ECONtribute
MARKETS & PUBLIC POLICY

ECONtribute
Discussion Paper No. 053

The Impact of the First Professional Police Forces on Crime

Anna Bindler Randi Hjalmarsson

January 2021 www.econtribute.de

UNIVERSITÄT BONN Universität zu Köln

Funding by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany´s Excellence Strategy – EXC 2126/1– 390838866 is gratefully acknowledged.

The Impact of the First Professional Police Forces on Crime*

Anna Bindler
University of Cologne and University of Gothenburg

Randi Hjalmarsson
University of Gothenburg and CEPR

This version: January 07, 2021

Abstract: This paper evaluates the effect on crime of creating a fundamental modern-day institution: centralized professional police forces tasked with preventing crime. We study the 1829 formation of the London Metropolitan Police – the first professional force worldwide. Using newly digitized and geocoded crime and police data together with difference-in-differences and pre-post designs, we find evidence of a significant reduction in violent crimes (despite the possibility of off-setting increases in clearance and reporting rates). In contrast, a reduction in property crime is not visible.

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

* 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. Bindler: Funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy – EXC 2126/1-390838866. We thank seminar and workshop participants at the University of Gothenburg, VATT Institute for Economics Research (Helsinki), University of Uppsala, University of Bologna, NBER Summer Institute (Crime), Transatlantic Workshop of Crime (Science Po Paris), IZA Bonn, CEP Workshop on Crime & Policing, Berlin's Applied Micro Seminar (BAMS), German Economists Abroad Christmas Meetings (Bonn), University of Cologne, University of Lund, RES Annual Conference (Warwick), SOLE Annual Meetings (Arlington), Brunel University (Crime Workshop) and the Warwick Applied Workshop for helpful comments and discussion. Authors: Randi Hjalmarsson, Department of Economics, University of Gothenburg, Email: randi.hjalmarsson@economics.gu.se (corresponding). Anna Bindler, University of Cologne and University of Gothenburg, Email: bindler@wiso.uni-koeln.de.

The test of police efficiency is the absence of crime and disorder, not the visible evidence of police action in dealing with them.

- Sir Robert Peel's 9th Principle of Law Enforcement (1829)

1. Introduction

Professional police forces are a fundamental component of criminal justice systems today. The origins of this modern-day institution date to 1829, when Sir Robert Peel founded the first professional force in the world – the London Metropolitan Police (the 'Met'). Many features distinguished this new force from the decentralized and community organized policing of the past, including a substantial increase in officer numbers, full-time salaried employees, accountability to a centralized government authority, an emphasis on officer quality, and equal policing independent of community wealth. One of the most innovative features of the Met was a shift from reactionary policing focused on catching criminals to tasking officers – for the first time ever – with *deterring* crime by slowly walking a small beat and being visibly present (Emsley, 2009). The London Met subsequently served as a model for forces around the world.[1]

Yet, despite the historical (and contemporary) importance of this institution, the impact of creating the Met has never been empirically evaluated.[2] Using newly digitized archival data sources together with quasi-experimental research designs, this paper provides the first empirical evidence on the causal effect of the formation of the Met on crime. We make two key contributions to the current understanding of the police-crime relationship. The first is to study old questions – the deterrence effect of more (visible) police – in a new context, i.e. the creation of a force rather than its expansion. The second emphasizes little studied aspects of policing (force administration and quality).

Specifically, two broad conclusions have been reached by the many empirical papers studying the deterrence effects of police – à la Becker's (1968) economic model of crime. The first, based largely on evaluations of (often temporary) expansions to existing police forces, is that increases in manpower reduce crime (Chalfin and McCrary, 2017).[3] The second, seen both in studies that rely on temporal variation in police deployment (e.g. Draca et al., 2011; Di Tella and Schargrosky, 2004) and spatial variation generated by geographic boundaries (MacDonald

[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] Historical crime in England has been studied by Wong (1995), who emphasizes opportunities for legal and illegal gains from 1857 to 1892, and Wolpin (1978), who looks at the role of clearance, conviction, imprisonment, and fines, as well as average sentences from 1894 to 1967. See Bignon et al. (2017), Mehlum et al. (2006) and Traxler and Burhop (2010) for studies of historical crime in France, Bavaria and Prussia, respectively.

[3] For specific examples, see for instance Evans and Owens (2007), Mello (2019) and Weisburst (2019).

et al., 2015; Heaton et al., 2016), is that visible police presence reduces crime.[4] Both of these commonly evaluated features of policing today – manpower and visibility – were fundamental to the newly founded Met. Do the crime deterring effects of more, visible police generalize to this different margin of police force expansion: the first recruits?[5] The answer can shed light on whether officer effectiveness depends on the institutional structures in place and whether officer visibility deters crime, even when forces are smaller and less experienced.

In contrast, "there is relatively scant evidence on the extent to which non-deployment related police policies reduce crime" (Owens, 2020). Two dimensions particularly salient in the context of the Met are the public versus private provision of policing and monitoring officer quality. Cheng and Long's (2018) study of the New Orleans French Quarter Task force is one of the few papers studying this question; it finds that monitoring and performance incentives under private provision decreased crime. In this context, our paper makes an important contribution: it evaluates the effect of replacing decentralized, local community organized policing with a centralized, public force in which officers of poor quality (e.g. shirking, inappropriate on-the-job-behavior, and corruption) were quickly dismissed.[6]

Finally, this paper contributes to a literature beyond the economics of crime –namely that studying state capacity.[7] While much of this literature takes a macro-perspective, our paper uses micro-data to study the institution of policing – an institution that is after all fundamental to a government's ability to implement and enforce its intended policies and laws. Moreover, studying the extent to which such a major institutional reform was successful sheds light on the degree of state capacity in early 19th century England.

We face three fundamental challenges to identifying the effect of the newly created Metropolitan Police on crime. The first is specific to the historical context: it is not easy to measure crime before the institution designed to monitor and systematically record it is created. We overcome this by digitizing multiple 'new' sources of archival crime and police data. The remaining challenges are not context specific: Even if crime behavior does not change, crime statistics could change after the formation of the Met due to changes in reporting behavior

[4] One exception is Blanes I Vidal and Mastrobuoni's (2018) study of non-terrorist attack related temporary patrol increases. Other deployment studies include Klick and Tabarrok (2005) and Blanes I Vidal and Kirchmaier (2018).
[5] Other studies have considered the temporary destruction of a police force, including the extensive margin effects of police strikes (Pfuhl, 1983) and slowdowns (Cann Chandrasekher, 2016).
[6] Other studies of private provision include business improvement districts (Brooks, 2008) and university policing (MacDonald et al., 2015; Heaton et al, 2016). Banerjee et al. (forthcoming) study incentives, community involvement, training, and managerial autonomy for police in India. Another dimension of policing that is increasingly being studied is the role of diversification; see for instance Linos (2018) and Miller and Segal (2019).
[7] See e.g. Acemoglu et al. (2015), Besley and Persson (2010), Dencecco and Katz (2014) or Johnson and Koyama (2017).

and/or clearance rates. The next section discusses these issues in more depth. To the extent that one believes the Met likely to increase both reporting and clearance rates, these potential confounders would work against us finding a deterrence effect of the Met.

Our analysis relies on two sources of London crime data. The first is felony 'trial' data for selected offenses – burglary, homicide and robbery – from London's Old Bailey Central Criminal Court; we include all cases, regardless of trial outcome. These data have three key advantages. First, we can geocode offenses and assign them to 'treated' and 'control' areas of London: with the exception of the 'City' of London (a 1-square mile area in Central London's business district, which still has its own force today), the initial catchment area of the Met was within an approximate 7-mile radius of Charing Cross. To implement a difference-in-differences design, we thus aggregate individual crimes into a panel data set of the number of crimes in each month and area from 1821 to 1837. A second advantage is that trial transcripts contain details about police witnesses, which allows us to document reform implementation: there is an instant shift in the type of police witness ('old'/e.g. pre-Met watchman to 'new'/ Met policeman) that is by far largest in the treated area. Finally, the selection of these most serious offenses limits concerns about biases in our estimates due to reporting changes.

Yet, given the historical felony classification of many offenses that are misdemeanors today (e.g. pickpocketing and simple assault), these three offenses (homicide, robbery, and burglary) comprise just 7% of felony trials. Concerns about generalizability to a broader set of property and violent crimes are addressed by our second data source, which consists of daily police reports for nine (magistrate run) police offices. These offices were tasked – both pre and post-Met – with processing crimes and apprehending offenders for all of London, including crimes outside the Met's catchment area; constables in these offices were not tasked with deterrence in either period. The daily reports include three types of crime measures: stolen property incident reports, 'information' (another type of incident report), and charges. Though all property, violent and other offenses are included in these reports (and not just selected felonies), these data also come with limitations. Offenses cannot be geocoded into treated and control areas, necessitating pre-post designs that estimate the effect of creating the Met on crime in all of London (i.e. net of any offsetting effects in the control areas).

Analyses of both London data sources provide evidence consistent with a crime-reducing effect, especially for violent crimes. In the Old Bailey data, a significant and persistent reduction in trials is seen for the violent crime of robbery (more than 40%), but there are no (consistently) significant effects for homicide and burglary trials. These results are robust to specification choice (OLS versus Poisson), sample windows, and the units of temporal and

geographic aggregation. We provide evidence in support of the underlying parallel trends assumption, and rule out biases due to a number of potential confounders (especially in a short window around the reform), including the 1832 cholera epidemic, contemporaneous criminal justice changes, population growth, police spill-overs and crime displacement. The lack of an observable crime reducing effect for the property crime of burglary can be due to there actually being no deterrence effect or crime-reducing effects being offset by increases in clearance and/or reporting rates, leading to more trials. The latter is consistent with the findings of the daily police reports analysis: A significant decrease is seen for all violent crime measures, but for property crimes, reductions in incidents are more than off-set by increases in charges.

Finally, we assess whether the crime reducing effects of new forces are specific to the London Met by evaluating the 1839 to 1856 roll-out of police forces to the counties of England and Wales. Taking advantage of variation in initial force characteristics, we use difference-in-difference models to estimate the effect of creating professional forces overall and by force size. Though there is no overall crime reducing effect of creating just any professional police force, we find that the largest forces per capita significantly reduced both violent and property crime; event study specifications show these effects are not immediate and increase over time. These results highlight the potential importance of force quality, and are in fact consistent with an additional finding of the London pre-post analyses – namely that effects increase over time.

The remainder of the paper is organized as follows. Section 2 highlights the differences between the pre- and post-Met 'police' and discusses the various channels through which the formation of the Met can impact crime and crime statistics. Sections 3 and 4 estimate the effect of the Met using the Old Bailey geocoded data and the daily reports data, respectively. Section 5 evaluates the roll-out of county forces throughout England and Wales. Section 6 concludes.

2. Institutional Background and Framework

2.1. 'Policing' Before the London Metropolitan Police

This section describes the forms of public and private 'policing' that existed in London before the 1829 Metropolitan Police Act. Local watchmen, tasked with guarding the streets at night, date to the 1285 Statute of Winchester, which required all towns to form a watch manned by the local residents (Rawlings, 2008). The unpopularity of this community policing system – neighbors watching out for (but also arresting) neighbors – led to a series of Watch Acts in the 1700s, "in which households exchanged the duty to serve in the watch for the duty to pay a watch rate" to hire substitutes (Rawlings, 2008). Watchmen initially stood guard in a fixed location, but by 1800, the better watches aimed to prevent crime by patrolling the streets at set

intervals and yelling out the time on the hour (Reynolds, 1998). Because they were locally funded, watch quality varied substantially with parish wealth.[8] Just three wealthy parishes (St Marylebone, St James, and St George) had superior watches, with larger numbers of relatively well-paid watchmen patrolling in multiple night shifts (Reynolds, 1998). By the early 1800s, the many criticisms of the local watches included: incompetence, improper and corrupt conduct, responsibility to only their local parish, and an inability to cope with major disturbances, such as riots and large crowds, without military support (Rawlings, 2008).[9]

Another form of (public) policing was London's Bow Street Runners, who were sworn constables of Westminster and date to around 1750 (Emsley, 2009). As there were initially only eight Runners, they did not have a physical presence and were not meant to deter crime but rather to clear it by locating and arresting serious offenders. Initially, these Runners were similar to the 18th century thief-takers, i.e. men who earned livings from rewards upon the convictions of 'serious' criminals. By the end of the 1700s, however, the Runners were essentially full-time policemen located at the Bow Street house, which became a centralized collection point of crime incidents. Other Bow Street forces were created: The Horse Patrol (1805) on the highways leading to London, the Foot Patrol (1790) in central London, and the Dismounted Horse Patrol beyond these jurisdictions. Both horse patrols remained following the creation of the Met, but the Foot Patrol was disbanded. Rawlings (2008) estimates that these patrols comprised about 400 persons when the Met was created.

Following the Bow Street template, seven additional Police Offices were established in the Middlesex Justices Act of 1792: Queen's Square, Great Marlborough Street, Worship Street, Lambeth Street, Shadwell (replaced by Marylebone High Street by 1816), Union Hall and Hatton Garden. A Thames River police was established in 1798. Each office was staffed by three magistrates and up to 12 constables, whose primary purpose was to follow up on crime reports (Emsley, 2009). According to Reynolds (1998), "these men did not patrol the streets on any regular basis." In contrast to the local watches, the jurisdictions of the Police Offices were not confined by parish boundaries (Rawlings, 2008). The magistrates' main responsibility was to process criminal cases. They retained this judiciary role upon the formation of the Met (Davis, 1984). That is, even post-Met, a known offender would be processed through these same offices (which existed until 1839) and recorded in the offices' daily reports.

[8] There are also examples of prosperous neighbors paying private watchmen to guard gates (Rawlings, 2008).
[9] The five days of rioting in the 1780 Gordon Riots are a prime example. Calling in the army was delayed given fear it would not arrive in time and would further incite unrest (Rawlings, 2008). The Gordon Riots led to some calls for police reform and more unified policing, but these failed and were eventually forgotten (Lyman, 1964).

The pre-Met 'police' were poorly paid. Office constables were paid 21 shillings per week (55 pounds per year) in 1811 (Radzinowicz, 1953) while 1820s watchmen salaries ranged from 12 to 16 shillings per week (Reynolds, 1998). These low salaries led to a system with additional sources of income: systematic schedules of charges for all 'extras' (e.g. witness testimony), rewards for catching criminals, and corrupt behavior.[10] Such a system inherently does not emphasize deterrence: no one can pay a reward for a crime that does not occur. A telling summary of the perception of the pre-Met police offices is provided in 1850 by Charles Dickens (Radzinowicz, 1953): "We are not by any means devout believers in the Old Bow Street Police. […] as a Preventive Police they were utterly ineffective, and as a Detective Police were very loose and uncertain in their operations, …".

The weaknesses of the de-centralized, parish wealth dependent, non-cooperative, poorly paid, too small for mass crowd control, and oftentimes-corrupt policing agents were not unrecognized before the creation of the Met. But, all earlier calls and attempts to reform (unify) policing encountered strong resistance by rich parishes – and especially the City of London – who wanted to control their own watch and worried that any replacement would reduce local coverage to provide for the poorer locales (Rawlings, 2008).

2.2. The Introduction of the London Metropolitan Police

The Metropolitan Police Act (10 Geo.4, c.44) created the London Metropolitan Police (the 'Met') on September 29, 1829.[11] Though the Act states that property crime was rising, it also highlights the many previously recognized deficiencies in the existing 'police':

> "… 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 […]"

Why did this Act pass, while other reform attempts failed? This perhaps comes down to Sir Robert Peel, who became Secretary of the Home Office in 1822. Peel emphasized legal reform, including (i) the consolidation of laws (1823), (ii) abolishing capital punishment (offense by offense), and (iii) improving prison conditions (1823). Peel learned how to negotiate the political landscape to get reforms through Parliament; importantly, he convinced parishes that the new police would not cost more than localities paid before (Reynolds, 1998).[12]

[10] With respect to corruption, Radzinowicz (1953) quotes witnesses examined during evaluations of pre-Met policing as saying an annual salary of 100-120 pounds is needed "to be above temptation and to do nothing mean."
[11] The bill to create the Met was introduced April 1829, amended in May and passed by both houses in June.
[12] The Watch Rate was replaced with a Police Rate (equal to the parish Poor Rate, a property tax used to provide poor relief) on property holders, with a maximum of 8 pence per pound of annual property value (Lyman, 1964).

The new Metropolitan Police initially consisted of 1000 men, but there were more than 3000 officers by May 1830. Panel A of Figure 1 documents the weekly number of hires from 1829 until 1856. It shows that (i) a first wave of hiring occurred for six inner divisions in September 1829 and a second wave for 11 outer divisions in February 1830 (see also Appendix Table A1) and (ii) the Met grew continuously to about 6000 men in 1856. Each parish was notified of the date that the Met would take over, and the existing watch disbanded, by a notice posted on the parish's church door two Sundays prior to that date (Reynolds, 1998).

The initial catchment area was within an approximately 7-mile radius from Charing Cross in Central London and extended to 15-miles in 1839.[13] Excluded were the City of London (which established its own force in 1832 that is still distinct today) and, until 1839, the Thames River Police. Panel A of Figure 2 presents a historical map of the original jurisdiction of the Met. Panel B shows that the pre-existing police offices were centrally located within the 7-mile radius (and even a smaller 4-mile radius). An equal number of police were hired into each division, regardless of the geographic size (see Appendix Table A1).[14] As beats in the outer divisions were often larger, it is plausible that police in these divisions were less visible on their beats (Emsley, 2009). There is thus a potentially more intense treatment in a shorter radius around Charing Cross.[15]

In addition to the sharp increase in the number of 'police' and the centralized structure, many other changes characterized the new police, including their tasks, uniforms, incentive structure, the ability to police across parishes, and an emphasis on quality and proper behavior. First, the primary task of an officer was deterrence. Met officers walked a regular route at a pace of 2.5 miles per hour; the beat was intentionally small to increase visibility. The deterrence goal was also salient in the provision of uniforms that "caught the eye of criminals and the law-abiding public so both would know the police were on hand" (Reynolds, 1998). There was little expectation that the new police would solve crimes; the Bow Street Runners remained until 1839, and were replaced with a Detective Division in 1842 (Reynolds, 1998).

[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. parish church); 85 lie within 7-miles from Charing Cross and all are within 8-miles. Moreover, 75% of the locations are within 4-miles. We test the robustness of our results to an 8-mile radius.

[14] Unfortunately, while we can measure the initial number hired per division, we cannot track officers per division over time.

[15] On October 6, 1829, the Met opened its first station on a street called Great Scotland Yard (near 4 Whitehall Place and Charing Cross). This became the home station of the Met, including the two police commissioners. As the force expanded, other buildings in this area were taken over, but eventually the Met started opening police stations throughout London. To the best of our knowledge, few if any stations were opened in the 1830s.

Second, while the new police received little formal training, there were clear behavioral guidelines, including Peel's nine principles of law enforcement. The higher quality achieved via enforcing these rules is seen in historical records. Panel B of Figure 1 shows the weekly number of leavers among those recruited before March 1831 while Panel C shows the weekly number of post-1833 dismissals by reason (drunk, neglect/misconduct, criminal behavior, other). There is high turnover, especially at the beginnings of the Met, including annual firing spikes for being drunk on duty around Christmas. Moreover, id numbers and division letters on uniform collars made it easier to identify officers behaving improperly (Reynolds, 1998).

Who were the new police? Selection criteria included being at least 170 centimeters, under 35, physically and mentally fit, and able to read and write (Reynolds, 1998). Were the new officers just the old 'police' with a new job title? Though this could have happened to some extent, it would have been far from a one to one replacement: Peel's new force consisted of more than 3000 men while Lyman (1964) estimates that there were 1000 men in total (constables and watchmen) in 1800. While some of the old became the new force, we can demonstrate that many did not last long in the job.[16] Anecdotally, many did not even qualify.[17]

Parliament was responsible for the finances of the new police. Appendix Table A2 lists the annual pay for constables (47 pounds), sergeants (55 pounds), inspectors (100 pounds), and superintendents (200 pounds). Thus, there was now a system with ranked officers, providing promotion incentives. But, constable and sergeant salaries were similar to that of pre-Met constables; continued reports of external fee schedules (e.g. witness testimony) are thus unsurprising (Radzinowicz, 1953). Did Peel keep his budgetary promises? Many parishes were actually surprised by the size of the initial fees, and had to rely on loans from the Treasury; continued struggles led to an 1833 statute that allowed parishes to pay 75% of the full amount, with the remainder covered by the government (Reynolds, 1998).

2.3. Framework: The New Police, Crime and Crime Statistics

The new police could have affected the actual number of crimes via two channels. As

[16] We compare registers of the first 3,000 officers hired by the Met (Source: MEPO 4/31, National Archives London) to the 156 men hired into the Bow Street Foot Parole between 1823 and the Met's 1829 creation (Source: MEPO 4/508, National Archives London). We can observe (using 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 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.

