

Working Paper in Economics No. 779

# **The Impact of the First Professional Police Forces on Crime**

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Department of Economics, October 2019

ISSN 1403-2473 (Print)  
ISSN 1403-2465 (Online)



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# The Impact of the First Professional Police Forces on Crime\*

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This version: October 9, 2019

*Abstract:* This paper evaluates how the introduction of professional police forces affected crime using two natural experiments in history: the 1829 formation of the London Metropolitan Police (the first police force ever tasked with *detering* crime) and the 1839 to 1856 county roll-out of forces in England and Wales. The London Met analysis relies on two complementary data sources. The first, trial data with geocoded crime locations, allows for a difference-in-differences estimation that finds a significant and persistent reduction in robbery but not homicide or burglary. A pre-post analysis of the second source, daily police reports of both cleared and uncleared crime incidents, finds a significant reduction in all violent crimes but offsetting changes in uncleared (decrease) and cleared (increase) property crimes. These (local) reductions in crime are not just due to crime displacement but represent true decreases in overall crime. Difference-in-difference analyses of the county roll-out find that only sufficiently large forces, measured by the population to force ratio, significantly reduced crime. The results are robust to controlling for spill-over effects of neighboring forces.

*JEL Codes:* K42, N93, H00

*Keywords:* police, crime, deterrence, economic history, institutions

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\* This paper would not have been possible without the tremendous effort of our research assistants Srinidhi Srinivasan and Vu Tran, and the financial support of Vetenskapsrådet, The Swedish Research Council, Grants for Distinguished Young Researchers. We thank seminar and workshop participants at the University of Gothenburg, 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](mailto:randi.hjalmarsson@economics.gu.se) (corresponding). Anna Bindler, Department of Economics, University of Gothenburg, Email: [anna.bindler@economics.gu.se](mailto:anna.bindler@economics.gu.se).

## 1. Introduction

An extensive empirical literature testing one of the core components of Becker's (1968) economic model of crime – the role played by the probability of apprehension – has resulted in a general “consensus that increases in police manpower reduce crime” (Chalfin and McCrary, 2017).<sup>1</sup> Most of this existing literature is based on (often temporary) expansions to the size of an existing police force, thereby estimating the marginal effect of an additional officer to an already established force. Yet, much is still unknown about the more fundamental relationship between police and crime. Specifically, how does the effectiveness of police in combatting crime depend on (i) the tasks assigned to an officer, (ii) police force characteristics, including size and/or age, and (iii) police (force) quality? To begin to address these questions, we study the introduction of professional forces on crime, which profoundly changed the nature of policing, by tasking officers - for the first time ever - with *detering* crime.

Specifically, our paper uses two natural experiments in history: the formation of the first professional force in the world – the London Metropolitan Police (the ‘Met’) in October 1829 – and the roll-out of rural county forces in England and Wales over the next 30 years. Cities across the United States and around the world modelled their own police departments on the Met and, most prominently, adopted its innovative emphasis on crime prevention or deterrence.<sup>2</sup> These newly created institutions, which still exist today, are a fundamental component of the contemporary criminal justice system.

In contrast to the existing police-crime literature, we study a large shock to the institution of ‘policing’ – the *introduction* of a large, professional and institutionalized police force with the explicit aim of crime prevention. Specifically, there were three distinguishing features of these new professional police forces. First, they represented a substantial increase in numbers: in London, 1000 men were hired over-night (an approximately ten-fold increase over the pre-existing informal ‘police’), and the force expanded to 3000 men soon after. Second, there was a shift from reactionary policing focused on catching criminals for financial reward towards prevention and deterrence by slowly walking a small beat and being visibly present (Emsley, 2009). Third, there was a new-found emphasis on police quality. Many features of modern-day

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<sup>1</sup> Empirical evidence of the crime reducing effect of police was elusive due to both simultaneity bias – more police are hired in higher crime locations or times – and measurement error in the number of police (Chalfin and McCrary, 2018). Levitt (1997) was amongst the first to try to causally identify a crime-reducing effect of police with natural experiments that locally or temporarily increased police numbers. See Chalfin and McCrary (2017) for a recent review and Cameron (1988) for a review of the early literature that did not account for this simultaneity bias.

<sup>2</sup> 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).

policing were thus *introduced* for the first time during this period. A second contrast with much of the existing literature is that our natural experiments consist of two *permanent* shocks to policing.<sup>3</sup> Finally, exploiting variation in the officer to population ratio across counties at the time of force formation, we can begin addressing the question of whether the (marginal) effect of police on crime depends on force characteristics.

The historical context of our study allows for contributions to two additional literatures. First, we enhance explanations of 19<sup>th</sup> century crime trends in England and Wales: Crime rose in the first half of the century but was followed by a decline in the latter half despite the growing population – an ‘English miracle’ (Taylor, 1998). Did the formation of professional police forces contribute to this pattern?<sup>4</sup> Second, we contribute to the literature studying the extent to which institutions, like mass education and public spending, explain long-run economic growth, the development of human and social capital, and state capacity – a state’s ability to implement its intended policies.<sup>5</sup> While this literature mostly takes a macro-perspective, our paper studies the micro-foundations of these questions for one such institution – police. Given the potentially important role of the level of societal crime for explaining economic growth and development (e.g. Mauro, 1995) and the state’s ability to govern, understanding the extent to which crime was affected by this new institution is an important contribution.

Identifying the effect of the new police on crime is not a simple matter. One potential confounder is an increase in reporting of crimes to the police (even if there was no change in criminal behavior). This would only have happened if there was increased societal trust in ‘police’. Yet, anecdotal evidence suggests that, at least initially, there were anti-police sentiments: even a magistrate stated that “a strong feeling existed against the new police” upon the Met’s formation.<sup>6</sup> We do not rely on only anecdotal evidence, however, and note that concerns regarding changes in reporting are further alleviated by the fact that crimes were reported to magistrates’ offices in London, both prior to and after the introduction of the Met,

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<sup>3</sup> These new police are likely to have been perceived as permanent given the magnitude of the intervention (in London) and the fact that the county roll-out started ten years after (and was modelled after) the Met.

<sup>4</sup> Wong (1995) emphasizes opportunities for legal and illegal gains from 1857 to 1892. Wolpin (1978) looks at the relationship between crime and the rates of clearance, conviction, imprisonment, and fines, as well as average sentences from 1894 to 1967. 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.

<sup>5</sup> See, for instance, Squicciarini and Voigtländer (2015), Becker et al. (2011), Besley and Persson (2009), and Acemoglu et al. (2016).

<sup>6</sup> The magistrate quote is published in an October 1, 1829 issue of *The Morning Journal*. On the same date, the *Morning Herald* quotes a member of a mob shouting “it is one of Peel’s bloody police; they are all thieves themselves”. Sir Robert Peel was the Home Secretary from 1822 to 1830 and regarded as the founder of modern-day policing. These articles are part of a collection on the Open University website (viewed May 7, 2019): <https://www.open.ac.uk/Arts/history-from-police-archives/MphcR1/Scrapbooks/sbIntro.html> .

and not directly to the new police. In addition, some of our analysis emphasizes the most serious crimes of robbery, burglary, and murder, which are arguably less subject to concerns about reporting. Another potential confounder is that an increased ability to detect crime could have led to more charges, even if the number of crimes committed did not change; i.e. there could have been an increase in clearance rates. This would have been expected as the new force was substantially larger than the previously existing informal police. Note that such increased detection would also have been expected to *reduce* crime through incapacitation (over and above deterrence). To disentangle whether the new forces reduced crime (through deterrence and/or incapacitation) from increases in crime reporting and clearance rates (the potential confounders), we rely on two types of crime measures – incidents and charges.<sup>7</sup> Incident level data is especially important as it allows us to abstract from the potential problem of crime reducing effects being masked by increased clearance rates (one of the confounders), which could potentially happen when studying an administrative measure like charges.

The closest existing literature to our study includes those papers studying police deployment on the streets.<sup>8</sup> A number of studies report a crime reduction following temporal variation in (often non-permanent) police deployment, including post-terrorist attack increases in deployment in London (Draca et al., 2011) and Buenos Aires (Di Tella and Schargrosky, 2004).<sup>9</sup> However, Blanes I Vidal and Mastrobuoni (2018) do not find significant effects of non-terrorist attack related temporary increases in patrols. Negative effects of visible police presence on crime have also been found in studies of private policing using geographic boundaries (MacDonald et al., 2015; Heaton et al., 2016); these studies use spatial variation in force allocation to understand the permanent effect of policing.<sup>10</sup> In contrast, our study estimates the effect of a permanent change in policing, exploiting variation both over time and across space.

The first part of our empirical analysis studies the impact of the 1829 formation of the Met on crime in London. With the exception of the ‘City’ of London (which still has its own force today), the initial catchment area of the Met was within an approximate 7-mile radius of Charing Cross, London.<sup>11</sup> Because not all of London is initially ‘treated’ by the formation of

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<sup>7</sup> Incidents refer to a reported offense (regardless of whether it is cleared or not), while charges can only be filed when there is a suspect associated with that offense.

<sup>8</sup> Studies have considered the extensive margin (temporary) destruction of a police force. As described by Nagin (2013), Andenaes finds a rise in crime rates, especially street crimes like robbery, after German soldiers arrested all members of the Danish police in 1944. Others have studied the effects of police strikes (Pfuhl, 1983) and slowdowns (Cann Chandrasekher, 2016), though the latter differs from the extensive margin.

<sup>9</sup> Negative effects of police on crime are also found by Klick and Tabarrok (2005) following increases in Washington DC terrorist alert levels and Weisburd (2017) using variation in officers leaving their beats unattended.

<sup>10</sup> To the extent that decreased response times imply an increase in police presence, Blanes I Vidal and Kirchmaier (2018) find a relationship between response time and the likelihood of clearing a crime.

<sup>11</sup> The City of London refers a 1-square mile area (today’s central business district) in Central London.

the Met, our analysis relies on geocoding historical crime data from two data sources into ‘treated’ and ‘control’ regions of London for periods before and after the Met was created.

First, we use felony trial data reported in the Proceedings of the Old Bailey (Central Criminal Court of London and County of Middlesex) for the most serious offenses of burglary, manslaughter/murder, and robbery, for which we manually coded the number and type of police witnesses and geocoded offense locations into treated and control regions. Thus, the Old Bailey data allow us to directly observe reform implementation: there is an instant shift in the type of police witnesses (‘old’ to ‘new’) that is by far largest in the treated area. Moreover, it allows for a difference-in-differences model. The second source consists of daily police reports for nine police offices run by the pre-1829 magistrates (and continued until 1839), who were tasked with processing crimes for all of London. These reports include three crime measures: ‘informations’ and stolen property reports (which one can interpret as uncleared crime incidents) and charges (which are cleared crimes). Though all offenses are included (as opposed to selected felonies at the Old Bailey), they cannot be geocoded, necessitating pre-post designs.

Both London analyses provide evidence consistent with a crime-reducing effect, especially for violent crimes (including robbery). A significant and persistent reduction in trials is seen for robbery (more than 40%) in the Old Bailey data, but no (consistently) significant effect for homicide and burglary trials. Using the daily reports data for London, we find a significant reduction in violent crime (driven by reductions in both cleared and uncleared crime), whereas there is a significant reduction in uncleared property crime incidents but an increase in cleared property crimes (charges). As the latter effect dominates in the daily reports data (which include a wide range of property offenses), these off-setting channels provide a potential explanation for the lack of a property crime (burglary) effect in the Old Bailey data.

The second part of our analysis studies the impact of the introduction of police forces to the counties of England and Wales; such forces were allowed for in 1839 but did not become mandatory until 1856. The county analysis complements the London analysis in a number of ways – but perhaps first and foremost, it increases the external validity of the results: Are they specific to London in the 1830s? Moreover, as the county forces were all of different sizes (relative to the population) upon creation, we can use the county roll-out of police forces to further our understanding of how the effect of a police force depends on its characteristics. Of the 48 counties in our analysis, 16 created forces in 1840, 23 in 1857, and 9 in the intermediate years. We use a difference-in-differences model to identify the effect of creating professional police forces on crime, overall and for forces of different sizes, where 1,000 people per officer was the nationally recommended (but rarely achieved) guideline. Our main crime measure (the

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

We find no overall effect of creating just any professional police force. But, creating a force that was closer in relative size to the nationally recommended threshold reduced crime overall (19%) and across categories (18% for violent, 14% for property, but no significant effect for other offenses); relatively smaller forces did not have a net crime reducing effect (i.e. observable in administrative data). Event-study specifications show that the crime-reducing effect of large forces is not immediate (delayed by one to two years) and increases over time. Insignificant leads support the parallel trend assumption and a lack of anticipatory effects.

