 miles from Charing Cross) to allow for a potentially more intense treatment in the inner divisions (i.e. more visible patrol presence 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 Met (*PostMet*). Offense type dummies are included to allow for differential police involvement across offenses. The difference-in-differences specification is presented in equation (1), where the coefficients of interest, β_1 and β_2, capture the effect of the Met on the within 4 miles radius (main treatment area) and the 4-7 miles radius (uncertain treatment intensity area), respectively. 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 significantly changed

[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.
[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.

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 the 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 some increase (19 percentage points) in 'new' and no change in 'old' police. Ideally, we would find a zero estimate on the presence of new police, as we do for old police, in the control area. The point estimate for new police is, however, much 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 in the Proceedings by court reporters, 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 *under*-estimation 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. 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 is plausible that they were increasingly present at the crime scene itself, either

[32] Indeed, the original Act does not actually say a 7-mile radius but provides a list of parishes than can be included. Therefore, we conduct the same analyses using an 8-mile instead of a 7-mile radius from Charing Cross to account for measurement error in the control group assignment (see Section 2.1). Yet, our conclusions remain unchanged.
[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.

by witnessing the crime or being close enough to be called upon for assistance, i.e. a shorter response time. This may depend crime type 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 share of trials with police present at the crime scene increase (the shading becomes darker). Columns (7) and (8) of Table 1 look at this more formally: 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 and is not robust to the difference-in-differences specification.

Main Empirical Specification (Old Bailey Data)

Having established that the creation of the Met affected '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 (but an increase from 1839 to 1850).

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 plus the City of London). Table 2 begins with a simple comparison of the average number of crimes before and after introducing the Met, for all crimes and separately by crime type (burglary, robbery and homicide). Panels A and B show means for 1820-1839 and 1828-1832, respectively.[34] In the larger time window, there is a significant reduction of 37% (from 6.46 to 4.10) in the average number of total monthly crimes in the treated area; similar reductions are seen in the shorter window (40%) and for burglary and robbery. In contrast, there is little evidence of a crime decrease for the (less intensively treated) uncertainty area and there is no significant change in total crime for the control area (though a reduction in burglary is 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 window that virtually disappears in the smaller window in Panel B (i.e. when excluding the period after 1832 when the City of London may have been partially treated).[35]

[34] Significance levels are based on simple pre-post regressions; the results are robust to including month dummies.
[35] Similar results are found when using an 8-mile instead of a 7-mile radius from Charing Cross.

To make the case that these post-Met crime reductions in the treated area have a causal interpretation, we turn to the difference-in-differences specification in equation (2), which uses the area outside the 7-mile radius and the City of London as the best possible control groups. We again split the potentially treated areas into areas with a certain (within 4-mile radius) and an uncertain treatment intensity (4 to 7 mile radius). 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). Year, month and area fixed effects are included.

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

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. We 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. It is possible that the City Day Police introduced a similar treatment to the City of London as 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 are 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. One potential spill-over is that of policing. Our above discussion of police witnesses already raised this possibility (as an explanation for the change in type of police witnesses in the control area). 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, a spill-over of policing to the control area would attenuate our estimates of a crime reducing effect of police. A second type of spill-over, however, is crime displacement. If criminals chose to commit crime in less policed areas than the newly treated Met jurisdiction, then this would bias the difference-in-differences estimates in the direction of a crime reducing effect. We argue that this is unlikely to be the case for three reasons: First, the pre-post estimations do not suggest any significant change in crime in either of the control areas (outside the 7-mile radius and in the City of London in the shorter 'cleaner' estimation window). Second, it is important to keep in mind the historical context – criminals would likely be travelling on foot.[36] 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. Third, if indeed criminals now travelled to the control areas to commit crime, the distance of crime locations from Charing Cross would be expected to change differentially in the treatment compared to the control area. We use a similar pre-post as well as difference-in-differences design as in equation (2) above, but using as an outcome the average distance to Charing Cross of crimes committed per month and area (of course, the caveat of that analysis is that it necessitates conditioning on observed crimes). The results (available upon request) suggest that there is no increase in the distance of crime locations from Charing Cross in the treatment compared to the control areas from 1828 to 1832.

A final potential confounder is that the 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 increases 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 comparable, then the pre-post bias is in fact eliminated. But, if the population grew faster (slower) in the control areas, this

[36] 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. For more detail, see also Heblich et al. (2018).

would bias us towards (against) a crime-reducing effect of the Met. Unfortunately, we cannot directly measure population growth within the various treatment and control areas. Our main analysis, however, mitigate this concern by using a narrow window around 1829.

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 by crime type. 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 area relative to the control area. This could imply that there was no change in crime levels in the uncertainty area (maybe due to a smaller deterrence effect as police were less visible in larger divisions) or that the crime reduction effect was offset by increased apprehensions. Another possibility is that any deterrence effect is 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). Further, moving to a narrower estimation window (and mitigating 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. Our main finding of the Met leading to significant and sizeable reductions in crime (trials) is robust to alternative estimation strategies and estimation windows.

Additional Identification and Robustness Tests (Old Bailey Data)

Figure 7 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 that interacts the

treatment indicator with dummies for 2-year intervals before and after the introduction of the Met.[37] To account for the mid-year timing of the Met's creation, we define a year from September to August. The purpose of these specifications is twofold: Use the leads to test the parallel trends assumption and study the dynamic effects of creating the Met. Were the effects immediate, and did they change over time (as officer quality increased with both experience on the beat and in recruiting)? The results are supportive 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 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 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).[38]

3.3. Analysis of The Daily Police Reports

Summary Statistics (Daily Police Reports)

[37] We similarly interact (but do not show) the indicator for the uncertainty area with these two-year dummies.
[38] 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. Though the raw data include nine offices, we exclude the Thames Police Office from the analysis as the Thames River Police are not in the jurisdiction of the Met.[39] Given the different nature and likely crime composition of the Thames jurisdiction (docks and water) and the surrounding offices, we opt to not include it as a control office in a difference-in-differences specification. 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 8 illustrates these patterns: Panels A and B show the weekly (Monday – Saturday) number of informations and charges, respectively, by crime category, while Panel C shows the weekly number of stolen property reports. As suggested by 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: we do not see continued decreases or increases in the years after the reform (1830-1832).[40]

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:

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

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 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

[39] We also exclude the "Metropolitan Police Office" as this office was created in 1832.

[40] 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.

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. Of course, in 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 Old Bailey analysis found the results to be robust to the smaller time window and both pre-post and difference-in-differences designs.

Table 6 presents the baseline pre-post results using the daily crime reports for each outcome: any and number of informations (Panels A and 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) and calendar week and day of the week fixed effects in columns (3) and (4).[41] Column (5) includes the daily reports for January to April of two additional post-reform years (1831 and 1832). For all outcomes, the size of the point estimates increase while the sign and precision remain the same. We 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%, 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 crime measures. Both informations and property stolen are proxies for criminal incidents, comparable to modern

[41] 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).

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.[42]

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 corresponds 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.[43]

[42] An alternative explanation for the pattern of results is that there was a substitution in the reporting of crime *incidents* between the old police offices and new police stations, but that the *charges* were still processed at the old police stations (as this is where the magistrates processing the charges were located). We believe this scenario to be unlikely for three reasons: First, we almost immediately see effects on crime incidents (see in particular Table 7 on the second wave of hiring). It is hard to imagine that reporting behavior changes so fast (especially given the general distrust in the new police). Second, similar findings are found in the Old Bailey trial data, especially for violent crimes; these data are not subject to the same substitution in reporting concerns. Third, the Daily Crime Reports indicate a decrease in both violent crime incidents and charges. If the incident finding was only due to a substitution in reporting locations, then one would not expect to see a decrease in violent crime charges.

[43] In particular, the 1832 Daily Crime Reports (MEPO 4/17) include hand written letters next to (almost) every 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.

We 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 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 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 (notwithstanding 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 Met on crime? First, we find convincing evidence of the immediate implementation of the Metropolitan Police Act in Old Bailey police witness testimony, with the greatest 'treatment' in the more intensively patrolled 4-mile radius around Charing Cross. Second, using the Old Bailey data, we find that the introduction of the Met significantly reduced trials 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 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 yields evidence consistent with both reduced criminal activity and increased apprehensions for all crimes (violent, property and other). However, we find that the former dominates for violent crimes (negative coefficient for both the incident related outcomes and charges) while the results are consistent with apprehension/reporting effects dominating for property crimes (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.

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 and 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 size for each county. Figure 9 illustrates the roll-out of forces in a map of all counties in England and Wales, making it apparent 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 or Middlesex.[44] Figure 10 demonstrates the evolution of the number of county forces over time: 16 counties created police forces in 1840 (just 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).[45]

Before the 1856 Act, 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 one county and year).[46] This measure is available for both the entire 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 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.

A potential disadvantage of using trials to measure crime is that it may confound changes in prosecution behavior (in which the police may have played a significant role at the time) with changes in criminal behavior. However, Appendix Figure A2 demonstrates that all three crime measures available in the *Judicial Statistics* after 1857, i.e. trials (our measure), total number of indictable crimes committed and the total number of individuals apprehended for indictable

[44] 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.
[45] 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.
[46] 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.

offenses, move in lock-step until the early 1890s. Another potential concern is the impact of the 1855 Criminal Justice Act on the number of trials. The Act gave judges the ability to summarily deal with larceny cases, which is reflected in the large decrease in the number of trials in the year before the mandatory creation of the police forces, specifically for property offenses (see Panel A of Figure 11). Given that this Act is a national shock (comparable figures by county are available upon request), our difference-in-differences approach mitigates related concerns. Moreover, we estimate the effect of creating a force for two categories unaffected by that reform (violent and other) and for the early reforming counties using a sample period prior to 1855.

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.[47] 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.[48] We use this population variable to create crime *rates*.

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 reliably identify the year of force creation and measure crime. The raw data include 52 counties. We drop Middlesex since it cannot be disentangled from London in the crime data, and at least part of Middlesex was already treated by the Met. We also drop three counties (York, Sussex, and Suffolk) that represent aggregates of regions with initially separate forces (but with crime data only available at the aggregate level).[49] Appendix Table A5 lists each of the remaining 48 counties included in the analysis.

Our main treatment variable measures whether county *c* has 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; for the former, the first treated year is

[47] We obtained the census data from *North Atlantic Population Project, UK Censuses.* https://www.nappdata.org/napp/

[48] 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).

[49] 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.

typically only partially treated whereas for 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 and use that to characterize the new force's 'efficiency'. 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 also 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

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):

$$Crime_{ct} = \beta Force_{ct} + \alpha_c + \alpha_t + X_{ct}\theta + \varepsilon_{ct} \quad (4)$$

The dependent variable, *Crime*, is the number of persons committed to trial in county *c* and fiscal year *t*. We consider 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 creating a professional force on crime, we make the usual parallel trends assumption that 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 11 illustrates 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 allowing for differential effects leading up to the reform. 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 right 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 measure of crime, trials, only allows us to estimate the combined effect of deterrence/incapacitation and these confounders. In contrast to the London analysis, here we do not have crime incident data. Therefore, 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 detect it in the data.

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 the estimation window 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 insight from Table 9 is that the creation of a police force, on average, does not have a significant effect on overall, violent or property crime. Second, creating a force appears to reduce 'other' crimes by 10 to 17 percent. Third, the estimates are comparable when using the log number of crimes or 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 rather than just part of the year. This is perhaps unsurprising, as forces cannot be created overnight: Officers needed to be recruited and trained.

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 observed 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, we must rely on the added assumption that 'efficiency' is conditionally random. The 1839 Act recommended 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 (maybe particularly motivated counties) that were efficient (using a 1,500 people per officer threshold); rather, similar proportions of early (20%), mid (33%) and late (17%) reformers 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, from 1,500 people per policeman in column (1) up to 2,500 in column (5). Under the strictest and weakest thresholds, there are 10 and 30 efficient forces, respectively. There is a differential impact of creating an efficient versus 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 categories (18% for violent, 14% for property and 24% for other offenses). In contrast, creating an inefficient force does not significantly affect crime overall; instead, it increases the number of property (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.

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 shown in Figure 12 for all crimes categories combined, and for each offense category separately in Appendix Figure A3. The top and bottom panels of Figure 12 present the estimates for the efficient and inefficient forces, respectively; note that both 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 starts three years after the reform. Second, the negative effects of efficient force creation 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 visibly 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 by 1840) is affected by lagged crime rates and a neighboring county having 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 prior years 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).[50] Again, lagged crime rates overall and by category never significantly predict reform adoption. Having a neighboring force decreases the chance of reform (significant at the 10% level and driven by 'inefficient' forces at the 1,500 threshold). This raises the question of whether creating 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) efficiency measures upon creation. We consider all potential determinants available to us, including fixed geographic variables (acreage, number of parishes and neighboring counties, English and Welsh counties), variables measured in the 1851 census (share farmers, male, married, native, employed, and age groups), as well as crime rates and whether any neighboring county had efficient or inefficient forces in the year before force formation.[51] There is little information, including crime in the year before formation, which consistently predicts the type/size of the police force. 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 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 a neighboring county in year *t* had an *efficient or inefficient* force. In all specifications, controlling for neighboring county police forces has no impact on the baseline estimates (though the coefficients on the neighboring forces are themselves significant): having an efficient force still decreases overall crime by almost 19%.[52]

4.6. Discussion of County Police Force Formation Results

To summarize, the above analysis of the roll-out of professional county 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

[50] 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.
[51] 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.
[52] Having an efficient (inefficient) neighbor significantly decreases (increases) local crime. There are multiple potential explanations that we unfortunately cannot disentangle, including the possibility of incapacitation effects or cooperation between neighboring forces (which was a criteria used by the inspectors to evaluate efficiency).

(trial) reducing effect. 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. 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 (the same is actually true in our London analysis). Finally, an interesting takeaway is the increase in the magnitude of the crime-reducing effect over time. This highlights the importance of quality: Police forces clearly improved in 'quality' over time as people per officer ratios continued to decrease, supervisors were increasingly hired, and experience was gained.

Linked to the notion of quality, 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 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). 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 visible 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.