[17] Some of the pre-Met police met clearly did meet the age criteria. Though our digitalization of those hired into the Bow Street Foot Patrol (Source: MEPO 4/508, National Archives London) indicates all men were between ages 20 and 35, this is not the case for watchmen. Reynolds (1998) documents that just one-third of the St Luke's watch met all Met eligibility requirements and that, in the early 1800s, many watchmen were older than 50.

highlighted above, Peel's ultimate goal was the absence of crime via *deterrence*: this was to be accomplished with more police, who were highly visible during the day and night, with uniforms, and constant patrols. The second crime-reducing channel is *incapacitation*: if the new police apprehended more offenders who were sentenced to prison, death or transportation to Australia, then crimes were prevented by removing these persons from London's society. Though incapacitation was not an intention of the new Met, we cannot rule out such effects. Despite the new police not being tasked with apprehending offenders (a task that remained in the hands of the Police Offices), officers could be more likely to clear crimes by catching offenders in the act on the streets. Our empirical analysis cannot disentangle such deterrence and incapacitation effects, which both move crime in the same direction. Both effects may increase in size over time as the new police become more experienced and disciplined.

We now turn to the potential confounders of measuring this crime reduction in administrative data. Estimates of the effect of any contemporary or historical policy on crime can be biased if the policy under consideration affects crime reporting and/or clearance rates (i.e. the share of incidents that have an identified/known offender associated).

First, did the introduction of the Met increase clearance rates? Even if crime remained constant, higher clearance rates could imply more arrests, charges and/or trials. Though this type of bias would not be relevant for incident data (i.e. reported crimes), it could be a potential confounder in outcomes measured later in the justice system, like charges or trials. As noted above, since the task of following up on crime reports remained with the pre-Met Police Office constables, one may not expect clearance rates to go up that much (except perhaps for street crimes, like pickpocketing, in which the new police could be present to immediately apprehend offenders). Moreover, if the new police impacted clearance rates, one would expect this to increase *administrative crime records*: i.e., it works against us finding a crime reducing effect.

A second potential confounder is that the new police affected crime reporting. In particular, crime reporting may have increased if people felt that there was a new 'trusted' authority to report to, who would take the report seriously regardless of the potential rewards. That is, creating the Met might have lowered the victim's opportunity cost of reporting a crime. This would increase observed crime (in all data types, ranging from reports to trials). This may be especially relevant for relatively minor property crimes, for which the effort of reporting pre-Met may not have been worth it. At the same time, there is anecdotal evidence, at least

initially, of anti-police sentiments towards the Met.[18] Thus, if changes in reporting biases crime measures upwards, one would expect this to increase over time with growing trust in the new police – as Peel's emphasis on a non-corrupt high quality force becomes recognized – and to be more prominent for minor offenses. For those most serious violent offenses, e.g. robbery and homicide, one would not expect reporting biases to play a significant role.

3. The Impact of the Met on Crime: Evidence from the Old Bailey Proceedings

3.1. Old Bailey Data Description

Our first data source is the Proceedings of the Old Bailey. The Old Bailey was the Central Criminal Court for felony crimes in both London and the surrounding county of Middlesex. In other words, offenses both inside and outside the Met's catchment area were trialed at the Old Bailey; the only difference is that there were different juries for offenses in London as opposed to Middlesex. Trials took place in (approximately) monthly sessions. At this time, victims often acted as prosecutors and a charge would go to trial if a grand jury decided that there was sufficient evidence. The verdict and sentences were decided by juries and judges, respectively.

The Proceedings were published after each court session, and include records of more than 200,000 trials from 1700 to 1913. *The Old Bailey Proceedings Online* (Hitchcock et al., 2013) has digitized and tagged many variables (e.g. offense type, verdict, and sentence). We complement these readily available data with manually extracted (i) crime locations, geocoded into treatment and control areas of London, (ii) offense dates to identify treatment exposure and (iii) police witness characteristics (number, type, and crime scene presence) to evaluate the 'first stage' implementation of the reform. Given the time-intensive nature of this work, we only transcribed and geocoded trials for homicide (murder/manslaughter), robbery, and burglary/housebreaking from 1821 to 1837, whose felony status (and thus representation at the Old Bailey rather than a lesser court for misdemeanors) did not change during this period.[19]

A potential limitation of the Old Bailey data is that it only contains those serious felony *charges* that resulted in a trial. Importantly, we use charges brought to trial (i.e. the occurrence of a trial and not its outcome) as a proxy for crime. It is of course possible though that crimes

[18] One magistrate is quoted as stating that "a strong feeling existed against the new police" upon the Met's formation in an October 1, 1829 issue of The Morning Journal. See Open University collection (viewed May 7, 2019): https://www.open.ac.uk/Arts/history-from-police-archives/MphcR1/Scrapbooks/sbIntro.html .

[19] We also geocoded robbery, burglary, and murder/manslaughter for 1820 to 1850, but focus on pre-1837 years prior to avoid the abolition of the death penalty for robbery and burglary as a confounder. In addition, after the initial data coding, we noted that hardly any offenses were labelled burglary from the mid-1820s to the mid-1830s, while there was a sharp increase in offenses labelled housebreaking. We thus geocoded housebreaking offenses for 1820-1837 and treat housebreaking and burglary as one combined offense category for the entire period.

did not result in a trial – either because the victim did not report/prosecute or because the report was not cleared (i.e. did not result in an arrest/charge of a suspect). However, our focus on the most serious offenses of murder, robbery and burglary limits concerns about the former; as highlighted in Section 2.3, such serious crimes are likely to always be reported. We further note that these historical trial data are fairly encompassing: cases that result in pleas and acquittals are reported in the Proceedings and included in our data. Acquittal rates were around 25% in the 1820s and 1830s, while pleading did not play a prominent role at this time. See Bindler and Hjalmarsson (2020) for more details about trials at this time.

We geocoded the data using the most detailed address available in the Proceedings (e.g. an intersection, parish/district name or street end/mid points) and mapped these locations into modern day London maps to obtain postcodes and geo-coordinates.[20] We measure the distance of each offense location to Charing Cross, and classify them as being in one of four areas. Only parishes within a 7-mile radius of Charing Cross (but outside the "City" of London) are treated, i.e. within the Met's original jurisdiction. We subdivide this 7-mile circle into two treatment areas: the more intensely treated area within a 4-mile radius and the less intensely treated area within a 4-7 mile radius of Charing Cross, respectively (see Section 2.2). The two control areas are those outside the 7-mile radius of Charing Cross and within the City of London. Using the coded date of offense to classify incidents as before and after the Met's introduction, we can thus estimate both simple pre-post and difference-in-difference specifications.

Appendix Table A3 overviews the data, and shows the number of trials by crime type and police witness details within and outside a 7-mile radius of Charing Cross and in the City of London. One statistic that stands out is the relatively low number of homicide trials (just 258 in all areas over 1821-1837). This does not appear to be driven by the quality of Old Bailey trial data: Appendix Figure A1 shows that trials track well with an alternative series of (non-trial) London homicides. A more likely explanation is the limited ability of coroners at this time to identify potential murders due to the limitations of forensic technology, for instance, to identify murder by poison (Emmerichs, 2001). These measurement issues may, of course, limit our ability to say a lot about the impact of the new police on homicides.

Finally, for our main analysis, we aggregate the data to an area by month panel – i.e. a dataset of the number of crimes (by crime type) in a given area and month. The baseline

[20] Whenever locations changed names (e.g. street names), we identify the current address using historical maps (roughly 40% of our 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., assign potentially untreated observations to the treatment area). Results are qualitatively robust to excluding either of those 'fuzzy' locations (see Appendix Table A4).

specification uses the four areas (two treatment and two control) described above. We study two windows around the reform: January 1821-December 1837 (4 areas x 17 years x 12 months = 816 observations) and January 1828 to December 1832 (240 observations).

The raw data (i.e. the number of crimes in month by area cells) are presented in time series plots in Appendix Figure A2 and summarized in Table 1, which presents, for each area and period (pre and post Met), the mean number of crimes per month. Panels A and B use the short and long windows respectively. These statistics emphasize that the pre-reform number of crimes in the treated area is greater than that in the uncertain and control areas. In a preview of the results in Section 3.4, Table 1 shows that there is a significant reduction in robbery in the 4 miles around Charing Cross that is seen in both the short and longer windows. No significant changes in burglary or homicide are observed.

3.2. The First Stage: Evidence of the Introduction of Met Police as Trial Witnesses

We use the Old Bailey trial reports to assess whether there is evidence of the introduction of the Met. Police witnesses were called constables (both before and after the creation of the Met), policeman (a post-Met label), watchman (a pre-Met label) and a handful of other labels that were either predominantly pre or post-Met. Do we see an increase in the number and/or different types of police witnesses at trial after the Met was created?

We estimate the effect of the Metropolitan Police Act on witness type for each (of the four) treatment and control areas *a* in simple pre-post regressions depicted in equation (1). Specifically, we regress each measure of police presence at trial *i* for offense *o* at date *t* on a dummy variable indicating whether the offense occurred after the introduction of the Met.

$$PoliceType_{iot}^{a} = \gamma_1 PostMet_{it} + \alpha_o + \varepsilon_{iot} \quad (1)$$

Offense type fixed effects (α_o) capture inherent differences across crime types in police presence; e.g. police might be more likely to be present for street crimes like robbery. In contrast to the main analysis, which aggregates crimes to the month by area unit, these first stage specifications keep the data at the individual trial level. Results for two windows –1821-1837 and 1828-1832 – are presented in Table 2.

There is little evidence that the creation of the Met affected the extensive margin of having *any* police witness at a trial (see columns (1) and (2)). But, columns (3)-(6) show that it significantly changed the *type* of witness: the likelihood of a trial having a 'new' police witness increased by 57 and 46 percentage points in the 4 and 4-7 miles radius areas (using the

1821-37 window) while the presence of 'old' police decreased by 49 and 25 percentage points, respectively. In the control area, there was an increase (16 percentage points) in 'new' but no change in 'old' police while in the City of London, potentially treated after April 1832, a shift from old to new police is seen in the larger window (including post-1832) but is much smaller and/or insignificant in the shorter window. Finally, as the Met officers were constantly walking a short beat, it is possible that they were increasingly present at the crime scene itself, either by witnessing the crime or being close enough to be called for assistance, i.e. a shorter response time. This may be especially relevant for street crimes. However, columns (7) and (8) of Table 2 only find support for this in the longer window for the treated area.

Table 2 shows indeed that the reform was implemented in both treated areas, but with a stronger intensity in the inner (4-mile) circle. Consistent with the immediate elimination of the parish watch described in the Act, the old police – i.e. watchmen – are observed to be substituted out. An unexpected finding, however, is the increase in new police in the control areas, albeit substantially smaller than that for the treated areas. This could be explained by (i) geocoding errors or (ii) an increased use of the term 'police' in the Proceedings by court reporters, regardless of the actual type (the same reporter is responsible for the entire Proceedings). Alternatively, (iii) there could be spill-overs of the Met police into the control areas. This could occur because the 7-mile radius/City of London is not a perfect boundary and some officers actually patrol this area or some crimes committed outside the 7-mile radius or in the City led to arrests within the 7-mile radius. If such spill-overs existed and the control group was partially treated, we would *under*-estimate the treatment effect in a difference-in-difference analysis of the effect of the Met on crime. To assess the plausibility of such spill-overs, Figure 3 presents kernel densities of post-Met trials with and without police at the crime scene (Panel A) and with at least one new or one old type of police witness (Panel B), both by distance from Charing Cross. If there were policing spill-overs beyond the Met's catchment area, then one would expect to see a non-zero density in police presence at distances from Charing Cross beyond 7-miles, with potentially larger densities at distances closer to the Met boundaries. The figures do not suggest a substantial spill-over of Metropolitan policing since the densities are close to zero around and outside the 7-mile mark.

3.3. Old Bailey Empirical Strategy

Having established that the creation of the Met affected 'policing' in London, we turn to its effect on crime. Equation (2) presents a difference-in-differences model, which uses the area outside the 7-mile radius and the City of London as the best possible control groups. Again,

we split the potentially treated areas into a 4-mile radius area with a more intense treatment and a 4 to 7-mile radius area with potentially less intense (i.e. uncertain intensity) treatment; these are labelled *Treatment* and *Uncertain*, respectively. The outcome is the number of trials for offense *o* in area *a* during month *m* of year *y.* The baseline analysis aggregates the data at the month (m) and area (a) level, and estimates equation (2) separately by offense category o.

$$(2)\quad Trials^{o}_{amy} = \gamma_1(Treatment * PostMet)_{amy} + \gamma_2(Uncertain * PostMet)_{amy} + \alpha_y + \alpha_m + \alpha_a + \varepsilon_{amy}$$

Year fixed effects (α_y) flexibly control for criminal justice and societal trends over our sample period that are common to both treated and control areas, while month of year fixed effects (α_m) capture seasonality. Area fixed effects (α_a) control for pre-existing differences between areas, such as population density. γ_1 and γ_2, the coefficients on the interaction between a treatment (or uncertain) area dummy and whether the month of offense is October 1829 or later, capture the treatment effect for the inner and outer circles, respectively. This specification allows for causal interpretations of the estimated treatment effects (γ_1 and γ_2) if the usual parallel trends assumption holds, i.e. nothing else changed differentially in the treatment and control areas that could have affected crime rates in the estimation window. We test for pre-reform differences between the treatment and control areas in an event-study design and discuss a number of potential confounders, including: the 1832 cholera epidemic, contemporaneous criminal justice changes, population growth, police spill-overs, and crime displacement.

We highlight four aspects of the baseline estimation. First, robust standard errors are used. Appendix Table A5 assesses the sensitivity to a wild cluster bootstrap procedure (Cameron et al., 2008), clustering by the four baseline areas: our findings are generally robust. Second, the baseline classifies the City of London as a control area. But, as the *City Day Police* (which was not part of the Met) became operational in April 1832, it is possible that a treatment similar to the Met was introduced in the City of London. We thus also estimate specifications that demonstrate robustness to which treatment or control group the City of London is assigned to: the treatment group after April 1832, the uncertainty group, or dropped completely. Third, we present results for two windows (1821-1837 and 1828-1832); the shorter window alleviates concerns about potential confounders discussed in Section 3.5.

Fourth, we estimate the baseline using OLS. Arguably, however, the correct model depends on a number of considerations. For example, should the impact of the Met be proportional to the population? Another example is a potential concern about differences in the

moments (mean and variance) of the distribution in the outcome variable across the treatment and control group; for instance, as seen in Table 1, mean crime levels are significantly higher in the treated than the control areas. Such differences would make the control group potentially unsuitable for a difference-in-difference setup. These arguments would speak in favor of a Poisson model: In absence of information on the number of officers per treatment/control area (note that we cannot reliably map 'divisions' of 150 officers each into these areas), a Poisson model allows for a proportional interpretation of the introduction of the Met. Moreover, the Poisson model transforms the outcome variable in ways that can mitigate the second concern (note that a simple log-transformation is not suitable here given the zeros in the outcome variable). However, given that there are reasons that speak against such a model (e.g., it is not obvious that a proportional effect should be expected, given differential population density and officer density per policed area), we remain more agnostic and show the results for a Poisson model alongside the results for the OLS model.[21] Furthermore, we estimate OLS specifications with a binary (extensive margin) outcome: whether there is any crime in a given area and period (using smaller areas or time periods than the baseline). These specifications are less dependent on the assumptions discussed above.

3.4. Main Results and Robustness Checks: Old Bailey Data

Table 3 presents these results. Columns (1) and (2) correspond to the baseline specification, estimated via OLS with the City as a control area, for the 1821-1837 and 1828-1832 windows, respectively. Panels A-C present the results for burglary, robbery, and homicide. The introduction of the Met significantly decreases the average number of monthly robbery trials in the treatment area (< 4 miles from Charing Cross) relative to the control areas by 1.0 (long window) and 1.3 (short window), respectively, or 12 to 15 per year. There are no significant effects for burglary and homicide. Relative to the average number of pre-Met robberies in the treatment group, robberies decrease by 40% and 46% in the long and short windows, respectively. These findings are consistent with Draca et al. (2011) who, using modern-day London data, also find no effect on burglary but a decrease in robbery. They argue that the nature of the offense makes burglary less susceptible to a "public deterrence technology" than robbery (which can be classified as a street-crime). An alternative explanation is that off-setting biases due to increased clearance and/or reporting rates are larger for burglary than robbery. Though at least partially treated, we find no significant effects of the Met on crime in the

[21] We use QML-Poisson to estimate the fixed-effects models and adjust standard errors (Wooldridge, 1999).

uncertainty area; in fact, most of the coefficients are positive. This could imply that crime levels did not change in this 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 or reporting. Columns (3)-(5) of Table 3 show that these results are robust to how the City of London is classified.

The rest of Table 3 presents three alternative estimation strategies for both sample windows. For ease of interpretation, columns (6) and (7) show incident rate ratios from estimating a QML Poisson model. These estimates generally have the same sign as baseline OLS (i.e. the Poisson estimates are less than one when the OLS estimates are negative), but differ in precision. Specifically, we find an approximately 30% reduction in robberies in the treatment area that is only significant (at the 10% level) in the short window. While the OLS found no significant effects on burglary and homicide in the main treatment area, the Poisson results find a significant reduction in the long window. In the area of uncertain treatment, the Poisson specification finds significant increases in burglary in the long window and robbery in the short window. Again, these results suggest that if there is any reduction in robberies or burglaries in the uncertainty area, it is (more than) offset by increases in either reporting or clearance rates.

Columns (8) – (11) present the results when we instead move to OLS specifications with a binary (extensive margin) outcome: whether there is any crime in a given area and period. To create enough variation in this new outcome variable (and, at the same time, test the robustness of our results to different aggregation levels), we aggregate the data to the week by area level in columns (8) and (9) (i.e. a smaller temporal period than in the baseline) and to the month by one-mile distance band level in columns (10) and (11) (i.e. smaller geographic areas). We see the same pattern of results as in the baseline. There is a significant reduction in robbery trials (ranging from 11.5-16.8 percentage points) after introducing the Met, regardless of window or aggregation level. In contrast to the baseline (but comparable to the Poisson estimation), we do observe a marginally significant reduction in the chance of burglary (6.9 percentage points) in column (8). As in the OLS specifications, no significant effects are seen in the uncertainty area.

Figure 4 shows the results from event study estimations by crime type (burglary – Panel A, robbery – Panel B, and homicide – Panel C) for both the treated and uncertain areas. We estimate a more flexible specification that interacts the treatment and uncertainty indicators in equation (2) with a dummy for each one-year interval before and after the introduction of the Met. In other words, we estimate a series of lead and lag effects that average all months within a given year, relative to the reform. To account for the mid-year creation of the Met, we define

a year from September to August. These specifications allow us to both test the plausibility of the parallel trends assumption and study the dynamic effects of creating the Met. The results are supportive of parallel trends for all crime categories, and for both treatment and uncertainty areas: The coefficients are not significantly different from zero in the years leading up to the reform (the only exception is the third lead for burglary in the treatment area). There is no evidence of a short or long-run effect for homicide or burglary. For robbery, the change in the point estimates in the treatment area is immediate and persistent (though not quite significant), and is close to the overall difference-in-differences estimate in every post reform year.[22]

Finally, Appendix Table A4 demonstrates the robustness of the results to: (i) area specific time trends, (ii) excluding crimes reported 'somewhere' on a long street, which could lead to miss-classified treated offenses, (iii) including only crimes for which we could identify the coordinates without referring to historical maps, and (iv) excluding offenses with missing crime dates (rather than instead assigning trial dates, as in the baseline). The main result – a significant reduction in robberies in the treated area with no observable effect for burglary – does not change. Controlling for area specific trends does yield marginally significant increases in robbery trials in the uncertainty area.

3.5. Spillovers, Crime Displacement and Other Potential Confounders

This section discusses a number of potential confounders to interpreting our results causally. The first is whether the 1832 cholera epidemic, which resulted in the deaths of almost 7,000 Londoners, differentially affected treatment and control areas.[23] The epidemic could have affected crime through public riots (Tynkynnen, 1995) or by affecting police resources (directly through ill/dying officers or indirectly as officer responsibilities are shifted away from crime prevention) or criminals (who may be incapacitated by the disease or driven to commit crimes). To explicitly look at the geographical and temporal distribution of cholera in London, we use a new data source (the *Returns to Death* of Met officers from 1829 to 1889), which includes the officer's police division and date and (often) reason of death. Appendix Figure A3 demonstrates that cholera arrived and peaked in 1832, diminished by 1833, and almost disappeared by 1834. 19 officers died; all but one death were in July-September (peaking in August). Despite the equal number of officers across divisions, deaths were not equally distributed, with more deaths in inner London. However, our shorter estimation window mostly

[22] These estimates are significant when using (less demanding) two-year intervals.
[23] See for instance https://www.choleraandthethames.co.uk/ (accessed April 29, 2019).

avoids this concern and the results are robust to dropping all trials after May 1832.