Finally, we pay careful attention to the potential role of spill-overs and crime displacement in the analyses of both the Met and county forces. Spill-overs may lead to a reduction in crime in neighboring areas, if there are spill-overs either in policing across catchment areas or in deterrence and/or incapacitation effects. In contrast, crime in neighboring areas can increase if criminals simply migrate away from the policed areas to commit crimes elsewhere (crime displacement). The extent to which the latter applies is clearly relevant to identify whether the introduction of police reduced crime overall or just locally. Using the geocoded Old Bailey data, we find little evidence of displacement within London from inside to just outside the Met's catchment area. Similarly, we do not find evidence suggestive of crime displacement from London/Middlesex to neighboring counties after the introduction of the Met. Finally, though our main county results are robust to controlling for spill-over effects of neighboring forces, the existence of neighboring forces does indeed have an impact on local county crime: neighbors with relatively large forces decrease local crime while those with relatively small forces increase local property crime.

The remainder of the paper proceeds as follows. Section 2 provides institutional details related to the 1829 creation of the Met and the subsequent roll-out of county police forces. Sections 3 and 4 present data and analysis for the Met and county roll-out, respectively. Section 5 concludes with a discussion of the external validity of these historical experiments to today.

## **2. Institutional Background**

This section highlights the institutional context and details of the introduction of the London Metropolitan Police Force and the county force roll-out. Other criminal justice reforms (e.g. abolishing capital punishment) and societal changes (e.g. population growth) that are relevant to the analysis will be discussed in detail throughout the paper.

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

The idea of policing did, of course, exist prior to the Metropolitan Police Act of 1829. In fact, unpaid and part-time local (night) watchmen date to the Westminster Watch Act of 1735. London's Bow Street Runners, who were sworn constables of Westminster, date to around 1750 (Emsley, 2009). As there were only eight, they did not have a physical presence and were not meant to deter crime, but rather to locate and arrest serious offenders. Initially, these Runners were similar to the 18<sup>th</sup> century thief-takers, i.e. men who earned their livings from private and public rewards upon the convictions of 'serious' criminals. By the end of the 1700s, however, the Runners were essentially full-time policemen located at the Bow Street house, which became a centralized collection point of crime incidents for the Runners to follow-up on.<sup>12</sup>

The Bow Street office was used as a model to establish seven additional Police Offices 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 Wapping in 1798. Each office was staffed by three magistrates and up to 12 constables (Emsley, 2009). That is, there were less than 150 policemen in London prior to the creation of the Metropolitan Police and their primary purpose was to follow up on crime reports. The magistrates had two main responsibilities: to supervise the constables and process criminal cases. Upon the creation of the Met, the magistrates lost control of the existing 'police' to the new Metropolitan Police commissioners but retained their judiciary role (Davis, 1984). Thus, despite the separation of policing and magistracy responsibilities, these police (or magistracy) offices played an essential role in crime processing. Even post-Met, a known offender would be processed through these offices (which existed until 1839) and recorded in the daily reports from these offices (as reflected in the introductory text of these reports).

Finally, the Metropolitan Police Act (10 Geo.4, c.44) created the London Metropolitan Police (the 'Met') on September 29, 1829. This was the first *professional* police force in the world. Initially 1,000 men strong, there were more than 3,000 officers by May 1830: in other words, in just six months, there was a more than 20-fold increase in the number of officers in London (from around 150 pre-Met). Panel A of Figure 1 documents the weekly number of hires from September 1829 to March 1830, and Panel B the Met's weekly growth until 1856. It shows that (i) initial hiring happened in two stages, with hiring of recruits for six inner divisions in September 1829 and the 11 outer divisions in February 1830 (see Appendix Table A1) and (ii)

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<sup>12</sup> This summary is based largely on the London Lives website (accessed February 6, 2018): <https://www.londonlives.org/static/Policing.jsp>



the Met grew almost constantly in the next 30 years to about 6,000 men in 1856.

The initial catchment area of the Met was within an approximately 7-mile radius from Charing Cross in Central London and extended to 15-miles in 1839.<sup>13</sup> Excluded from the initial catchment area, however, were the City of London (which established its own force in 1832, expanded in 1839 and still distinct today) and, until 1839, the Thames River Police.<sup>14</sup> Panel A of Figure 2 presents a historical map of the original jurisdiction of the Metropolitan Police. Panel B shows that the (geocoded) pre-existing police offices were centrally located within the 7-mile radius (and even a smaller 4-mile radius). Appendix Table A1 shows that an equal number of police were hired into each division, regardless of the geographic size. This implies that the Met Police were likely to be more visible on their beats in the smaller inner divisions. There is thus a potentially more intense treatment in a shorter radius around Charing Cross.

On October 6, 1829, the Metropolitan Police opened its first station – Scotland Yard – 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.<sup>15</sup> 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. The earliest schedule of Met Police stations we have found dates to May 1, 1873, listing each station by police division, including the tenure at the current building (though not all have dates). There are more than 175 stations listed but less than 10 with leases dating to the early 1830s (and this includes Bow Street, Marylebone, Marlborough, and Scotland Yard).<sup>16</sup>

In addition to the sharp and large increase in the sheer number of ‘police’, there was a shift in the primary task of an officer to deterrence. To this end, Metropolitan Police officers were assigned to walk a beat – a regular route – at a pace of 2.5 miles per hour; the beat was intentionally small to increase visibility and the new policemen ‘were supposed to get to know

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<sup>13</sup> 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. Our main analysis uses the 7-mile radius to define all potentially treated areas, but also breaks this up into a treated inner circle and potentially less intensely treated outer circle (i.e. patrols are less visible due to the larger geographic area). We also test the robustness of our results to an 8-mile radius.

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

<sup>15</sup> [http://www.historyhouse.co.uk/articles/scotland\\_yard.html](http://www.historyhouse.co.uk/articles/scotland_yard.html), accessed April 29, 2019.

<sup>16</sup> See MEPO 4/234 from the National Archives.

everyone who lived on these beats'.<sup>17</sup> Increased standards and quality may also have increased the effectiveness of the new police. Documents reporting the reason of removal of officers from the force make clear that 'police quality' was taken seriously. Panel A of Figure 3 shows the weekly number of leavers among those recruited before March 1831. Panels B and C show the weekly number of post-1833 removals by broad reason (resignation, dismissal or death) and dismissals by reason (drunk, neglect or misconduct, criminal behavior or other). These figures demonstrate high turnover especially at the very beginnings of the Met; one can even observe annual firing spikes for being drunk on duty around Christmas.

Who were the men in the new police? When the Met was created, selection criteria were not yet strict and men between the ages of 18 and 35 were eligible to apply. The job was physically demanding and subject to strict discipline as highlighted above, but offered more security than other work places and, as such, attracted, among others, previously unemployed workers (see Dell, 2004). The new police were paid a wage comparable to that of an unskilled agricultural worker, in an effort to recruit men who did not resemble gentlemen and who could gain the trust of the everyday man.<sup>18</sup> As work conditions became more attractive over time, recruitment became more restrictive in terms of age and physical requirements. The new police received very little training – the first formal training school was only established in 1907.<sup>19</sup>

One question that naturally arises is whether the new officers were just the old 'police' with a new job title. We argue that this is unlikely given that the size of the 'old police' amounts to only less than 3% of the size of the Met by May 1830.<sup>20</sup> Moreover, even if some of the old police did become Met officers, they were not doing the same job – they were now patrolling the streets of London to deter crime. This newfound emphasis on deterrence is a fundamental component of the institutional changes being studied here.

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

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<sup>17</sup> While this was possible in the inner divisions in Central London, beats in the outer divisions were often larger and it is plausible that policemen in these divisions were not able to fulfill these tasks (see Emsley, 2009).

<sup>18</sup> See <https://www.npcc.police.uk/Publication/History%20of%20Police%20Office%20Pay%20Framework.pdf>, last accessed October 22, 2018.

<sup>19</sup> See <http://www.open.ac.uk/Arts/history-from-police-archives/Met6Kt/PoliceWorker/pwRecPol.html> and <http://www.open.ac.uk/Arts/history-from-police-archives/Met6Kt/PoliceWorker/pwTrain.html> for further detail (both last accessed on October 4, 2019).

<sup>20</sup> We support this conclusion by comparing registers of the first 3,000 officers hired by the Met (Source: MEPO 4/31, National Archives London) to those hired into the Bow Street Foot Patrol in the years leading up to 1829 (Source: MEPO 4/508, National Archives London). Only 156 men were hired into Bow Street between 1823 and 1829. Since turnover is high in the early years, this 156 only corresponds to *hires* and not the existing level of pre-Met 'police'. We can observe (using names) that a number were hired by the Met in the initial hiring wave; 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.

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

But, anecdotal evidence also points towards alternative reasons for forming the Met, including a need for a centralized (non-military) body to maintain order, police provision independent of parish wealth, and a desire for order and tidiness.<sup>21</sup> The first of Sir Robert Peel’s nine Principles of Law Enforcement highlights these alternative reasons: “The basic mission for which police exist is to prevent crime and disorder as an alternative to the repression of crime and disorder by military force and severity of legal punishment.”<sup>22</sup>

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

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

The 1835 Act required the boroughs to appoint both a watch committee and sufficient number of fit men to act as constables, tasked with preserving the peace and preventing crime. There was general resistance, such that by 1837 only 93 of 171 boroughs even claimed to have established a force (Hart, 1955). Many admitted to fulfilling ‘statutory obligations’ by re-appointing previous ‘police’ (rather than selecting new recruits; Hart, 1955). Rather than studying the limited and fuzzy implementation of the 1835 Act, we focus on the rural county forces created by the 1839 and 1856 Acts.

The 1839 Act gave the Quarter Sessions’ justices in each county the power to create a police force for all or part of the county *if they chose*. This act also provided guidance regarding the structure of such a force (Stallion and Wall, 1999), including a pay scale set by the Home Office.<sup>23</sup> Why were the 1835 and 1839 Acts passed? Hart (1955) argues that these acts were not a response to criminals fleeing already treated areas, a conclusion that our analysis of spillovers from London to the neighboring counties supports. Rather, she argues that an increased concern about relying on the military and deficiencies in the implementations of

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<sup>21</sup> See <http://www.open.ac.uk/Arts/history-from-police-archives/Met6Kt/MetHistory/mhFormMetPol.html> (last accessed on May 17, 2018).

<sup>22</sup> The military had a limited pre-Met role, not extending to patrolling the streets in the sense of everyday policing. In particular, they were sent in to dissolve unlawful gatherings or to (violently) suppress riots (Dell, 2004).

<sup>23</sup> A county constable should be paid somewhat more than an agricultural worker. Last accessed October 22, 2018, see <https://www.npcc.police.uk/Publication/History%20of%20Police%20Office%20Pay%20Framework.pdf>.

earlier acts motivated the 1839, and ultimately, 1856 Acts (Hart, 1956).

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

In 1856, three inspection districts – Northern, Midlands, and Southern – were formed, each with an assigned inspector. According to Cowley and Todd (2006), the initial (unofficial) inspections in 1857 found many counties with inefficient or even non-existent forces. The inspectors assessed efficiency according to (i) the size of the force, (ii) the ratio of officers to the population, (iii) the quality of supervision, and (iv) the degree of cooperation with neighboring forces. Stipulated by the 1839 Act, one officer per 1,000 people was taken as the norm by the inspectors. Following unofficial advice given during the preliminary inspections in early 1857, only five districts were declared inefficient in the first official inspection, but just one (Rutland) the following year (Cowley and Todd, 2006).<sup>24</sup> The Inspectors rigidly interpreted the requirement of a *sufficient* ratio of officers per population; even the Home Secretary, Sir Vernon Harcourt, highlighted this in 1883: “...the fanciful cast-iron rule of so many [police]men per 1,000 inhabitants. Nothing can be more ridiculous than to apply the same measure to all places alike regardless of circumstances.”

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

#### **3.1. London Data Description**

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

The first is the *Proceedings of the Old Bailey*. The Old Bailey is the Central Criminal Court of London and the surrounding county of Middlesex, and responsible for all felony trials. The Proceedings were published after each monthly court session and include the records of more than 200,000 trials from 1700 to 1913; these have since been digitized by *The Old Bailey Proceedings Online*. Though many variables (e.g. offense type, verdict, and sentence) are tagged and easily identifiable, we also manually extracted and coded the location and date of

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<sup>24</sup> Rutland remained inefficient until the 1861/62 inspection year.

the crime as well as the characteristics of police witnesses (number, type, and crime scene presence), which we will use to assess the first-stage implementation of the Metropolitan Police Act.<sup>25</sup> Given the time-intensive nature of the transcription and historical geocoding, we focus on the most serious offenses of murder/manslaughter, robbery, and burglary/housebreaking from 1821 to 1837.<sup>26</sup> For these, we can assume that their felony status (and hence representation at the Old Bailey versus a lesser court) did not change. Importantly, the emphasis on the most serious offenses further alleviates concerns regarding changes in crime reporting following the introduction of the Met - i.e. a robbery would always be reported.