Given the lack of estimates of the extensive margin effect of police on crime, it is hard to ultimately compare our findings to the existing literature. 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 Schargrodsky (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 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% subject to 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).[53] Data from *Eurostat* report similar numbers for 2016 with 3.2 police officers per 1,500 population in England and Wales (4.5 in Germany and 3.04 in Sweden).[54] 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 study offers a number of contemporary insights. First, the extensive margin effect of the modern-day police institution is in and of itself still a relevant policy question today. In countries like the U.S. and U.K., police departments and/or local police stations are currently being closed due to shrinking budgets.[55] What will be the effect of these closures on crime? The existing literature is limited in its ability to answer this question, as such an extrapolation would be based on the assumption that the marginal effects per officer (as identified in the literature) apply equally (linearly) across all officers.[56] This is an assumption that (to the best of our knowledge) has no empirical support to date, and a question we hope to shed light on in further studies of our historical context: does the marginal effect of police on crime depend on the size and age of the force? Rather than making such

[53] https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/tables/table-25, last accessed March 15, 2018.
[54] Eurostat, "Personnel in the criminal justice system by sex" (last accessed on October 31, 2018): https://data.europa.eu/euodp/data/dataset/4IAKUAgPA5yFoNEwJp1syA
[55] A 2018 New York Times article (https://www.nytimes.com/2018/07/31/us/blandford-police-officers-quit.html) highlights the US closures while articles in series of UK newspapers highlight the English closures (see e.g. https://www.dailymail.co.uk/news/article-4764324/40-police-stations-close-violent-crime-surges.html and https://www.independent.co.uk/news/uk/home-news/police-station-close-shut-budget-cuts-police-federation-a8521501.html.)
[56] In a recent study, Blesse and Diegmann (2018) exploit variation from police station closures in one federal state in Germany to estimate the effect of police closures on crime.

assumptions, our results are indicative of the potential effect of eliminating the entire force. There are, of course, some caveats in making such inference from our study. First, there were many other institutional and societal differences between 19th century England and today. The extensive margin effect of a police force today on crime may not the same as it was historically. Second, it may not be the case that the extensive margin effect of a force is symmetric upon both its creation and collapse. Nevertheless, given the lack of research to date on the extensive margin (in either direction), we believe our research fills a significant gap in the literature.

In other (less-developed) countries, police forces today are being disbanded and new forces created in an effort to eliminate police corruption. In July 2015, 2,000 new officers, named the Patrol Police, were placed on the streets of Kiev in the Ukraine; these individuals were carefully selected from a pool of more than 34,000 and received 10 weeks of training (by Americans); the old police (militsiya) were not disbanded but were ordered off the streets. In the Republic of Georgia, the "old militsiya and traffic police were abolished, their 16,000 jobs eliminated, and an all-new-in-every-way force called the Patrol Police hit the streets. They [were] also paid a decent wage, to discourage corruption" (Gessen, 2015). There are many similarities between these modern-day contexts and the historical creation of the Met, including the corruption of the 'old' police. Thus, our results may have important policy implications today with respect to institution building in developing countries; one potential 'lesson' is that the quality of the institution plays a fundamental role.

Finally, our findings may have implications on how to interpret the 2nd Amendment to the US Constitution ("…the right of the people to keep and bear Arms…"). Many interpret this as a right to self-defense. Yet, it must be noted that the Constitution was written *before* the creation of modern-day police forces – in other words before the systematic provision of *public* crime prevention and protection. Given the significant crime-reducing effect of creating professional police forces, an open question is whether the 2nd Amendment would read the same had it been written *after* the creation of these institutions.[57]

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[57] We thank John Donohue for raising this debate.

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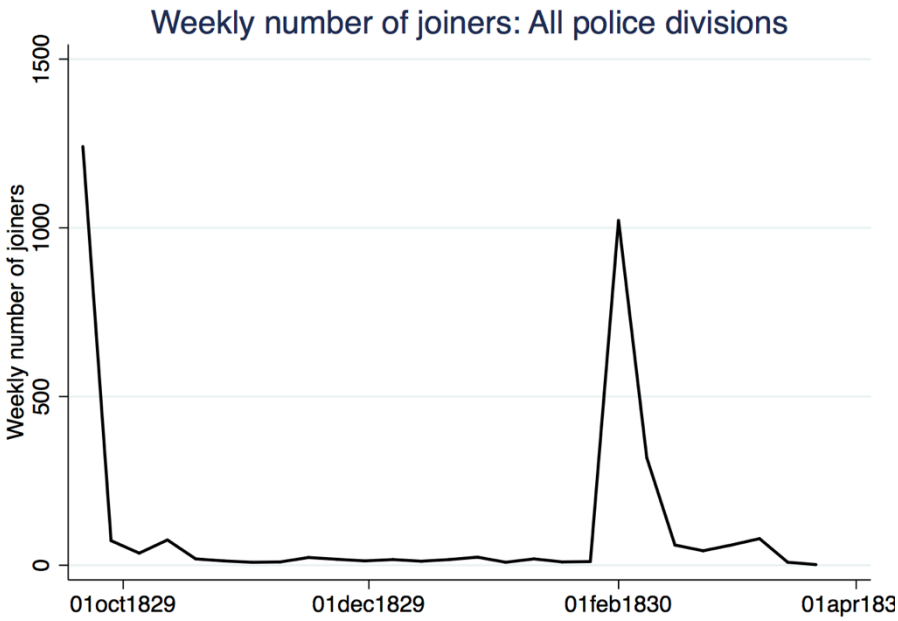
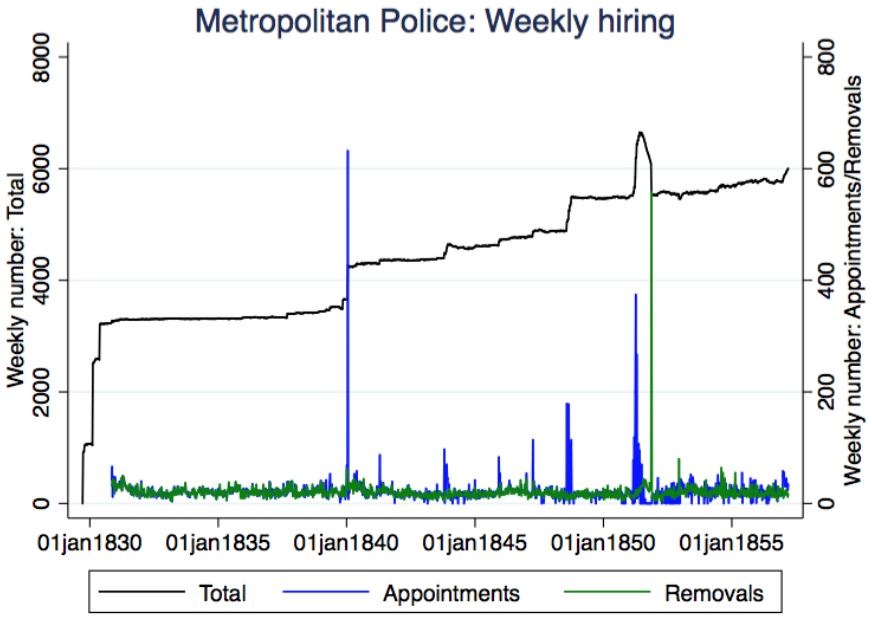
Figure 1. London Metropolitan Police – Weekly Hires

Panel A. 1829-1831



Panel B. 1830-1856

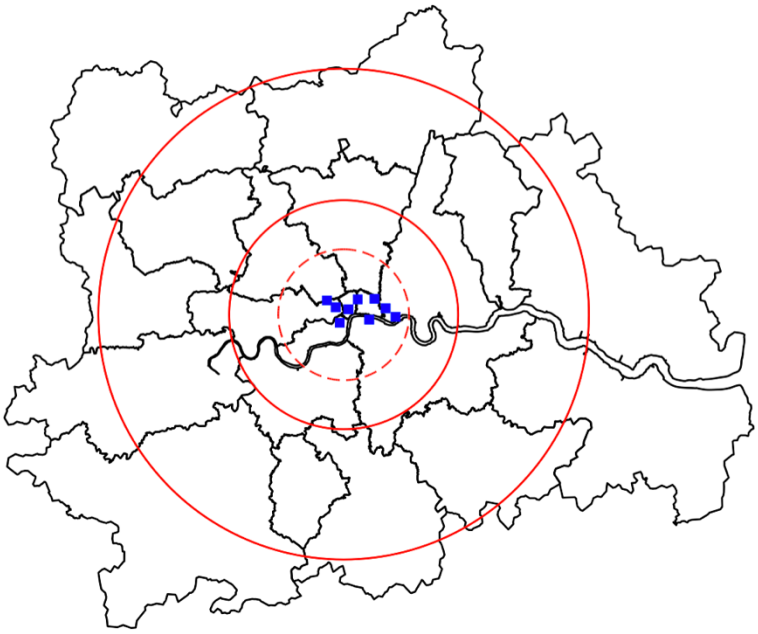


NOTES – Panel A shows the weekly number of police joining the Metropolitan Police across all police divisions between September 1829 and March 1830. The data underlying this figure were manually transcribed from the *Register of recruits into the Metropolitan Police* available at the London National 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

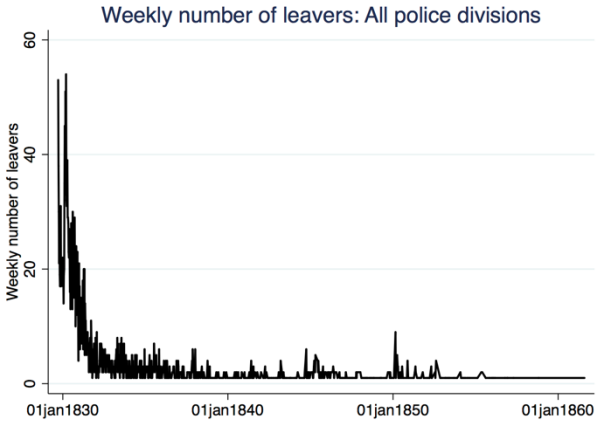
Panel B. Police Stations (Existing Before the Metropolitan Police and until 1839)



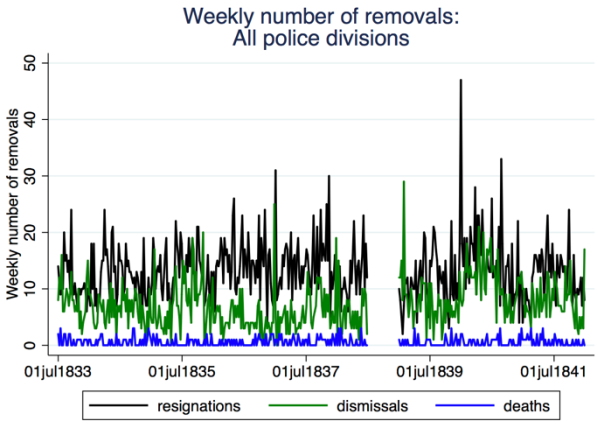
NOTES – Panel A presents a map of the original London Metropolitan Police District. Shaded in red is the City of London Police area, outside of the Met's jurisdiction. The large letters indicate the various districts of the Metropolitan Police. The map is available from the British Library's online map collection: *http://www.bl.uk/onlinegallery/onlineex/crace/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- (dashed), 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

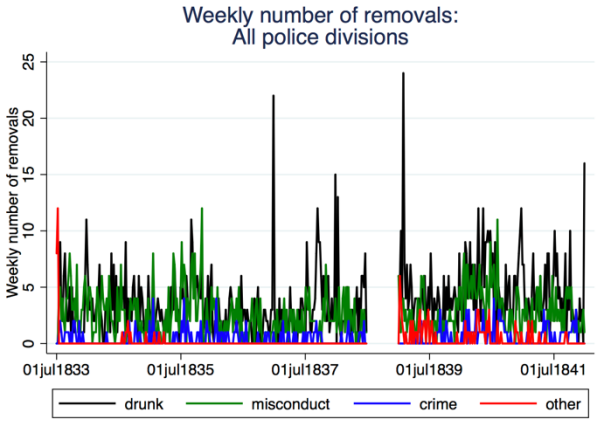
Panel A. Weekly Number of Leavers



Panel B. Weekly Number of Removals, by Reason

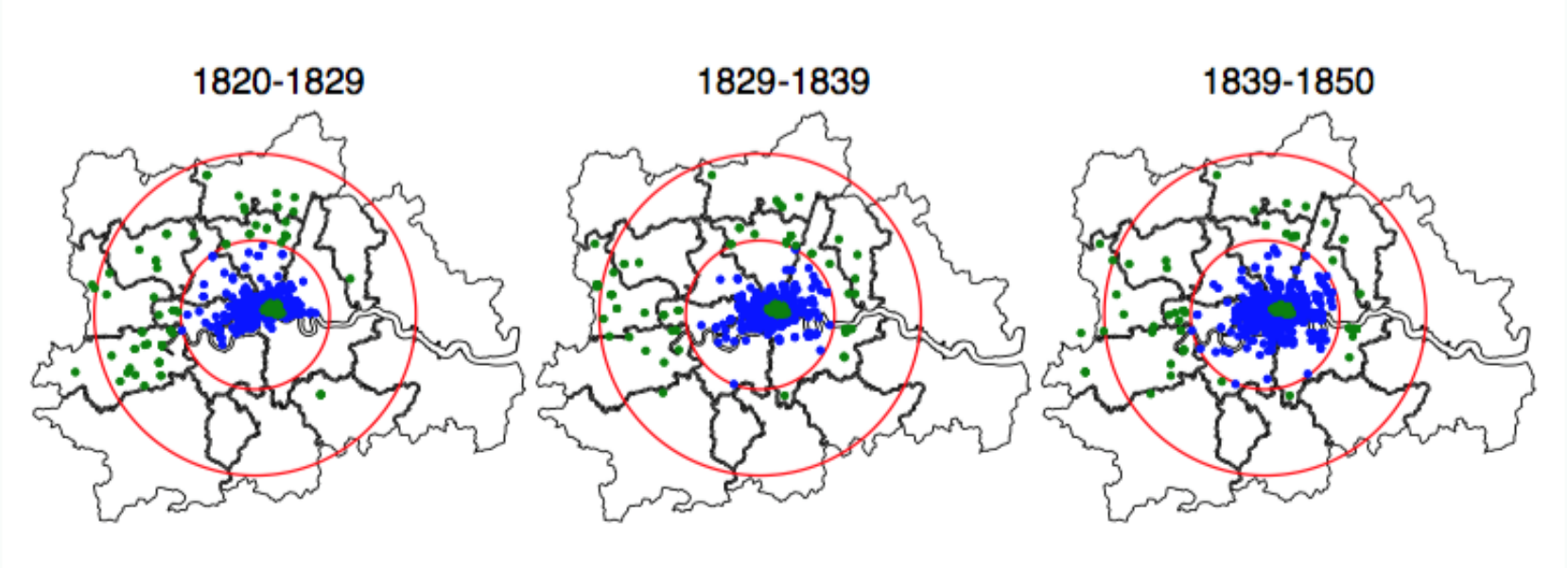


Panel C. Weekly Number of Dismissals, by Reason



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).