A second concern is whether other criminal justice reforms differentially affected treatment and control areas. The main reform is the abolition of capital punishment for burglary and robbery in 1837, which we avoid by ending our sample before 1837. The shorter time window further limits the possibility of omitting such shocks. Our previous research (Bindler and Hjalmarsson, 2018) documents that there are no anticipation effects related to the timing of the offense by offense capital punishment reforms. Other legal reforms pushed by Peel (highlighted in Section 2) occurred in 1823, well outside the short window.

A third potential confounder is dynamic population growth. Appendix Figure A4 shows decadal population estimates and growth for Inner London, Outer London and the City of London.[24] In the first half of the 19th century, Inner London grows at the highest rate (almost 25%) between each census, with Outer London not far off, while the City of London does not grow at all. To the extent that population growth implies more potential criminals and increases in crime, this biases us against finding a crime-reducing effect in the pre-post analysis. Likewise, faster growth in the treated areas would yield a similar bias in a difference-in-difference analysis. Using a short window around the reform mitigates these concerns.

Finally, to determine whether the introduction of professional police decreased crime overall or just locally (where police were introduced), one needs to understand whether police spill-over or crime displacement effects exist. Section 3.2 concluded that there was little evidence of a substantial police spill-over; however, if it did exist, it would attenuate any crime reducing effects of the police. With regards to crime displacement, if criminals chose to commit crime in less policed areas than the newly treated Met jurisdiction, this would bias the difference-in-difference estimates in the direction of a crime reducing effect. To assess the extent to which displacement is a concern, we return to Table 1, which compares the average number of pre- and post-Met offenses. In the shorter window, there is a significant reduction of 53% (2.88 to 1.33) in the average number of monthly robberies in the treated area. A comparable decrease is not seen in the less intensively treated uncertainty area nor in the control area or City of London. There were no significant changes to burglary or homicide, nor any evidence of an increase in crime in the control areas. In other words, there is no evidence of a substitution of offenses from the treated to control areas in the short window.

However, such displacement cannot be ruled out in the longer window, as although it is

[24] The classification of Inner and Outer London areas is done by the historical census, and cannot be matched to the 'areas' in our data as it does not necessarily correspond to a 7 mile boundary.

still an order of magnitude smaller than in the treated area, the average number of monthly robberies outside the 7-mile radius actually doubles. While this increase could reflect displacement, it could be due to other potential confounders in the longer window. To take a closer look at potential displacement, Figure 3 plots kernel densities of crime locations (relative to Charing Cross) for pre and post-Met periods. If there is displacement, one would expect an increase in the post-reform density just outside the 7-mile radius, where the Met was not introduced. While one can in fact see such a 'blip' for each crime category around this distance, we highlight that (i) it is negligible relative to the amount of crime in the treated area and (ii) similar blips are seen in the pre-Met period, suggesting it is not completely driven by displacement.[25] Appendix Figure A5 reinforces this conclusion by plotting the pre-post Met differences in the mean number of trials (by crime type) separately for each one-mile radius from Charing Cross. The only evidence of a crime reduction is for robbery in the treated area.

4. The Impact of the Met on Crime: Evidence from Daily Police Office Reports

A limitation of the Old Bailey data used in Section 3 is its restriction to three offenses – robbery, homicide, and burglary. Though the severity of these offenses mitigates concerns about changes in reporting, they make up a small fraction of Old Bailey trials: taken together, they represent about 7% of all trials from 1820 to 1830.[26] Thus, a natural question is whether the Old Bailey findings – that the Met led to an observable reduction in robbery but not burglary – generalize to other types of violent crimes (e.g. simple assault) and property crimes (e.g. shoplifting, pickpocketing, and simple/petty larceny). Many of these offenses were also classified as felonies at this time and trialed at the Old Bailey, but not transcribed and geocoded by us, as described in Section 3. This section thus complements the Old Bailey trial analysis with an alternative data source (the *Report or Account of the Proceedings at the several Police Offices*) that is all encompassing in terms of offense categories and that measures crime at an earlier stage of the judicial process than trials.

4.1. Daily Police Reports: Data Description

Appendix Figure C1 presents an excerpt from this data source, which contains the reports by the nine police offices (described in Section 2) that were run by the pre-1829 magistrates (and

[25] In this period, criminals would likely be travelling on foot. Horse drawn stage coaches could be hired, and from 1829, the first 'omnibuses' were introduced in central London (horse-drawn buses), but these alternatives were expensive. See https://www.oldbaileyonline.org/static/Transport.jsp, last accessed June 19, 2018, and Heblich et al. (2020). In this context, the control area with a radius of 7 to 15 miles from Charing Cross is not small.

[26] This is based on tabulations at the Old Bailey website: https://www.oldbaileyonline.org/forms/formStats.jsp.

continued until 1839). We sourced the reports from the National Archives and manually transcribed the data from January to April of: 1828 (the year pre-reform), 1830 (the year post-reform), 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 from the Archives. For each office and day (except Sundays), a detailed description of 'persons charged', 'informations' and 'property stolen' are reported. As can be seen from the appendix excerpt, 'informations' are varied in nature, and typically provide details about a specific incident, including descriptions of missing property, suspect names, and potential awards.

For each day and office, we create three crime measures: (i) number of 'property stolen' entries, (ii) number of property, violent, and other 'informations', and (iii) number of charges by crime type (property, violent, and other). The first two measure crime incidence (i.e. they report a crime incident and while the suspect may be known, he is not caught), while charges is most similar to modern day arrest data; there is a known suspect who is caught. One possible interpretation of these measures is to distinguish between *uncleared* and *cleared crimes,* where the informations and stolen property reports would be classified as the former and charges as the latter. The main offenses included in violent crime are robbery, assault, stabbing, wounding, and murder/manslaughter, while the main property offenses are burglary, theft, animal theft, and pickpocketing. Thus, while the violent and property crime categories used here include the Old Bailey offenses of homicide, robbery and burglary, they encompass many other offenses. To address the possibility that the introduction of police only shifts uncleared crimes into the cleared category, we also create a measure that aggregates all types of incidents (informations, stolen property, and charges) together; this is our best proxy of the total number of crimes.

Figure 2 shows that all of the police offices are physically located in the 4-mile inner circle around Charing Cross. These police offices cannot be uniquely matched to the treatment and control areas used in the first part of the paper. And, in fact, the jurisdiction of these offices extends beyond the treatment areas to include crimes in the City of London as well as beyond the 7-mile radius.[27] We thus, instead, use a simple pre-post design to estimate *the net effect on crime in London* (i.e. accounting for displacement to control areas) in a narrow window around the reform. We also note that an implication of the wider range of crime categories included in the Daily Reports analysis is a potentially increased susceptibility to concerns about reporting

[27] Specifically, the 1832 Daily Police Reports (National Archives MEPO 4/17) often (but not always) include hand-written notes indicating the Metropolitan Police division in which the offense is located. There are two important takeaways. First, this is not a one-to-one match, with some divisions corresponding to multiple police offices and vice versa. Second, crimes from the City of London and beyond the 7-mile radius are included in these documents, i.e. the catchment area for the police offices includes both treated and control areas of London.

biases than for those offenses (murder, robbery, burglary) studied in the Old Bailey.

4.2. Daily Police Reports: Summary Statistics

These historical records are digitized into a panel data set of crime measures at the office by date level for the nine offices and each Monday-Saturday in January through April of 1828, 1830, 1831, and 1832. We exclude the Thames Police Office as the Thames River Police is not in the jurisdiction of the Met and the composition and nature of crimes in this jurisdiction (docks and water) is likely to differ from other offices.[28] The full analysis sample includes 3,232 office x day observations while the 1828-1830 sample window includes 1,616 observations. Table 4 presents summary statistics for the entire sample and by period: pre-reform (1828), one-year post (1830) and three-year post (1830-1832). For the entire period, there are on average 0.5 informations, 6.4 charges and 0.4 reports of stolen property per day and office. Aggregating all crime measures together (top panel of Table 4), we find on average 7.3 crime reports per day and station. For all measures, property offenses comprise the largest crime category. Looking at the total number of crimes across years (1828 versus 1830), we see a reduction in the average number of violent crimes (per day and office) of 40%, which persists until 1832, but a small increase in property crimes (8%) and other crimes (4%). These increases are due to increases in the number of charges, while decreases are seen in both stolen property reports and property and other informations.

4.3. Daily Police Reports: Empirical Specification

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

$$Y_{it} = \beta PostMet_t + \alpha_w + \alpha_d + \alpha_i + \varepsilon_{it} \quad (3)$$

Y is the daily measure of crime reported in office *i* on date *t*. Our main variable of interest, *PostMet*, equals one on all dates *t* after the introduction of the Met (i.e. 1830 to 1832 in our analysis sample) and zero in the year before (i.e. 1828). Our baseline specification includes police office fixed effects (α_i) to control for unobservable office characteristics, such as

[28] We also exclude the "Metropolitan Police Office" as this 10th office was created only in April 1831; this restriction is only relevant for the larger estimation window. It is not the case that crimes in the Met jurisdiction were displaced to the Thames office; a pre-post analysis of the Thames office indicates a reduction in all crime measures in the first year after the Met and the baseline results are robust to including the Thames in the analysis.

differences across magistrates, as well as fixed effects for calendar weeks (α_w) and day of the week (α_d) to control for seasonality and variation in crime rates over the days of a week.

These data and simple pre-post specification clearly have limitations. One is an inability to control for confounding factors that changed at the same time as the Met. Based on the Old Bailey discussion, however, we believe these concerns are alleviated by (i) the short window emphasized for most of this analysis, (ii) the lack of contemporaneous reforms in these years, and (iii) comparability between the Old Bailey pre-post and difference-in-differences estimates. Second, having only one pre-period of data (January to April of 1828) limits our ability to test for pre-existing crime trends. But, as one argument for the new police was rising crime rates, it is hard to imagine deterrence being confounded by a downward trend in crime.

How is the parameter, β, interpreted? Compared to the Old Bailey analysis, where we could geocode offenses into two treated and controls areas, the catchment area for the pre-Met police offices includes both the 0-4 mile treatment and 4-7 mile uncertainty areas, as well as the control areas. Thus, this specification estimates the net effect of introducing the Met on crime in all of London; a perhaps unintended advantage is that we do not need to worry about crime displacement effects. As highlighted above, the other main difference from the Old Bailey analysis is the crime measures used: the daily police report data include all offenses, regardless of offense type or severity. While this increases the generalizability of the results beyond the selected offenses of robbery, burglary, and homicide, it does increase the possibility that the results are confounded by changes in reporting behavior.

4.4. Daily Police Reports: Main Results

Table 5 presents the results of estimating equation (3) using the daily crime reports for each outcome: any and number of informations (Panels B and C), any stolen property reports (Panel D), and number of charges (Panel E). Panel A presents the total number of incidents, i.e. the aggregated number of informations, stolen property offenses and charges. Column (1) shows the raw pre-post difference with the short sample window (i.e. 1828 and 1830 only) for all offenses. There is a significant reduction in the chance of observing *any* informations by 14.9 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%). Panel A shows that the combined effect on the total number of incidents (aggregated across categories) is positive (0.34 or 5.1%) and marginally significant. Adding police office, calendar week, and day of

week fixed effects in column (2) has little impact on the raw estimates.[29] Expanding the post-period to include 1831 and 1832 results in larger estimates, with unchanged sign and precision. We study possible reasons for this pattern in Table 6. Finally, consistent with the Old Bailey analysis, we present results from QML Poisson model estimations for the non-binary outcomes; column (7) combines all offense categories. Similar effect sizes are seen for the number of informations (-38%), charges (16.8%) and total incidents (5.2%). Except for the latter, these estimates are statistically significant.

Columns (4) to (6) of Table 5 look at property, violent and other crimes with OLS. The respective results using a Poisson model are shown in columns (8) to (10). The OLS results are consistent with the comparison of pre- and post-means. For informations, we see negative effects for all crime categories, with a reduction of any property and violent informations of 24% and 57%, respectively (the corresponding Poisson estimates are -30% and -64.1% though only the latter is statistically significant). For charges, there is a more heterogeneous pattern: charges increase (decrease) by about 21% (-26%) for property (violent) crime. Aggregating informations and charges, Panel A shows that the overall effect is a significant increase (8%) and decrease (-39%) in property and violent crime, respectively, but no effect on other crime. The corresponding Poisson estimates are almost exactly the same in magnitude for both property (7.8%) and violent crime (-39.1%), but less precisely estimated (not significant for property crime). For violent crime, the overall number of incidents decreases on average by 0.115 per day and office: this corresponds to a reduction throughout London of 287 violent crimes per year (0.115 x 8 offices x 6 days per week x 52 weeks). A comparison to the Old Bailey analysis suggests that about 5% (15 of 287 violent crimes averted per year) of these offenses are robberies. Of course, to the extent that the estimates are biased by offsetting increases in clearances or reporting, these numbers of violent crimes averted are a lower bound.

The combined measure – informations plus stolen property plus charges – is our best proxy for all criminal incidents, regardless of whether there are known suspects and associated charges. To summarize, the above analysis found a reduction in total violent crime that was driven by reductions in both violent informations (uncleared offenses) and charges (cleared offenses). This in fact parallels the reduction seen for robbery, a violent offense, in the Old Bailey analysis. In contrast, there is an increase in observed property crime incidents overall,

[29] Appendix Table A6 presents a number of robustness checks, including estimates: (i) excluding one office at a time, (ii) at the weekly instead of daily level, (iii) excluding incomplete weeks of data, e.g. due to holidays, and (iv) based on alternative specifications, including logarithms of the dependent variable (where appropriate). Table A7 shows the robustness to conventional clustering (by police office) as well as a wild cluster bootstrap procedure.

which is driven by an increase in charges that outweighs decreases in informations and stolen property reports. Increases in clearance rates or reporting are plausible drivers of the increase in charges: Increased clearance is possible as the physical presence of the Met officers walking the streets may have allowed them to apprehend property offenders, such as pick pocketers, as crimes were being committed. And given that the data include more (and less serious) offenses than just burglary (as in the Old Bailey analysis), reporting changes are also feasible. In this respect, the results are consistent with the Old Bailey analysis, where there was no burglary effect – i.e. deterrence and clearance/reporting channels offset each other.

4.5. Daily Police Reports Extension: Short- and Medium-Term Dynamics

What are the dynamic effects of creating the Met? We take advantage of the two-stage initial hiring described in Section 2.2 (i.e. inner divisions in September 1829 and outer divisions in February 1830) to estimate an extended version of equation (3) that allows for different coefficients on the treatment variable in: (i) January 1830 (post-Met but before the second hiring wave), (ii) all other months in 1830 (after the second wave), (iii) 1831 and (iv) 1832. This allows us to study the immediate effect of the large February 1830 hiring wave and whether the impact of the Met changes over time. Table 6 shows the results for the combination of all incidents in columns (1) – (3), the number of charges in (4) – (6), any informations in (7) – (9), and stolen property in column (10). There are two key takeaways. First, the point estimates generally increase over time, which may not be surprising given the increasing quality of police after the initial introduction of the Met and the continued hiring. Second, while some of the reduction in 'uncleared' crimes is immediate (for violent informations and stolen property reports), the significant increase in property crime charges does not kick in until the second wave. This could be because reporting did not increase until the new police began to gain the trust of the people or because the new police needed to gain experience and manpower to sufficiently increase apprehension/clearance rates, which was at least initially not their explicit task.

5. Extension: The Introduction of Professional County Police

The results thus far show that the creation of the London Met significantly reduced violent crimes: reductions in crime due to deterrence or incapacitation dominate increases in clearance and reporting. For property crimes, the latter effects are at least as large (in the Old Bailey) or larger (in the daily reports), such that there is less visible evidence of crime deterrence. Are these patterns specific to the creation of a professional force in London in the 1830s?

We answer this question by studying the roll-out of forces to the rural counties of England and Wales via the County Police Act of 1839 and the County and Borough Police Act of 1856. The former allowed for the creation of county forces while the latter made it mandatory. We digitized the year of county force formation and *initial* force size (see Appendix Table B1) from the Police History Society (Stallion and Wall, 1999). Panel A of Figure 5 maps the roll-out, and demonstrates no obvious clustering in the years of force creation by neighboring counties. Panel B depicts the evolution over time: 16 forces were created in 1840 (when permitted), 23 in 1857 (when mandatory), and 9 in the intervening years. The 1856 Act also established a national Inspectorate to annually certify force 'efficiency'. One important input was whether the officer to population ratio was sufficiently large; the 1839 Act recommended one officer per 1000 people.[30] We study the effect of having *any* county force as well as whether higher quality forces – measured by initial force size per capita – have differential effects.

To measure crime, we digitized the annual number of persons committed or bailed for trial for each fiscal year ending September 29, as reported in the *Judicial Statistics*, for six crime classes (see Appendix Table B2). We focus on the total number of charges and the sub-categories of violent, property, and other property crimes.[31] We also use 1851 and 1861 census records to create county level controls.[32] Of the 52 counties, we drop Middlesex, York, Sussex and Suffolk, as they represent regions where multiple forces were created (at different times) but crime data are only available for the aggregate (entire) county. We use 1832 to 1865 as the baseline sample years, resulting in a county by year panel of 1,632 observations (48 counties x 34 years). Appendix Table B3 shows the summary statistics.

We first estimate the overall effect of having *any* professional force (regardless of its characteristics) on crime with the following difference-in-differences model.

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

Crime is the log number of trials in county *c* and fiscal year *t* while *Force* equals one for county-year combinations with a professional force for all of year *t*.[33] County fixed effects (α_c) control for unobservable but constant differences across counties, e.g. pre-existing crime levels that

[30] See Bindler and Hjalmarsson (2019) for a more in-depth discussion of the county force roll-out.
[31] Violent includes crimes against person and violent property classes. Property includes the class of non-violent property and other property includes the classes of malicious property and forgery. Appendix Figure B1 shows that alternative crime measures (available after 1857) move in lockstep with our measure – charges.
[32] Census data are from *North Atlantic Population Project, UK Censuses,* https://www.nappdata.org/napp/; the controls are share male, married, native, in various age groups, unemployed or out of the labor force, and farmers.
[33] Results are robust to using log trials *per capita*; this is not our preferred choice given imperfect population data.

may be related to force creation decisions as well as any fixed characteristics of neighboring counties. Year fixed effects (α_t) capture national shocks, such as other criminal justice reforms (e.g. abolition of the death penalty and transportation and an 1855 Act that shifted non-violent larceny cases out of the courtroom). Column (1) of Table 7 presents the results for all charges in Panel A and violent, property, and other property charges in Panels B-D, respectively. The creation of a county force, on average, does not significantly affect trials in any crime category.

Yet, this non-effect could mask differential effects for forces of varying quality. We observe one dimension of quality – force size upon creation relative to the population. We thus evaluate the differential effect of introducing sufficiently or insufficiently large forces in an expanded specification, where superscript *j* indicates the threshold used to measure sufficiency.

$$Crime_{ct} = \beta_s SuffForce_{ct}^{j} + \beta_{ins} InsuffForce_{ct}^{j} + \alpha_c + \alpha_t + X_{ct}\theta + \varepsilon_{ct} \quad (5)$$

Columns (2)-(4) of Table 7 present the results for selected thresholds of 1500, 2000 and 2500 people per policeman. Under the strictest and weakest criteria, there are 10 and 30 sufficiently large forces, respectively. Force size matters: creating a sufficiently large force with less than 1500 people per policeman decreases the overall number of crimes by about 19%; comparable effects are seen for violent and property crime (18% and 14%, respectively) but not for other offenses. In contrast, creating an insufficiently large force (insignificantly) increases the number of property and other crimes. Moving across the table, a significant crime reducing effect of a 'sufficiently large' force is also seen using the weaker threshold of 2000. The effects of the sufficiently and insufficiently large forces on total charges are significantly different from each other for the 1500 and 2000 thresholds (p-values are shown in the table).

A causal interpretation of these findings relies on the assumptions of parallel trends, random timing of force formation, and conditional randomness of having a sufficiently large force. Appendix B summarizes and presents a number of robustness tests addressing these assumptions. Specifically, event study specifications in Appendix Figure B2 support the difference-in-differences assumptions and show that the crime reducing effect of relatively large forces is not immediate but starts around three years post reform and increases over time. Appendix Table B4 further shows that the timing of force adoption is unaffected by lagged crime rates in the county or neighboring counties, the size of pre-existing local forces, or neighboring county force characteristics. Appendix Table B5 finds no evidence of crime displacement from London to neighboring counties upon the creation of the Met. Finally, few

observables predict the force's initial type/size (Appendix Table B6).[34]

What do we learn from these findings – i.e. a crime reducing effect of sufficiently large forces but null effect of insufficient forces? Similar to what we argue in the London case, the net negative effect for sufficiently large forces suggests that deterrence and incapacitation (decreasing crime) can outweigh reporting and apprehension channels (increasing *reported* crime). But, these results also highlight that this will only happen when a force's quality is high enough (at least in the dimension measured here). Moreover, the increasing size of the crime-reducing effect over time further highlights the potential role of quality: Force 'quality' likely improved over time as people per officer ratios decreased (more officers were hired), more supervisors were hired, and experience was gained. These results reinforce our findings in the London daily police reports analysis (Table 6) that the estimates increase over time.