To geocode the data, a research assistant transcribed 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 geocoordinates.<sup>27</sup> Using the geocoded location and date of offense, we classify offenses as in the treatment and control areas (within/outside a 7-mile radius of Charing Cross and within/outside the City of London, respectively) before and after the Met's introduction. Thus, with the Old Bailey data, we can estimate both simple pre-post and difference-in-differences specifications.

Figure 4 shows maps of each crime for 1821 – September 1829 (pre-Met) and October 1829 – 1837 (post-Met). Each dot represents the location for a defendant-crime observation; treated locations are blue, while control locations (the City of London or outside 7-miles) are green. The borders indicate modern date postcode areas and the red circles indicate radii of 7 and 15 miles from Charing Cross, respectively.<sup>28</sup> Appendix Table A2 provides the number of trials by crime type as well as details regarding police witnesses within and outside a 7-mile radius of Charing Cross and in the City of London for different time windows. One statistic that stands out is the relatively low number of murder/manslaughter trials (just 258 in all areas over 1821-1837). Appendix Figure 1 demonstrates that this low murder rate is not driven by only

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<sup>25</sup> We have previously used the Old Bailey data in projects studying (i) the impact of abolishing the death penalty on jury verdicts, (ii) path dependency in jury decisions, and (iii) the gender gap in jury and judge decisions from 1715 to 1900 (see Bindler and Hjalmarsson, 2017, 2018 and forthcoming).

<sup>26</sup> We have also geocoded robbery, burglary, and murder/manslaughter for the longer period of 1820 to 1850, but focus on this shorter window to avoid the 1837 abolition of the death penalty for robbery and burglary. In addition, after the initial data coding, we noted an unusual dip in burglary from the mid-1820s to the mid-1830s. There were hardly any offenses labelled as burglary during this time, while there was a sharp increase in the offenses labelled housebreaking. We therefore geocoded housebreaking offenses for the 1820-1837 window to supplement our analysis. Housebreaking and burglary are treated as one combined offense category for the entire period.

<sup>27</sup> The same RA coded all locations, and though they were aware of the general question, they were unaware of the specifics of the research design. There was no opportunity for manipulation in the geocoding. Whenever locations have changed names (e.g. street names), we identify the current address using historical maps (roughly 40% of our regression sample). When the most detailed address is a long street (about 11% of our sample), we geocode the nearest street endpoint as the location (i.e., assign potentially untreated observations to the treatment area). Results are qualitatively robust to excluding either of those 'fuzzy' locations (see Appendix Table A3).

<sup>28</sup> Shapefiles for the postcode areas were obtained from Maproom's UK Postcodes Shapefiles.

measuring crimes that go to trial; Old Bailey murder trials track well with an alternative time series of London homicides that is not based on trials. A more likely explanation is the limited ability of coroners at this time to identify potential murders (Emmerichs, 2001).

The limitation of the Old Bailey data only including serious felonies that go to trial is addressed by our second data source – the *Report or Account of the Proceedings at the several Police Offices*. These are reports by the nine police offices that were run by the pre-1829 magistrates, which continued until 1839.<sup>29</sup> We manually transcribed the data from January to April of 1828 (the year pre-reform), 1830 (the year post-reform) as well as 1831 and 1832. Unfortunately, these daily police reports did not exist before 1828 and those for the second half of 1828 and 1829 are missing.<sup>30</sup> For each office and day (except Sundays), a detailed description of ‘charges’, ‘informations’ and ‘property stolen’ are reported. We use these data to create two measures of crime incidence: (i) the daily number of ‘property stolen’ entries and (ii) the daily number of property, violent, and other ‘informations’. Most comparable to modern day arrest data, we also code the daily number of charges by crime category (property, violent, other). Our interpretation of these different measures is that daily informations and stolen property reports represent *uncleared* crime incidents while charges represent *cleared* crime incidents. To address the possibility that the introduction of police simply shifts uncleared crimes into the cleared category, we also create a measure that aggregates all types of incidents.

In contrast to the Old Bailey trial data, the daily report data include both felonies and misdemeanors and cleared and uncleared crimes (even those not going to trial). Yet, we cannot geocode the offenses into treated and control areas of London (the magistrates deal with all of London) and are thus restricted to a pre-post design. Given the high frequency of the data, we can, however, estimate the total effect on crime in London (the sum of any crime reduction and potential counteracting displacement to control areas) in a narrow window around the reform.

### ***3.2. Analysis of The Old Bailey Proceedings***

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

We begin by assessing whether there is evidence of the introduction of the Met in the Old Bailey trial reports. 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

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<sup>29</sup> See Appendix Figure B1 for an example page of data, which are publicly available from the National Archives.

<sup>30</sup> 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).

that were either predominantly pre or post-Met.<sup>31</sup> Do we see an increase in the number and/or these different types of police witnesses at trial after the Met was created? An important caveat is that this analysis conditions on crimes brought to trial: We cannot control for the possibility that the new police affect the number of crimes committed or the likelihood of a trial.

Panel A of Figure 5 plots the annual share of trials with a police witness of any sort for both the treated (i.e. within 7-miles but not the City of London) and potential control area (outside 7-miles or in the City of London). There is no obvious change in the proportion of trials with *any* police witness around the 1829 introduction of the Met. Panel B presents the share of trials in the treated and control areas with different types of police witnesses.<sup>32</sup> In the treated area, the share of trials with an ‘old’ labelled witness (watchman or other) drops sharply from about 70% to 20% while the share with a ‘new’ label (policeman or other) increased from 0% to almost 50%. In the control areas, we also see an increase in the share with ‘new’ police, but it is much smaller and more gradual than in the treatment area.

To account for the different sample sizes (and precision) in the different areas (see Panel C of Figure 5, which looks at the number rather than share of trials), as well as the potentially different composition of offenses, Table 1 looks at the ‘first-stage’ more formally by estimating pre-post designs for each potential treatment and control area. We divide the treatment area into two areas (within 4 miles and 4-7 miles from Charing Cross) to allow for a potentially more intense treatment in the inner divisions (i.e. more visible patrol presence as highlighted in Section 2.1). The two control areas include (i) offenses outside the 7-mile area and (ii) the City of London. These are simple regressions of each measure of police presence at crime trial  $i$  for offense  $o$  in area  $a$  at date  $t$  on a dummy indicating whether the offense occurred after the introduction of the Met and offense type fixed effects. These results are presented for two windows –1821-1837 and 1828-1832 – which we carry through the Old Bailey analysis. The latter mimics the estimation window of our second data source (daily police reports).

Consistent with the descriptive figure, Table 1 shows little evidence that the creation of the Met increased the presence of *any* police at a trial. But, it significantly changed the *type* of witness: The pre-post specifications show that 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, respectively (using the 1821-37 window) while the presence of ‘old’ police decreased by 49

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<sup>31</sup> Other predominantly pre-Met labels include beadle, conductor, marshalsman, officer, patrol and street keeper. Other predominantly post-Met labels include inspector, sergeant, superintendent, captain and Thames.

<sup>32</sup> Type of the police witness refers to any of the first five police witnesses; less than 1% of sample trials have more witnesses. The presence of constables, a label that is not distinctively pre- or post-Met, is excluded from this figure.

and 25 percentage points respectively. Thus, the pre-post analysis confirms that there was a treatment, and suggests it may have been stronger in the inner (4-mile) circle. Also consistent with the figure, the pre-post specification for the control area (more than 7-miles) indicates some increase (16 percentage points) in ‘new’ but no change in ‘old’ police. 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 plausible 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 depend on crime type and be especially relevant for street crimes. Columns (7) and (8) of Table 1 look at this in the pre-post regressions: There is a significant 8 percentage point increase in police presence at a crime scene in the 4-mile radius for the larger sample period (the estimate is similar but less precise for the uncertainty 4-7 mile area, while there is no such effect in the control areas). These changes are not seen, however, in the short time window.

Though substantially smaller than for the treated area, the above analysis finds some evidence of an increase in new police in the control areas. Why? There are a number of plausible explanations. It could simply be (i) measurement error in our geocoding or (ii) that the term ‘police’ is increasingly used in the Proceedings by court reporters, regardless of the actual type of police (the same court reporter is responsible for the entire Proceedings, regardless of offense location). Alternatively, (iii) there could be spill-overs of the Metropolitan Police into the control areas. This could occur because the 7-mile radius/City of London is not a perfect boundary and some Met police 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-differences analysis of the effect of the Met on crime. To assess the plausibility of such spill-overs, Figure 6 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 no spill-overs, then one would expect to see a drop in the density just after the 7- mile threshold. The figures do not suggest a substantial spill-over of Metropolitan policing (the densities are close to zero around and outside the 7-mile mark), but they do reinforce the basic findings from this section. The new police are observed to be present in the treated area, and to a greater extent in the 4-mile radius than the 4 to 7-mile radius. Police are also more likely to be present at the crime scene in the 4-mile radius.



### *Pre-Post Comparison of Means (Old Bailey Data)*

Having established that the creation of the Met affected ‘policing’ in London, we turn to whether it affected crime. To do so, we have to temporally and geographically aggregate the data. In our baseline, we aggregate at the month by area level: treated (less than 4 miles from Charing Cross), uncertain (4 to 7 miles from Charing Cross) and control areas (more than 7 miles from Charing Cross plus the City of London). Table 2 compares the average number of crimes overall and by crime type (burglary, robbery and homicide) before and after introducing the Met. Panels A and B show means for 1821-1837 and 1828-1832, respectively.<sup>33</sup> In the larger window, there is a significant reduction of 12% (8.24 to 7.29) in the average number of total monthly crimes in the treated area; there are similar reductions (20%) in the shorter window. This change is driven in both windows by robbery, which decreases by 44% and 53%, respectively. Such a decrease is not seen in the less intensively treated uncertainty area (if anything, there is a significant increase in burglaries in the longer window). No significant changes are observed in the control area or the City of London in the short window, though a significant increase in robbery is seen in the control and an increase (decrease) in burglary (robbery) in the City in the longer window.<sup>34</sup> Appendix Figure A2 takes these differences in means a step further, and plots them separately for each one-mile radius from Charing Cross. The only evidence of a crime reduction is for robbery within the treated area.

### *Main Empirical Specification (Old Bailey Data)*

To make the case that these post-Met crime reductions in the treated area have a causal interpretation, we turn to the difference-in-differences model in equation (1), which uses the area outside the 7-mile radius and the City of London as the best possible control groups. We again split the potentially treated areas into areas with a certain (within 4-mile radius) and an uncertain treatment intensity (4 to 7-mile radius), consistent with the higher treatment intensity in the inner circle and suggestive evidence from the difference-in-means comparison above. The outcome variable is the number of trials overall and for offense  $o$  in area  $a$  during time period  $t$ . The baseline analysis aggregates the data at the month ( $t$ ) and area ( $a$ ) level, using the four previously defined areas (treatment, uncertain, control and City of London). We later conduct robustness tests to alternative aggregation levels (weeks and circles around Charing Cross). Year, month and area fixed effects are included.

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<sup>33</sup> Significance levels are based on simple pre-post regressions; the results are robust to including month dummies.

<sup>34</sup> Similar results are found when using an 8-mile instead of a 7-mile radius from Charing Cross.

$$(1) \quad Trials_{at}^o = \gamma_1(Treat * PostMet)_{at} + \gamma_2(Uncertain * PostMet)_{at} + \alpha_y + \alpha_m + \alpha_a + \varepsilon_{at}$$

Intuitively, we estimate the change in crime in the treated areas before and after the introduction of the Met compared to the change in crime in the control areas. Compared to the simple pre-post analyses, this allows us to account for general trends in crime that would have occurred independently of the reform. For this to be the case, the usual parallel trend assumption must hold and we must assume that during the estimation window nothing else changed in the treatment but not in the control group (or vice versa) that could have affected crime rates. We formally test for pre-reform differences between the treatment and control areas when we move from the difference-in-differences to an event-study design and discuss potential confounders.<sup>35</sup>

#### *Spillovers, Crime Displacement and Other Potential Confounders (Old-Bailey Data)*

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

A second potential confounder is the first cholera epidemic of 1832, to which the deaths of almost 7,000 in London have been attributed.<sup>36</sup> The epidemic could certainly have affected crime through multiple channels: directly through public riots (Tynkynnen, 1995), by affecting police resources (directly through ill/dying officers or indirectly as officers responsibilities are shifted away from crime prevention), or by impacting the population of criminals (who may be incapacitated by the disease or driven to commit crimes). If the epidemic differentially impacted the population in the treatment and control areas, then this *could* violate the difference-in-differences assumptions. To explicitly look at the geographical and temporal distribution of cholera in London, we use a new source of data - the *Returns to Death* of the Metropolitan Police officers from 1829 to 1889, which provides the date of death, police division to which the officer is assigned and often the reason of death. Appendix Figure A3 demonstrates that cholera arrived and peaked in 1832, diminished by 1833, and almost disappeared by 1834. 19

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<sup>35</sup> Robust standard errors are used in the baseline. Appendix Table A4 assesses the sensitivity of these standard errors to a wild cluster bootstrap clustering by area (treatment, uncertainly, control, and City): if anything, the findings become more precise.