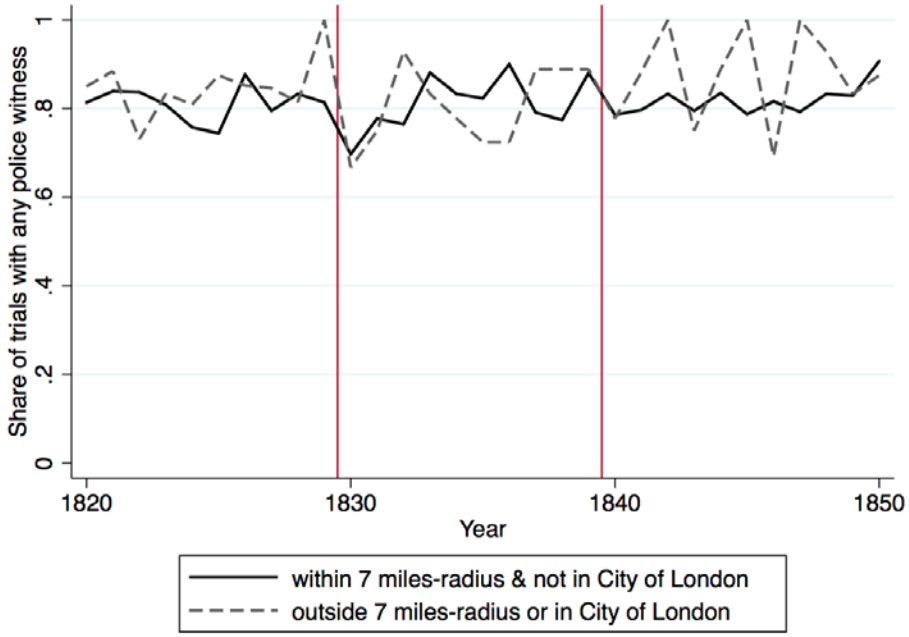
Figure 4. Geocoded Data from the Old Bailey Proceedings



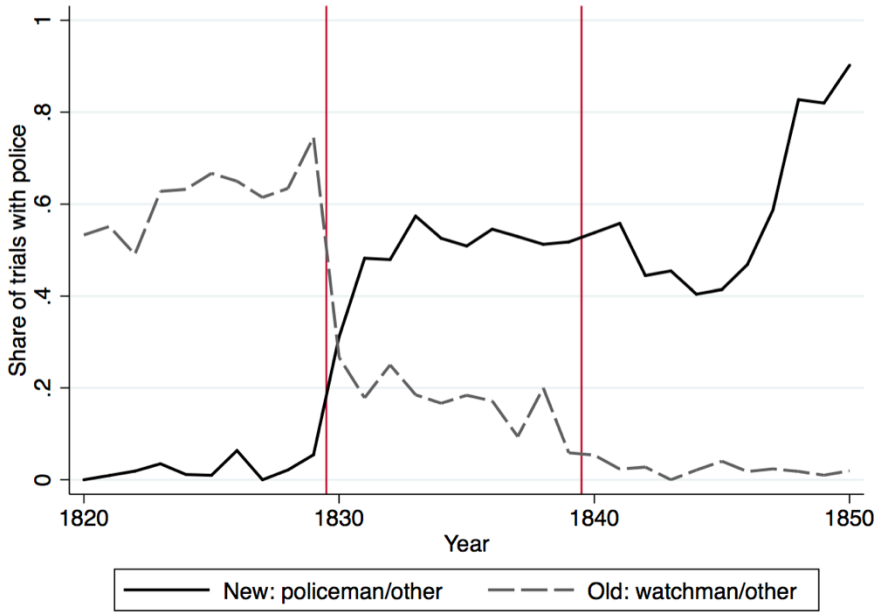
NOTES – The figure plots geocoded crime locations of murders, manslaughters, robberies and burglaries trialed at the Old Bailey between 1820 and 1850. The two red circle mark a 7- and 15-mile radius from Charing Cross, respectively. Each dot represents a trial-defendant observation; the green dots represent crime locations inside the City of London (within 7-mile radius) as well as outside the 7-mile radius and the blue dots represent crime locations within the 7-mile radius and not in the City of London. 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



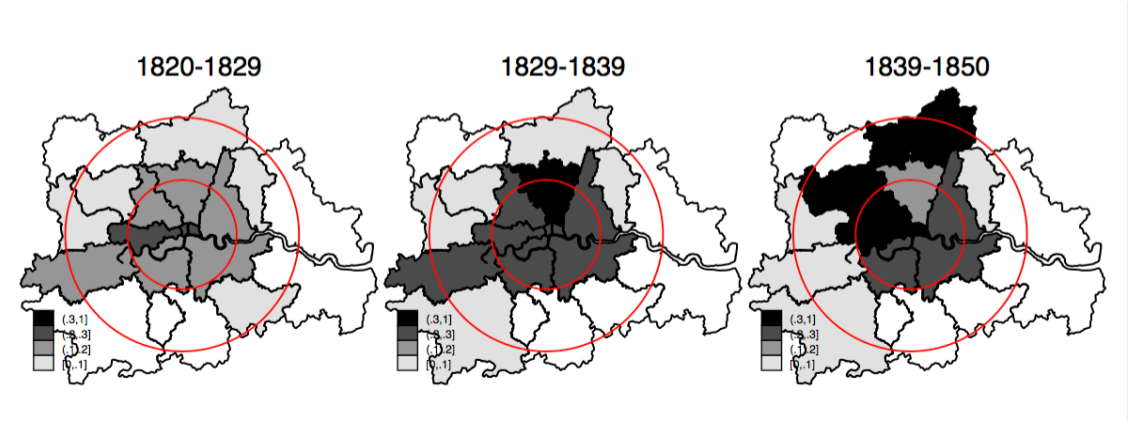
Panel B. Change in Type of Police Witnesses at Trial



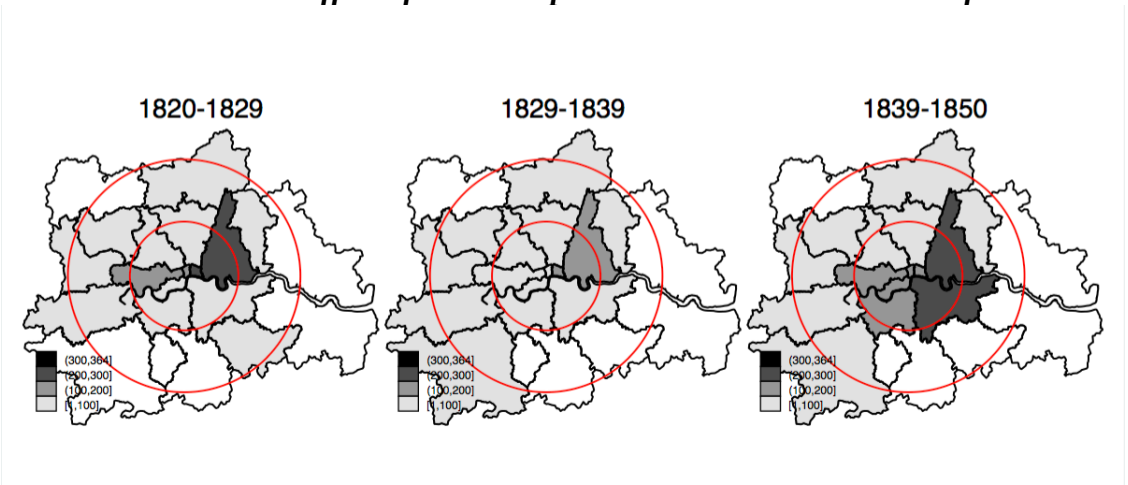
NOTES – Panel A shows the annual share of homicide, robbery, and burglary trials at the Old Bailey from 1820 to 1850 with at least one police present as a witness. The black solid line represents trials for crimes located in the treatment group (within 7 miles from Charing Cross), the grey dashed line 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



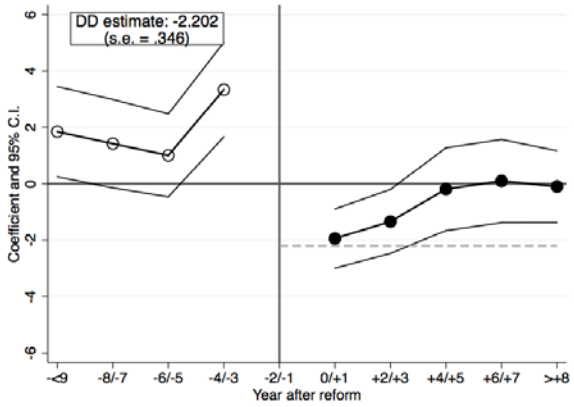
Panel B. Reduced Form Effect of the Metropolitan Police on the Number of Trials



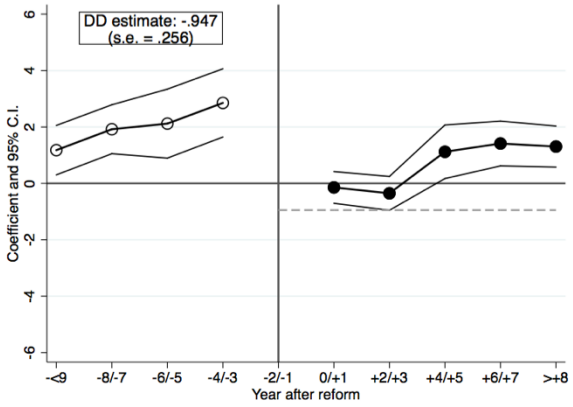
NOTES – Panel A shows maps of London with the share of trials at the Old Bailey from 1820 to 1829 (left), 1829 to 1839 (middle) and 1839 to 1850 (right) with police present at the crime scene, each by (modern-day) postcode area. Darker shaded areas correspond to higher shares of trials with police at 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. Event-Study for Reduced Form from the Old Bailey Proceedings

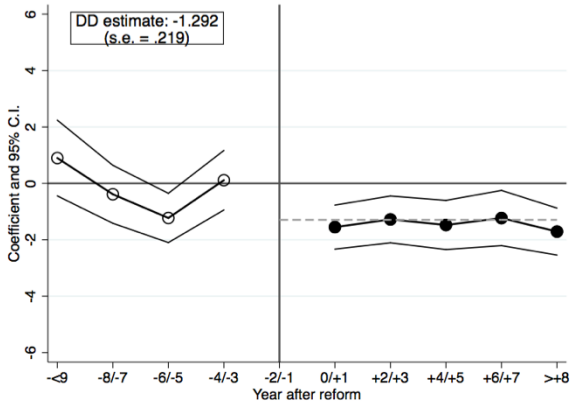
Panel A. Total Crime



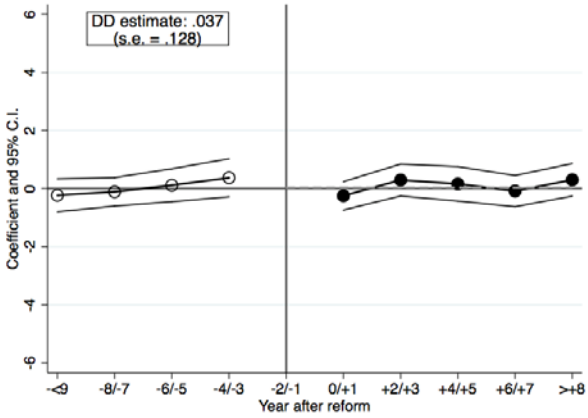
Panel B. Burglary



Panel C. Robbery



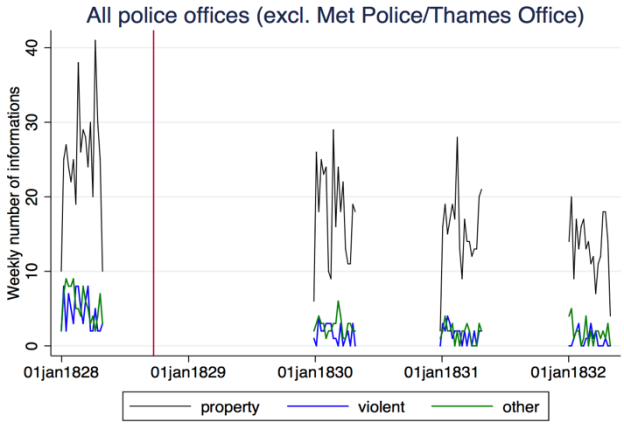
Panel D. Homicide



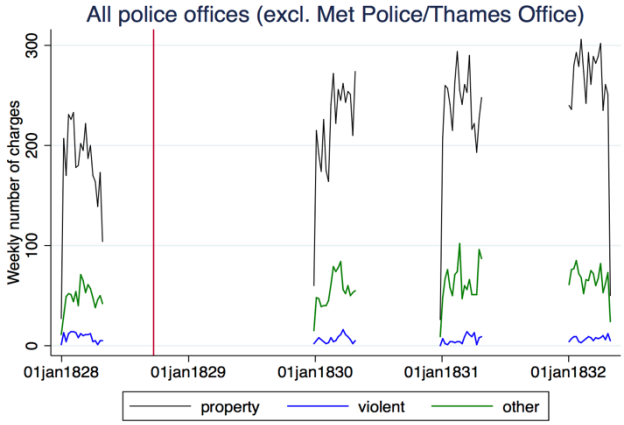
NOTES – The figures show the estimated coefficients and 95% confidence intervals corresponding to the event-study specifications described in Section 3.2. Panel A shows the results for all trials (pooled), Panel B to D for burglary, robbery and homicide trials, respectively. A year is defined as September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