6. Conclusion

This paper estimates the effect on crime of the *introduction* of the London Metropolitan Police – the first professional force in the world – and provides evidence that this new force significantly decreased violent crime. Specifically, we estimate an annual reduction in violent crimes of *at least* 15 fewer robberies and 300 violent crimes more generally; given the potential biases due to increased clearance rates and (especially in the daily reports data) reporting, these estimates are likely to be lower bounds. For property crime, these potential biases may off-set any crime reducing effects, such that a crime reduction is not visible (though it may exist) in administrative charge or trial data.

The relative size of the violent crime reduction (about 40%) is of similar magnitude to contemporary studies. Using terror-related shocks to deployment, Draca et al. (2011) and DiTella and Schargrodsky (2004) find a decrease in crime of approximately 0.3% with a 1% increase in police (elasticity of -0.3). MacDonald et al. (2016) find a 45-85% difference in the number of crimes across boundaries of police catchment areas. But, comparisons of this unique historical context to policing studies today are not perfect. Thus, one can also think about the size of these effects relative to the size of the reform or its cost. About 3000 officers were hired by May of 1830, with annual salary costs estimated in Appendix Table A2 to be about 157,000 pounds. Using these numbers and our results (i.e a lower bound of about 300 violent crimes reduced), one could infer that one violent crime per year is deterred per 10 officers on the force

[34] Appendix Table B7 presents a number of additional robustness checks, including controlling for whether neighboring counties had insufficiently or sufficiently large forces.

or per approximately 500 pounds (measured in 1829). These ratios are clearly conservative given both the potential biases discussed above, especially for property crimes, and the fact that we over-estimate the cost of the reform (since we do not measure the marginal cost relative to what was spent on pre-Met 'policing'). Finally, given the inherent difficulties, even in a contemporary context, in estimating the (averted) cost of crime, we hesitate to benchmark our findings with such comparisons.

Our study demonstrates that the creation of this first professional police force was successful: crime decreased. Even an increase in clearance and/or reporting can be thought of as an achievement for an institution aiming to protect society. The successful implementation of a reform of this scale is evidence in and of itself of state capacity in early 19th century England. The consequences that this modern day institution had on societal dimensions besides crime, ranging from local economic activity and the enforcement of property rights to inequality and even political outcomes, is an important question for future research.

References

Acemoglu, Daron, Camilo Garcia-Jimeno, and James A Robinson (2015) "State capacity and economic development: A network approach," *American Economic Review,* 105(8): 2364-2409.

Banerjee, Abhijit, Raghabendra Chattopadhyay, Esther Duflo, Daniel Keniston, and Nina Singh (forthcoming) "Improving Police Performance in Rajasthan, India: Experimental Evidence on Incentives, Managerial Autonomy and Training", *American Economic Journal: Economic Policy*.

Becker, Gary (1968) "Crime and Punishment: An Economic Approach," *Journal of Political Economy*, 76(2): 169-217.

Besley, Timothy and Torsten Persson (2010) "State capacity, conflict, and development," *Econometrica,* 78(1): 1-34.

Bindler, Anna and Randi Hjalmarsson (2018) "How Punishment Severity Affects Jury Verdicts: Evidence from Two Natural Experiments," *American Economic Journal: Economic Policy*, 10(4): 36-78.

Bindler, Anna and Randi Hjalmarsson (2019) "The Impact of the First Professional Police Forces on Crime," *CEPR Discussion Paper 14068*.

Bindler, Anna and Randi Hjalmarsson (2020) "The Persistence of the Criminal Justice Gender Gap: Evidence from 200 Years of Judicial Decisions," *Journal of Law and Economics*, 63(2): 297-339.

Bignon, Vincent, Eve Caroli, and Roberto Galbiati (2017) "Stealing to Survive: Crime in XIXth Century France," *Economic Journal*, 127(599): pp.19–49.

Blanes I Vidal, Jordi and Tom Kirchmaier (2018) "The Effect of Police Response Time on Crime Clearance Rates," *Review of Economic Studies*, 85(2): 855-891.

Blanes I Vidal, Jordi and Giovanni Mastrobuoni (2018) "Police Patrols and Crime", IZA Discussion Paper No. 11393.

Brooks, Leah (2008) "Volunteering to Be Taxed: Business Improvement Districts and the Extra Governmental Provision of Public Safety," *Journal of Public Economics*, 92(1): 388-406.

Buckles, Kasey, Melanie Guldi, and Joseph Price (2011) "Changing the Price of Marriage: Evidence from Blood Test Requirements," *The Journal of Human Resources*, 46(3): 539-567.

Cameron, A. Colin, Jonah B. Gelbach and Douglas L. Miller (2008) "Bootstrap-based improvements for inference with clustered errors," *The Review of Economics and Statistics*, 90(3): 414-427.

Cann Chandrasekher, Andrea (2016) "The Effect of Police Slowdowns on Crime," *American Law and Economics Review*, 18(2): 385-437.

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

Cheng, Cheng and Wei Long (2018) "Improving police services: Evidence from the French Quarter Task Force," *Journal of Public Economics*, 164(C): pp. 1-18.

Davis, Jennifer (1984) "A Poor Man's System of Justice: The London Police Courts in the Second Half of the Nineteenth Century," *The Historical Journal*, 27(2): 309-335.

Dincecco, Mark and Gabriel Katz (2014) "State capacity and long-run economic performance," *The Economic Journal,* 126(590): 189-218.

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

Draca, Mirko, Stephen Machin, and Robert Witt (2011) "Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks," *American Economic Review*, 101(5): 2157-2181.

Emmerichs, Mary Beth (2001) "Getting Away with Murder? Homicide and the Coroners in Nineteenth-Century London," *Social Science History*, 25(1): 93-100.

Emsley, Clive (2009) *The Great British Bobby: A History of British Policing from the 18th Century to the Present*. Trafalgar Square.

Evans, William, and Emily Owens (2007) "COPS and crime," *Journal of Public Economics*, 91(1-2): 181-201.

Goldin, Claudia, and Cecilia Rouse (2000) "Orchestrating Impartiality: The Impact of "Blind" Auditions on Female Musicians," *American Economic Review,* 90(4): 715-741.

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

Heblich, Stephan, Stephen Redding and Daniel Sturm (2020) "The Making of the Modern Metropolis: Evidence from London," *Quarterly Journal of Economics*, 135(4): 2059-2133.

Hitchcock, Tim, Robert Shoemaker, Clive Emsley, Sharon Howard, Jamie McLaughlin, et al. "The Old Bailey Proceedings Online, 1674-1913," www.oldbaileyonline.org (version 7.1, April 2013).

Johnson, Noel D and Mark Koyama (2017) "States and economic growth: Capacity and constraints," *Explorations in Economic History*, 64(C): 1–20.

Judicial Statistics. 1857-1892. England and Wales. Part I. Police – Criminal Proceedings – Prisons. Part II. Common-Law – Equity – Civil and Canon Law. Obtained from the House of Commons Parliamentary Papers Online.

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

Linos, Elizabeth (2018) "More Than Public Service: A Field Experiment on Job Advertisements and Diversity in the Police," *Journal of Public Administration Research and Theory* 28(1): 67–85.

Lyman, J.L. (1964) "The Metropolitan Police Act of 1829" *Journal of Criminal Law and Criminology*, 55(1): 141-154.

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

Mehlum, Halvor, Edward Miguel, and Ragnar Torvik (2006) "Poverty and crime in 19th century Germany," *Journal of Urban Economics*, 59(3): 370–388.

Mello, Steven (2019) "More COPS, less crime," *Journal of Public Economics* 172: 174-200.

Miller, Amalia and Carmit Segal (2019) "Do Female Officers Improve Law Enforcement Quality? Effects on Crime Reporting and Domestic Violence," *Review of Economic Studies*, 86: 2220-2247.

Owens, Emily (2020) "The Economics of Policing," In: Zimmermann K. (eds) *Handbook of Labor, Human Resources and Population Economics*. Springer, Cham. https://doi.org/10.1007/978-3-319-57365-6_146-1.

Pfuhl, Erdwin (1983) "Police Strikes and Conventional Crime: A Look at the Data," *Criminology*, 21: 489–503.

Radzinowicz, Leon (1953) "Trading in Police Services: An Aspect of the Early 19th Century Police in England," *University of Pennsylvania Law Review*, 102(1): 1-30.

Rawlings, Philip (2008) "Policing Before the Police," *Handbook of Policing*, Routledge.

Reynolds, Elaine (1998) *Before the Bobbies: The Night Watch and Police Reform in Metropolitan London, 1720-1830*. MacMillan Press, Ltd. Pp. 234.

Stallion, Martin and David Wall (1999) "The British Police: Police Forces and Chief Officers 1829-2000", Athenaeum Press, Gateshead, Tyne & Wear. Pp. 262.

Traxler, Christian and Carsten Burhop (2010) "Poverty and crime in 19th century Germany: a reassessment," Working Paper, Max Planck Institute for Research on Collective Goods.

Tynkkynen, K. (1995) "Four cholera epidemics in nineteenth-century London," *Hippokrates*, 12:62-88.

Uchida, Craig (2015) "The Development of the American Police: An Historical Overview," In Roger Dunham and Geoffrey Alpert (Eds), *Critical Issues in Policing* (7th edition, pp. 11-30). Long Grove, Illinois: Waveland Press, Inc.

Weisburst, Emily (2019) "Safety in Police Numbers: Evidence of Police Effectiveness from Federal COPS Grant Applications," *American Law and Economics Review* 21(1): 81-109.

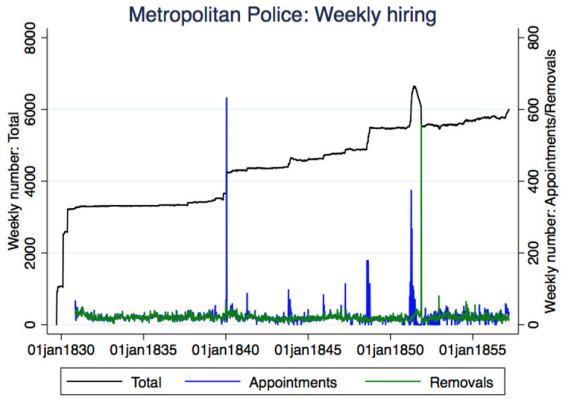
Wolpin, Kenneth I. (1978) "An Economic Analysis of Crime and Punishment in England and Wales, 1894-1967," *Journal of Political Economy*, 86(5): 815-840.

Wong, Yue-Chim Richard (1995) "An Economic Analysis of the Crime Rate in England and Wales, 1857-92," *Economica*, 62(246): 235-246.

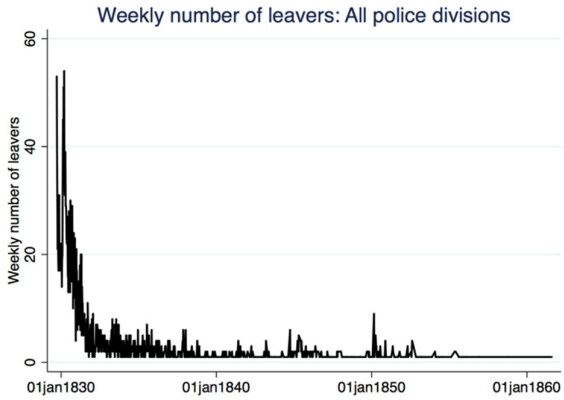
Wooldridge, Jeffrey M. (1999) "Distribution-free estimation of some nonlinear panel data models," *Journal of Econometrics* 90(1): 77-97.

Figure 1. London Metropolitan Police – Weekly Hires and Dismissals

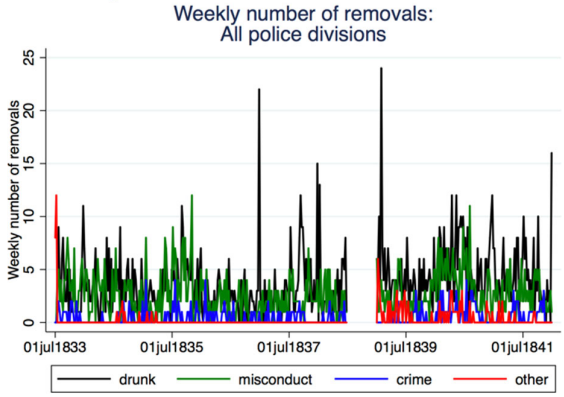
Panel A. Weekly Hiring: 1830-1856



Panel B. Weekly Leavers



Panel C. Weekly Dismissals by Reason

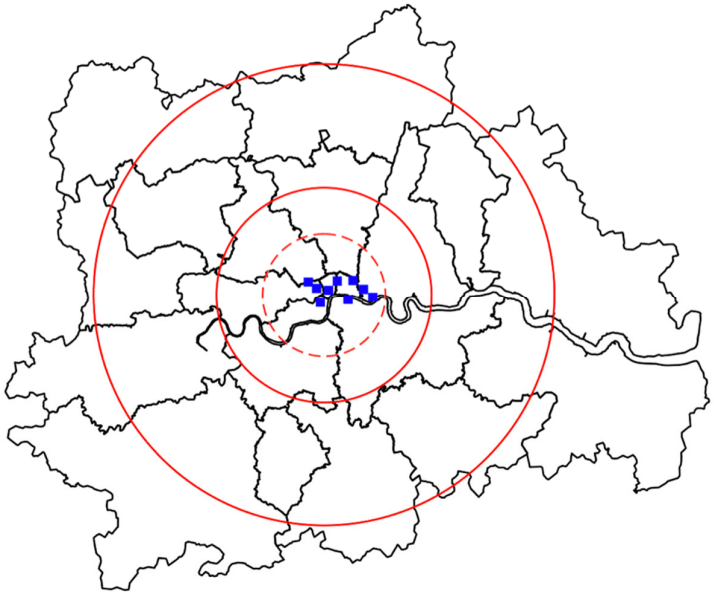


NOTES – Panel A shows the weekly number of total police, appointments as well as removals from the Metropolitan Police between 1829 (1830 for appointments and removals) and 1857. This figure is based in manually transcribed data from the Weekly State of the Metropolitan Police 1829-1857 available at the London National Archives (MEPO 4/1). Panel B 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 C shows the weekly number of dismissals split up by detailed reason (drunkenness, neglect or misconduct, criminal behavior, other). This figure is 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 2. The London Metropolitan Police Jurisdiction (1829)

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

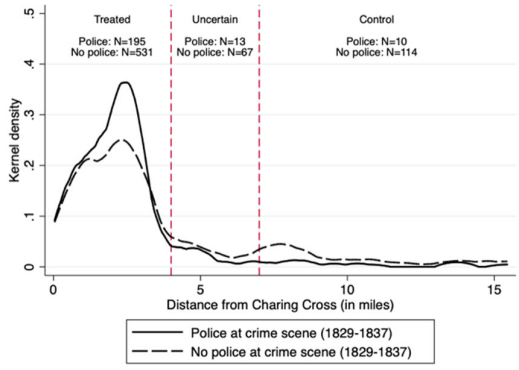
Panel B. Police Stations (Existing Before the Metropolitan Police and until 1839)



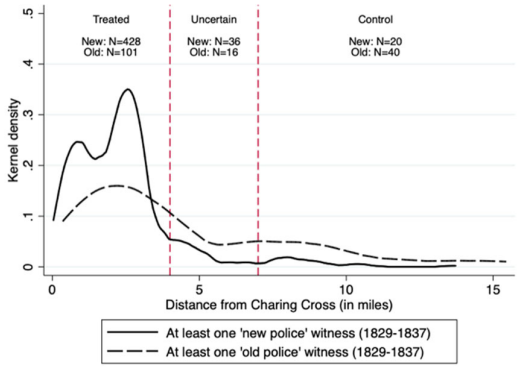
NOTES – Panel A presents a map of the original London Metropolitan Police District. Shaded in red is the City of London Police area, outside of the Met's jurisdiction. The large letters indicate the various districts of the Metropolitan Police. The map is available from the British Library's online map 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. Spillovers in Policing and Crime Displacement

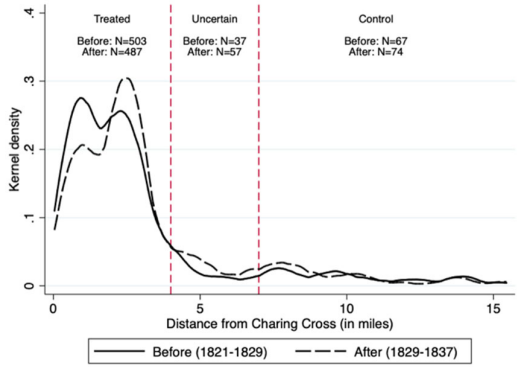
Panel A. Police at Crime Scene



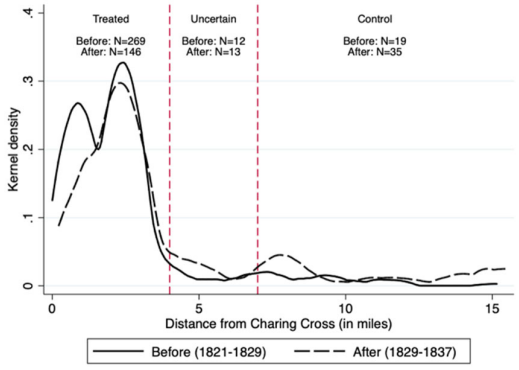
Panel B. New/Old Type Police Witnesses



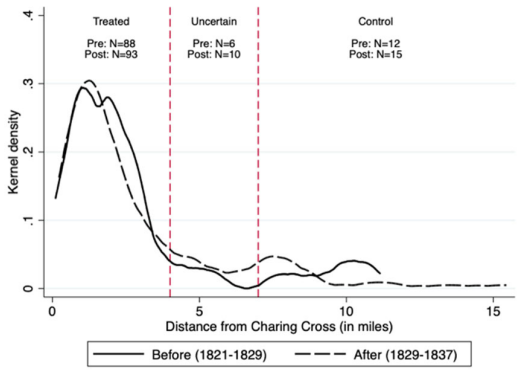
Panel C. Crime Locations: Burglary



Panel D. Crime Locations: Robbery

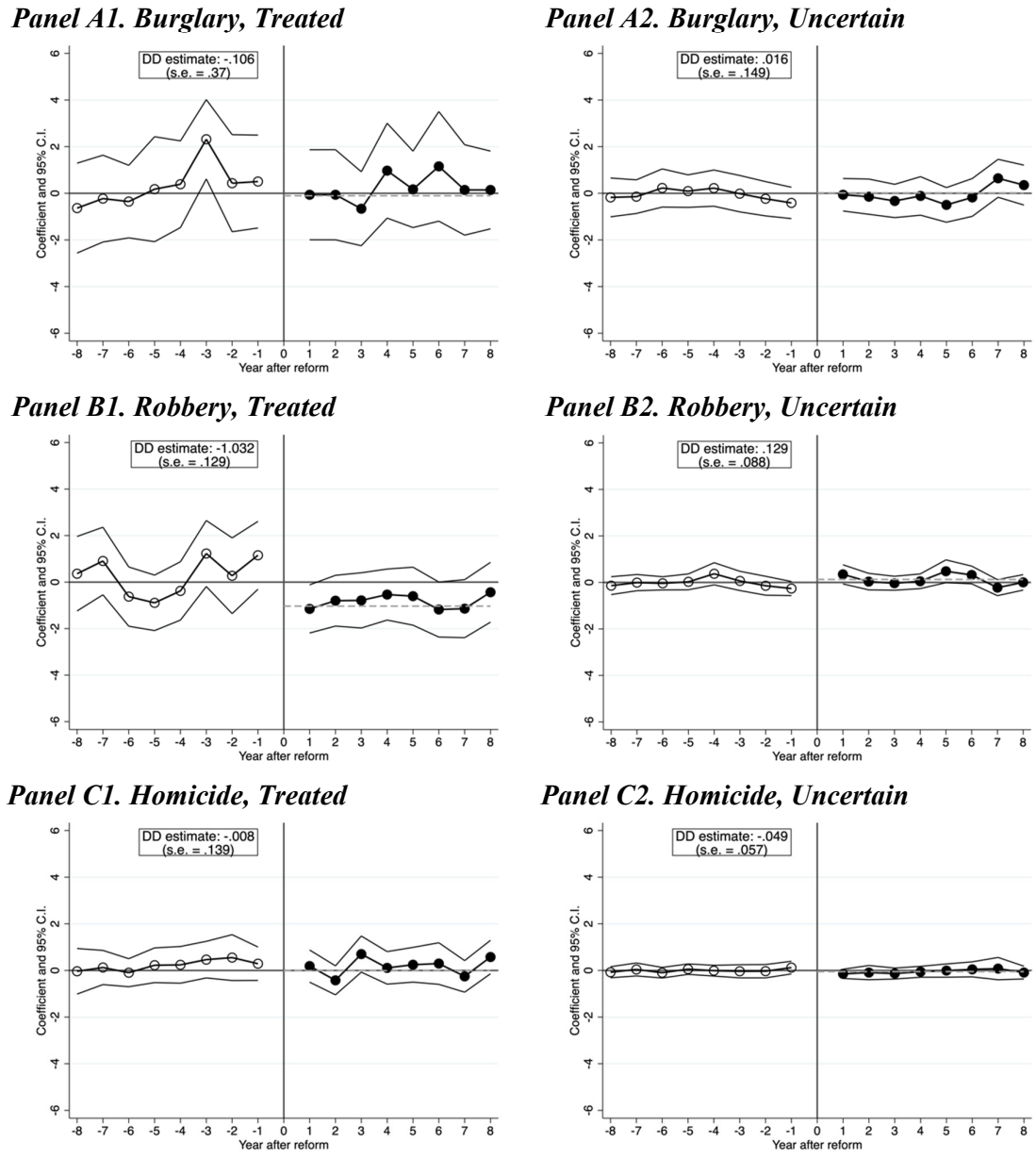


Panel E. Crime Locations: Homicide



NOTES – Panel A shows the kernel density of homicide, robbery and burglary trials at the Old Bailey from 1829 (after the introduction of the Met) to 1837 with/without police at the crime scene by distance from Charing Cross (in miles). Panel B shows the respective kernel density for trials with at least one "new" or "old" type of police witness at the trial (see the text for details on the types of police). Panels C to E show the kernel densities of crime locations for trials at the Old Bailey for 1821-1829 (before the introduction of the Met) and 1829-1837 (after the introduction of the Met), for burglary, robbery and homicide. All figures exclude the City of London. The dashed vertical lines mark the thresholds in terms of distance from Charing Cross for the treated, uncertain and control area, respectively. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