<sup>36</sup> See for instance <https://www.choleraandthethames.co.uk/> (accessed April 29, 2019).

officers died from cholera, and all but one of these deaths 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 avoids this concern; in addition, our results are robust to further dropping all trials after May 1832, i.e. when only looking at the pre-cholera period. Moreover, while the differential exposure of the treated and control areas to the cholera epidemic raises the possibility of a violation of the parallel-trends assumption, empirically we do not find this to be the case: Event-study specifications with leads and lags (presented later in this section) suggest parallel pre-trends in crime and hence support the assumption of no differential trends in crime.

Third, is there the possibility of other (potentially relevant) criminal justice changes during this period that differentially affect the treatment and control areas? The main reform we are aware of is the abolition of capital punishment for burglary and robbery in 1837, which we avoid by restricting our large time window to end before 1837. Again, the shorter time window limits the possibility of omitting other such shocks. Moreover, our previous research (Bindler and Hjalmarsson, 2018), studying the effect of abolishing capital punishment on jury verdicts, documents that there are no anticipation effects related to the timing of the abolition of capital punishment. The abolition occurred offense by offense over a period of 40 years, with no crime-specific movements determining the year a specific category was reformed.

A fourth potential confounder is that the period is characterized by dynamic population growth. Appendix Figure A4 shows decadal population estimates and growth for Inner London (excluding the City), Outer London and the City of London.<sup>37</sup> In the first half of the 19<sup>th</sup> century, Inner London grows at the highest rate (almost 25%) between each census, with Outer London not too far off (though the Inner London population is substantially larger), 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 would bias us against finding a crime-reducing effect in the pre-post analysis. Likewise, the faster growth in the treated areas would bias us against finding a crime-reducing effect of the Met in a difference-in-difference analysis. However, the extent to which such differential population growth is a concern is again mitigated by our emphasis on the short window around the reform and by observing parallel pre-trends in crime.

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. Our above discussion of witnesses already raised

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<sup>37</sup> The classification of Inner and Outer London areas is done by the historical census, but these are likely to overlap with our less than and greater than 7-mile radius classifications.

the possibility of the former. Though it would actually attenuate our estimates of a crime reducing effect of police, we concluded that there was little evidence of a substantial police spill-over. 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-differences estimates in the direction of a crime reducing effect. One could even conclude crime was reduced even if all crimes from the treated area were simply re-located to the control area. To assess the extent to which displacement is a concern, we return to the pre-post estimations in Table 2. There is no evidence of an increase in crime in control areas in the short window. A spill-over cannot be ruled out in the longer window, however, as the average number of monthly robberies outside the 7-mile radius actually doubles, though it is still an order of magnitude smaller than in the treated area. To take a closer look at potential displacement, Figure 6 plots kernel densities of crime locations (relative to Charing Cross) for the periods before and after the introduction of the Met. 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.<sup>38</sup>

#### *Main Results and Robustness Tests (Old-Bailey Data)*

The results from the difference-in-differences estimation are shown in Table 3. Columns (1) to (3) correspond to the baseline specification (with the City of London classified as a control area) for three windows: 1821-1837, 1825-1835, and 1828-1832. Panel A shows the results for total crime, and Panels B to D separately by crime type. Using the largest window, we find that the introduction of the Met leads to highly significant decreases in trials in the treatment relative to the control area for total crime, which is driven by robbery. The baseline effects are sizeable: Relative to the average number of pre-Met crimes in the treatment group, the point estimates (in the larger window) translate into a reduction in combined crime of 14% and 40% for robbery. Though at least partially treated, we do not find any effects of the Met on crime in the uncertainty area; this could imply that there was no change in crime levels in the uncertainty area (maybe due to a smaller deterrence effect as police were less visible in larger

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<sup>38</sup> 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. (2018). In this context, the control area with a radius of 7 to 15 miles from Charing Cross is not small.

divisions) or that the crime reduction effect was offset by increased apprehensions.

Focusing on the inner 4-mile radius, we note that the difference-in-differences estimates are close to the simple pre-post comparison of means. Further, moving to a narrower estimation window (and mitigating potential confounders), the difference-in-differences specification yields similarly sized effects (18% overall and 46% for robbery, respectively). The remaining columns of Table 3 show the sensitivity of the main results to alternatively assigning the City of London to the treatment group after the introduction of the City Day Police in April 1832, the uncertainty group, and excluding the City of London from the analysis completely. Unsurprisingly (as the treatment is distorted), the former attenuates the point estimates (though they remain significant), while the latter cases result in robbery point estimates only marginally different from the baseline. Our main finding that the Met led to significant and sizeable reductions in robbery (trials) is robust to alternative estimation strategies and windows.

Figure 7 shows the results from event study estimations by crime type (burglary – Panel A, robbery – Panel B, and homicide – Panel C) for the treated and uncertain areas in columns (1) and (2) respectively. We estimate a more flexible specification that interacts the treatment and uncertainty indicator with dummies for 2-year intervals before and after the introduction of the Met. To account for the mid-year timing of the Met’s creation, we define a year from September to August. The purpose of these specifications is twofold: Use the leads to test the plausibility of the parallel trends assumption and study the dynamic effects of creating the Met. Were the effects immediate, and did they change over time (as officer quality increased with both experience and in recruiting)? 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. There is no evidence of a short or long-run effect for homicide or burglary. For robbery, the effect is immediate and persistent.

Table 4 presents robustness checks of the level of temporal and geographic aggregation. Columns (1) - (3) aggregate the data to the week by area level (i.e. a smaller temporal period) while (4) - (6) consider the month by 1-mile distance band level (i.e. smaller geographic areas). Since crime is a rarer event in these smaller units, we adopt an extensive margin measure of crime (*any crime*) for this table; we cannot use the same margin in the baseline aggregation due to a lack of variation (100% of treated areas have, on average, at least one crime per month). We see the same pattern of results. A significant reduction in robbery trials (ranging from 11.5-16.8 percentage points) is seen after introducing the Met regardless of the window or level of aggregation. In contrast to the baseline, we do observe a sometimes significant reduction in the chance of burglary of 6-9 percentage points; precision is generally lost in the smallest window.

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

### ***3.3. Analysis of The Daily Police Reports***

#### *Summary Statistics (Daily Police Reports)*

The second part of the London analysis uses a simple pre-post design to analyze the daily crime reports described in Section 3.1. Though the raw data include nine offices, we exclude the Thames Police Office from the analysis as the Thames River Police are not in the jurisdiction of the Met and the composition and nature of crimes in the Thames jurisdiction (docks and water) is likely to differ from the surrounding offices.<sup>39</sup> Table 5 presents summary statistics for the remaining eight offices for the entire period, the pre-reform period (1828), a one-year post period (1830) and a three-year post period (1830-1832). For the entire period, there are on average 0.5 informations, 6.4 charges and 0.4 reports of stolen property per day and station. To the extent that informations and stolen property reports proxy for uncleared crimes while charges represent cleared crimes, the combined total of informations, stolen property reports and charges can be interpreted as the total number of crimes (presented in the top panel of Table 5): there are on average 7.3 crime reports per day and station. The largest share of informations and charges is for property crimes. ‘Other’ informations include non-crime incidents such as escaped prisoners or lost and found reports, while the property and violent categories refer to actual crimes. Looking at the total number of crimes across years (1828 versus 1830), we see a reduction in the average number of violent crimes of 40%, which persists until 1832, but a small increase in property crimes (8%) and other crimes (4%). These increases are driven, however, by increases in the number of charges (*cleared crimes*), while there are decreases in uncleared crimes – stolen property reports (25%), property informations (31%), other informations (52%).

Figure 8 illustrates these patterns: Panel A shows the weekly (Monday–Saturday) number

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<sup>39</sup> We also exclude the “Metropolitan Police Office” as this office was created only in April 1831. This is only a relevant restriction for the larger estimation window; most of the analysis focuses on 1828-1830. As in the Old Bailey analysis, a relevant question is the possibility of crime displacement. The jurisdiction of the magistrates in the police offices includes all of London, which implies that we capture the total effect (including displaced crime, if it existed). Moreover, 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 introduction and the baseline results are robust to including the Thames in the analysis (available on request).

of crime incidents (all measures combined) by crime category, while Panels B-D show each component separately. As suggested by the summary statistics, there is an overall decrease in informations and stolen property reports and an increase in charges after the introduction of the Met. The figures do not suggest that this is purely due to crime trends over time: we do not see continued decreases or increases in the years after the reform (1830-1832).

*Main Empirical Specification and Results (Daily Police Reports)*

Equation (2) presents the baseline pre-post model used to estimate the effect of the introduction of the Metropolitan Police on daily crime reported to the different police offices:

$$(2) \quad Y_{ywdi} = \beta PostMet_{ywd} + \alpha_w + \alpha_d + \alpha_i + \varepsilon_{ywdi}$$

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

Table 6 presents the baseline pre-post results using the daily crime reports for each outcome: the number of all cleared and uncleared incidents (Panel A), any and number of informations (Panels B and C), any stolen property reports (Panel D), and number of charges (Panel E). Column (1) shows the raw pre-post difference when the sample is restricted to one year before and after the reform only (i.e. 1828 and 1830) including all crime categories. There is a significant reduction in the chance of observing *any* informations by 15 percentage points (32% relative to the 1828 mean), the *number* of informations by 0.302 (38%), and the likelihood of any stolen property incidents by 9.8 percentage points (25%). In contrast, there is an increase

in the total number of charges by 0.88 (16.6%). Panel A shows that the combined effect on the total number of incidents is positive (0.34 or 5.1%) and marginally significant. We build up to the baseline specification by adding police office fixed effects in column (2) and calendar week and day of the week fixed effects in columns (3) and (4); these have little impact on the raw estimates.<sup>40</sup> Column (5) includes the daily reports for January to April of two additional post-reform years (1831 and 1832). For all outcomes, the point estimates increase while their sign and precision are unchanged. We study possible reasons for this pattern in Table 7.

Columns (6) to (8) of Table 6 look at property, violent and other crimes. Not surprisingly, these results are consistent with the comparison of pre- and post-means. For informations, we see negative point estimates for all crime categories, with a reduction of any property and violent informations of 24% and 57%, respectively. For charges, there is a more heterogeneous pattern: charges increase by about 21% for property crime and decrease by 26% for violent crime. The overall effect on crime incidents (Panel A) is such that there is a significant increase in property crime (8%) and reduction in violent crime (39%), but no effect on other crime.

To interpret these results, one must recall the potential differences between crime measures. Both informations and stolen property reports are proxies for criminal incidents when there is either no known suspect or the suspect has not been caught: these are *uncleared* crime incidents. For charges, on the other hand, there is always a known suspect in hand – in this sense, they are *cleared* crime incidents. The combined measure of cleared and uncleared incidents is our best proxy for total crime incidents. The above analysis finds that the introduction of the Met significantly reduced violent crime incidents (both cleared and uncleared), but for property crimes, there was a reduction in uncleared but increase in cleared offenses, with the magnitude of the latter being larger. That is, for property crime, it appears as if the apprehension/clearance effect of the police is greater than the deterrence/incapacitation channels. Why? One reason is that 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. Did the police actually deter crimes, or just substitute crimes from the uncleared to cleared categories? It is impossible to say for property crime, but the total reduction for violent crime suggests that there was at least some true reduction in criminal behavior.<sup>41</sup>

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<sup>40</sup> Appendix Table A5 presents a number of robustness checks, including estimates: (i) at the weekly instead of the daily level, (ii) excluding incomplete weeks of data, as occur at the beginning of each year or in weeks with holidays, (iii) excluding one office at a time to rule out that our results are driven by one particular office, and (iv) based on alternative specifications, including logarithms of the dependent variable (where appropriate).

<sup>41</sup> The crime reductions in the daily reports is not driven by a shift in reporting from the old offices to new police stations. As discussed earlier, there were no new police stations during this period (except Scotland Yard) and the magistrates housed in these offices were responsible for processing crimes both before *and* after the Met.



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

This section aims to better understand the dynamic effects of creating the Met. As described in Section 2.1, there were two initial hiring waves, the inner divisions in September 1829 and the outer divisions in February 1830.<sup>42</sup> We take advantage of this two-stage initial hiring and estimate a specification that allows for different coefficients on the treatment variable in (i) January 1830 (after the introduction of the Met and before the second hiring wave), (ii) all other months in 1830 (after the second hiring wave), (iii) 1831 and (iv) 1832. That is, we estimate the baseline specification presented in equation (2), but decompose the treatment into multiple time periods. We can thus study the immediate effect of a large hiring wave in February 1830 (and thereby implicitly allow for heterogeneous effects of the two hiring stages) and whether the impact of the formation of the Met changes over time. Table 7 shows the results for the combination of all incidents in columns (1) – (3), the number of charges in (4) – (6), any informations in (7) – (9), and stolen property reports 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 the clearance (charges) of property crimes does not kick in until the second wave. This may mean two things: (i) Visible police (notwithstanding low quality) may deter crime even if they do not increase clearance rates, and (ii) clearance rates may have been higher already in those places with the first wave of hiring than those locations affected by the second hiring wave.