Figure 8. Daily Crime Reports – Weekly Aggregated Crime

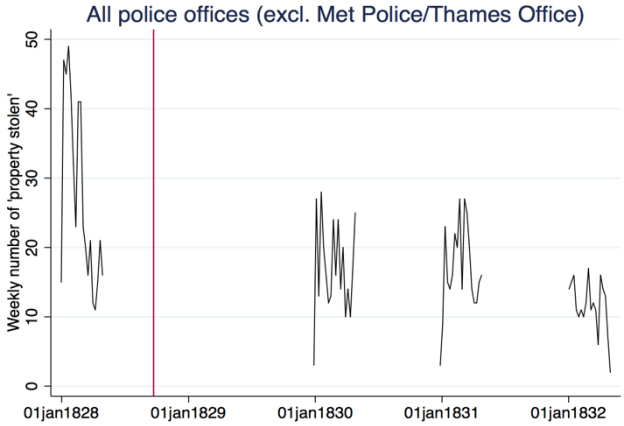
Panel A. Weekly Number of Informations



Panel B. Weekly Number of Charges

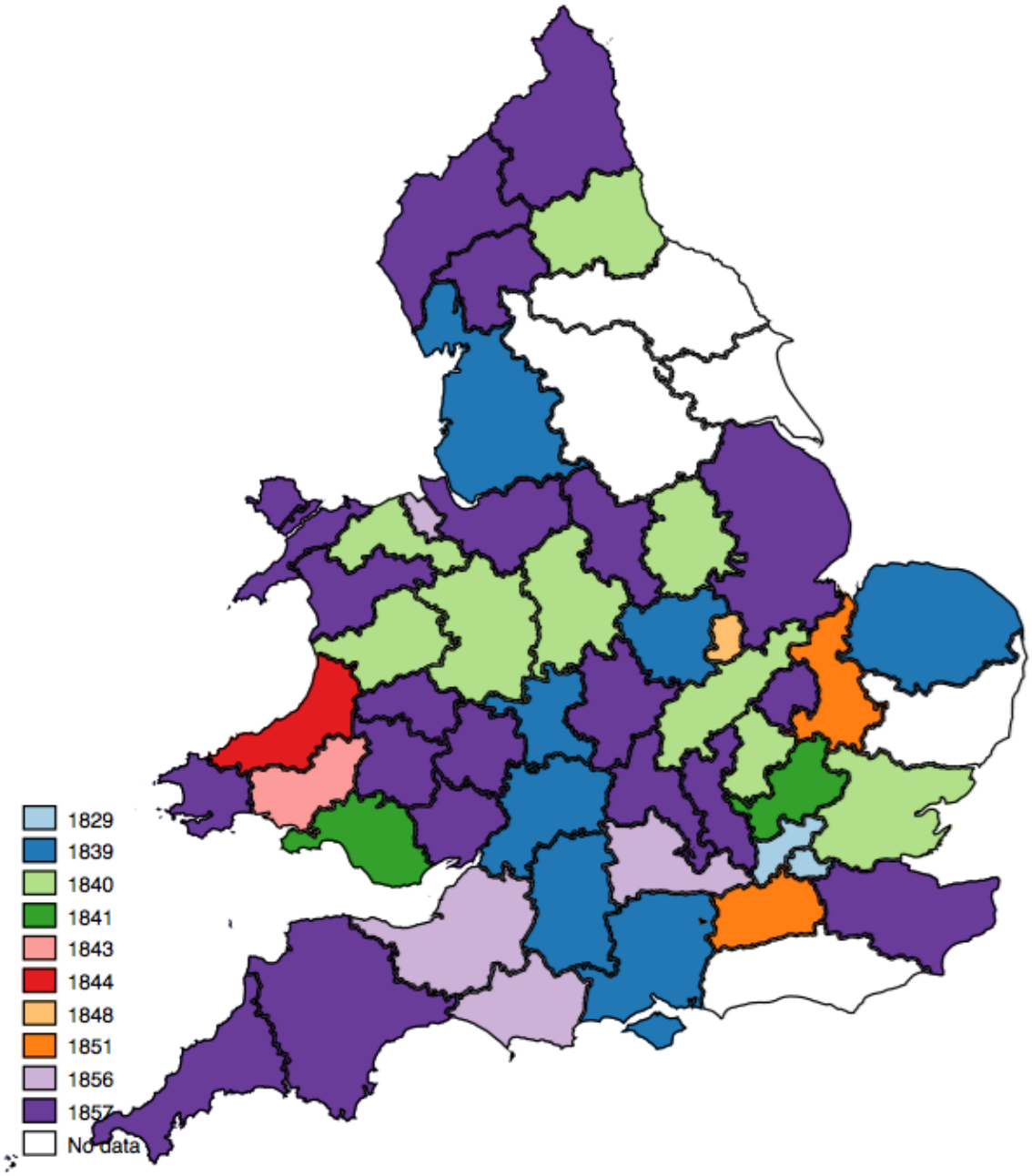


Panel C. Weekly Number of Property Stolen Incidents



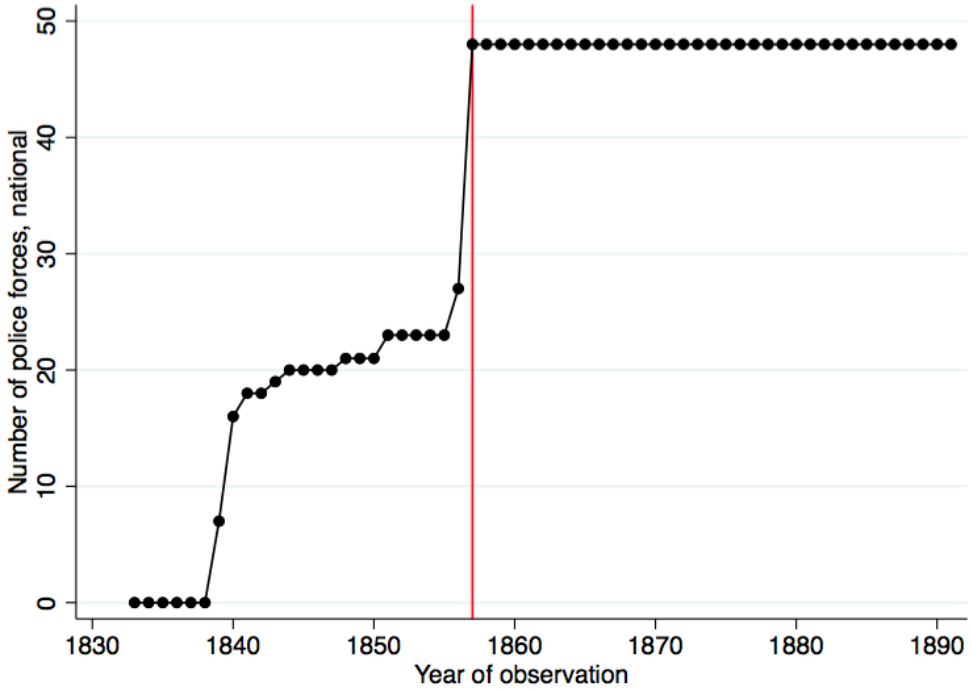
NOTES – Panel A shows the weekly number of informations for property (black line), violent (blue line) and other (green line) incidents for all London police offices (except Thames and the Metropolitan Police, see Section 3.1 for details). Panel B shows the weekly number of charges, again for property, 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 9. 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.

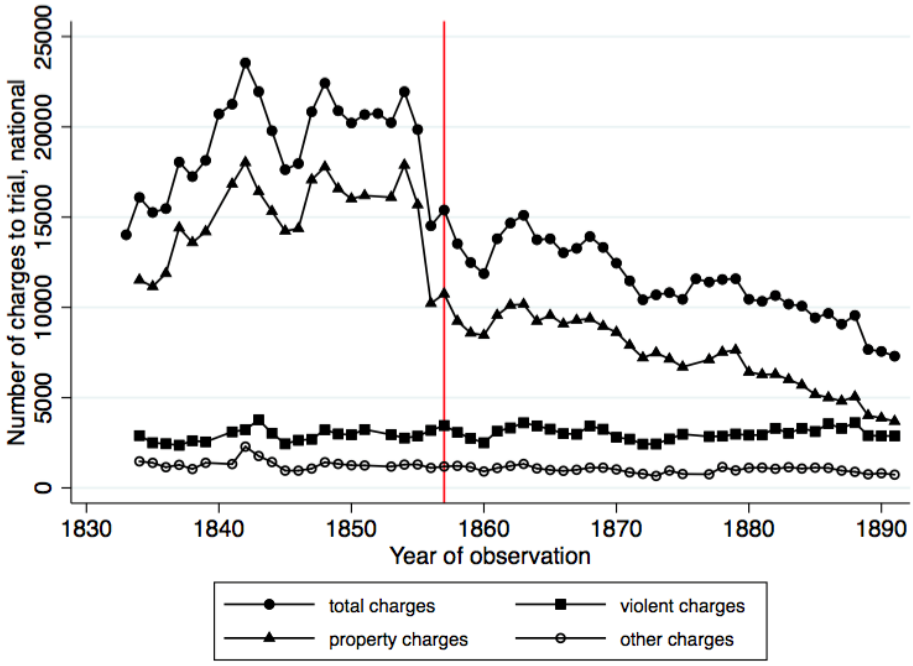
Figure 10. Number of Professional County Police Forces in England and Wales



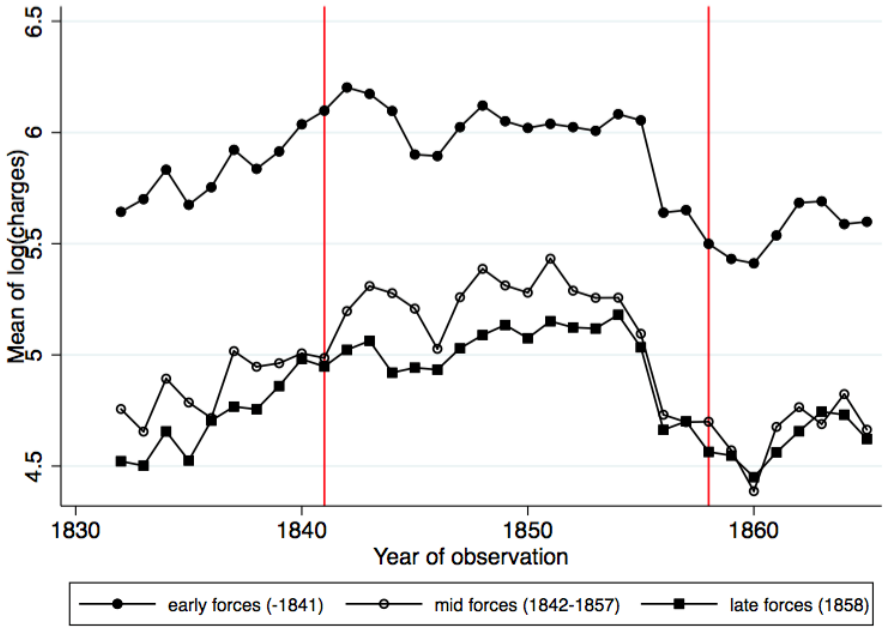
NOTES – This figure shows the number of county police forces in each year for our analysis sample of 48 counties, i.e. excluding Middlesex, York, Suffolk, and Sussex. The red vertical line marks 1857, the year when the creation of a county police force became mandatory. See Section 4.1 and 4.2 for details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

Figure 11. County-Level Data on Charges Brought to Trial

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



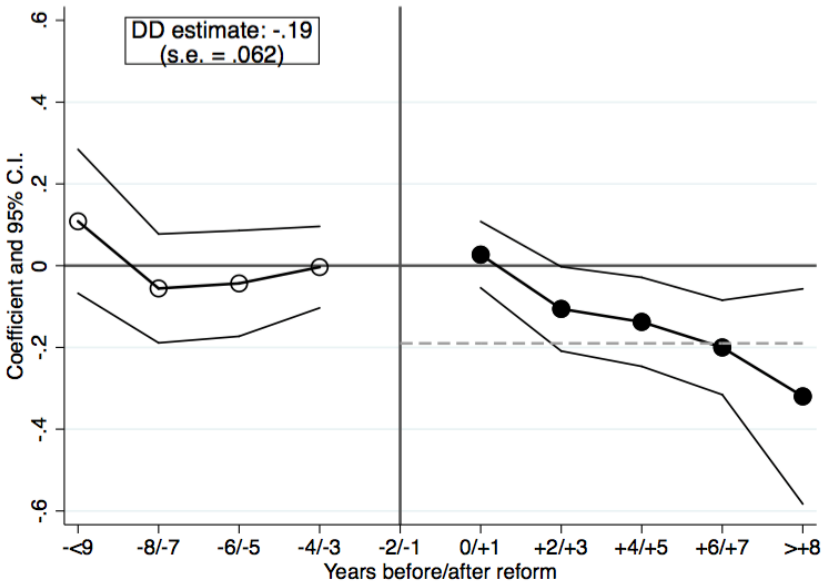
Panel B. Average Log Charges for Early, Mid and Late Reforming Counties



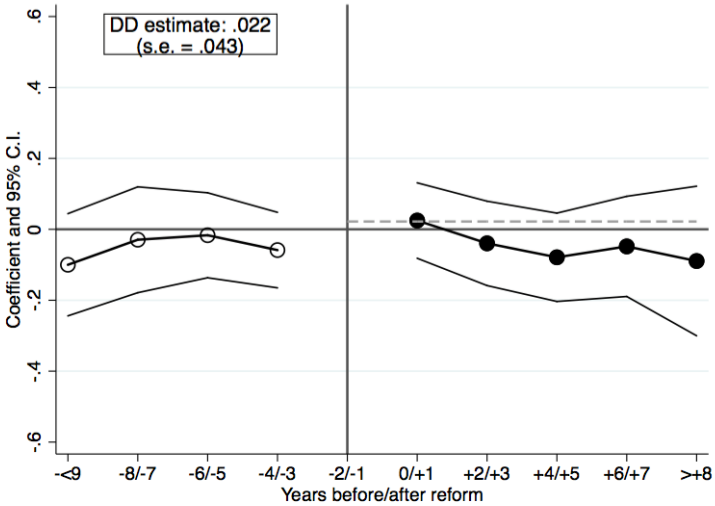
NOTES – Panel A shows the annual number of charges brought to trial in England and Wales, overall and by crime type and for all counties included in the analysis sample, i.e. excluding Middlesex, York, Suffolk, and Sussex. The red vertical line marks 1857, the year when the creation of a county police 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 12. 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/lines correspond to the point estimates and 95% confidence intervals. The vertical line represents the two years before the police force is created (the omitted category). The dashed horizontal line represents the (average) diff-in-diff estimate.