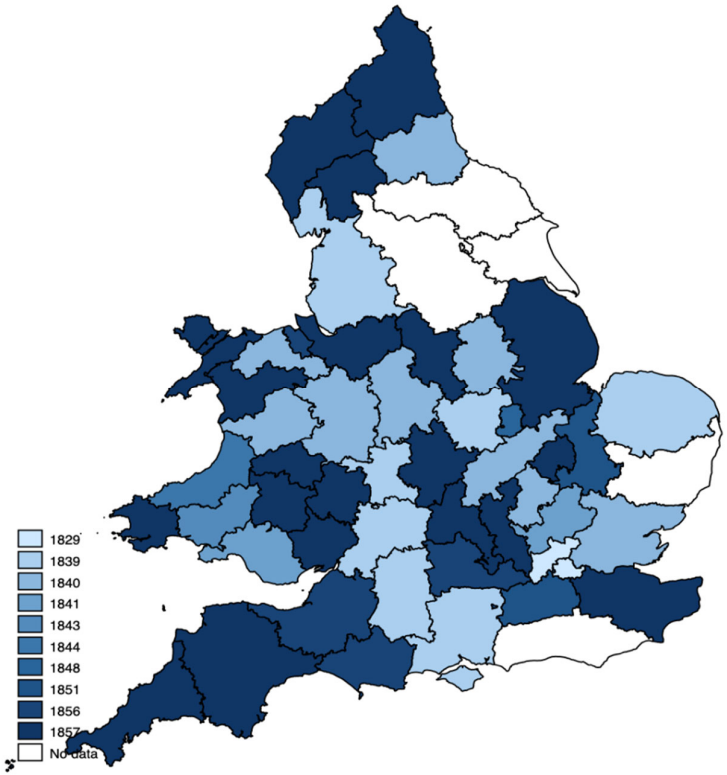
Figure 4. Event Study: The Effect of the Met on Crime (Old Bailey Proceedings)



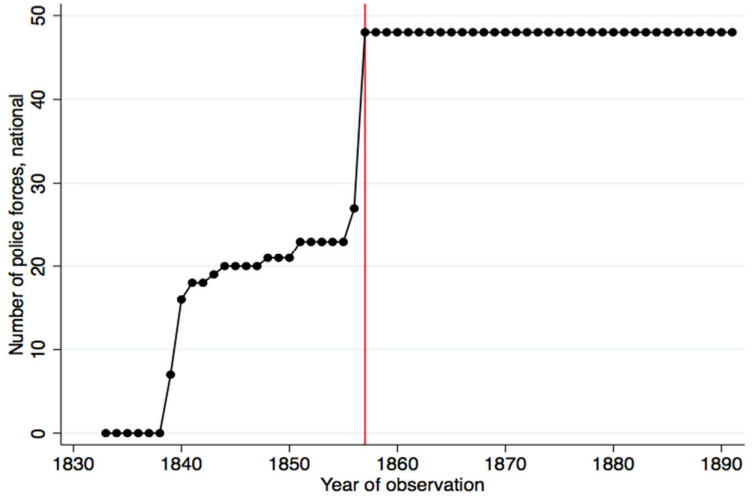
NOTES – The figures show the estimated coefficients and 95% confidence intervals corresponding to the event-study specifications described in Section 3.4. The figures are based on expanding equation (2) by allowing interactions between the treatment and uncertainty dummies with each year leading up to/following the introduction of the Met: $Trials^{o}_{amy} = \sum_{t=-8}^{8} \gamma_1^t (Treatment * PostMet)_{amy} \mathbb{I}(t = y - 1829) + \sum_{t=-8}^{8} \gamma_2^t (Uncertain * PostMet)_{amy} \mathbb{I}(t = y - 1829) + \alpha_y + \alpha_m + \alpha_a + \varepsilon_{amy}$. The figures to the left represent the estimates for the treated area (γ_1), the figures to the right for the uncertain area (γ_2). Panels A1 and A2 show the results for burglary, Panels B1 and B2 for robbery, Panels C1 and C2 for homicide. A year is defined as September to August. The vertical line represents the year 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 5. The Roll-Out of Police Forces for English and Welsh Counties

Panel A. Map of County Force Roll-out



Panel B. Number of Professional County Police Forces



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'. Boundary data were downloaded from https://vision.port.ac.uk/downloads/download_free/boundaries.jsp. Panel B 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 5 for details on the data and sample.

Table 1. Differences in Means in the Old Bailey Proceedings

	Burglary				Robbery				Homicide			
	Before	After	△		Before	After	△		Before	After	△	
Panel A. 1828-1832, Y = Number of crimes per month/area												
Treated	4.70	4.40	-0.30		2.80	1.33	-1.48	***	0.70	0.85	0.15	
Uncertain	0.40	0.40	0.00		0.05	0.10	0.05		0.05	0.03	-0.03	
Control	1.15	1.03	-0.13		0.30	0.28	-0.03		0.25	0.08	-0.17	
City of London	0.95	1.05	0.10		0.50	0.35	-0.15		0.15	0.30	0.15	
All	1.80	1.72	-0.08		0.91	0.51	-0.40	**	0.29	0.31	0.03	
Panel B. 1821-1837, Y = Number of crimes per month/area												
Treated	4.81	4.90	0.09		2.59	1.46	-1.13	***	0.85	0.93	0.08	
Uncertain	0.36	0.57	0.21	**	0.11	0.14	0.03		0.06	0.10	0.04	
Control	0.68	0.78	0.10		0.18	0.35	0.17	**	0.12	0.15	0.03	
City of London	0.79	1.14	0.35	**	0.57	0.25	-0.32	***	0.11	0.23	0.12	**
All	1.66	1.85	0.19		0.86	0.55	-0.31	***	0.28	0.35	0.07	

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 each offense as well as by area (separately and all areas together). Trials are allocated to areas according to the following definitions: *Treated* (within 4 miles from Charing Cross), *Uncertain* (between 4 and 7 miles from Charing Cross), *Control* (more than 7 miles from Charing Cross), and *City of London* (in the City of London). Panel A shows the results for 1828-1832, Panel B for 1821-18327 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 2. 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:	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832
Treated (<4 miles)								
Post Met	0.025	-0.018	0.574***	0.540***	-0.487***	-0.485***	0.082***	0.007
	(0.018)	(0.032)	(0.019)	(0.034)	(0.021)	(0.044)	(0.021)	(0.045)
Observations	1.586	427	1.586	427	1.586	427	1.586	427
Uncertain (4-7 miles)								
Post Met	0.034	-0.071	0.455***	0.357***	-0.254***	-0.133	0.074	-0.092
	(0.064)	(0.145)	(0.057)	(0.117)	(0.082)	(0.211)	(0.057)	(0.187)
Observations	135	31	135	31	135	31	135	31
Control (>7 miles)								
Post Met	0.014	-0.007	0.155***	0.088**	-0.056	0.059	-0.005	-0.052
	(0.052)	(0.083)	(0.033)	(0.038)	(0.063)	(0.096)	(0.038)	(0.077)
Observations	230	89	230	89	230	89	230	89
City of London								
Post Met	-0.062	-0.096	0.268***	0.113	-0.259***	-0.201*	-0.031	-0.156
	(0.039)	(0.059)	(0.042)	(0.081)	(0.056)	(0.103)	(0.052)	(0.103)
Observations	314	100	314	100	314	100	314	100

NOTES - The table shows the results of estimating equation (1). Specifically, it regresses the first stage outcomes of police presence (dummy variables for any police witness at the trial, any "new" police witness, any "old" police witness, and whether any police was at the crime scene) on a dummy indicating whether the offense occurred after the creation of the Metropolitan Police (*Post Met)* and offense fixed effects. These results are estimated separately for each treatment and control area (see each panel) and for the long and short windows (odd and even columns, respectively). As highlighted in the text, these regressions are based on a data set of individual trials, where we consider the police presence at each trial. The data are sourced from the *Old Bailey Proceedings Online* and own transcriptions/calculations; the sample includes trials for robbery, burglary and homicide. See Section 3.1 for details. Robust standard errors are shown in parentheses below the coefficient. * 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)	(10)	(11)
Sample:	1821-1837	1828-1832	1828-1832	1828-1832	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832
Specification:	Intensive margin: By month/area					Intensive margin: By month/area		Extensive margin (1/0): By week/area		Extensive margin (1/0): By month/distance band	
City of London:	Control	Control	Treated from 02 April 1832	Uncertain	Excluded	Control		Control		Control	
	OLS					QML Poisson		OLS		OLS	
Panel A. Burglary											
Post Met x Treatment Area	-0.106	-0.294	-0.285	-0.234	-0.120	0.821*	0.951	-0.069*	-0.062	-0.032	-0.064
	(0.370)	(0.660)	(0.474)	(0.686)	(0.701)	(0.091)	(0.124)	(0.036)	(0.069)	(0.036)	(0.072)
Post Met x Uncertainty Area	0.016	0.006	-0.013	0.116	0.180	1.290**	1.015	-0.006	-0.029	0.031	-0.028
	(0.149)	(0.288)	(0.298)	(0.319)	(0.377)	(0.144)	(0.133)	(0.026)	(0.049)	(0.031)	(0.059)
Panel B. Robbery											
Post Met x Treatment Area	-1.032***	-1.297***	-0.832***	-1.281***	-1.336***	0.727	0.693*	-0.115***	-0.127*	-0.116***	-0.168**
	(0.219)	(0.428)	(0.299)	(0.433)	(0.438)	(0.341)	(0.141)	(0.033)	(0.065)	(0.034)	(0.069)
Post Met x Uncertainty Area	0.129	0.228	0.288*	0.144	0.189	1.705	2.928***	0.020	-0.002	0.018	0.027
	(0.088)	(0.162)	(0.170)	(0.189)	(0.208)	(0.800)	(0.597)	(0.016)	(0.031)	(0.018)	(0.030)
Panel C. Homicide											
Post Met x Treatment Area	-0.008	0.120	0.072	0.188	0.222	0.618***	1.053	0.026	0.064	0.000	0.044
	(0.139)	(0.251)	(0.181)	(0.262)	(0.266)	(0.094)	(0.482)	(0.027)	(0.047)	(0.028)	(0.051)
Post Met x Uncertainty Area	-0.049	-0.055	-0.062	0.100	0.047	0.975	0.433*	-0.005	-0.007	0.002	-0.008
	(0.057)	(0.115)	(0.119)	(0.133)	(0.143)	(0.148)	(0.198)	(0.012)	(0.024)	(0.015)	(0.024)
Observations	816	240	240	240	180	816	240	3,604	1,060	3,672	1,080
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Week fixed effects	No	No	No	No	No	No	No	Yes	Yes	No	No
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to the baseline difference-in-differences model in equation (2). Panel A shows the results for burglary, Panel B for robbery and Panel C for homicide. The dependent variable is the number of crimes (that are brought to trial) per month and area in columns (1) to (7), a dummy variable indicating whether there is any crime in a given week and area in columns (8) and (9), and a dummy variable indicating whether there is any crime in a given month and distance band from Charing Cross in columns (10) and (11). For all columns, 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 (2) to (5), we vary whether we treat the City of London as part of the control area, as treated from April 1832 when they introduced their own police force, as part of the uncertainty area or exclude the City of London from the analysis. Columns (6) and (7) report incident rate ratios from QML Poisson regressions (using the *xtpqml* Stata program); the other columns report the estimated coefficients from OLS regressions. Distance bands in columns (10) and (11) are circles around Charing Cross: less than 1 mile, 1-2 miles, 2-3 miles, ... 13-14 miles and more than 14 miles. Robust standard errors are shown in parentheses below the coefficient. Regressions are based on manually geocoded data from the Old Bailey Proceedings Online and own transcriptions/calculations; see the text for details. * p<0.1, ** p<0.05, *** p<0.01.

Table 4. Summary Statistics: Daily Crime Reports

Sample Period	All		Pre-Met: 1828		Post-Met: 1830		Post-Met: 1830-1832	
N	3,232		800		816		2,432	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD
All incidents: Informations + charges + property stolen								
Number of incidents: All	7.299	3.801	6.685	3.569	7.025	3.672	7.501	3.854
Number of incidents: Property	5.748	3.264	5.189	3.139	5.592	3.138	5.932	3.283
Number of incidents: Violent	0.203	0.492	0.295	0.599	0.178	0.445	0.173	0.447
Number of incidents: Other	1.348	1.373	1.201	1.320	1.255	1.322	1.396	1.386
Informations								
Number of informations: All	0.513	0.982	0.791	1.154	0.489	0.875	0.421	0.899
Number of informations: Property	0.401	0.806	0.566	0.922	0.396	0.743	0.346	0.757
Number of informations: Violent	0.048	0.242	0.101	0.362	0.036	0.192	0.030	0.182
Number of informations: Other	0.065	0.282	0.124	0.386	0.059	0.274	0.0453	0.236
Any informations: All	0.311	0.463	0.465	0.499	0.316	0.465	0.260	0.439
Any informations: Property	0.269	0.444	0.371	0.483	0.279	0.449	0.236	0.424
Any informations: Violent	0.043	0.204	0.086	0.281	0.037	0.188	0.029	0.168
Any informations: Other	0.057	0.232	0.107	0.310	0.053	0.224	0.040	0.197
Charges								
Number of charges: All	6.382	3.590	5.281	3.154	6.161	3.419	6.744	3.651
Number of charges: Property	4.946	3.064	4.010	2.746	4.834	2.878	5.254	3.101
Number of charges: Violent	0.155	0.421	0.194	0.479	0.143	0.402	0.143	0.399
Number of charges: Other	1.284	1.355	1.077	1.271	1.199	1.306	1.352	1.375
Any charges: All	0.991	0.0943	0.983	0.131	0.990	0.099	0.994	0.078
Any charges: Property	0.976	0.153	0.949	0.221	0.979	0.143	0.985	0.121
Any charges: Violent	0.136	0.343	0.164	0.370	0.127	0.334	0.127	0.333
Any charges: Other	0.660	0.474	0.598	0.491	0.627	0.484	0.681	0.466
Property stolen								
Number of incidents	0.405	0.750	0.613	0.951	0.376	0.655	0.337	0.656
Any incident	0.292	0.455	0.394	0.489	0.295	0.456	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 4.1. The first two columns show 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 number of observations is shown at the top of each column. 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 5. Daily Crime Reports: Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample:	1828-1830	1828-1830	1828-1832	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830
Crime type:	total	total	total	property	violent	other	total	property	violent	other
Estimator	OLS						QML Poisson			
Panel A. Number of all incidents per day/station										
Post Met Police	0.340*	0.347**	0.821***	0.406***	-0.115***	0.056	1.052	1.078	0.609**	1.046
	(0.180)	(0.154)	(0.134)	(0.135)	(0.026)	(0.063)	(0.047)	(0.060)	(0.119)	(0.144)
Panel B. Any informations per day/station										
Post Met Police	-0.149***	-0.148***	-0.206***	-0.090***	-0.049***	-0.055***	-	-	-	-
	(0.024)	(0.022)	(0.019)	(0.022)	(0.012)	(0.013)				
Panel C. Number of informations per day/station										
Post Met Police	-0.302***	-0.301***	-0.371***	-0.170***	-0.064***	-0.065***	0.619**	0.700	0.359***	0.473**
	(0.051)	(0.046)	(0.041)	(0.039)	(0.014)	(0.016)	(0.149)	(0.171)	(0.129)	(0.140)
Panel D. Any 'stolen property' per day/station										
Post Met Police	-0.098***	-0.099***	-0.137***	-	-	-	-	-	-	-
	(0.024)	(0.023)	(0.019)							
Panel E. Number of charges per day/station										
Post Met Police	0.879***	0.890***	1.471***	0.827***	-0.050**	0.126**	1.168***	1.206**	0.742	1.117
	(0.164)	(0.140)	(0.120)	(0.121)	(0.022)	(0.061)	(0.068)	(0.092)	(0.146)	(0.157)
Observations	1,616	1,616	3,232	1,616	1,616	1,616	1,616	1,616	1,616	1,616
Office FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar week FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of week FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to equation (3), discussed in Section 4.3. For a description of the underlying data, see Section 4.1. The dependent variable in Panel A is the number of all incidents (i.e. the sum of informations, stolen property incidents and charges), in Panel B a dummy variable indicating whether there are any informations, in Panel C the number of informations, in Panel D a dummy variable indicating whether there are any stolen property reports and in Panel E the number of charges. The independent variable of interest is a dummy variable *Post Met Police* equal to one for any date after the introduction of the Met Police. The top of each column indicates the years included in the sample and where appropriate the crime category, as well as the estimation details (OLS, QML Poisson). Robust standard errors are shown in parentheses below the estimated coefficients. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Daily Crime Reports: Different Stages of Police Hiring

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample:	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832	1828-1832
Y:	*Number of all incidents*			*Number of charges*			*Any informations*			*Any 'stolen property'*
Crime type:	total	property	violent	total	property	violent	total	property	violent	property
Post Met: 1830, January	-0.496*	-0.158	-0.130***	0.030	0.214	-0.056	-0.114***	-0.045	-0.055***	-0.114***
	(0.268)	(0.233)	(0.043)	(0.246)	(0.209)	(0.037)	(0.038)	(0.038)	(0.019)	(0.040)
Post Met: 1830, > January	0.632***	0.606***	-0.111***	1.177***	1.040***	-0.049**	-0.160***	-0.106***	-0.046***	-0.094***
	(0.176)	(0.153)	(0.027)	(0.163)	(0.140)	(0.023)	(0.024)	(0.023)	(0.012)	(0.025)
Post Met: 1831	0.772***	0.688***	-0.131***	1.382***	1.146***	-0.065***	-0.220***	-0.141***	-0.054***	-0.125***
	(0.165)	(0.141)	(0.026)	(0.151)	(0.129)	(0.021)	(0.021)	(0.021)	(0.011)	(0.023)
Post Met: 1832	1.350***	1.147***	-0.117***	2.157***	1.783***	-0.034	-0.250***	-0.174***	-0.068***	-0.187***
	(0.175)	(0.151)	(0.026)	(0.160)	(0.137)	(0.022)	(0.021)	(0.021)	(0.011)	(0.022)
p-value	*0.000*	*0.000*	*0.853*	*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	3,232	3,232	3,232
Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to an expansion of equation (3), where we allow 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). In other words, we allow the Metropolitan Police to have differential effects in four post-met periods (January 1830, February – April 1830, Jan-April 1831, Jan-April 1832). For a description of the underlying data, see Section 4.1. The dependent variable in columns (1) to (3) is the total number of incidents (charges + informations + property stolen incidents), in columns (4) to (6) the number of charges, in columns (7) to (9) a dummy variable indicating whether there are any informations, and in column (10) 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