### ***3.4. Summary and Discussion of the London Metropolitan Police Findings***

The first key takeaway of the London Met analyses is that there is convincing evidence of the immediate implementation of the Metropolitan Police Act in Old Bailey police witness testimony, with the greatest ‘treatment’ in the more intensively patrolled 4-mile radius around Charing Cross. Second, using the Old Bailey data, we found that the Met significantly (and persistently) reduced robbery trials by about 40% but no evidence of an effect on homicides and little evidence for burglary. Given that trials represent cleared crimes, this suggests that the crime reducing effects of deterrence and/or incapacitation dominate any apprehension and reporting effects for the violent crime of robbery. The lack of a crime-reducing effect for burglary at the Old Bailey, however, does not rule out a deterrence effect, as it could be the case

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<sup>42</sup> There is not, however, a one-to-one mapping of pre-existing police offices to the new Met Police divisions.

that a higher clearance rate completely offsets such an effect. Third, our pre-post analysis of the daily police report data supports these interpretations of the Old Bailey results: A significant reduction in (both cleared and uncleared) violent crimes, including robbery, is observed, while evidence of off-setting channels are seen for property crime: a reduction in uncleared crimes (which could include deterrence) but increase in cleared crimes. We hesitate to say more than that the findings from the two analyses are consistent with each other, or to make direct comparisons of the Old Bailey and daily reports analyses: the selected Old Bailey offenses represent just a small subset (the most serious) of the wide range offenses in the daily reports. Fourth, we find little evidence of spill-overs in policing or crime displacement.

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

##### **4.1. County Data**

Our evaluation of the roll-out of county police forces uses manually transcribed archival records to measure police force creation and crime. Year of force formation and initial size for each county (see Appendix Table A6) were obtained from a Police History Society book (Stallion and Wall, 1999). Annual force size data is only available in the *Judicial Statistics* yearbooks after the 1856 mandatory creation of forces. Figure 9 illustrates the roll-out of county forces in a map of England and Wales, demonstrating that (i) there is no obvious clustering in the years of force creation by neighboring counties and (ii) the earliest reformers are not just those closest to London or Middlesex.<sup>43</sup> Figure 10 demonstrates the evolution of the number of county forces over time: 16 counties created forces in 1840 (just after permission was granted), 2 in 1841, 1 each in 1843, 1844, 1848, 1851 and 1852, 2 in 1856, and 23 in 1857 (when mandatory).<sup>44</sup>

Before the 1856 Act, the only systematic measure of crime that we can collect from the *Judicial Statistics* is the annual number of persons committed or bailed for *trial*; see Appendix Figure B2 for a sample page of data (in one county and year). This measure is available for both the entire period and the six main crime categories: class 1 (offenses against persons), class 2 (offenses against property with violence), class 3 (offenses against property without violence), class 4 (malicious offenses against property), class 5 (forgery), and class 6 (other). Appendix Table A7 lists the specific offenses included in each class. Our analysis focuses on the total number of charges as well as three broad sub-categories: violent (crimes against person and

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<sup>43</sup> The map's boundary data are based on the 1851 registration districts of England and Wales and was downloaded from [https://vision.port.ac.uk/downloads/download\\_free/boundaries.jsp](https://vision.port.ac.uk/downloads/download_free/boundaries.jsp).

<sup>44</sup> As in the main analysis, these figures exclude York, Sussex, Suffolk and Middlesex counties because forces were created for sub-county level areas in different years and crime data are only available for the whole county.

violent property offenses), property (non-violent property), and other property crimes (malicious property and forgery). We omit the ‘other’ other offenses (class 6), as they vary widely by type and severity, making it hard to give them a clear interpretation.<sup>45</sup>

A potential disadvantage of using trials to measure crime is that it may confound changes in prosecution behavior (in which the police may have played a significant role at the time) with changes in criminal behavior. However, Appendix Figure A5 demonstrates that all three crime measures available in the *Judicial Statistics* after 1857, i.e. trials (our measure), total number of indictable crimes committed and the total number of individuals apprehended for indictable offenses, move in lock-step until the early 1890s. Another potential concern is the impact of the 1855 Criminal Justice Act on the number of trials. The Act gave judges the ability to summarily deal with larceny cases, which is reflected in the large decrease in the number of trials in the year before the mandatory creation of the police forces, specifically for property offenses (see Panel A of Figure 11). Given that this Act is a national shock (comparable figures by county are available upon request), our difference-in-differences approach mitigates related concerns. Moreover, we estimate the effect of creating a force for two categories unaffected by that reform (violent and other) and for the early reforming counties using a sample period prior to 1855.

Finally, we use available census records from 1851 and 1861 to generate relevant control variables at the county level: the share male, married, native, in various age groups, unemployed or out of the labor force, and farmers.<sup>46</sup> We have coded the annual county population from the *Judicial Statistics* after 1857, and use the 1851 and 1841 censuses to estimate the population in earlier years.<sup>47</sup> We use this population variable to create crime *rates*.

#### ***4.2. Sample Creation, Treatment Definition and Summary Statistics***

We use a difference-in-differences model to estimate the impact of creating rural county police forces on crime. We restrict our sample to rural county jurisdictions for which we can both reliably identify the year of force creation and measure crime. The raw data include 52 counties; we drop Middlesex, York, Sussex and Suffolk, as these counties represent aggregates of regions

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<sup>45</sup> In 1834, there was a change in the table title from the number of persons committed to the number of persons committed or bailed. We therefore demonstrate the robustness of our results to beginning the sample in 1835.

<sup>46</sup> We obtained the census data from *North Atlantic Population Project, UK Censuses*.

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

<sup>47</sup> For 1851 to 1857, we assign counties the population reported in the 1857 *Judicial Statistics* (which in turn were based on the 1851 Census), and we use the 1841 Census for the years before 1851. Note that the county population in the Census includes the entire county population whereas the county population in the *Judicial Statistics* includes the rural areas of the counties only (i.e. the catchment area of the county police force). To use consistent measures of population, we thus weight the 1841 Census measures with the share of the rural population as in the 1851 Census (i.e. the ratio of the county population in the *Judicial Statistics* compared to the 1851 Census).

with initially separate forces (but with crime data only available at the aggregate level).<sup>48</sup>

Consistent with the reporting year for crime data in *Judicial Statistics*, our main treatment variable measures whether county  $c$  has a professional force in fiscal year  $t$ , ending on September 29 of that year. Specifically, we identify whether a county had an existing police force for *any* or *all* of the fiscal year; for the former, the first treated year is typically only partially treated whereas for the latter, the first treated year is fully treated. The above-described treatment only captures whether there existed any professional county police force, but nothing about the quality of the force. One measure of quality explicitly used by the inspectors tasked with annually certifying each force is the relative size of the force, i.e. the number of people per officer in the county. We can measure this upon force formation, and use this to characterize whether the new force was *sufficiently* large. Finally, our baseline analysis uses a window of eight years before and after the earliest and latest reform years, respectively, i.e. 1832 to 1865, but also conduct sensitivity checks with respect to the start and end years of the sample.

Table 8 presents summary statistics for all analysis sample counties (N=48) and for those characterized as early (1839 or 1840), mid, and late reformers (after the 1856 Act was passed). The average number of charges per year (for all counties over the entire time period) is 367, which corresponds to 1.79 charges per 1,000 population (1.3 property, 0.3 violent, and 0.06 other, respectively). 75% of the counties are in England and the average county population was close to 200,000 in 1858. It is also clear that the police forces became larger relative to the population over time: the ratio of people to police averaged 2,857 at the time of force formation but was down to 1,700 by 1858. In terms of characterizing early, mid and late reformers, Table 8 shows that early reformers were on average largest in terms of population and acreage, while the mid-reformers were smallest in both of these measures. In addition, the earliest reformers did *not* have the highest crime rate (based on the whole time period): the average crime rate per 1,000 population was 1.9 for early reformers, 2.5 for mid-reformers, and 1.5 for late reformers.

### ***4.3. Empirical Approach: County Police Force Formation***

The difference-in-differences model is presented in equation (3):

$$(3) \quad \text{Crime}_{ct} = \beta \text{Force}_{ct} + \alpha_c + \alpha_t + X_{ct}\theta + \varepsilon_{ct}$$

The dependent variable, *Crime*, is the number of persons committed to trial in county  $c$  and

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<sup>48</sup> Forces were created in East and West Sussex in 1840 and 1857; near the end of 1856 and beginning of 1857 for the York sub-parts and 1840 and 1845 for the Suffolk sub-parts.

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

For  $\beta$  to represent the causal effect of creating a professional force on crime, we make the usual parallel trends assumption that the change in crime (trial) rates in treated counties would have been the same as in control counties in the absence of the reform. Panel B of Figure 11 illustrates the plausibility of this assumption by presenting the average annual log charges separately for the early, mid and late reformers. Crime rates are remarkably parallel for these three groups. We more formally test the plausibility of the parallel trends assumption in an event study design allowing for differential effects leading up to the reform. Another identifying assumption is that the timing of police force formation is random. Anecdotally, this seems reasonable, at least for the earliest and latest reformers. The earliest reformers created a force right after the passage of the 1839 Act, but did not lobby for this Act or know it was coming. The last reformers only created a force when they had to after the 1856 Act; again, (to the best of our knowledge) they did not know it was coming. We test this assumption in Section 4.5.

In analyzing the formation of county police forces, the same potential confounders of increased reporting and/or clearance rates exist as in London. Using trials to measure crime only allows us to estimate the combined effect of deterrence/incapacitation and these confounders. Without any measure of uncleared crime incidents, we can only detect a deterrence and/or incapacitation effect if it is larger than these offsetting channels: a null or increasing effect of police on charges does not rule out the existence of such a crime reducing effect, but does not allow us to detect it in the data.

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

Table 9 presents the results of estimating the baseline specification for the estimation window

1832 to 1865. The dependent variable is the log number of trials in columns (1) and (2) and the log number of trials per capita (crime rate) in columns (3) and (4). Panel A considers all charges while panels B to D consider violent, property, and other property charges, respectively. The variable of interest, *Force*, is equal to one in any county-year combination in which there exists a police force for at least part of the year (columns (1) and (3)) or all of the year (columns (2) and (4)), and equal to zero otherwise. The first insight from Table 9 is that the creation of a police force, on average, does not have a significant effect on overall, violent, property or other crime. Second, the estimates are comparable when using the log number of crimes or the log crime rate; going forward, we emphasize the log number given the measurement error concerns in the denominator of the crime rate. Third, estimates are comparable when defining treatment as having a police force for any or all of the year; going forward, we use the latter (which should if anything bias us against finding a crime-reducing effect).

The results in Table 9 show the effect of creating *any* police force, regardless of its quality. Yet, some forces may have been in name only or thought to be insufficiently large by the inspectors. The lack of an overall effect of force formation on crime could be masking differential effects of forces of varying quality. We use the relative size of the force, i.e. the number of people per policeman, to assess whether the effect on crime depends on whether the force is sufficiently large. In studying that question in an expanded specification, we must rely on the added assumption that ‘sufficiently large’ is conditionally random. Though the 1839 Act recommended having 1,000 people per policeman, few (if any) forces initially achieved this standard. Some initial evidence regarding the determinants (or lack thereof) of force type can be seen in Table 8. Simply put, it is not just early reformers (maybe particularly motivated counties) that had a sufficiently large force (using a 1,500 threshold); rather, similar proportions of early (20%), mid (33%) and late (17%) reformers were sufficiently large at formation. We return to the determinants of the relative size of the new forces in the next section.

Table 10 estimates the impact of sufficiently versus insufficiently large force formation, using various thresholds in defining ‘sufficient’, from 1,500 people per policeman in column (1) to 2,500 in column (5). Under the strictest and weakest thresholds, there are 10 and 30 sufficiently large forces, respectively. There is a differential impact of force size: Column (1) shows that creating a sufficiently large force with less than 1,500 people per policeman decreases the overall number of crimes by approximately 19%; this effect is seen for both violent and property crime (18% and 14%, respectively) but not for other offenses. In contrast, creating an insufficiently large force does not significantly affect crime overall; instead, it (insignificantly) increases the number of property and other crimes. It is the positive effect of

such forces on the largest crime category of property offenses that is masking the crime reducing effect of creating a sufficiently large force in the baseline regressions. While the crime reducing effect of a sufficiently large force gets smaller as we relax the sufficiency threshold in columns (2) to (5), we still see an overall reduction in crime.