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
Panel A. Pre-Post Analysis								
Treated (<4 miles)								
Post Met	0.059***	-0.009	0.586***	0.491***	-0.493***	-0.528***	0.111***	-0.017
	(0.020)	(0.052)	(0.023)	(0.052)	(0.023)	(0.061)	(0.025)	(0.065)
Observations	1,247	202	1,247	202	1,247	202	1,247	202
Uncertain (4-7 miles)								
Post Met	0.036	-	0.446***	-	-0.245**	-	0.034	-
	(0.074)		(0.070)		(0.095)		(0.070)	
Observations	99	10	99	10	99	10	99	10
Control (>7 miles)								
Post Met	0.031	-	0.191***	-	-0.069	-	0.012	-
	(0.061)		(0.046)		(0.078)		(0.045)	
Observations	168	35	168	35	168	35	168	35
City of London								
Post Met	0.000	-0.025	0.356***	0.240**	-0.247***	-0.185	0.013	-0.138
	(0.042)	(0.098)	(0.053)	(0.119)	(0.065)	(0.150)	(0.063)	(0.147)
Observations	239	50	239	50	239	50	239	50
Panel B. Difference-in-Differences Analysis								
Post Met x Treatment Area	0.032	0.026	0.253***	0.279***	-0.293***	-0.357***	0.064	0.039
	(0.041)	(0.078)	(0.040)	(0.072)	(0.051)	(0.101)	(0.044)	(0.096)
Post Met x Uncertainty Area	0.002	-0.187	0.111	-0.129	-0.045	-0.027	0.011	-0.261
	(0.083)	(0.315)	(0.073)	(0.080)	(0.105)	(0.313)	(0.079)	(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	
Panel A. 1820-1839, Y = Number of crimes per month/area																
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	**
Panel B. 1828-1832, Y = Number of crimes per month/area																
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:	*Four miles from Charing Cross*			*Four miles from Charing Cross*			*Four miles from Charing Cross*		
Specification:	City of London = Control			City of London = Treated from 02 April 1832			City of London = Uncertain		
Panel A. Total crime (by month/area)									
Post Met x Treatment Area	-2.202***	-2.766***	-1.574***	-1.483***	-1.637***	-0.984**	-2.286***	-2.732***	-1.431***
	(0.346)	(0.491)	(0.537)	(0.209)	(0.289)	(0.382)	(0.355)	(0.500)	(0.531)
Post Met x Uncertainty Area	0.234*	0.228	0.151	0.099	0.251	0.233	-0.016	0.171	0.344
	(0.134)	(0.185)	(0.239)	(0.154)	(0.208)	(0.247)	(0.152)	(0.210)	(0.246)
Panel B. Burglary (by month/area)									
Post Met x Treatment Area	-0.947***	-1.220***	-0.397	-0.447***	-0.595***	-0.224	-0.827***	-1.142***	-0.338
	(0.256)	(0.376)	(0.267)	(0.150)	(0.205)	(0.190)	(0.260)	(0.376)	(0.272)
Post Met x Uncertainty Area	0.166	0.082	-0.022	0.214*	0.155	0.007	0.273***	0.171	0.100
	(0.103)	(0.132)	(0.104)	(0.112)	(0.145)	(0.104)	(0.106)	(0.133)	(0.109)
Panel C. Robbery (by month/area)									
Post Met x Treatment Area	-1.292***	-1.345***	-1.297***	-0.978***	-0.885***	-0.832***	-1.504***	-1.439***	-1.281***
	(0.219)	(0.284)	(0.428)	(0.129)	(0.165)	(0.299)	(0.224)	(0.290)	(0.433)
Post Met x Uncertainty Area	0.123	0.190	0.228	-0.017	0.157	0.288*	-0.276***	-0.061	0.144
	(0.083)	(0.119)	(0.162)	(0.093)	(0.127)	(0.170)	(0.096)	(0.132)	(0.189)
Panel D. Homicide (by month/area)									
Post Met x Treatment Area	0.037	-0.200	0.120	-0.058	-0.157	0.072	0.046	-0.151	0.188
	(0.128)	(0.186)	(0.251)	(0.085)	(0.116)	(0.181)	(0.133)	(0.191)	(0.262)
Post Met x Uncertainty Area	-0.055	-0.043	-0.055	-0.099	-0.061	-0.062	-0.013	0.061	0.100
	(0.054)	(0.074)	(0.115)	(0.065)	(0.083)	(0.119)	(0.061)	(0.082)	(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:	*Four miles from Charing Cross*			*Four miles from Charing Cross*		
Specification:	City of London = Control			City of London = Control		
	Extensive margin (1/0): By Week/area			Extensive margin (1/0): By Month/distance bands		
Panel A. Total crime						
Post Met x Treatment Area	-0.121***	-0.166***	-0.099	-0.110***	-0.145***	-0.087
	(0.032)	(0.043)	(0.070)	(0.033)	(0.044)	(0.071)
Post Met x Uncertainty Area	0.037	0.017	-0.030	0.035	-0.000	-0.018
	(0.024)	(0.032)	(0.043)	(0.027)	(0.037)	(0.047)
Panel B. Burglary						
Post Met x Treatment Area	-0.125***	-0.149***	-0.087*	-0.103***	-0.116***	-0.063
	(0.031)	(0.039)	(0.051)	(0.032)	(0.040)	(0.054)
Post Met x Uncertainty Area	0.028	0.005	-0.017	0.026	-0.014	-0.032
	(0.017)	(0.022)	(0.025)	(0.020)	(0.025)	(0.030)
Panel C. Robbery						
Post Met x Treatment Area	-0.136***	-0.159***	-0.127*	-0.134***	-0.161***	-0.168**
	(0.031)	(0.042)	(0.065)	(0.032)	(0.043)	(0.069)
Post Met x Uncertainty Area	0.018	0.029	-0.002	0.013	0.022	0.027
	(0.015)	(0.022)	(0.031)	(0.017)	(0.025)	(0.030)
Panel D. Homicide						
Post Met x Treatment Area	0.028	0.000	0.064	0.013	-0.040	0.044
	(0.024)	(0.034)	(0.047)	(0.026)	(0.036)	(0.051)
Post Met x Uncertainty Area	-0.008	-0.005	-0.007	0.001	-0.004	-0.008
	(0.011)	(0.016)	(0.024)	(0.014)	(0.019)	(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

	All			Before: 1828			After: 1830			After: 1830-1832		
Variable	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Informations												
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	0.048	0.242	800	0.101	0.362	814	0.036	0.192	2,430	0.030	0.182
Number of informations: Other	3,230	0.065	0.282	800	0.124	0.386	814	0.059	0.274	2,430	0.045	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	0.043	0.204	800	0.086	0.281	816	0.037	0.188	2,432	0.029	0.168
Any informations: Other	3,232	0.057	0.232	800	0.107	0.310	816	0.053	0.224	2,432	0.040	0.197
Charges												
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
Property stolen												
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
Panel A. Any informations per day/station								
Post Met Police	-0.149***	-0.149***	-0.148***	-0.148***	-0.206***	-0.090***	-0.049***	-0.055***
	(0.024)	(0.022)	(0.022)	(0.022)	(0.019)	(0.022)	(0.012)	(0.013)
Panel B. Number of informations per day/station								
Post Met Police	-0.302***	-0.302***	-0.302***	-0.301***	-0.371***	-0.170***	-0.064***	-0.065***
	(0.051)	(0.046)	(0.046)	(0.046)	(0.041)	(0.039)	(0.014)	(0.016)
Panel C. Any 'stolen property' per day/station								
Post Met Police	-0.098***	-0.098***	-0.099***	-0.099***	-0.137***	na	na	na
	(0.024)	(0.023)	(0.023)	(0.023)	(0.019)			
Panel D. Number of charges per day/station								
Post Met Police	0.879***	0.879***	0.881***	0.890***	1.471***	0.827***	-0.050**	0.126**
	(0.164)	(0.146)	(0.144)	(0.140)	(0.120)	(0.121)	(0.022)	(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:		*Number of charges*			*Any informations*		*Any 'stolen property'*
Crime type:	total	prop	viol	total	prop	viol	total
Post Met: 1830, January	0.030	0.214	-0.056	-0.114***	-0.045	-0.055***	-0.114***
	(0.246)	(0.209)	(0.037)	(0.038)	(0.038)	(0.019)	(0.040)
Post Met: 1830, > January	1.177***	1.040***	-0.049**	-0.160***	-0.106***	-0.046***	-0.094***
	(0.163)	(0.140)	(0.023)	(0.024)	(0.023)	(0.012)	(0.025)
Post Met: 1831	1.382***	1.146***	-0.065***	-0.220***	-0.141***	-0.054***	-0.125***
	(0.151)	(0.129)	(0.021)	(0.021)	(0.021)	(0.011)	(0.023)
Post Met: 1832	2.157***	1.783***	-0.034	-0.250***	-0.174***	-0.068***	-0.187***
	(0.160)	(0.137)	(0.022)	(0.021)	(0.021)	(0.011)	(0.022)
p-value	*0.000*	*0.000*	*0.483*	*0.000*	*0.000*	*0.079*	*0.000*
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
Snap Shot Variables												
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	642,642	347,403	16	733,137	277,302	9	507,433	244,367	23	632,598	412,494
Population (1858 Jud.Stats.)	48	191,492	153,919	16	272,118	200,542	9	132,879	93,997	23	158,340	112,680
People per police (initial)	47	2,857	2,493	15	3,098	1,974	9	3,074	3,223	23	2,615	2,572
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
1851 Census Variables												
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
Panel A: All Charges				
Force	0.008	-0.024	0.015	-0.018
	[0.037]	[0.033]	[0.038]	[0.034]
Observations	1,632	1,632	1,632	1,632
R-squared	0.959	0.959	0.891	0.891
Panel B: Violent Charges				
Force	-0.018	-0.031	-0.006	-0.020
	[0.053]	[0.050]	[0.054]	[0.051]
Observations	1,431	1,431	1,431	1,431
R-squared	0.894	0.894	0.712	0.712
Panel C: Property Charges				
Force	0.029	0.017	0.041	0.027
	[0.044]	[0.042]	[0.043]	[0.042]
Observations	1,440	1,440	1,440	1,440
R-squared	0.958	0.958	0.896	0.896
Panel D: Other Charges				
Force	-0.109	-0.179**	-0.095	-0.166*
	[0.086]	[0.081]	[0.088]	[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 *People Per Policeman* (upon police force creation)				
	1,500	1,750	2,000	2,250	2,500
Panel A: All Charges					
Force Efficient	-0.190***	-0.127**	-0.118**	-0.109**	-0.079
	[0.062]	[0.058]	[0.055]	[0.049]	[0.048]
Force Inefficient	0.022	0.040	0.053	0.088	0.066
	[0.043]	[0.051]	[0.057]	[0.068]	[0.075]
Panel B: Violent Charges					
Force Efficient	-0.183*	-0.134	-0.129*	-0.130**	-0.093
	[0.104]	[0.080]	[0.070]	[0.062]	[0.062]
Force Inefficient	-0.002	0.020	0.034	0.081	0.048
	[0.058]	[0.068]	[0.078]	[0.094]	[0.104]
Panel C: Property Charges					
Force Efficient	-0.143**	-0.073	-0.063	-0.050	-0.028
	[0.065]	[0.069]	[0.066]	[0.058]	[0.057]
Force Inefficient	0.064	0.080	0.090	0.113	0.101
	[0.050]	[0.055]	[0.060]	[0.073]	[0.083]
Panel D: Other Charges					
Force Efficient	-0.243**	-0.146	-0.125	-0.157*	-0.132
	[0.112]	[0.099]	[0.091]	[0.092]	[0.088]
Force Inefficient	-0.151*	-0.192*	-0.219**	-0.198*	-0.253*
	[0.090]	[0.102]	[0.109]	[0.116]	[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