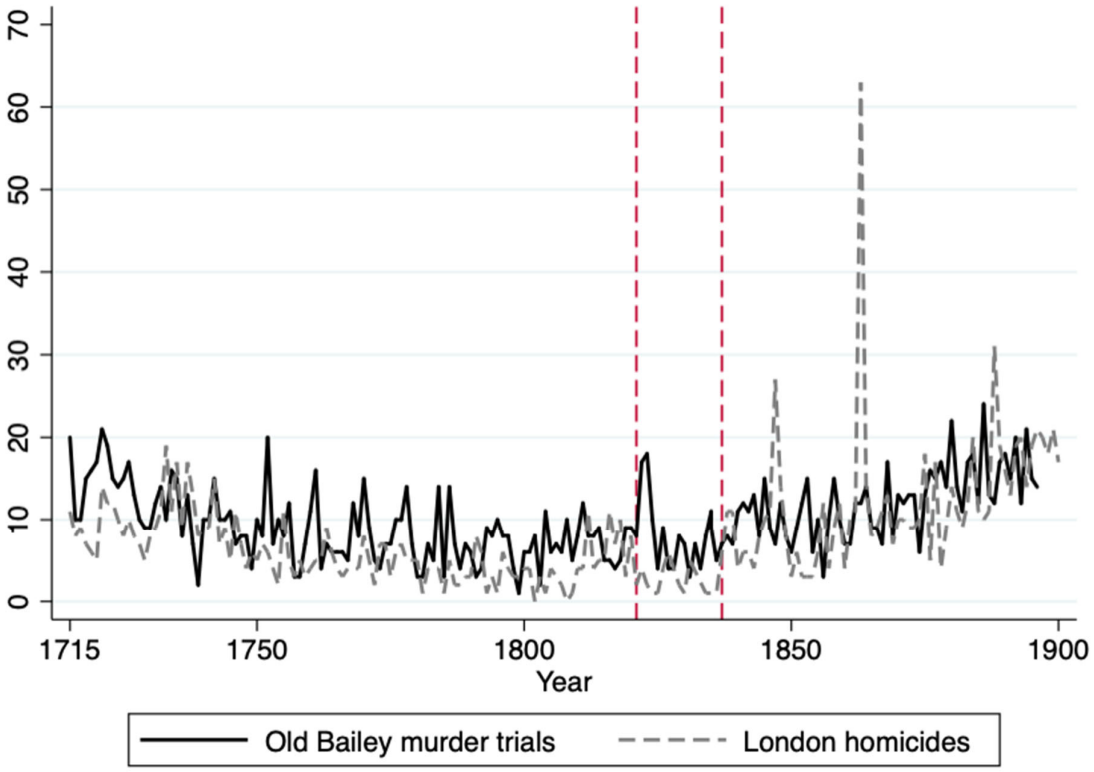
Table 7. The Effect of County Police Forces on Crime

	(1)	(2)	(3)	(4)
	Dep. Variable: Log (Number of Charges)			
	Police Force Size Defined According to the Below Thresholds in the Number of People Per Policeman (upon police force creation)			
	Any Force	1500	2000	2500
Panel A: All Charges				
Force	-0.024			
	[0.033]			
Sufficiently Large Force		-0.190***	-0.118**	-0.079
		[0.062]	[0.055]	[0.048]
Insufficiently Large Force		0.022	0.053	0.066
		[0.043]	[0.057]	[0.075]
p-Value		*0.011*	*0.048*	*0.151*
Panel B: Violent Charges				
Force	-0.031			
	[0.050]			
Sufficiently Large Force		-0.183*	-0.129*	-0.093
		[0.104]	[0.070]	[0.062]
Insufficiently Large Force		-0.002	0.034	0.048
		[0.058]	[0.078]	[0.104]
p-Value		*0.129*	*0.128*	*0.267*
Panel C: Property Charges				
Force	0.017			
	[0.042]			
Sufficiently Large Force		-0.143**	-0.063	-0.028
		[0.065]	[0.066]	[0.057]
Insufficiently Large Force		0.064	0.090	0.101
		[0.050]	[0.060]	[0.083]
p-Value		*0.010*	*0.090*	*0.2351*
Panel D: Other Charges				
Force	0.002			
	[0.078]			
Sufficiently Large Force		-0.181	-0.055	-0.049
		[0.120]	[0.097]	[0.089]
Insufficiently Large Force		0.027	0.009	0.030
		[0.077]	[0.092]	[0.107]
p-Value		*0.082*	*0.549*	*0.489*

NOTES – The table presents the results of the county-level difference-in-differences model (see equation (4) for column (1) and equation (5) for columns (2)-(4)). The dependent variable in all columns is the log number of charges overall (Panel A), for violent crime (Panel B), for property crime (Panel C) and for other crime (Panel D). In column (1), 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 in place for the entire year. In columns (2) to (4), the variables of interest "Sufficiently Large Force" and "Insufficiently Large Force" are equal to one for a county c in any year t after which a sufficiently large or insufficiently large county police force has been created. Sufficiency is defined according to the number of people per officer, with varying thresholds as indicated at the top of each column. The p-value (in italics) corresponds to a hypothesis test of equality of the two parameters for "Sufficiently Large Force" and "Insufficiently Large Force". All specifications include county and year fixed effects. The baseline sample includes 48 counties for the years 1832-1865. For four years (1832, 1833, 1840, 1852) we do not have charges by each crime type. In the few instances when there are zero charges in a given year/county, we drop this observation when taking the logarithm of the outcome variable. This results in sample sizes of 1632 observations in Panel A, 1431 observations in Panel B, 1440 observations in Panel C and 1267 observations in Panel D. Standard errors are clustered by county and shown in brackets below the coefficient. *** p<0.01, ** p<0.05, * p<0.1

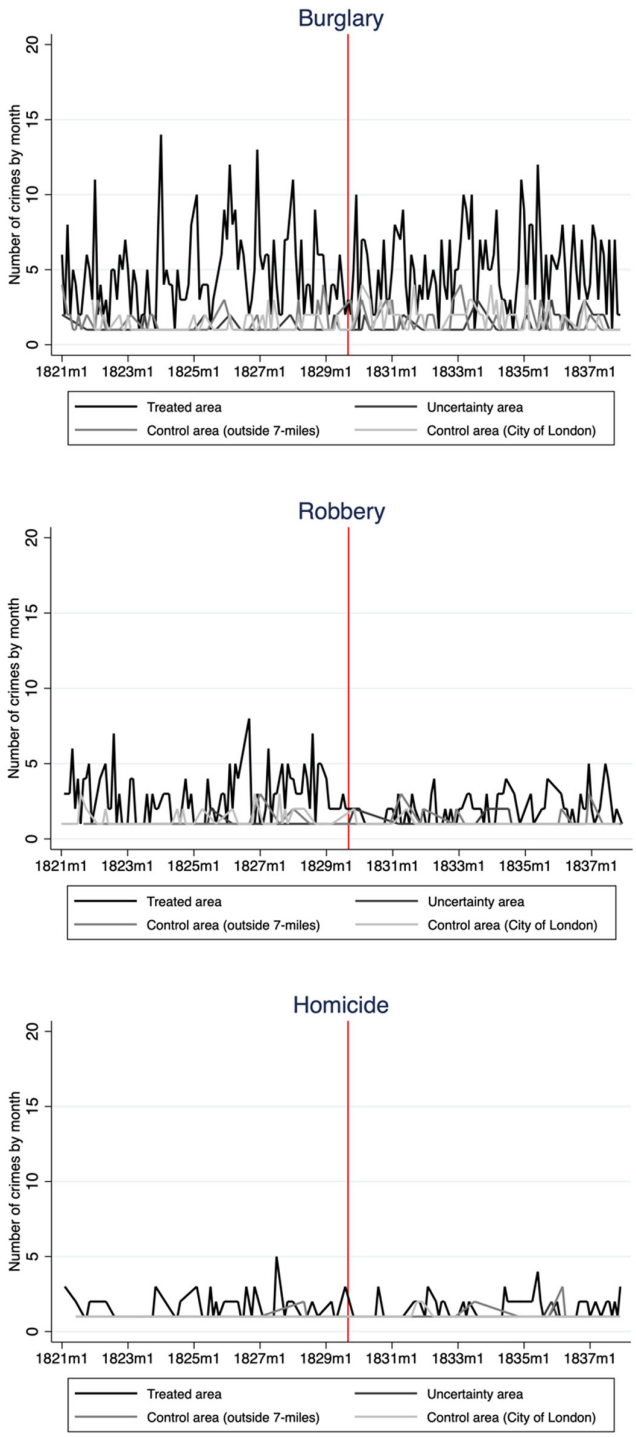
Online Appendix A. Additional Tables and Figures - London Metropolitan Police

Appendix Figure A1. London Murders and Homicides



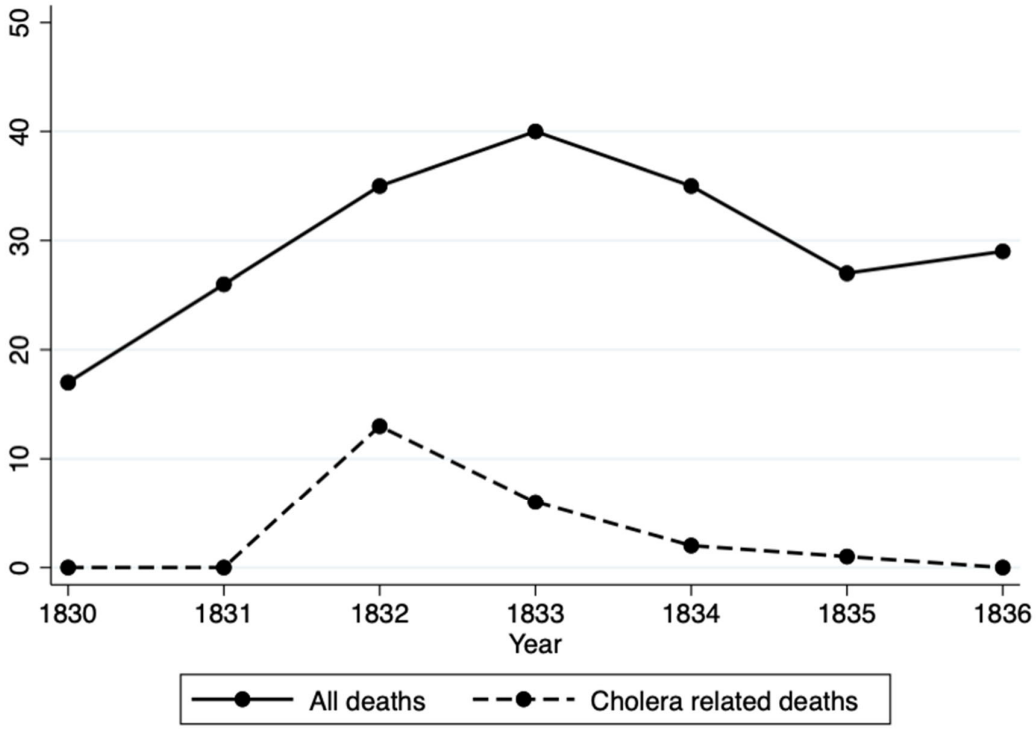
NOTES – The figure compares two measures of homicides in London between 1715 and 1900. The black solid line shows the number of trials for murder at the Old Bailey, retrieved from *The Old Bailey Proceedings Online*. The grey dashed line shows the number of London homicides compiled in "Homicides in New York City, 1797-1999 [And Various Historical Comparison Sites]" by Eric Monkkonen (retrieved on April 25, 2019 from https://cjrc.osu.edu/research/interdisciplinary/hvd/europe/london). The dashed vertical lines mark the years 1821-1837 (our sample period).

Appendix Figure A2. Time Series of Old Bailey Cases



NOTES – The figures show the number of crimes measured in the Old Bailey data for the longer estimation window (1821-1837). The observational unit is one calendar month by area (treated area within 4 miles of Charing Cross, uncertainly treated area within 4 to 7 miles from Charing Cross, control area outside the 7-mile radius around Charing Cross and control area within the City of London). The solid vertical line indicates the introduction of the Metropolitan Police in September 1829. The figures are based on data from the Old Bailey Proceedings Online and own transcriptions/calculations.

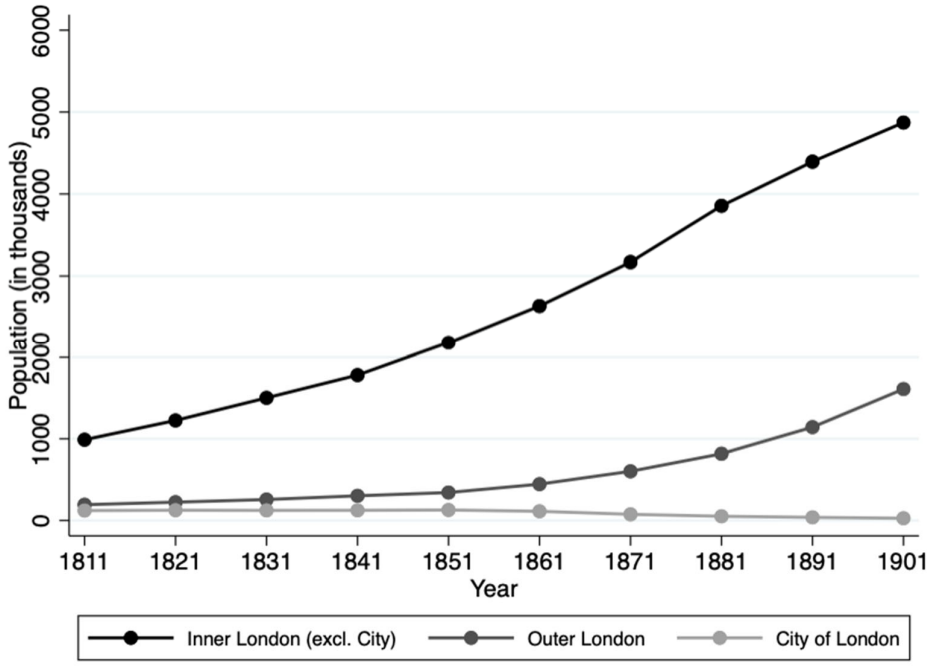
Appendix Figure A3. Deaths of Metropolitan Police Officers



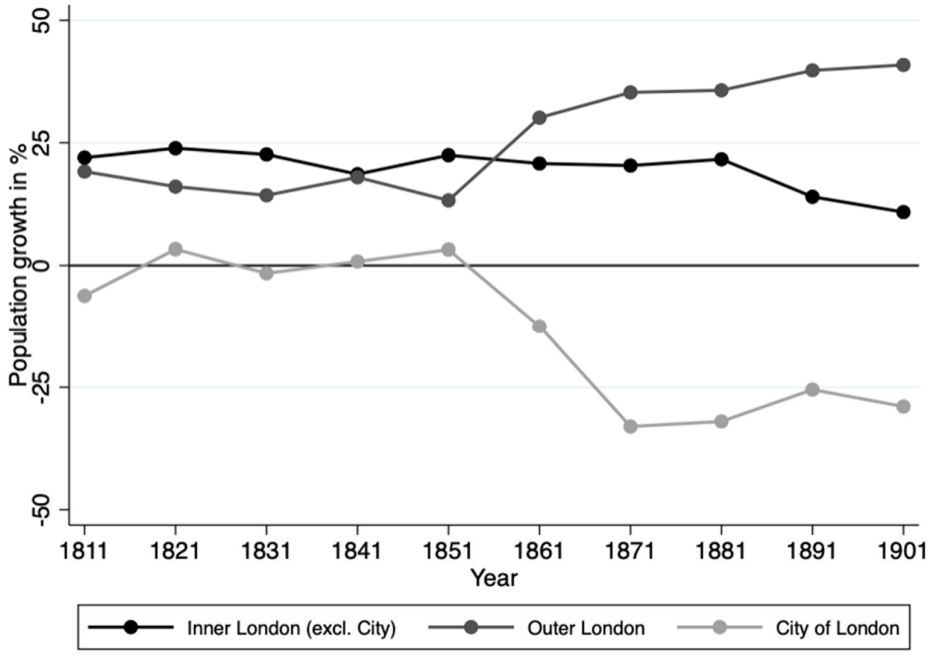
NOTES –The figure shows the number of deaths of Metropolitan Police Officers between 1830 and 1836, overall (solid black line) and due to Cholera (dashed black line). The figures as based on manually transcribed data from the *Returns to Death 1829-1889* sourced from the National Archives (MEPO 4/2).

Appendix Figure A4. Historical Population Estimates by Region of London

Panel A. Population Levels



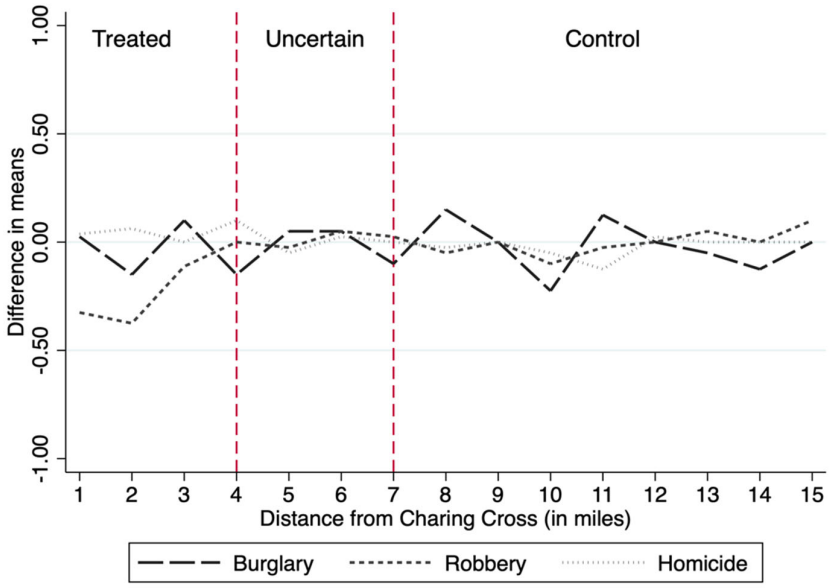
Panel B. Population Growth



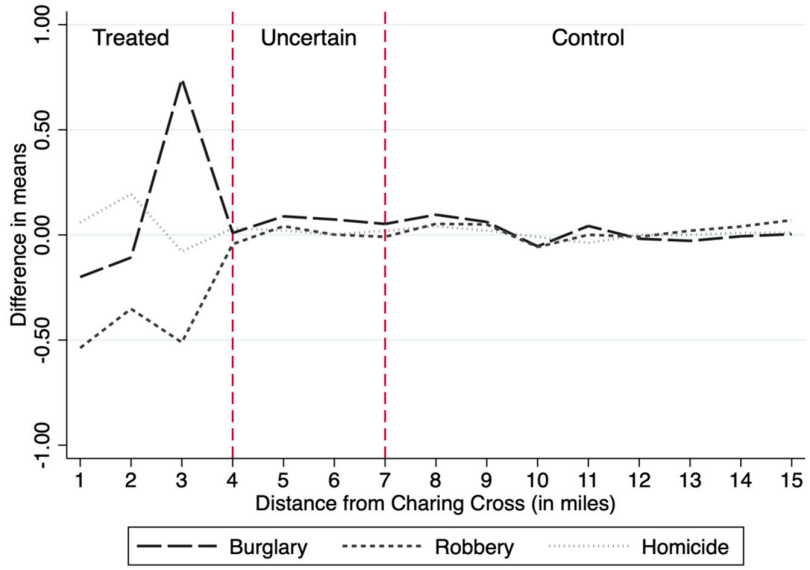
NOTES – Panel A shows the historical population estimate (in thousands) for the different regions of London (black line: inner London excluding the City of London; dark grey line: outer London; light grey line: City of London) from 1811 to 1901. Panel B shows the resulting population growth rate in percent. The data were retrieved from the Historical Census Population provided by the Office for National Statistics.

Appendix Figure A5. Differences in Means by Distance Band (Old Bailey)

Panel A. 1828-1832



Panel B. 1821-1837



NOTES – The figures plot the pre-post Met differences in means of the number of trials per month and each 1-mile distance band for burglary (long dashed line), robbery (short dashed line) and homicide (dotted line). The difference in means is separately computed for each 1-mile distance band around Charing Cross. Panel A shows the results for the 1828 to 1832 sample period and Panel B for the 1821 to 1837 sample period, respectively. The dashed vertical lines mark the thresholds in terms of distance from Charing Cross for the treated, uncertain and control area, respectively. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

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

	(1)	(2)	(3)	(4)	(5)
Met. Police Divison	All hires: 21 Sep 1829 - 27 Mar 1830	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
	all	service length >= 250 days			Early/late
A	125	60	49	11	early
B	261	136	111	25	early
C	300	146	132	14	early
D	280	150	126	24	early
E	263	155	139	16	early
F	289	137	118	19	early
G	226	156	24	132	late
H	221	125	18	107	late
K	210	164	19	145	late
L	202	154	20	134	late
M	207	146	16	130	late
N	95	51	0	51	late
P	231	140	12	128	late
R	39	30	0	30	late
S	260	158	40	118	late
T	9	6	0	6	late
V	9	6	0	6	late

NOTES – This table shows the number of hires by the Metropolitan police separately for each of its police divisions (A-V). 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). Columns (1) and (2) look at all hires from the creation of the Met until March 27, 1830; column (2) conditions on those officers who last at least 250 days. Columns (3) and (4) decompose this sample of hires into the two hiring waves. Specifically, 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, i.e whether the division is an early or late hiring division.