To study the dynamic effects of force creation, we estimate event-study specifications where we interact our treatments (creation of sufficiently and insufficiently large forces) with dummies for two-year intervals leading up to and following the reform. The omitted category is the two years immediately prior to the first fully treated (fiscal) year. The results are shown in Figure 12 for all crimes categories combined, and for each offense category separately in Appendix Figure A6. The top and bottom panels of Figure 12 present the estimates for the sufficiently and insufficiently large forces, respectively; note that both come from the same regression. The following conclusions can be drawn: First, the negative effect of sufficiently large police force formation on crime is generally not immediate but starts around three years after the reform. Second, the crime-reducing effects of sufficiently large forces become larger in magnitude over time. Third, for forces that were insufficient in size upon creation, no negative effect on crime is seen in any of the eight years after the force is created. These event study specifications also provide tests of the plausibility of the parallel trends assumption and the ‘random’ timing of force creation: There are no significant differences in crime rates in the years leading up to the reform for either (in)sufficiently large forces (neither overall nor by crime category). Additional robustness and identification tests are presented in the next section.

#### ***4.5. Sensitivity and Identification Tests for County Police Analysis***

Appendix Table A8 presents a sensitivity analysis of our main finding that only the creation of a *sufficiently large* force visibly reduces crime (using the 1,500 threshold). The results are 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) reducing the sample period by three years on both sides of the window, (iii) breaking the sample into two periods: 1832 to 1849 (identified off early reformers) and 1850 to 1865 (identified off late reformers), and (iv) restricting the sample to the 36 English counties (excluding Wales).

We next turn to tests of the identifying assumptions of randomness in (i) the timing of force formation and (ii) the relative size of the created force. One potential question is whether forces were adopted as a result of crime being displaced from neighboring counties that had previously adopted a force. We first look at this directly in the context of the 1829 introduction of the London Met: Did this increase crime in the surrounding counties, and trigger force

adoption? Though the patterns in Figure 9 suggest otherwise, Appendix Table A9 assesses this more formally: 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 of London and Middlesex). No clear pattern emerges that would suggest a systematic displacement of crime to neighboring counties of London. 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 as a response to the 1829 reform (see Panel B).

Appendix Table A10 looks more broadly, i.e. for all counties, at the determinants of being an early reformer (adopted by 1840) in columns (1)-(6) and the year of adoption for all counties in columns (7) – (13). For the latter, the dependent variable is a dummy equal to one in the year a county creates a force and zero in prior years; counties exit the sample once a force is created, as there is no longer a choice to be made.<sup>49</sup> We consider the role of: the county's own lagged charge rate, whether the neighboring counties had forces (and their relative sizes), population, the lagged charge rate in neighboring counties, and the number of initial police officers in the county area who are not part of the county force (measured before the formation of the county force and includes previously existing borough police). We find little evidence that these variables predict the timing of adoption. The most consistently significant variable is population: larger counties were significantly more likely to adopt early, but (i) the point estimates are close to zero, (ii) county fixed effects control for larger versus smaller counties, and (iii) population does not predict timing for later adopters. Having an insufficiently large neighboring force decreases the chance of adoption in a given year, raising the question of whether creating a force has spill-over effects on nearby counties, which we address shortly.

Appendix Table A11 looks at the determinants of initial relative force size: people per police and dichotomous variable indicating whether there were less than 1,500 people per police. We consider all potential determinants available, including fixed geographic variables (acreage, number of parishes and neighboring counties, English vs. Wales), 1851 census variables (share farmers, male, married, native, employed, age groups, and population), crime rates, and whether any neighboring county had sufficiently or insufficiently large forces in the year before force formation.<sup>50</sup> There is little information, including crime in the year before

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<sup>49</sup> 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.

<sup>50</sup> These regressions are purely descriptive. 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).



formation, which consistently predicts the type/size of the police force. Moreover, to the extent that these variables are constant over time, they are captured by county fixed effects.

Finally, Table 11 assesses the robustness of our results to spill-over effects of neighboring forces. Specifically, we estimate the effect of having an (in)sufficiently large force (using the 1,500 threshold) while controlling for whether (i) a neighbor in year  $t$  (i.e. a border-sharing county) had *any* force or (ii) a neighbor in year  $t$  had a *sufficient or insufficient* force. In all specifications, controlling for neighboring county forces has no impact on the baseline estimates (though the coefficients on neighboring forces are themselves significant): having a sufficiently large force still decreases overall crime by almost 19%. Having a sufficiently large neighboring force significantly decreases local violent, property and other crime, while an insufficiently large neighboring force increases local property crime. What could explain these patterns? There are a number of potential channels (which we unfortunately cannot disentangle) through which a relatively large neighboring force can decrease local crime. One possibility is cooperation between forces, which to some extent would reflect ‘true’ spillovers in policing. This was even a criterion that the inspectors evaluated; to the extent that a large force signals high quality, it could also be correlated with a high degree of cooperation. Another possible channel is that a large enough neighboring force decreases crime by incapacitating criminals who would have committed crimes in multiple counties. With respect to the increase in property crime due to a relatively small neighboring force, we can put forward one main channel: this small neighboring force does not have the capacity (or cooperative nature) to police criminals who commit their crimes elsewhere. That is, offenders may steal in one county but hide from the authorities in a neighboring (poorly policed) county. Anecdotal evidence of this is provided in the 1839 Police Commissioner’s Report (page 280, paragraph 208): “The state of insecurity produced in guarded towns by the unprotected state of surrounding districts is not confined to the facilities of escape furnished to delinquents for crimes committed within the towns, but the subsistence given in the unprotected districts to the predatory classes who harbor in the towns increases the expense of guarding against them.”<sup>51</sup>

#### ***4.6. Discussion of County Police Force Formation Results***

To summarize, the above analysis of the roll-out of professional county forces has four key findings. First, the creation of ‘sufficiently large’ county forces reduces trials overall and for both violent and property crime. Second, the formation of ‘insufficiently large’ forces does not

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<sup>51</sup> <https://babel.hathitrust.org/cgi/pt?id=uc1.a0000872473;view=1up;seq=7>, last accessed April 28, 2019.

have an observable crime (trial) reducing effect. Third, the effect of creating a sufficient force is not immediate and increases over time. Fourth, the introduction of the *London Met* did not displace crime to neighboring counties. Fifth, there are spill-over effects of neighboring *county* forces, with an insufficiently (sufficiently) large neighbor increasing (decreasing) ‘local’ crime.

What do these findings tell us about why the creation of a county force decreases crime? On the one hand, there are two main channels through which crime can be reduced: deterrence and incapacitation. On the other hand, creating a police force might increase measures of ‘crime’ through increased reporting of crime incidents and apprehensions. The net negative effect for sufficiently large forces suggests that deterrence and incapacitation outweigh reporting and apprehension channels. However, while (anecdotally) the aim of the new forces was deterrence, we cannot empirically disentangle it from incapacitation (the same is actually true in our London analysis). Finally, the increase in the size of the crime-reducing effect over time highlights the importance of quality: Force ‘quality’ clearly improved over time as people per officer ratios decreased, supervisors were increasingly hired, and experience was gained.

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

## **5. Conclusion**

This paper exploits two natural experiments in history – the introduction of the London Metropolitan Police in 1829 and the subsequent roll-out of county police forces throughout England and Wales – to estimate the effect on crime of the *introduction* of professional forces, which were for the first time explicitly tasked with deterrence. In London, we find evidence consistent with a crime-reducing effect, especially for violent crimes (including robbery); for property crimes, we find clear evidence of a reduction in uncleared crimes but also an increase in cleared crimes. Our county analysis finds that introducing ‘sufficiently large’ police forces reduced crime overall and across crime categories, while relatively small forces did not have a visible net crime reducing effect.

The most comparable studies to ours are those of police deployment and those of additional, private police. Using terror-related shocks to deployment, Draca et al. (2011) and DiTella and Schargrodsy (2004) find elasticities of crime with respect to police of around -0.3, i.e. a decrease in crime of approximately 0.3% with a 1% increase in police. MacDonald et

al. (2016) study the effect of private police patrols within defined boundaries using a geographic regression discontinuity design. They find a 45-85% increase in the number of crimes outside of the boundaries of the private police catchment area, which they convert to an elasticity of -0.33 (-0.2 for property crime and -0.7 for violent crime). Our findings for both the London and county analyses are generally in line with these results. Sufficiently large county forces decreased rural crime by 19%, while the London Met decreased robbery by 40 to 46%.

Finally, the county results are based on a sufficiently large force having fewer than 1,500 people per officer. How does that compare to today's force sizes? Data from the UCR's *Crime in the United States* suggest that overall there were 3.5 law enforcement officers per 1,500 population in the U.S. in 2016 (3.9 for metropolitan areas and 4.5 for non-metropolitan areas, respectively).<sup>52</sup> Data from *Eurostat* report similar numbers for 2016 with 3.2 police officers per 1,500 population in England and Wales (4.5 in Germany and 3.04 in Sweden).<sup>53</sup> That is, using a ratio of one officer per 1,500 people as a threshold is conservative in today's terms.

Lastly, despite the historical setting, our study offers a number of contemporary insights. First, the extensive margin effect of the modern-day police institution is in and of itself still a relevant question. In countries like the U.S. and U.K., police departments and/or local police stations are being closed due to shrinking budgets.<sup>54</sup> What will be the effect of these closures on crime? The existing literature is limited in its ability to answer this question, as such an extrapolation would be based on the assumption that the marginal effects per officer (identified in the literature) apply equally (linearly) across officers.<sup>55</sup> This assumption (to the best of our knowledge) has no empirical support to date. Our study begins to shed light on this question, suggesting that the effects are not linear, as a crime reduction effect is not seen for smaller forces. This could be because of how tasks are prioritized in forces of different sizes, and how the tasks of the marginal officer change. In future work, we aim to shed more light on this question in this historical setting: does the marginal effect of police on crime change as these new forces began to grow? There are, of course, caveats in making inference to today's context from our study, including the many other institutional and societal differences between 19<sup>th</sup> century England and today and the fact that the (quasi) extensive margin effect may not be

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<sup>52</sup> <https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/tables/table-25>, last accessed March 15, 2018.

<sup>53</sup> Eurostat, "Personnel in the criminal justice system by sex" (last accessed on October 31, 2018): <https://data.europa.eu/euodp/data/dataset/4IAKUAgPA5yFoNEwJp1syA>

<sup>54</sup> See, e.g., <https://www.nytimes.com/2018/07/31/us/blandford-police-officers-quit.html> for the US and for the UK <https://www.independent.co.uk/news/uk/home-news/police-station-close-shut-budget-cuts-police-federation-a8521501.html>.

<sup>55</sup> In a recent study, Blesse and Diegmann (2018) exploit variation from police station closures in one federal state in Germany to estimate the effect of police closures on crime.

symmetric upon creation and collapse of a force. Nevertheless, given the lack of research to date on this margin, we believe our study fills a significant gap in the literature.

Finally, police forces in less developed countries today are being disbanded and new forces created in an effort to eliminate police corruption.<sup>56</sup> Our results may have important policy implications today with respect to institution building in these countries; one potential ‘lesson’ is that the quality of the institution plays a fundamental role.

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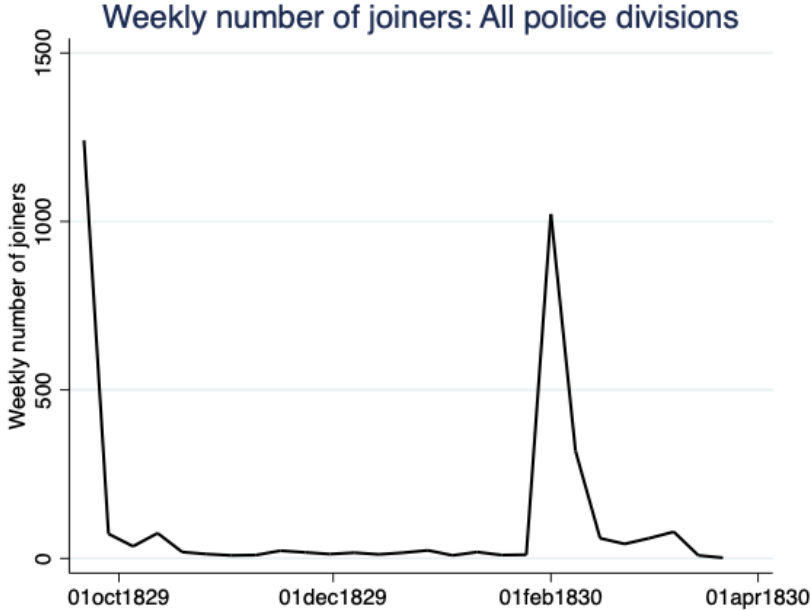
<sup>56</sup> In July 2015, 2000 new officers (carefully selected and trained from more than 34000) were placed on the streets of Kiev in the Ukraine; the old police (militsiya) were ordered off the streets. In the Republic of Georgia, the “old militsiya and traffic police were abolished, their 16000 jobs eliminated, and an all-new-in-every-way force called the Patrol Police hit the streets. They [were] also paid a decent wage, to discourage corruption” (Gessen, 2015).