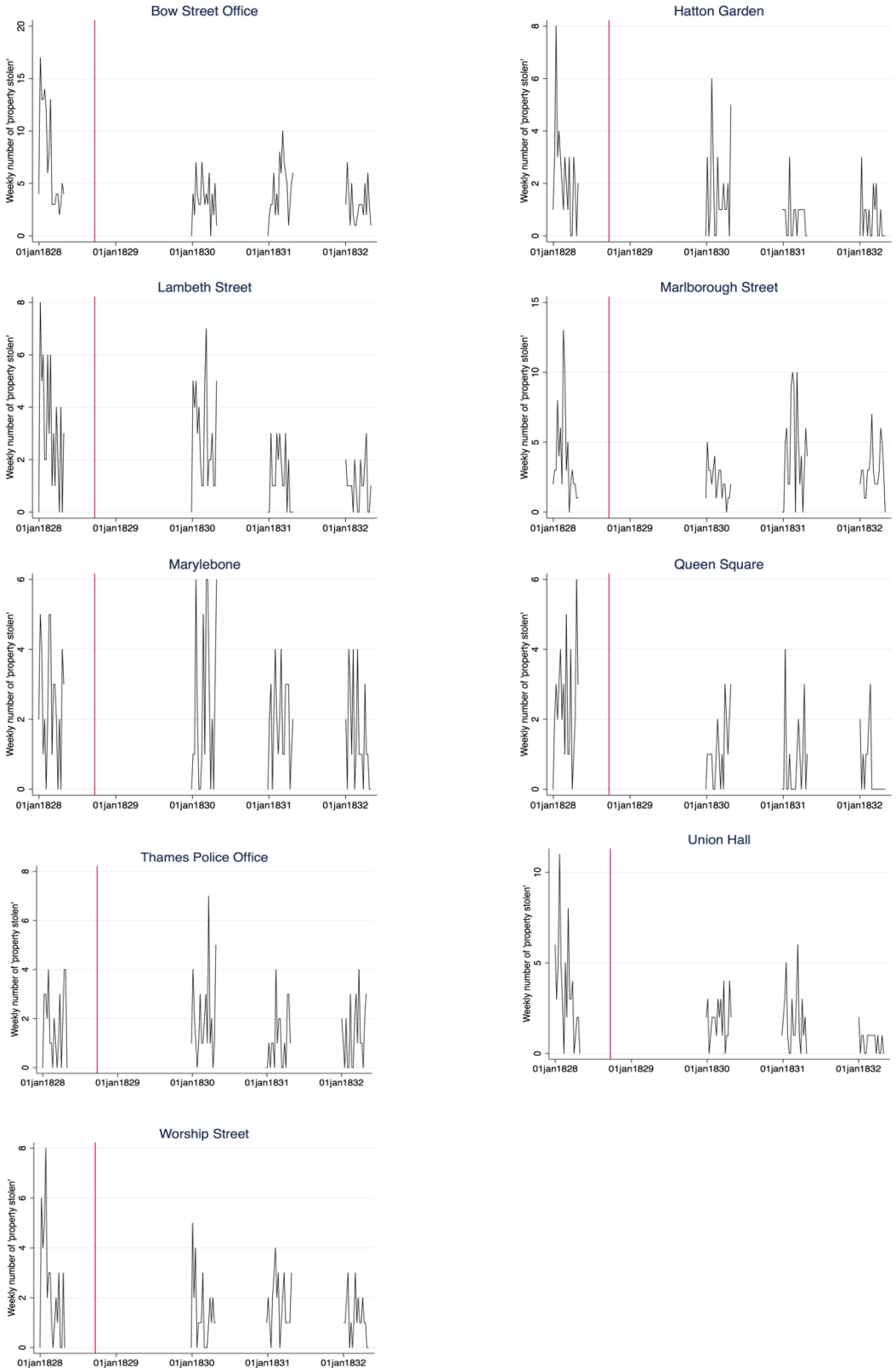
Table 11. Spillover Effects of Neighboring Police Forces

	(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***	-0.190***	-0.184***	-0.183*	-0.183*	-0.182*	-0.143**	-0.143**	-0.140**	-0.243**	-0.243**	-0.244**
	[0.062]	[0.069]	[0.060]	[0.104]	[0.108]	[0.101]	[0.065]	[0.068]	[0.063]	[0.112]	[0.111]	[0.109]
Own Force Inefficient	0.022	0.021	-0.006	-0.002	-0.003	-0.030	0.064	0.062	0.040	-0.151*	-0.151*	-0.166*
	[0.043]	[0.044]	[0.045]	[0.058]	[0.059]	[0.059]	[0.050]	[0.049]	[0.052]	[0.090]	[0.088]	[0.089]
Any Neighboring Force		0.189***			0.109			0.253***			-0.106	
		[0.059]			[0.112]			[0.055]			[0.096]	
Any Neighboring Efficient Force			-0.158**			-0.192**			-0.149**			-0.106
			[0.060]			[0.074]			[0.068]			[0.086]
Any Neighboring Inefficient Force			0.131**			0.064			0.199***			0.013
			[0.055]			[0.137]			[0.054]			[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

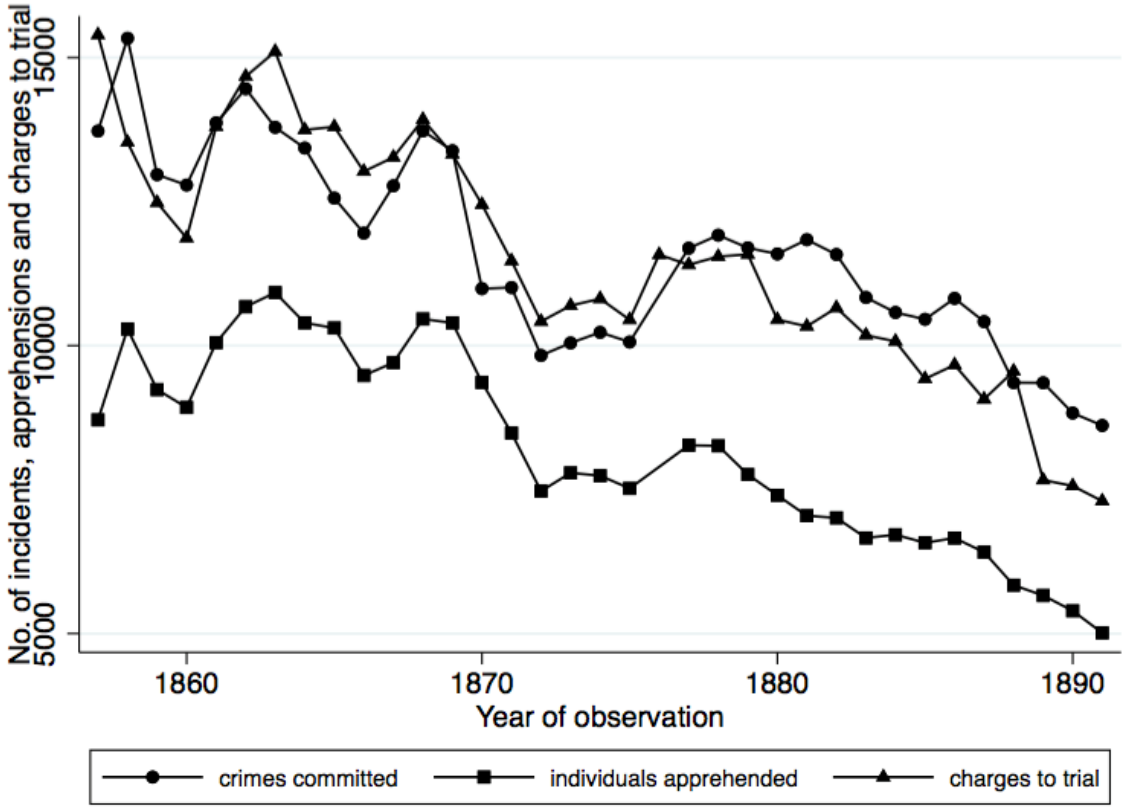
Appendix A: Additional Figures and Tables

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).

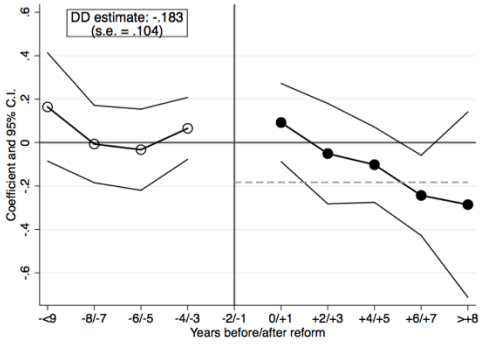
Appendix Figure A2. Charges Brought to Trial as a Crime Proxy (County Analysis)

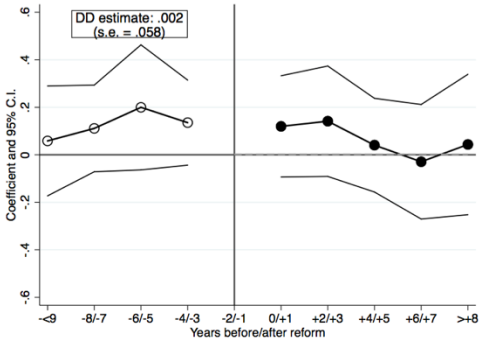


NOTES – The figure presents the national annual number of crimes committed, charges brought to trial, and individuals apprehended in all England and Wales counties, excluding Middlesex, York, Suffolk, and Sussex from 1857 to 1891. The number of charges to trial is the main outcome variable used in the 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.

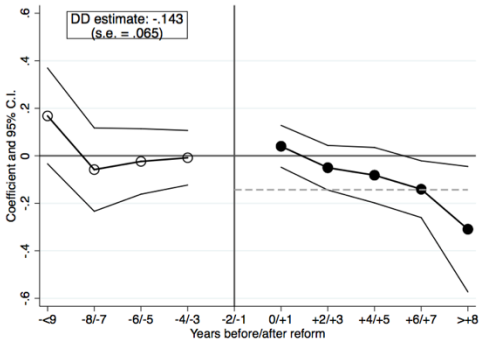
Appendix Figure A3. Event-Study of Efficient/Inefficient County Police Forces, By Crime Type

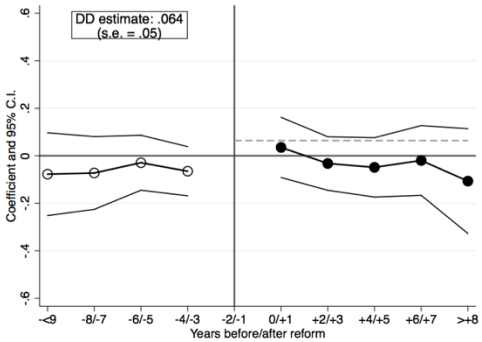
Panel A: Violent Charges



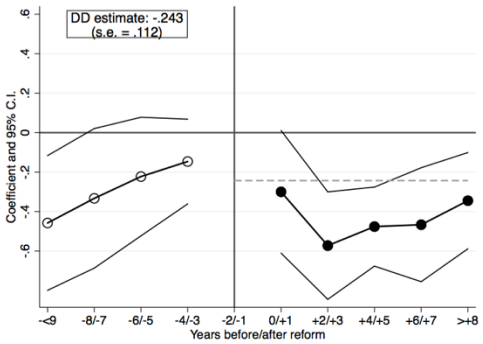


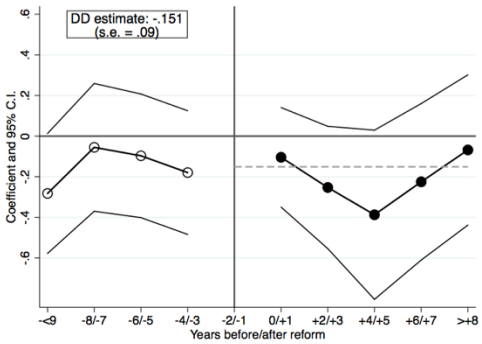
Panel B: Property Charges





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 12 for details.

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

		(1)	(2)	(3)	(4)	(5)
Police division(s): Metropolitan Police	Police office(s): Pre-existing Police	All hires: 21 Sep 1829 - 27 Mar 1830 all	All hires: 21 Sep 1829 - 27 Mar 1830	Early hires: 21 Sep 1829 - 31 Jan 1830	Late hires: 01 Feb 1830 - 27 Mar 1830	Category Early/late
			service length >= 250 days			
Panel A. Separately by division						
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
Panel B. Aggregated by divsion-office						
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

	within 7-miles radius				City of London				outside 7-miles radius			
Variable	1820-1829	1829-1839	1825-1835	1828-1832	1820-1829	1829-1839	1825-1835	1828-1832	1820-1829	1829-1839	1825-1835	1828-1832
Crime												
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
Police												
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	
Panel A. Total crime								
Post Met x Treatment Area	-2.959***	-1.602***	-2.033***	-1.583***	-0.874***	-0.669*	-2.227***	-1.561***
	(0.612)	(0.584)	(0.300)	(0.491)	(0.252)	(0.365)	(0.346)	(0.521)
Post Met x Uncertainty Area	0.279	0.467	0.218*	0.092	0.055	0.031	0.233*	0.139
	(0.267)	(0.403)	(0.126)	(0.221)	(0.109)	(0.188)	(0.134)	(0.238)
Panel B. Burglary								
Post Met x Treatment Area	-1.719***	0.066	-0.978***	-0.358	-0.314*	-0.340	-0.947***	-0.397
	(0.444)	(0.370)	(0.239)	(0.267)	(0.179)	(0.217)	(0.256)	(0.267)
Post Met x Uncertainty Area	0.119	-0.114	0.170*	0.017	0.132*	-0.040	0.166	-0.022
	(0.181)	(0.121)	(0.098)	(0.104)	(0.079)	(0.105)	(0.103)	(0.104)
Panel C. Robbery								
Post Met x Treatment Area	-0.935**	-1.449***	-1.020***	-1.222***	-0.690***	-0.521*	-1.292***	-1.297***
	(0.391)	(0.476)	(0.188)	(0.385)	(0.152)	(0.304)	(0.219)	(0.428)
Post Met x Uncertainty Area	0.227	0.565*	0.087	0.128	-0.018	0.129	0.123	0.228
	(0.169)	(0.300)	(0.076)	(0.143)	(0.065)	(0.114)	(0.083)	(0.162)
Panel D. Homicide								
Post Met x Treatment Area	-0.306	-0.219	-0.036	-0.003	0.131	0.192	0.011	0.132
	(0.251)	(0.373)	(0.119)	(0.227)	(0.095)	(0.181)	(0.126)	(0.254)
Post Met x Uncertainty Area	-0.067	0.017	-0.039	-0.053	-0.059	-0.058	-0.056	-0.068
	(0.098)	(0.157)	(0.053)	(0.113)	(0.043)	(0.100)	(0.054)	(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:				Drop one office at the time:						Weekly	
	Bow Street	Hatton Garden	Lambeth Street	Marlye-bone	Marlborough Street	Queen Square	Union Hall	Worship Street	All weeks	Complete weeks	Log outcome
Panel A. Any informations											
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)	
Panel B. Any 'stolen property'											
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)	
Panel C. Number of charges											
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; but crime data was not available for same sub-jurisdicitons.					
Suffolk						

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, Harbouring 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
Panel A. Dependent Variable = Log Total Charges											
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]
Panel B. Dependent Variable = Log Violent Charges											
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]
Panel C. Dependent Variable = Log Property Charges											
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]
Panel D. Dependent Variable = Log Other Charges											
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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Early adopter? 1840 Cross-section				All Counties: 1840 (earliest possible) -1857 (last possible) adoption				
Variable	Dependent Variable = adoption (1 in year of adoption)								
Lag1: Charge rate	0.010	-0.012	-0.005		-0.005	-0.006	-0.004	-0.004	
	[0.032]	[0.016]	[0.022]		[0.004]	[0.005]	[0.005]	[0.005]	
Lag1: Any neighboring force			-0.106	-0.105			-0.070*		
			[0.197]	[0.239]			[0.040]		
Lag1: Any efficient neighboring force								-0.003	-0.007
								[0.025]	[0.027]
Lag1: Any inefficient neighboring force								-0.067*	-0.089*
								[0.036]	[0.045]
Lag1: Violent crime charge rate				-1.252					0.080
				[0.993]					[0.132]
Lag1: Property crime charge rate				0.154					-0.036
				[0.164]					[0.028]
Lag1: Other crime charge rate				0.664					0.260
				[0.540]					[0.186]
Population		0.000***	0.000***	0.000**		0.000	0.000	0.000	0.000
		[0.000]	[0.000]	[0.000]		[0.000]	[0.000]	[0.000]	[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