Appendix Table A2. Estimated Salary Costs of the Metropolitan Police

Position	Pay Rate (in Peel letter)	Initial Force (October 1829) Force Size	Initial Force (October 1829) Salary Costs	Expanded Force (May 1830) Force Size	Expanded Force (May 1830) Salary Costs
Police force					
Superintendents	annual 200 pounds	8	1,600	19	3,800
Inspectors	annual 100 pounds	20	2,000	42	4,200
Sergeants	daily 3 shillings 6 pence (est. annual 55 pounds)	88	4,805	220	12,012
Constables	daily 3 shillings (est. annual 47 pounds)	895	41,886	2919	136,609
Administration					
Commissioners clerks					
Clerk 1	annual 200 pounds	1	200	1	200
Clerk 2	annual 150 pounds	1	150	1	150
Clerk 3	annual 90 pounds	1	90	1	90
Receivers clerks					
Chief clerk	annual 200 pounds	1	200	1	200
Clerk 2	annual 150 pounds	1	150	1	150
Total annual admin cost (1829 pounds)			790		790
Total police force cost (1829 pounds)			50,291		156,621
Total annual cost (1829 pounds)			790		790
Total police force cost (2018 pounds)			5,421,348		16,883,765

NOTES – The table shows estimates of the salary costs of the Metropolitan Police. The initial force size (in October 1829) and the pay are obtained from the text of a letter written by Sir Robert Peel in July of 1829; see: http://www.open.ac.uk/Arts/history-from-police-archives/Met6Kt/MetHistory/mhDocsTx4.html. For sergeants and constables, only the daily pay is listed in the letter. Annual pay is determined using an assumption of a 6-day work week for all 52 weeks of the year. Note that there are 12 pence in a shilling and 20 shillings in a pound. Estimates of the police forces by May 1830 are based on our own assumptions that for the 11 new districts, there are: 1 superintendent, 2 inspectors, and 12 sergeants per district. We know from historical records that the total force in May of 1830 is about 3,200; we determine the number of constables by subtracting superintendents, inspectors, and sergeants from the total. Finally, we translate the cost to 2018 pounds by assuming that a pound in 1829 is worth 107.8 times that in 2018; this value is based on the Bank of England inflation calculator.

Appendix Table A3. Descriptive Statistics – Old Bailey Proceedings Data

	within 7-miles radius			City of London			outside 7-miles radius		
Variable	1821-1829	1829-1837	1828-1832	1821-1829	1829-1837	1828-1832	1821-1829	1829-1837	1828-1832
Crime									
Number of crime incidents	915	806	458	153	161	100	102	128	89
Burglary	540	544	294	82	114	61	71	78	64
Manslaughter	35	73	30	8	16	11	3	7	5
Murder	59	30	20	4	6	4	9	8	3
Robbery	281	159	114	59	25	24	19	35	17
Distance to Charing Cross (miles)	1.935	2.208	2.058	1.659	1.637	1.624	10.371	10.244	10.631
Distance to Charing Cross (in km)	3.114	3.553	3.311	2.670	2.635	2.612	16.687	16.483	17.104
Number of co-defendants	1.414	1.297	1.376	1.320	1.261	1.260	1.578	1.359	1.562
Days crime to session start	33.547	29.211	34.590	67.283	30.416	33.545	60.657	79.570	49.584
Police									
Any police witness (1/0)	0.830	0.839	0.852	0.869	0.783	0.870	0.794	0.805	0.831
No. of police first 5 witnesses who are:	1.403	1.371	1.504	1.490	1.385	1.450	1.186	1.281	1.337
Constable	0.309	0.315	0.293	0.301	0.261	0.260	0.559	0.633	0.809
Policeman	0.001	0.733	0.395	0.000	0.342	0.180	0.000	0.172	0.079
Watchman	0.363	0.032	0.242	0.386	0.230	0.250	0.108	0.102	0.079
Other (pre-Met type)	0.573	0.145	0.410	0.706	0.342	0.600	0.373	0.281	0.258
Other (post-Met type)	0.023	0.127	0.116	0.039	0.124	0.090	0.000	0.070	0.056
Missing	3.715	3.644	3.517	3.562	3.696	3.620	3.951	3.734	3.708
Police at crime scene (1/0)	0.198	0.258	0.286	0.294	0.230	0.310	0.088	0.078	0.124

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 trial. Except for the number of crime trials, the table reports means for each respective sample. The sample restrictions for each column are indicated at the top of the column.

Appendix Table A4. Sensitivity Analysis for Difference-in-Differences Estimation (Old Bailey Data)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832
Specification:	Area specific trend		Exclude "long streets" locations		Include only "no issue" locations		Exclude missing crime dates	
Panel A. Burglary								
Post Met x Treatment Area	-0.913	0.257	-0.371	-0.462	-0.003	-0.831*	-0.106	-0.294
	(0.716)	(1.156)	(0.339)	(0.630)	(0.262)	(0.453)	(0.370)	(0.660)
Post Met x Uncertainty Area	-0.155	-0.132	0.028	0.088	-0.041	-0.156	0.016	0.006
	(0.281)	(0.418)	(0.144)	(0.274)	(0.118)	(0.213)	(0.149)	(0.288)
Panel B. Robbery								
Post Met x Treatment Area	-1.408***	-1.449***	-0.803***	-1.222***	-0.563***	-0.521*	-1.032***	-1.297***
	(0.410)	(0.476)	(0.190)	(0.385)	(0.161)	(0.304)	(0.219)	(0.428)
Post Met x Uncertainty Area	0.304*	0.565*	0.096	0.128	-0.016	0.129	0.129	0.228
	(0.177)	(0.300)	(0.081)	(0.143)	(0.069)	(0.114)	(0.088)	(0.162)
Panel C. Homicide								
Post Met x Treatment Area	-0.320	-0.219	-0.033	-0.003	0.115	0.192	-0.030	0.132
	(0.271)	(0.373)	(0.130)	(0.227)	(0.102)	(0.181)	(0.138)	(0.254)
Post Met x Uncertainty Area	-0.107	0.017	-0.031	-0.053	-0.059	-0.058	-0.050	-0.068
	(0.103)	(0.157)	(0.056)	(0.113)	(0.046)	(0.100)	(0.057)	(0.114)
Observations	816	240	816	240	816	240	816	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) and (2) 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 A5. Alternative Standard Errors (Old Bailey Data)

Sample:	(1) 1821-1837	(2) 1828-1832	(3) 1828-1832	(4) 1828-1832	(5) 1828-1832
Specification:	Intensive margin: By month/area				
City of London:	Control	Control	Treated from 02 April 1832	Uncertain	Excluded
	OLS				
Panel A. Burglary					
Post Met x Treatment Area	-0.106	-0.294	-0.285***	-0.234	-0.120
	0.496	*0.108*	*0.002*	*0.254*	*0.748*
Post Met x Uncertainty Area	0.016	0.006	-0.013	0.116	0.180
	0.880	*0.890*	*0.850*	*0.742*	*0.536*
Panel B. Robbery					
Post Met x Treatment Area	-1.032	-1.297***	-0.832	-1.281***	-1.336***
	0.504	*0.002*	*0.244*	*0.002*	*0.002*
Post Met x Uncertainty Area	0.129	0.228	0.288	0.144	0.189
	0.494	*0.154*	*0.308*	*0.762*	*0.762*
Panel C. Homicide					
Post Met x Treatment Area	-0.008	0.120	0.072	0.188	0.222
	0.746	*0.455*	*0.492*	*0.387*	*1.000*
Post Met x Uncertainty Area	-0.049	-0.055	-0.062	0.100	0.047
	0.496	*0.508*	*0.680*	*0.491*	*1.000*
Observations	816	240	240	240	180
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Week fixed effects	No	No	No	No	No
Area fixed effects	Yes	Yes	Yes	Yes	Yes

NOTES – For details regarding the specification, see the notes of Table 3. This table shows the estimation results with alternative standard errors, here clustered by area (treated, uncertain, control, city). Clustered standard errors are based on a Wild Cluster Bootstrap (1,000 repetitions); p-values are shown in italics below the coefficient.

Appendix Table A6. Robustness Checks for Pre-Post Estimation (Daily Crime Reports)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample:	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1830	1828-1832	1828-1830	1828-1830
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	All weeks	Complete weeks	Log outcome
Panel A. Number of all incidents per day/station												
Post Met Police	0.173	0.291*	0.349**	0.365**	0.585***	0.203	0.423**	0.385**	2.667**	5.090***	0.673	0.126***
	(0.164)	(0.165)	(0.167)	(0.168)	(0.162)	(0.169)	(0.165)	(0.159)	(1.190)	(1.090)	(1.411)	(0.044)
Panel B. Any informations												
Post Met Police	-0.177***	-0.151***	-0.141***	-0.125***	-0.151***	-0.148***	-0.122***	-0.168***	-0.139***	-0.220***	-0.125***	-
	(0.024)	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)	(0.043)	(0.034)	(0.047)	
Panel C. Any 'stolen property'												
Post Met Police	-0.089***	-0.099***	-0.116***	-0.105***	-0.099***	-0.090***	-0.093***	-0.102***	-0.514***	-0.734***	-0.712***	-
	(0.024)	(0.025)	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.140)	(0.119)	(0.165)	
Panel D. Number of charges												
Post Met Police	0.719***	0.865***	0.887***	0.906***	1.141***	0.762***	0.879***	0.957***	5.569***	8.625***	3.981***	0.284***
	(0.150)	(0.148)	(0.152)	(0.153)	(0.147)	(0.152)	(0.150)	(0.142)	(1.108)	(1.003)	(1.333)	(0.045)
Observations	1,414	1,414	1,414	1,414	1,414	1,414	1,414	1,414	288	576	240	576
Office FE	Yes	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	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 (13) present the results when the data are 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 A7. Alternative Standard Errors (Daily Crime Reports)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	1828-1830	1828-1830	1828-1832	1828-1830	1828-1830	1828-1830
Crime type:	total	total	total	property	violent	other
	OLS	OLS	OLS	OLS	OLS	OLS
Panel A. Number of all incidents per day/station						
Post Met Police	0.340	0.347	0.821	0.406	-0.115	0.056
SE1	*0.321*	*0.321*	*0.173*	*0.231*	*0.065**	*0.768*
SE2	*0.344*	*0.334*	*0.194*	*0.266*	*0.086**	*0.780*
Panel B. Any informations per day/station						
Post Met Police	-0.149	-0.148	-0.206	-0.090	-0.049	-0.055
SE1	*0.015***	*0.017***	*0.003****	*0.125*	*0.038***	*0.031***
SE2	*0.018***	*0.018***	*0.002****	*0.122*	*0.046***	*0.034***
Panel C. Number of informations per day/station						
Post Met Police	-0.302	-0.301	-0.371	-0.170	-0.064	-0.065
SE1	*0.027***	*0.030***	*0.018***	*0.130*	*0.056**	*0.031***
SE2	*0.034***	*0.034***	*0.030***	*0.126*	*0.016***	*0.050***
Panel D. Any 'stolen property' per day/station						
Post Met Police	-0.098	-0.099	-0.137	-	-	-
SE1	*0.003****	*0.003****	*0.001****			
SE2	*0.002****	*0.002****	*0.002****			
Panel E. Number of charges per day/station						
Post Met Police	0.879	0.890	1471	0.827	-0.050	0.126
SE1	*0.026***	*0.027***	*0.024***	*0.034***	*0.199*	*0.481*
SE2	*0.000****	*0.000****	*0.020***	*0.030***	*0.186*	*0.492*
Observations	1,616	1,616	3,232	1,616	1,616	1,616
Office FE	No	Yes	Yes	Yes	Yes	Yes
Calendar week FE	No	Yes	Yes	Yes	Yes	Yes
Day of week FE	No	Yes	Yes	Yes	Yes	Yes

NOTES – For details regarding the specification, see notes of Table 5. P-values when using clustered standard errors with conventional clustering (SE1) and with a Wild Cluster Bootstrap procedure with 1,000 repetitions (SE2) are shown in italics below the coefficient. Standard errors are clustered by police office (8 offices, see Sections 2 and 4.1 for more detail).

Online Appendix B. Additional Tables and Figures – County Police Forces

This appendix presents the results of additional robustness and specification checks related to the effect of the staggered introduction of county police forces on crime. We list each figure and table heading here, with a brief description (if necessary) of what it contains and/or finds. Additional details are contained in the notes to the tables and figures.

Appendix Figure B1. Charges Brought to Trial as a Crime Proxy. Before 1857, there is only one measure of crime (the number of persons brought to trial) available in the *Judicial Statistics*. Therefore, we must use the number of persons brought to trial as our crime proxy. After 1857, there are two additional measures (crimes committed and individuals apprehended) which we can use to cross-validate our measure. This figure demonstrates that the three variables indeed move together.

Appendix Figure B2. Event-Study – Sufficiently/Insufficiently Large County Forces. These figures present the results of estimating equation (5) in the paper, expanded such that the sufficiently large (ratio<1500) and insufficiently large (ratio>1500) force dummies are interacted with one-year lead and lag intervals (similar to the event-study design in the London analysis, see the equation in the notes of Figure 4). Panels A-D show the results by crime type (separate regressions), and the left and right figures show the results for sufficiently and insufficiently large forces, respectively (from the same regression). This event-study approach assesses the plausibility of the parallel trends and random timing assumptions and provides information on the dynamic effects of the roll-out.

Appendix Table B1. Dates of County Police Force Formation and Initial Force Size. This presents the raw data gathered from the Police History Society (Stallion and Wall, 1999).

Appendix Table B2. Offence Categories in the Judicial Statistics. This table lists each offense in the six reported offense classes, and documents how we aggregate them into the categories used in our analysis.

Appendix Table B3. Summary Statistics for County-Level Analysis. This table shows summary statistics for all analysis sample counties (N=48) and those characterized as early (1839 or 1840), mid (in-between years), and late reformers (1857). The average number of charges per year (for all counties over the period) is 367, which corresponds to 1.79 charges per 1000 population (1.3 property, 0.3 violent, and 0.06 other). Early reformers were on average largest in terms of population and acreage, while the mid-reformers were smallest. In addition, the earliest reformers did *not* have the highest crime rate (based on the whole time period).

Appendix Table B4. Determinants of the Timing of County Police Force Formation. This table looks at the determinants of being an early reformer (force created by 1840) and the year of adoption for all counties. For the former, we use an 1840 cross-section of the 48 counties and as a dependent variable a dummy equal to one if the county is an early reformer. For the latter, the dependent variable equals one in the year a county creates a force; counties exit the sample once a force is created, as there is no longer a choice to be made. 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. We consider the role of: the county's own lagged charge rate, whether the neighbors had forces (and their relative sizes), population, the lagged charge rate in neighboring counties, and the number of initial officers in the county area not part of the county force (i.e. borough police that existed prior to the county force). We find that little predicts the timing of adoption. The most consistently significant variable is population: larger counties

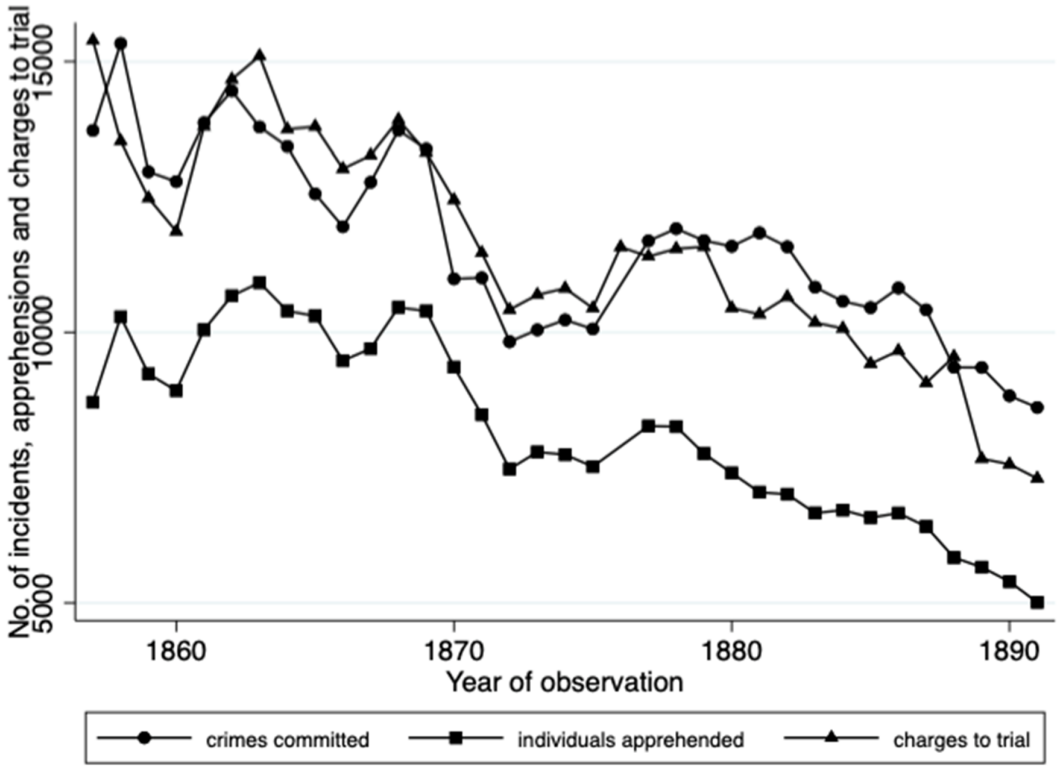
were more likely to adopt early, but (i) the estimates are small, (ii) county fixed effects control for larger versus smaller counties, and (iii) population does not predict timing for later adopters.

Appendix Table B5. Crime Displacement from London to Neighboring Counties. This table assesses whether police forces were created as a result of crime displacement from neighboring counties that had previously created a police force. We first look at whether the 1829 introduction of the London Met increased crime in the surrounding counties, and triggered reform adoption. Panel A reports pre-post comparisons in mean log charges around the 1829 introduction of the Met, both for direct neighbors of London/Middlesex and indirect neighbors ('second' neighbors/neighbors of neighbors of London and Middlesex). Panel B estimates a regression of log charges on indicators for Middlesex direct and second neighbors interacted with a post-1829 dummy (other counties being the reference group). No clear pattern emerges suggesting systematic crime displacement.

Appendix Table B6. Determinants of Initial Force Size to Population Ratio. This table presents (descriptive) regressions of the correlates of initial type of force (sufficiently large or not) and size of force. It considers fixed geography, 1851 census characteristics, crime rates and neighboring counties' forces. The regressions suggest that few observables predict the initial force size. However, there is clearly limited power with a cross-section of 48 counties; moreover, force formation could have occurred prior to the measurement of some of these variables (e.g. census).

Appendix Table B7. Sensitivity Analysis for Sufficiently and Insufficiently Large County Police Force Results. This table documents the results from a number of sensitivity tests. It shows that the effect sufficiently large forces is robust to (i) controlling for county population, England and inspection region dummies, and inspector specific and large county (above median acreage) specific time trends, (ii) using a smaller sample window, (iii) looking separately at 1832 to 1849 (early reformers) and 1850 to 1865 (late reformers), and (iv) excluding Wales. It also shows that the results are robust to controlling for whether a neighbor county in year t had a sufficient or insufficient force.

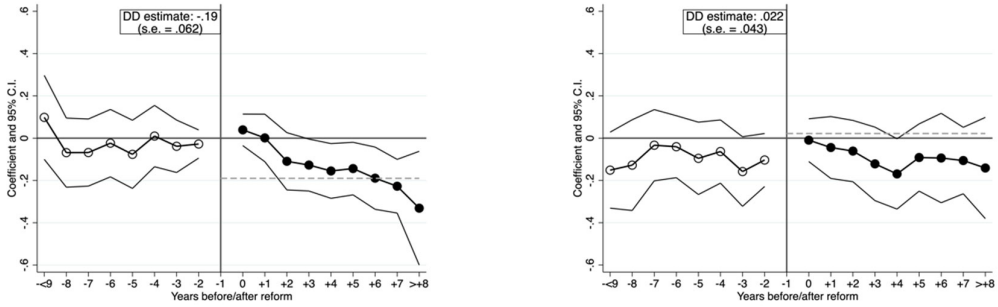
Appendix Figure B1. Charges Brought to Trial as a Crime Proxy



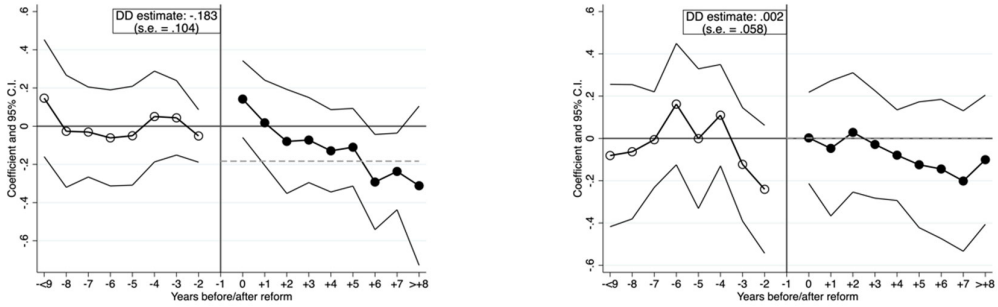
NOTES – The figure presents the national annual number of crimes committed, charges brought to trial, and individuals apprehended in all England and Wales counties, excluding Middlesex, York, Suffolk, and Sussex from 1857 to 1891. The number of charges to trial is the main outcome variable used in the county-level analysis, as it is the only measure available prior to 1857. The underlying data were digitized by the authors from the annual *Judicial Statistics* reports.