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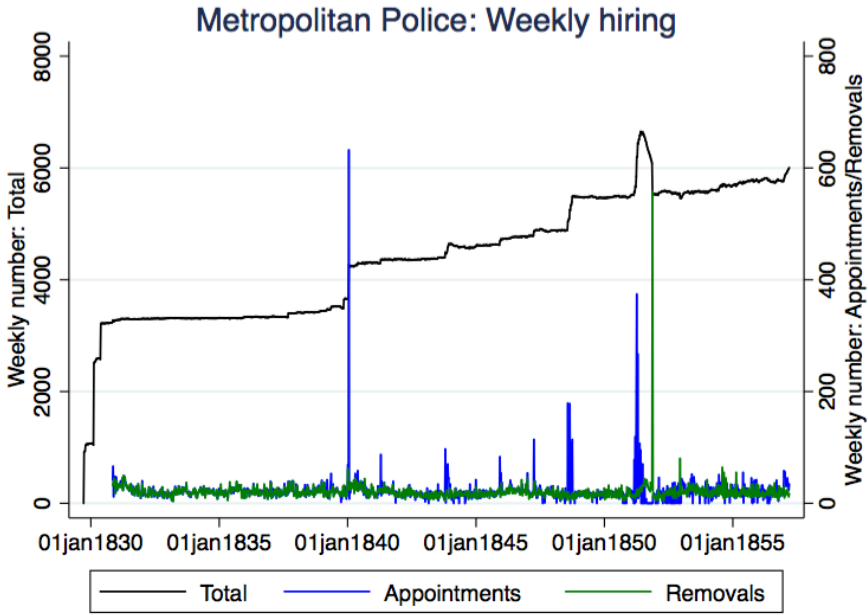
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**Figure 1. London Metropolitan Police – Weekly Hires**

*Panel A. 1829-1831*



*Panel B. 1830-1856*



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

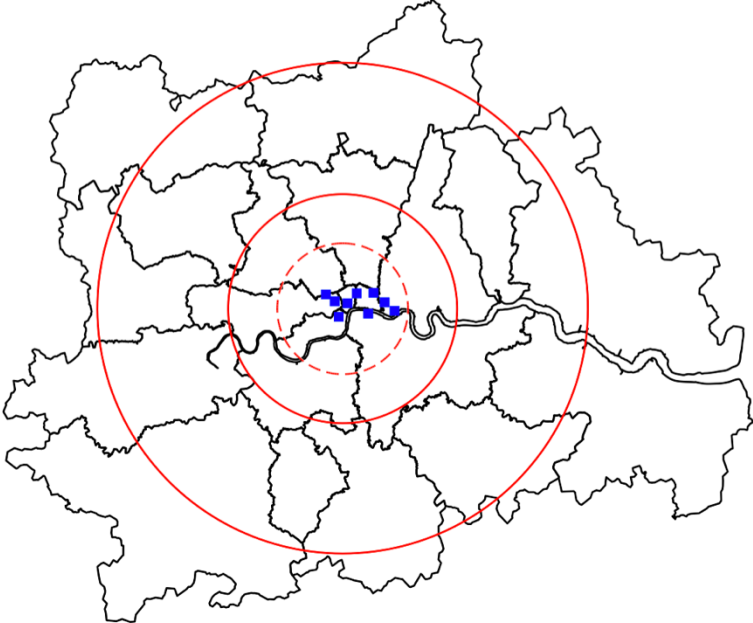


**Figure 2. The London Metropolitan Police Jurisdiction (1829)**

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



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

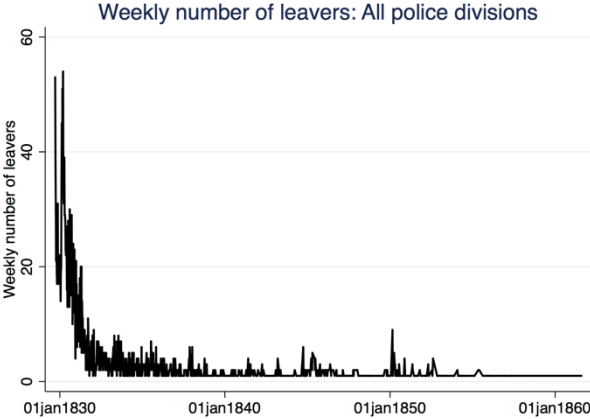


NOTES – Panel A presents a map of the original London Metropolitan Police District. Shaded in red is the City of London Police area, outside of the Met’s jurisdiction. The large letters indicate the various districts of the Metropolitan Police. The map is available from the British Library’s online map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

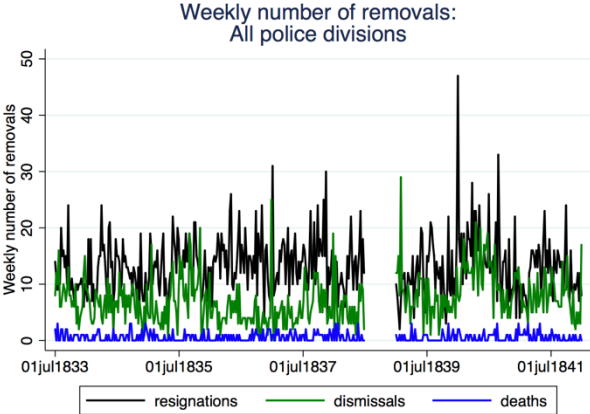


**Figure 3. London Metropolitan Police – Turnover and Quality**

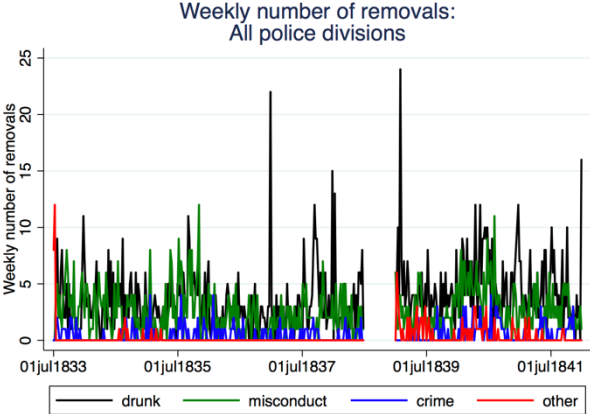
*Panel A. Weekly Number of Leavers*



*Panel B. Weekly Number of Removals, by Reason*

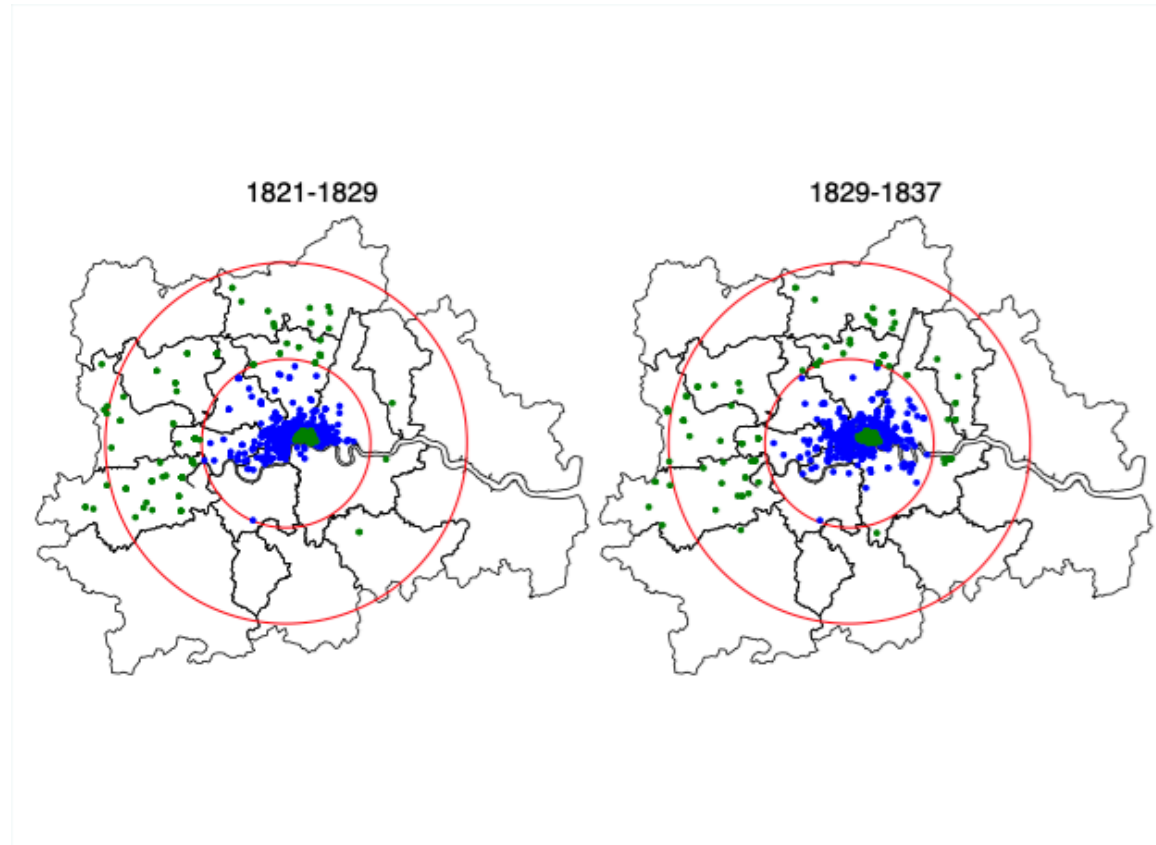


*Panel C. Weekly Number of Dismissals, by Reason*



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

**Figure 4. Geocoded Data from the Old Bailey Proceedings**



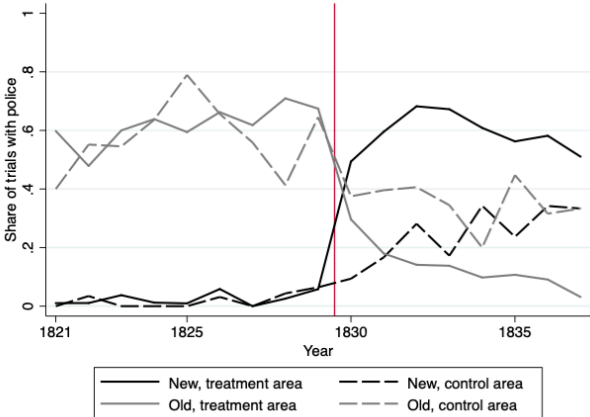
NOTES – The figure plots geocoded crime locations of murders, manslaughters, robberies and burglaries trialed at the Old Bailey between 1821 and 1837. The two red circle mark a 7- and 15-mile radius from Charing Cross, respectively. Each dot represents a trial-defendant observation; the green dots represent crime locations inside the City of London (within 7-mile radius) as well as outside the 7-mile radius and the blue dots represent crime locations within the 7-mile radius and not in the City of London. The borders represent modern day postcode areas; the respective shapefiles were obtained from Maproom’s UK Postcodes Shapefiles and contain OS, Royal Mail and National Statistics data.