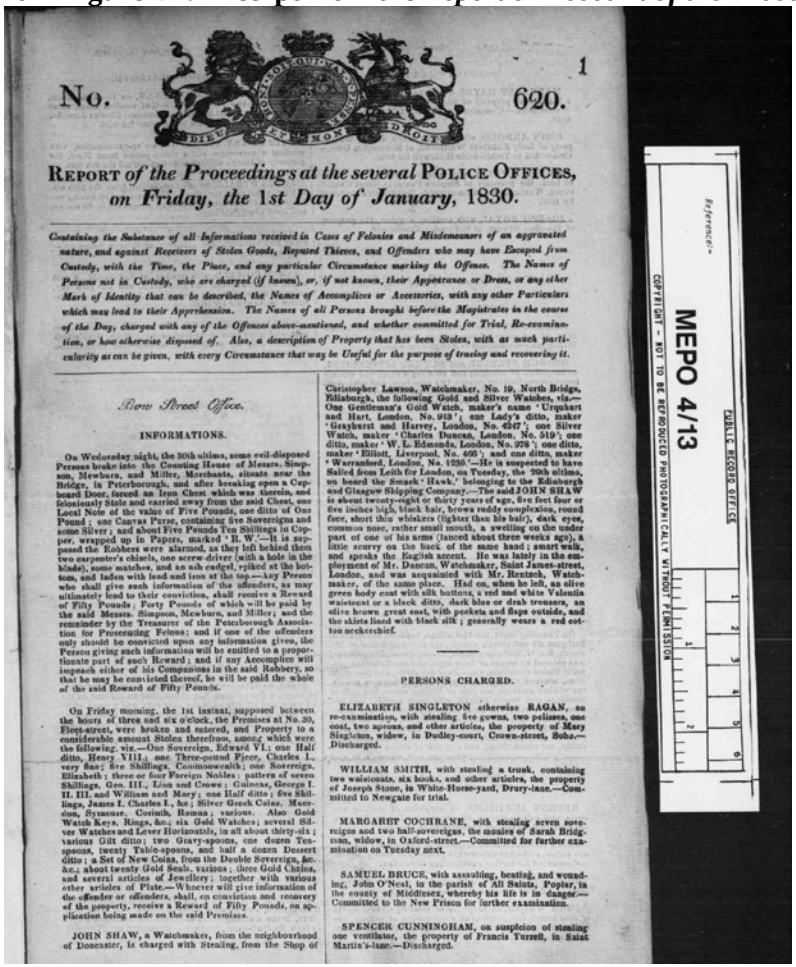
Appendix Table A9. Determinants of Initial Force Efficiency

	(1)	(2)	(3)	(4)
	Dependent Variable (measured at force creation):			
	Efficient? People/Police < 1500?	People/Police	Efficient? People/Police < 1500?	People/Police
Fixed Geographic Variables				
Acres	0.000	-0.003	0.000	-0.004*
	[0.000]	[0.002]	[0.000]	[0.002]
Number of parishes	0.000	0.499	-0.000	2.186
	[0.001]	[3.844]	[0.002]	[4.554]
Number of neighbors	0.029	-35.477	0.030	33.447
	[0.048]	[239.006]	[0.049]	[245.052]
England	-0.153	888.542	-0.064	1,915.566
	[0.179]	[1,435.166]	[0.296]	[2,424.436]
Variables measured in the 1851 census (shares)				
Farmer	-0.017	-8.729	-0.021	41.065
	[0.017]	[112.116]	[0.022]	[152.019]
Male	-0.104**	522.620	-0.114*	379.598
	[0.048]	[388.323]	[0.063]	[398.124]
Married	0.050	267.177	0.052	60.880
	[0.073]	[427.595]	[0.099]	[524.287]
Native	-0.049	-102.064	-0.045	-70.625
	[0.088]	[325.609]	[0.108]	[450.446]
Employed	-0.007	-246.094	0.005	-285.889
	[0.031]	[176.390]	[0.036]	[192.385]
Age 0-15	-0.131	-536.135	-0.164	-146.516
	[0.127]	[589.462]	[0.174]	[748.652]
Age 16-25	0.010	1,363.391	-0.071	1,318.013
	[0.162]	[1,019.616]	[0.192]	[1,411.150]
Age 26-35	-0.219	23.037	-0.183	178.901
	[0.209]	[1,314.718]	[0.277]	[1,319.783]
Age 36-45	0.005	-1,844.465	-0.135	34.450
	[0.457]	[1,780.253]	[0.620]	[2,241.751]
Age 46-55	-0.263	3,415.117**	-0.340	3,518.238*
	[0.326]	[1,645.359]	[0.380]	[1,794.485]
Variables measured in the year before force adoption				
Violent crime rate			-0.201	1,419.548
			[1.024]	[4,230.184]
Property crime rate			0.014	-633.048
			[0.170]	[661.370]
Other crime rate			0.378	776.423
			[0.621]	[3,286.349]
Any neighbors with eff. force			-0.078	-739.059
			[0.252]	[1,187.424]
Any neighbors with ineff. force			0.225	-705.270
			[0.211]	[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*



No. 1

620.

REPORT *of the Proceedings at the several* POLICE OFFICES, *on Friday, the 1st Day of January,* 1830.

Containing the Substance of all Informations received in Cases of Felonies and Misdemeanors of an aggravated nature, and against Receivers of Stolen Goods, Reputed Thieves, and Offenders who may have Escaped from Custody, with the Time, the Place, and any particular Circumstance marking the Offence. The Names of Persons not in Custody, who are charged (if known), or, if not known, their Appearance or Dress, or any other Mark of Identity that can be described, the Names of Accomplices or Accessories, with any other Particulars which may lead to their Apprehension. The Names of all Persons brought before the Magistrates in the course of the Day, charged with any of the Offences above-mentioned, and whether committed for Trial, Re-examination, or how otherwise disposed of. Also, a description of Property that has been Stolen, with as much particularity as can be given, with every Circumstance that may be Useful for the purpose of tracing and recovering it.

Bow Street Office.

INFORMATIONS.

On Wednesday night, the 30th ultimo, some evil-disposed Persons broke into the Counting House of Messrs. Simpson, Mewburn, and Miller, Merchants, situate near the Bridge, in Peterborough, and after breaking open a Cupboard Door, forced an Iron Chest which was therein, and feloniously Stole and carried away from the said Chest, one Local Note of the value of Five Pounds, one ditto of One Pound; one Canvas Purse, containing five Sovereigns and some Silver; and about Five Pounds Ten Shillings in Copper, wrapped up in Papers, marked 'R. W.'—It is supposed the Robbers were alarmed, as they left behind them two carpenter's chisels, one screw-driver (with a hole in the blade), some matches, and an ash cudgel, spiked at the bottom, and laden with lead and iron at the top.—Any Person who shall give such information of the offenders, as may ultimately lead to their conviction, shall receive a Reward of Fifty Pounds; Forty Pounds of which will be paid by the said Messrs. Simpson, Mewburn, and Miller; and the remainder by the Treasurer of the Peterborough Association for Prosecuting Felons; and if one of the offenders only should be convicted upon any information given, the Person giving such information will be entitled to a proportionate part of such Reward; and if any Accomplice will impeach either of his Companions in the said Robbery, so that he may be convicted thereof, he will be paid the whole of the said Reward of Fifty Pounds.

On Friday morning, the 1st instant, supposed between the hours of three and six o'clock, the Premises at No. 30, Fleet-street, were broken and entered, and Property to a considerable amount Stolen therefrom, among which were the following, viz.—One Sovereign, Edward VI.; one Half ditto, Henry VIII.; one Three-pound Piece, Charles I., very fine; five Shillings, Commonwealth; one Sovereign, Elizabeth; three or four Foreign Nobles; pattern of seven Shillings, Geo. III., Lion and Crown; Guineas, George I. II. III. and William and Mary; one Half ditto; five Shillings, James I. Charles I., &c.; Silver Greek Coins, Macedon, Syracuse, Corinth, Roman; various. Also Gold Watch Keys, Rings, &c.; six Gold Watches; several Silver Watches and Lever Horizontals, in all about thirty-six; various Gilt ditto; two Gravy-spoons, one dozen Tea-spoons, twenty Table-spoons, and half a dozen Dessert ditto; a Set of New Coins, from the Double Sovereign, &c. &c.; about twenty Gold Seals, various; three Gold Chains, and several articles of Jewellery; together with various other articles of Plate.—Whoever will give information of the offender or offenders, shall, on conviction and recovery of the property, receive a Reward of Fifty Pounds, on application being made on the said Premises.

JOHN SHAW, a Watchmaker, from the neighbourhood of Doncaster, is charged with Stealing, from the Shop of Christopher Lawson, Watchmaker, No. 19, North Bridge, Edinburgh, the following Gold and Silver Watches, viz.—One Gentleman's Gold Watch, maker's name 'Urquhart and Hart, London, No. 913'; one Lady's ditto, maker 'Graybarst and Harvey, London, No. 6247'; one Silver Watch, maker 'Charles Duncan, London, No. 519'; one ditto, maker 'W. L. Edmonds, London, No. 978'; one ditto, maker 'Elliott, Liverpool, No. 466'; and one ditto, maker 'Warranford, London, No. 1235.'—He is suspected to have Sailed from Leith for London, on Tuesday, the 29th ultimo, on board the Smack 'Hawk,' belonging to the Edinburgh and Glasgow Shipping Company.—The said JOHN SHAW is about twenty-eight or thirty years of age, five feet four or five inches high, black hair, brown ruddy complexion, round face, short thin whiskers (lighter than his hair), dark eyes, common nose, rather small mouth, a swelling on the under part of one of his arms (lanced about three weeks ago), a little scurvy on the back of the same hand; smart walk, and speaks the English accent. He was lately in the employment of Mr. Duncan, Watchmaker, Saint James-street, London, and was acquainted with Mr. Rentzch, Watchmaker, of the same place. Had on, when he left, an olive green body coat with silk buttons, a red and white Valentia waistcoat or a black ditto, dark blue or drab trousers, an olive brown great coat, with pockets and flaps outside, and the shirts lined with black silk; generally wears a red cotton neckerchief.

PERSONS CHARGED.

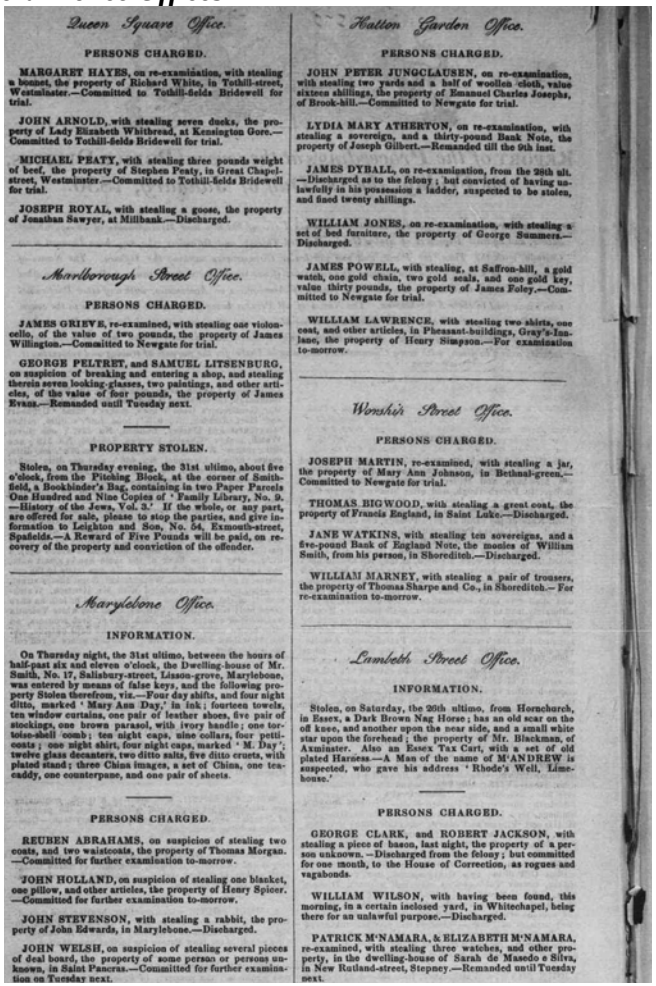
ELIZABETH SINGLETON otherwise RAGAN, on re-examination, with stealing five gowns, two petticoats, one coat, two aprons, and other articles, the property of Mary Singleton, widow, in Dudley-court, Crown-street, Soho.—Discharged.

WILLIAM SMITH, with stealing a trunk, containing two waistcoats, six books, and other articles, the property of Joseph Stone, in White-Horse-yard, Drury-lane.—Committed to Newgate for trial.

MARGARET COCHRANE, with stealing seven sovereigns and two half-sovereigns, the monies of Sarah Bridgman, widow, in Oxford-street.—Committed for further examination on Tuesday next.

SAMUEL BRUCE, with assaulting, beating, and wounding, John O'Neal, in the parish of All Saints, Poplar, in the county of Middlesex, whereby his life is in danger.—Committed to the New Prison for further examination.

SPENCER CUNNINGHAM, on suspicion of stealing one ventilator, the property of Francis Turrell, in Saint Martin's-lane.—Discharged.



Queen Square Office.

PERSONS CHARGED.

MARGARET HAYES, on re-examination, with stealing a bonnet, the property of Richard White, in Tothill-street, Westminster.—Committed to Tothill-fields Bridewell for trial.

JOHN ARNOLD, with stealing seven ducks, the property of Lady Elizabeth Whitbread, at Kensington Gore.—Committed to Tothill-fields Bridewell for trial.