Appendix Figure B2. Event-Study - Sufficiently/Insufficiently Large County Forces

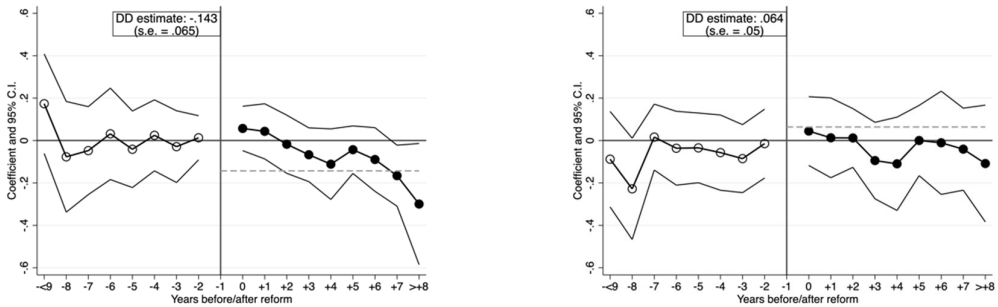
Panel A: All Charges



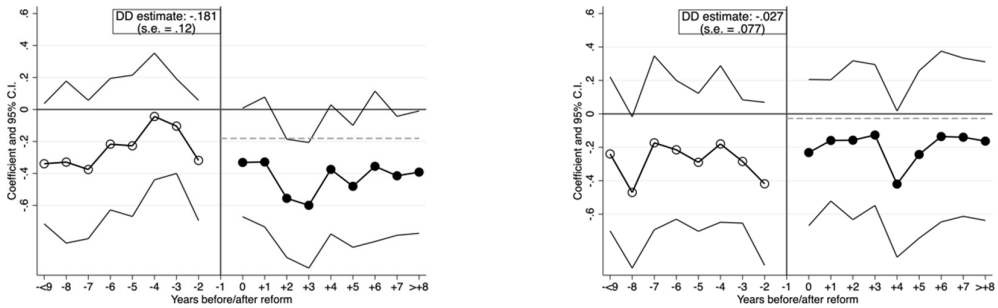
Panel A: Violent Charges



Panel B: Property Charges



Panel C: Other Charges



NOTES – The figures plot the estimated coefficients and 95% confidence intervals resulting from event-study regressions based on equation (5): specifically, we expand equation (5) to include interactions of sufficient and insufficient county force creation with dummies for each year to and from county police force formation. The left figures show the results for the estimated coefficients for sufficiently large forces (ratio < 1500) and the right figures for insufficiently large forces (ratio > 1500), respectively. Both sets of coefficients are obtained from the same specification. All years eight or more years after police force formation and nine or more years before police force formation are combined in the tails of the figures. The omitted category is the year before the police force is created (marked by the vertical line), where the first year (0) is defined as the first full fiscal year following the creation of a police force. The dashed horizontal line represents the (average) difference-in-difference estimate as reported in Table 7.

Appendix Table B1. Dates of County Police Force Formation and Initial Force Size

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-jurisdictions.					
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.

Appendix Table B2. 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 Property	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
Other	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.

Appendix Table B3. 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	1440	12	18	480	18	24	270	13	19	690	9	11
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)	1440	0.06	0.09	480	0.06	0.04	270	0.11	0.18	690	0.05	0.05
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 suff.large (<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	1,700	632	16	1,554	377	9	1,850	1215	23	1,742	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.

Appendix Table B4. Determinants of the Timing of County Police Force Formation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	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	-0.003	-0.004			
	[0.032]	[0.016]	[0.022]				[0.004]	[0.005]	[0.005]	[0.005]			
Lag1: Any neighboring force			-0.106	0.002	-0.005	-0.010			-0.070*				
			[0.197]	[0.204]	[0.207]	[0.230]			[0.040]				
Lag1: Any sufficiently large neighboring force										-0.003	-0.006	-0.005	-0.005
										[0.025]	[0.028]	[0.029]	[0.030]
Lag1: Any insufficiently large neighboring force										-0.067*	-0.094**	-0.082	-0.079
										[0.036]	[0.046]	[0.050]	[0.051]
Lag1: Violent crime charge rate				-0.862	-0.884	-0.747					0.108	0.091	0.120
				[1.020]	[1.024]	[1.234]					[0.127]	[0.127]	[0.139]
Lag1: Property crime charge rate				0.260	0.260	0.219					-0.038	-0.030	-0.042
				[0.155]	[0.156]	[0.179]					[0.028]	[0.027]	[0.030]
Lag1: Other crime charge rate				-4.605**	-4.650**	-4.007*					0.324	0.313	0.446*
				[2.052]	[2.073]	[2.240]					[0.242]	[0.236]	[0.246]
Population (in 10,000)		0.017***	0.017***	0.011*	0.011*	0.011		0.002	0.001	0.001	0.001	0.002	0.002
		[0.005]	[0.005]	[0.006]	[0.006]	[0.009]		[0.001]	[0.001]	[0.001]	[0.002]	[0.002]	[0.002]
Lag1: Average charge rate, all neighbors					0.021	0.036						-0.020*	-0.016
					[0.059]	[0.069]						[0.012]	[0.012]
Initial pre-county police						0.000							0.000
						[0.001]							[0.000]
Observations	48	48	48	48	48	41	511	511	511	511	454	454	392
R-squared	0.002	0.151	0.155	0.240	0.243	0.244	0.001	0.005	0.015	0.016	0.027	0.032	0.037

NOTES – The table shows regression results testing for determinants of the timing of county police force formation. The outcome variable in columns (1) to (6) 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 (7) to (13) 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 (7) to (13)) are shown in brackets below the estimated coefficient. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table B5. Crime Displacement from London to Neighboring Counties?

Panel A. Pre-post comparison of means (1828-1832)				
	Log charges before 1829	Log charges after 1829	△	
Middlesex direct neighbors	5.70	5.85	0.15	
Berks	5.30	5.36	0.06	
Bucks	5.13	5.33	0.20	
Essex	6.13	6.38	0.24	
Hertford	5.38	5.52	0.14	
Surrey	6.55	6.67	0.12	
Middlesex second neighbors	5.56	5.73	0.17	
Bedford	4.79	4.71	-0.08	
Cambridge	5.17	5.13	-0.04	
Gloucester	6.38	6.69	0.30	**
Kent	6.45	6.53	0.08	
Northampton	5.01	5.14	0.13	
Oxford	5.03	5.41	0.38	*
Southampton	5.93	6.18	0.25	*
Wilts	5.74	6.07	0.33	
Remaining counties	4.38	4.57	0.20	

Panel B. Regressions (1828-1832)		
Y:	Log charges (total)	Log charges (total)
Middlesex direct neighbour	-0.043	-0.043
x Post 1829	(0.081)	(0.060)
Middlesex second neighbour	-0.025	-0.025
x Post 1829	(0.064)	(0.083)
Standard errors	robust	clustered (county)
County fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	240	240

NOTES - Panel A shows pre-post comparisons of log (total) charges before and after the introduction of the London Metropolitan Police in 1829 as well as the difference, respectively. This is shown for both direct neighbors of London/Middlesex and indirect neighbors ('second' neighbors of London and Middlesex). Direct neighbors are counties that border London and Middlesex, indirect neighbors are counties that border these direct neighbor counties. Statistical significance is based on simple pre-post regressions (with limited numbers of observations; N=5 for single counties). No clear pattern emerges that would suggest a systematic displacement of crime: There are no systematic increases in crime in particular in directly neighboring counties. Moreover, when we estimate a regression of log charges on indicators for Middlesex direct and second neighbors interacted with a post-1829 dummy (other counties being the reference group), we do not find significant increases in crime. These regressions (shown in Panel B) are essentially pre-post regressions of log charges on a dummy *Post 1829* interacted with the type of county (direct neighbor, indirect neighbor, other = reference group). London/Middlesex itself is excluded from the regressions; the regressions include county and year fixed effects. Robust/clustered (by county) standard errors are shown in parentheses below the coefficient. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table B6. Determinants of Initial Force Size to Population Ratio

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable (measured at force creation):					
	Suff. Large: Ppl/Pol < 1500?	Ppl/Pol	Suff. Large: Ppl/Pol < 1500?	Ppl/Pol	Suff. Large: Ppl/Pol < 1500?	Ppl/Pol
Fixed Geographic Variables						
Acres (in 10,000)	0.001	-27.099	-0.001	-31.207	-0.004	-34.354
	[0.004]	[18.663]	[0.006]	[23.103]	[0.005]	[30.762]
Number of parishes (in 100)	0.010	49.912	0.051	25.820	-0.003	-19.884
	[0.118]	[384.362]	[0.147]	[470.753]	[0.118]	[481.501]
Number of neighbors	0.029	-35.477	0.032	9.968	0.043	19.787
	[0.048]	[239.006]	[0.046]	[223.389]	[0.040]	[224.012]
England	-0.153	888.542	0.035	1,310.160	0.161	1,417.821
	[0.179]	[1,435.166]	[0.319]	[2,266.308]	[0.332]	[2,107.863]
Variables measured in the 1851 census (shares)						
Farmer	-0.017	-8.729	-0.010	-2.527	-0.017	-7.902
	[0.017]	[112.116]	[0.026]	[148.072]	[0.024]	[163.879]
Male	-0.104**	522.620	-0.103*	396.943	-0.066	429.000
	[0.048]	[388.323]	[0.058]	[397.435]	[0.060]	[470.022]
Married	0.050	267.177	0.065	90.110	0.026	57.382
	[0.073]	[427.595]	[0.094]	[478.618]	[0.080]	[534.110]
Native	-0.049	-102.064	-0.122	137.937	-0.001	240.613
	[0.088]	[325.609]	[0.109]	[392.508]	[0.094]	[655.567]
Employed	-0.007	-246.094	0.006	-284.430	-0.003	-292.068
	[0.031]	[176.390]	[0.038]	[194.710]	[0.035]	[219.775]
Age 0-15	-0.131	-536.135	-0.162	-222.318	-0.297*	-337.007
	[0.127]	[589.462]	[0.171]	[767.984]	[0.167]	[1,005.164]
Age 16-25	0.010	1,363.391	-0.061	1,423.109	-0.170	1,330.720
	[0.162]	[1,019.616]	[0.212]	[1,448.194]	[0.217]	[1,226.612]
Age 26-35	-0.219	23.037	-0.245	37.629	-0.137	130.006
	[0.209]	[1,314.718]	[0.272]	[1,269.969]	[0.281]	[1,235.988]
Age 36-45	0.005	-1,844.465	-0.142	-22.887	-0.574	-390.638
	[0.457]	[1,780.253]	[0.600]	[2,128.982]	[0.535]	[2,373.003]
Age 46-55	-0.263	3,415.117**	-0.303	3,489.476*	-0.460	3,355.611*
	[0.326]	[1,645.359]	[0.399]	[1,873.414]	[0.429]	[1,838.108]
Variables measured in the year before force adoption						
Violent crime rate			-0.453	2,916.493	-0.131	3,191.165
			[0.978]	[3,928.376]	[0.890]	[4,152.820]
Property crime rate			-0.071	-161.968	-0.165	-242.323
			[0.192]	[658.132]	[0.195]	[834.754]
Other crime rate			2.823	-11,572.963	3.467	-11,024.540
			[2.640]	[9,164.806]	[2.554]	[9,995.851]
Any neighbors with suff. large force			-0.124	-560.707	-0.105	-544.154
			[0.220]	[1,073.534]	[0.184]	[1,142.446]
Any neighbors with insuff. large force			0.069	-95.433	0.165	-13.718
			[0.215]	[977.603]	[0.207]	[996.988]
Population control						
Population (in 10,000)					0.026***	22.536
					[0.009]	[83.500]
Observations	47	47	45	45	45	45
R-squared	0.349	0.370	0.412	0.447	0.534	0.449

NOTES - The dependent variable measures police force sufficiency at the time of force formation, measured by the number of people per police (ppl/pol). In the odd columns, the dependent variable is a dummy variable if the police force is sufficiently large and in the even columns it is the actual ratio of people per police. 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. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

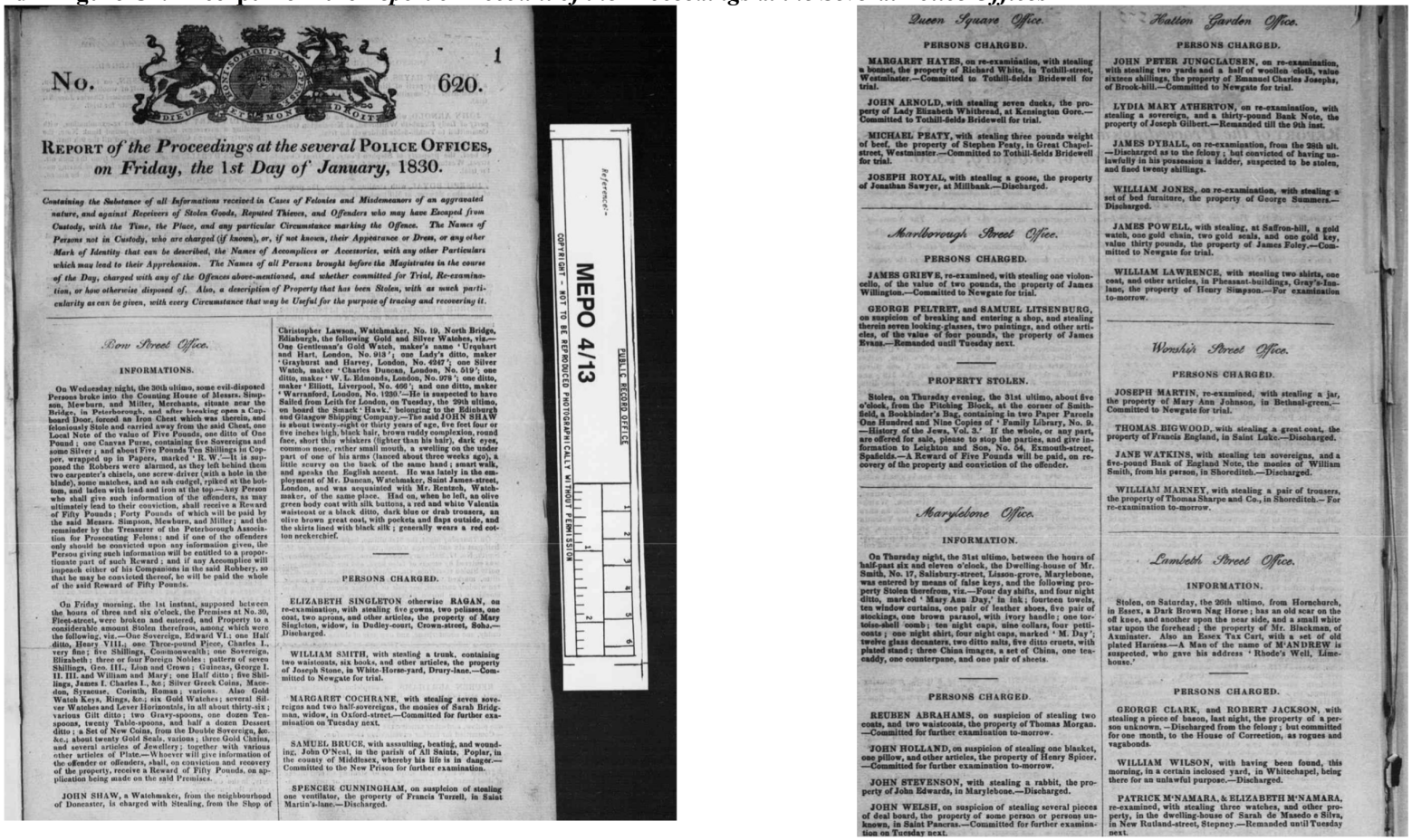
Appendix Table B7. Sensitivity Analysis for Sufficiently and Insufficiently Large County Police Force Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample Years:	1832-1865	1832-1865	1832-1865	1832-1865	1832-1865	1835-1862	1832-1849	1850-1865	1832-1865	1832-1865
Sample Counties	all	all	all	all	all	all	all	all	English	all
N	1,632	1,632	1,632	1,632	1,632	1,344	864	768	1,224	1,632
Panel A. Charges Overall										
Force Sufficiently Large	-0.190***	-0.207***	-0.202***	-0.207***	-0.190***	-0.153**	-0.088**	-0.074	-0.099*	-0.184***
	[0.062]	[0.062]	[0.062]	[0.070]	[0.062]	[0.062]	[0.035]	[0.072]	[0.056]	[0.060]
Force Insufficiently Large	0.022	0.043	0.051	0.034	0.022	0.020	0.008	0.005	-0.024	-0.006
	[0.043]	[0.046]	[0.046]	[0.043]	[0.043]	[0.044]	[0.061]	[0.073]	[0.046]	[0.045]
Panel B. Violent Crime Charges										
Force Sufficiently Large	-0.183*	-0.199**	-0.184**	-0.220**	-0.183*	-0.134	-0.049	-0.094	-0.114	-0.182*
	[0.104]	[0.086]	[0.086]	[0.103]	[0.104]	[0.119]	[0.194]	[0.101]	[0.104]	[0.101]
Force Insufficiently Large	-0.002	0.040	0.055	0.013	-0.002	0.005	0.068	-0.103	-0.058	-0.030
	[0.058]	[0.058]	[0.056]	[0.058]	[0.058]	[0.060]	[0.089]	[0.108]	[0.054]	[0.059]
Panel C. Property Crime Charges										
Force Sufficiently Large	-0.143**	-0.152**	-0.148**	-0.147*	-0.143**	-0.124**	-0.025	-0.026	-0.046	-0.140**
	[0.065]	[0.067]	[0.069]	[0.077]	[0.065]	[0.061]	[0.064]	[0.069]	[0.054]	[0.063]
Force Insufficiently Large	0.064	0.087*	0.095*	0.071	0.064	0.059	0.050	0.038	0.008	0.040
	[0.050]	[0.050]	[0.051]	[0.049]	[0.050]	[0.050]	[0.069]	[0.071]	[0.055]	[0.052]
Panel D. Other Crime Charges										
Force Sufficiently Large	-0.181	-0.183	-0.170	-0.115	-0.181	-0.195	-0.186	-0.197*	-0.188	-0.188
	[0.120]	[0.113]	[0.113]	[0.124]	[0.120]	[0.130]	[0.254]	[0.101]	[0.125]	[0.117]
Force Insufficiently Large	0.027	0.049	0.072	0.046	0.027	0.021	0.117	-0.004	0.046	0.001
	[0.077]	[0.077]	[0.075]	[0.077]	[0.077]	[0.082]	[0.123]	[0.094]	[0.082]	[0.074]
+ population	no	yes	yes	no	no	no	no	no	no	no
+ Eng. and region dummies	no	no	yes	no	no	no	no	no	no	no
+ region specific trend	no	no	no	yes	no	no	no	no	no	no
+ > median acre trend	no	no	no	no	yes	no	no	no	no	no
+ neighbor force suff./insuff. large	no	no	no	no	no	no	no	no	no	yes

NOTES – This table presents sensitivity analyses of the baseline difference-in-differences specification in equation (5) (see Table 7), where the variables of interest Force Sufficiently and Insufficiently Large are equal to one for a county c in any year t after which sufficiently large or insufficiently large force has been created, using a threshold of 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. The dependent variable in all columns is the log number of charges overall (Panel A), for violent crime (Panel B), for property crime (Panel C) and for other crime (Panel D). 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. The number of observations for Panel A are listed at the top of the table for each column; as noted in Table 7, due to missing years of data, there are fewer observations in the crime specific panels. Standard errors are clustered by county and shown in brackets below the estimated coefficients. *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix C. Data Sources

Appendix Figure C1. 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 'Graysbrut and Harvey, London, No. 6347'; 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. 1230.'—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. Rentzsch, 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 skirts 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. 3.' 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 26th 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).