MICHAEL PEATY, with stealing three pounds weight of beef, the property of Stephen Peaty, in Great Chapel-street, Westminster.—Committed to Tothill-fields Bridewell for trial.

JOSEPH ROYAL, with stealing a goose, the property of Jonathan Sawyer, at Millbank.—Discharged.

Marlborough Street Office.

PERSONS CHARGED.

JAMES GRIEVE, re-examined, with stealing one violoncello, of the value of two pounds, the property of James Willington.—Committed to Newgate for trial.

GEORGE PELTRET, and SAMUEL LITSENBURG, on suspicion of breaking and entering a shop, and stealing therein seven looking-glasses, two paintings, and other articles, of the value of four pounds, the property of James Evans.—Remanded until Tuesday next.

PROPERTY STOLEN.

Stolen, on Thursday evening, the 31st ultimo, about five o'clock, from the Pitching Block, at the corner of Smithfield, a Bookbinder's Bag, containing in two Paper Parcels One Hundred and Nine Copies of 'Family Library, No. 9. —History of the Jews, Vol. 2.' If the whole, or any part, are offered for sale, please to stop the parties, and give information to Leighton and Son, No. 54, Exmouth-street, Spafields.—A Reward of Five Pounds will be paid, on recovery of the property and conviction of the offender.

Marylebone Office.

INFORMATION.

On Thursday night, the 31st ultimo, between the hours of half-past six and eleven o'clock, the Dwelling-house of Mr. Smith, No. 17, Salisbury-street, Lisson-grove, Marylebone, was entered by means of false keys, and the following property Stolen therefrom, viz.—Four day shifts, and four night ditto, marked 'Mary Ann Day,' in ink; fourteen towels, ten window curtains, one pair of leather shoes, five pair of stockings, one brown parasol, with ivory handle; one tortoise-shell comb; ten night caps, nine collars, four petticoats; one night shirt, four night caps, marked 'M. Day'; twelve glass decanters, two ditto salts, five ditto cruets, with plated stand; three China images, a set of China, one tea-caddy, one counterpane, and one pair of sheets.

PERSONS CHARGED.

REUBEN ABRAHAMS, on suspicion of stealing two coats, and two waistcoats, the property of Thomas Morgan.—Committed for further examination to-morrow.

JOHN HOLLAND, on suspicion of stealing one blanket, one pillow, and other articles, the property of Henry Spicer.—Committed for further examination to-morrow.

JOHN STEVENSON, with stealing a rabbit, the property of John Edwards, in Marylebone.—Discharged.

JOHN WELSH, on suspicion of stealing several pieces of deal board, the property of some person or persons unknown, in Saint Pancras.—Committed for further examination on Tuesday next.

Hatton Garden Office.

PERSONS CHARGED.

JOHN PETER JUNGCLAUSEN, on re-examination, with stealing two yards and a half of woollen cloth, value sixteen shillings, the property of Emanuel Charles Josephs, of Brook-hill.—Committed to Newgate for trial.

LYDIA MARY ATHERTON, on re-examination, with stealing a sovereign, and a thirty-pound Bank Note, the property of Joseph Gilbert.—Remanded till the 9th inst.

JAMES DYBALL, on re-examination, from the 28th ult. —Discharged as to the felony; but convicted of having unlawfully in his possession a ladder, suspected to be stolen, and fined twenty shillings.

WILLIAM JONES, on re-examination, with stealing a set of bed furniture, the property of George Summers.—Discharged.

JAMES POWELL, with stealing, at Saffron-hill, a gold watch, one gold chain, two gold seals, and one gold key, value thirty pounds, the property of James Foley.—Committed to Newgate for trial.

WILLIAM LAWRENCE, with stealing two shirts, one coat, and other articles, in Pheasant-buildings, Gray's-Inn-lane, the property of Henry Simpson.—For examination to-morrow.

Worship Street Office.

PERSONS CHARGED.

JOSEPH MARTIN, re-examined, with stealing a jar, the property of Mary Ann Johnson, in Bethnal-green.—Committed to Newgate for trial.

THOMAS BIGWOOD, with stealing a great coat, the property of Francis England, in Saint Luke.—Discharged.

JANE WATKINS, with stealing ten sovereigns, and a five-pound Bank of England Note, the monies of William Smith, from his person, in Shoreditch.—Discharged.

WILLIAM MARNEY, with stealing a pair of trousers, the property of Thomas Sharpe and Co., in Shoreditch.—For re-examination to-morrow.

Lambeth Street Office.

INFORMATION.

Stolen, on Saturday, the 26th ultimo, from Hornchurch, in Essex, a Dark Brown Nag Horse; has an old scar on the off knee, and another upon the near side, and a small white star upon the forehead; the property of Mr. Blackman, of Axminster. Also an Essex Tax Cart, with a set of old plated Harness.—A Man of the name of M'ANDREW is suspected, who gave his address 'Rhode's Well, Limehouse.'

PERSONS CHARGED.

GEORGE CLARK, and ROBERT JACKSON, with stealing a piece of bacon, last night, the property of a person unknown.—Discharged from the felony; but committed for one month, to the House of Correction, as rogues and vagabonds.

WILLIAM WILSON, with having been found, this morning, in a certain inclosed yard, in Whitechapel, being there for an unlawful purpose.—Discharged.

PATRICK M'NAMARA, & ELIZABETH M'NAMARA, re-examined, with stealing three watches, and other property, in the dwelling-house of Sarah de Masedo e Silva, in New Rutland-street, Stepney.—Remanded until Tuesday next.

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).

Appendix Figure B2. Excerpt of the *Judicial Statistics* (County of Bedford, 1844)

COUNTY OF BEDFORD. 1844. 1

TABLE showing the Number of PERSONS COMMITTED for TRIAL or BAILED, and the Result of the Proceedings.

OFFENCES.	TOTAL NUMBER OF OFFENDERS.	CONVICTED. DEATH.	Transportation. Life.	Transportation. Above 15 Years.	Transportation. 15 Years and above 10 Years.	Transportation. 10 Years and above 7 Years.	Transportation. 7 Years.	Imprisonment, with, in some Cases, Whipping, Fine, &c. Above 3 Years.	Imprisonment. 3 Years and above 2 Years.	Imprisonment. 2 Years and above 1 Year.	Imprisonment. 1 Year and above 6 Months.	Imprisonment. 6 Months and under.	Whipping, Fine, and Discharge on Sureties.	Sentence respited and Pardon.	TOTAL.	INSANE to be detained. Found Insane on Arraignment.	Acquitted as being Insane.	ACQUITTED. Not Guilty on Trial.	No Bill found.	No Prosecution.	TOTAL.	EXECUTION of CAPITAL SENTENCES, or COMMUTATION or PARDON recommended by the Judge. Execution.	Commutation. Transportation. Life.	15 Years and above 10 Years.	10 Years and above 7 Years.	7 Years.	Commutation. Imprisonment. 3 Years and above 2 Years.	2 Years and above 1 Year.	1 Year and above 6 Months.	6 Months and under.	Free Pardon.
No. 1. Murder	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Attempts to Murder, attended with dangerous bodily injuries	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Attempts to Murder, unattended with bodily injuries	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Shooting at, Stabbing, Wounding, &c., with intent to maim, disfigure, &c.	2	-	-	-	2	-	-	-	-	-	-	-	-	-	2	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Manslaughter	4	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	3	1	-	4	-	-	-	-	-	-	-	-	-	-
Attempts to procure the Miscarriage of Women	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Concealing the Births of Infants	1	-	-	-	-	-	-	-	-	-	-	1	-	-	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Sodomy	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assaults, with intent to commit Sodomy, and other unnatural Misdemeanors	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	1	-	-	1	-	-	-	-	-	-	-	-	-	-
Rape, and carnally abusing Girls under the Age of Ten Years	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assaults, with intent to ravish and carnally abuse	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Carnally abusing Girls between the Age of Ten and Twelve Years	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Abduction	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Bigamy	1	-	-	-	-	-	-	-	-	-	1	-	-	-	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Child Stealing	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assaults	1	-	-	-	-	-	-	-	-	-	-	-	1	-	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assaults on Peace Officers in the Execution of their Duty	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total of No. 1.	10	-	-	-	2	-	-	-	-	-	1	1	1	-	5	-	-	4	1	-	5	-	-	-	-	-	-	-	-	-	-
No. 2. Offences against Property committed with Violence. Sacrilege	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Burglary	4	-	1	-	1	2	-	-	-	-	-	-	-	-	4	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Burglary, attended with violence to Persons	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Housebreaking	6	-	-	-	-	1	-	-	-	-	-	2	-	-	3	-	-	1	2	-	3	-	-	-	-	-	-	-	-	-	-
Breaking within the Curtilage of Dwelling-houses, and Stealing	2	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	1	1	-	2	-	-	-	-	-	-	-	-	-	-
Breaking into Shops, Warehouses, and Counting-houses, and Stealing	1	-	-	-	-	-	-	-	-	-	1	-	-	-	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Misdemeanors, with intent to commit the above Offences	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Robbery	1	-	-	-	-	-	-	-	-	-	-	1	-	-	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Robbery and attempts to rob, by persons armed, in company, &c.	2	-	-	-	1	-	-	-	-	-	-	-	-	-	1	-	-	1	-	-	1	-	-	-	-	-	-	-	-	-	-
Robbery, attended with cutting or wounding	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Obtaining Property by threats to accuse of unnatural Crimes	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assaults, with intent to rob, and demanding Property with Menaces	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Stealing in Dwelling-houses, Persons therein being put in fear	1	-	-	-	-	1	-	-	-	-	-	-	-	-	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Sending menacing Letters to extort Money	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Piracy	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total of No. 2.	17	-	1	-	2	4	-	-	-	-	1	3	-	-	11	-	-	3	3	-	6	-	-	-	-	-	-	-	-	-	-
No. 3. Offences against Property committed without Violence. Cattle Stealing	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Horse Stealing	3	-	-	-	-	2	-	-	-	-	-	-	-	-	2	-	-	1	-	-	1	-	-	-	-	-	-	-	-	-	-
Sheep Stealing	4	-	-	-	-	2	-	-	-	-	1	-	-	-	3	-	-	1	-	-	1	-	-	-	-	-	-	-	-	-	-
Larceny to the Value of £5, in Dwelling-houses	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Larceny, from the Person	12	-	-	-	-	3	-	-	-	-	1	3	-	-	7	-	-	4	1	-	5	-	-	-	-	-	-	-	-	-	-
Larceny, by Servants	11	-	-	-	-	-	-	-	-	-	1	11	-	-	12	-	-	2	-	-	2	-	-	-	-	-	-	-	-	-	-
Larceny, simple	107	-	1	-	-	8	5	-	-	-	5	66	-	-	85	-	1	10	10	1	21	-	-	-	-	-	-	-	-	-	-
Stealing from Vessels in Port, on a River, &c.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Stealing Goods in Process of Manufacture	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Stealing Fixtures, Trees, & Shrubs growing, &c.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Misdemeanors, with intent to steal	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Embezzlement	3	-	-	-	-	-	-	-	-	-	2	1	-	-	3	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Stealing and receiving Letters Stolen from the Post-Office, by Servants	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Receiving Stolen Goods	3	-	-	-	-	-	-	-	-	-	2	1	-	-	3	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Frauds, and attempts to defraud	3	-	-	-	-	-	-	-	-	-	-	3	-	-	3	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total of No. 3.	149	-	1	-	-	15	5	-	-	-	12	85	-	-	118	-	1	18	11	1	30	-	-	-	-	-	-	-	-	-	-
No. 4. Malicious Offences against Property. Setting Fire to a Dwelling-house, or Shop, Persons being therein	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Setting Fire to a House, Warehouse, Corn-stack, &c.	9	-	3	-	-	-	-	-	-	-	-	-	-	-	3	-	-	2	3	1	6	-	-	-	-	-	-	-	-	-	-
Setting Fire to Crops, Plantations, Heath, &c.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Attempts to commit Arson, set Fire to Crops, &c.	1	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	1	-	1	-	-	-	-	-	-	-	-	-	-
Riot, and feloniously demolishing Buildings, Machinery, &c.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Destroying Silk, Woollen, Linen, or Cotton Goods, in Process of Manufacture	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Destroying Hop-binds, Trees, and Shrubs growing, &c.	2	-	-	-	1	-	1	-	-	-	-	-	-	-	2	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Killing and maiming Cattle	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Sending Letters threatening to burn Houses, &c.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Other Malicious Offences	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total of No. 4.	12	-	3	-	1	-	1	-	-	-	-	-	-	-	5	-	-	2	4	1	7	-	-	-	-	-	-	-	-	-	-
No. 5. Forgery and Offences against the Currency. Forging and uttering forged Bank of England Notes	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Forging and uttering other forged Instruments	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Having in Possession, &c. forged Bank of England Notes	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Counterfeiting the current Gold and Silver Coin	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Having in Possession, &c. Implements for Coining	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Buying and putting off counterfeit Gold and Silver Coin	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Uttering and having in Possession Ditto	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total of No. 5.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
No. 6. Other Offences not included in the above Classes. High Treason	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assembling armed, &c. to aid Smugglers	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Assaulting and obstructing Officers employed to prevent Smuggling	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Deer Stealing, and felony resisting Deer-keepers	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Being out armed, &c. to take Game by Night, taking Game by Night, and assaulting Game-keepers	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Taking and destroying Fish in enclosed Water	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Being at large under Sentence of Transportation	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Prison-breaking, harbouring and aiding the Escape of Felons	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Perjury and Subornation of Perjury	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Riot, Sedition, &c.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Riot, Breach of the Peace, and Pound-breach	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Rescue, and refusing to aid Peace Officers	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Keeping Disorderly Houses	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Indecently exposing the Person	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Felonies, not included in above Denominations	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Misdemeanors, Ditto	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total of No. 6.	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Grand Total	188	-	5	-	5	19	6	-	-	-	14	89	1	-	139	-	1	27	19	2	48	-	-	-	-	-	-	-	-	-	-

B

NOTES – The above page shows an example excerpt from the *Judicial* Statistics. We coded data from the first column of this table, the total number of offenders committed to trial, for each year and county for each broad crime category: No. 1 – No. 6.