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

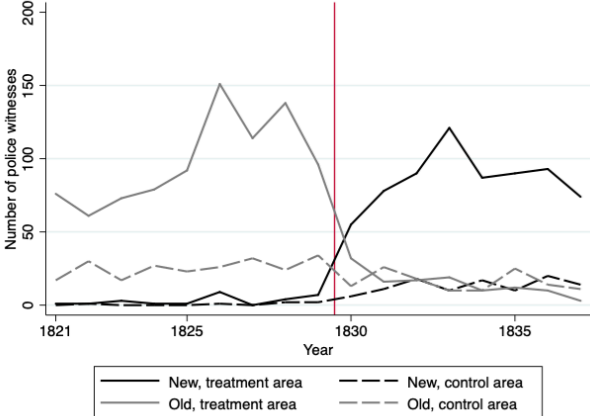
**Panel A. Presence of Any Police Witnesses at Trial**



**Panel B. Change in Type of Police Witnesses at Trial (Share of Trials)**



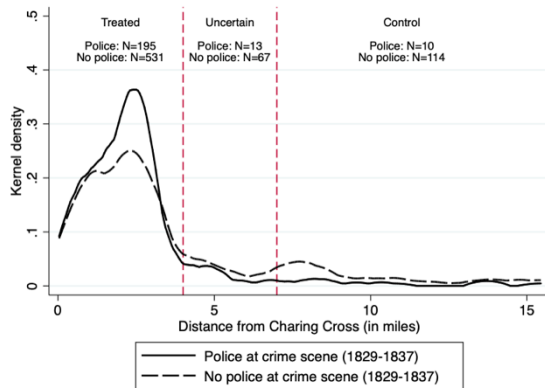
**Panel C. Change in Type of Police Witnesses at Trial (Number of Trials)**



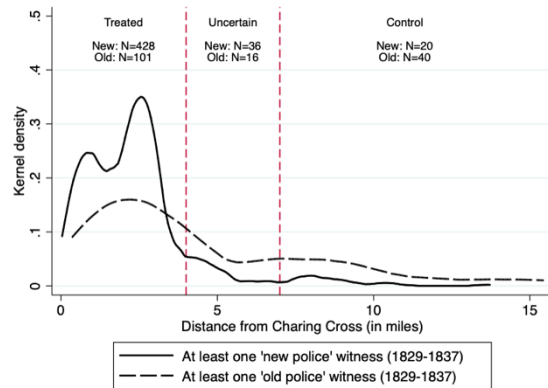
NOTES – Panel A shows the annual share of homicide, robbery, and burglary trials at the Old Bailey from 1821 to 1837 with at least one police present as a witness. The black solid line represents trials for crimes located in the treatment group (within 7 miles from Charing Cross), the grey dashed line trials for crimes located in the control group (more than 7 miles from Charing Cross or in the City of London). Panel B (C) shows the annual share (number) of trials that, among the first five witnesses present at the trial, had at least one of either the new type (black) or the old type (grey) of police both for the treatment group (solid lines) and the control group (dashed lines). See the text for details on the types of police. The red vertical line in all panels represents the timing of the introduction of the Metropolitan Police in 1829. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

**Figure 6. Spill-overs in Policing and Crime Displacement**

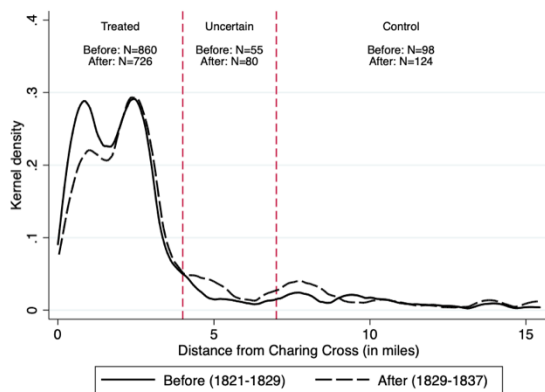
**Panel A. Police at Crime Scene**



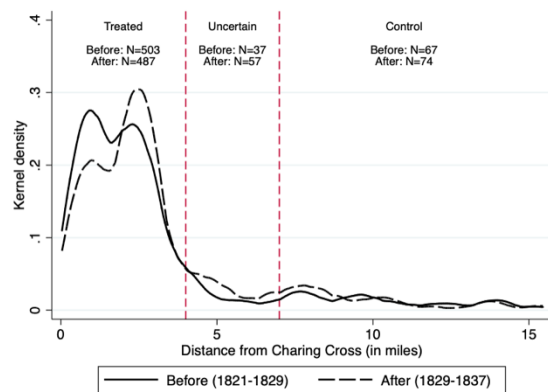
**Panel B. New/Old Type Police Witnesses**



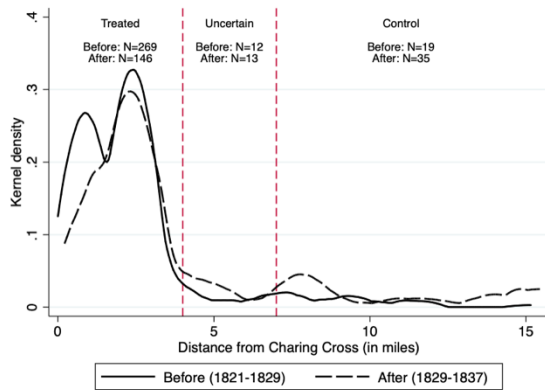
**Panel C. Crime Locations: Total Crime**



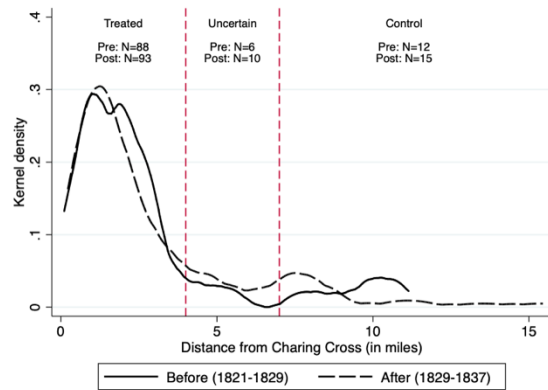
**Panel D. Crime Locations: Burglary**



**Panel E. Crime Locations: Robbery**



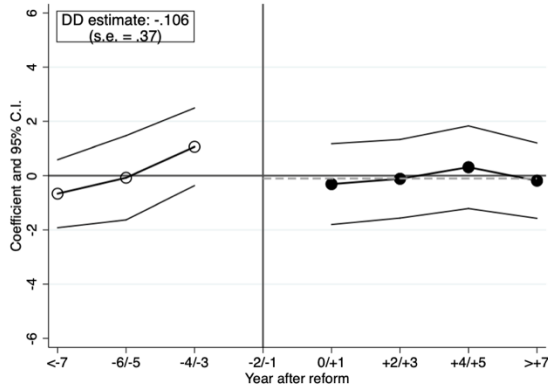
**Panel F. Crime Locations: Homicide**



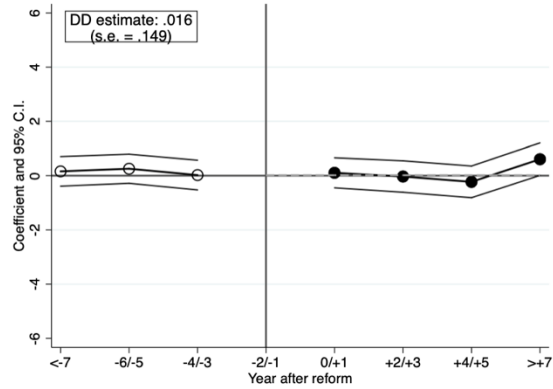
NOTES – Panel A shows the kernel density of homicide, robbery and burglary trials at the Old Bailey from 1829 (after the introduction of the Met) to 1837 with/without police at the crime scene by distance from Charing Cross (in miles). Panel B shows the respective kernel density for trials with at least one “new” or “old” type police witness at the trial (see the text for details on the types of police). Panel C to F 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 all types of crime and separately 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 7. Event-Study for Reduced Form from the Old Bailey Proceedings**

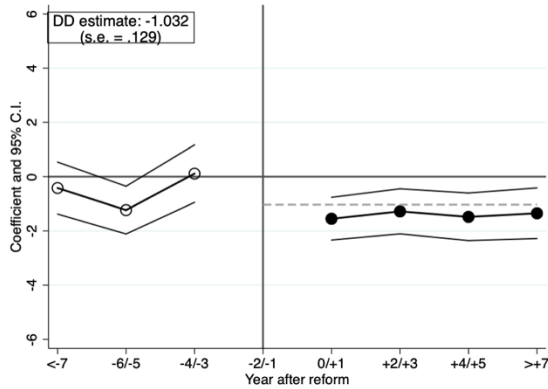
**Panel A1. Burglary, Treated**



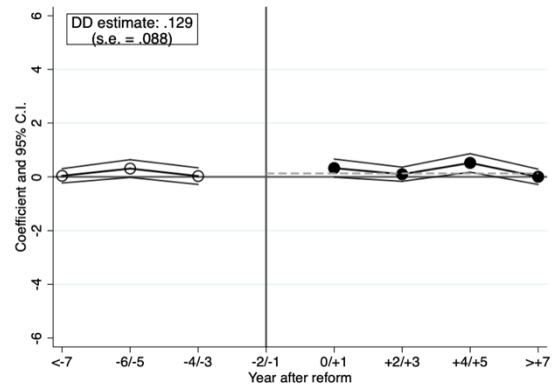
**Panel A2. Burglary, Uncertain**



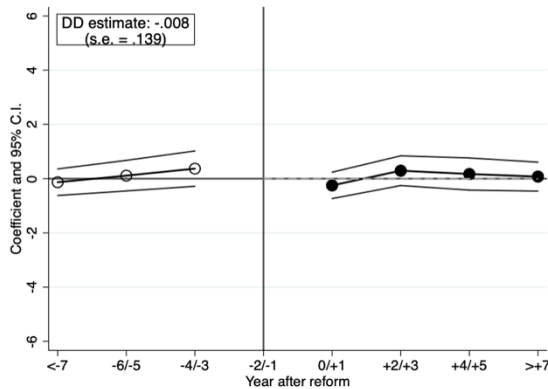
**Panel B1. Robbery, Treated**



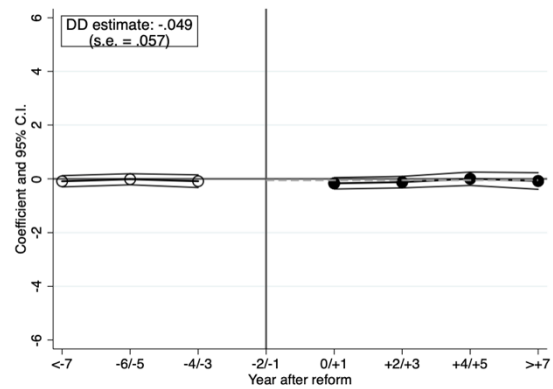
**Panel B2. Robbery, Uncertain**



**Panel C1. Homicide, Treated**



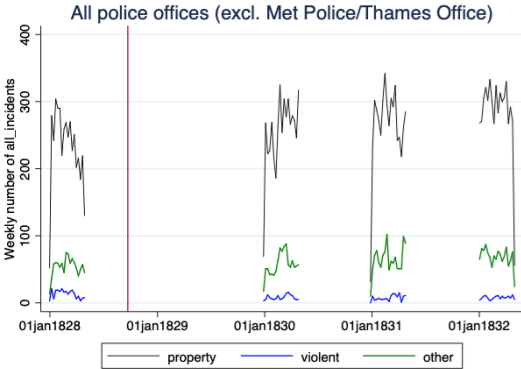
**Panel C2. Homicide, Uncertain**



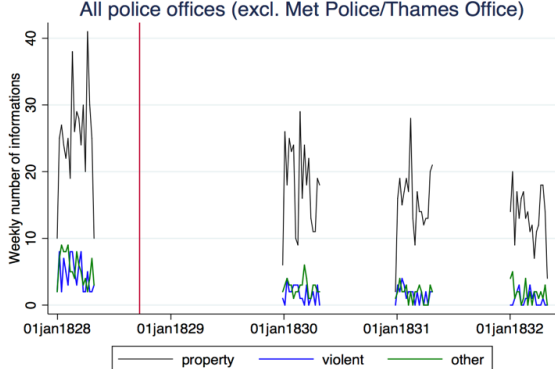
NOTES – The figures show the estimated coefficients and 95% confidence intervals corresponding to the event-study specifications described in Section 3.2. The figures to the left represent the results for the treated area, the figures to the right for the uncertain area (see text for explanations; note that the estimates come from the same regression). 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 two years before the introduction of the Metropolitan Police (September 1829) which is the omitted category. The dashed horizontal line represents the (average) diff-in-diff estimate. The figures are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations.

**Figure 8. Daily Crime Reports – Weekly Aggregated Crime**

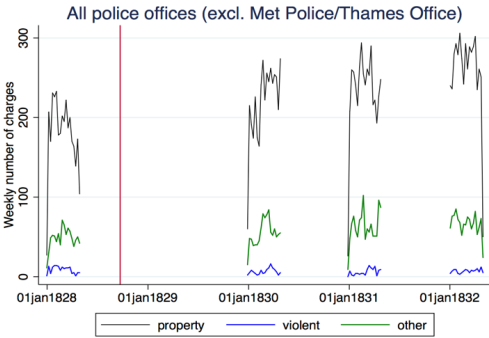
**Panel A. Weekly Number of All Incidents**



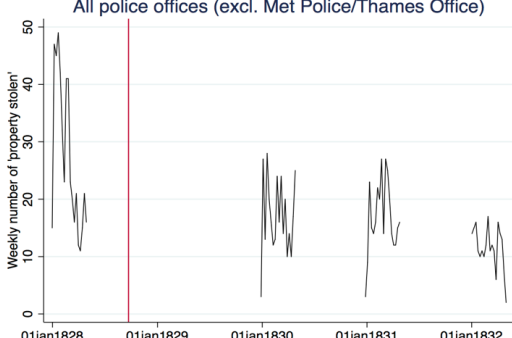
**Panel B. Weekly Number of Informations**



**Panel C. Weekly Number of Charges**

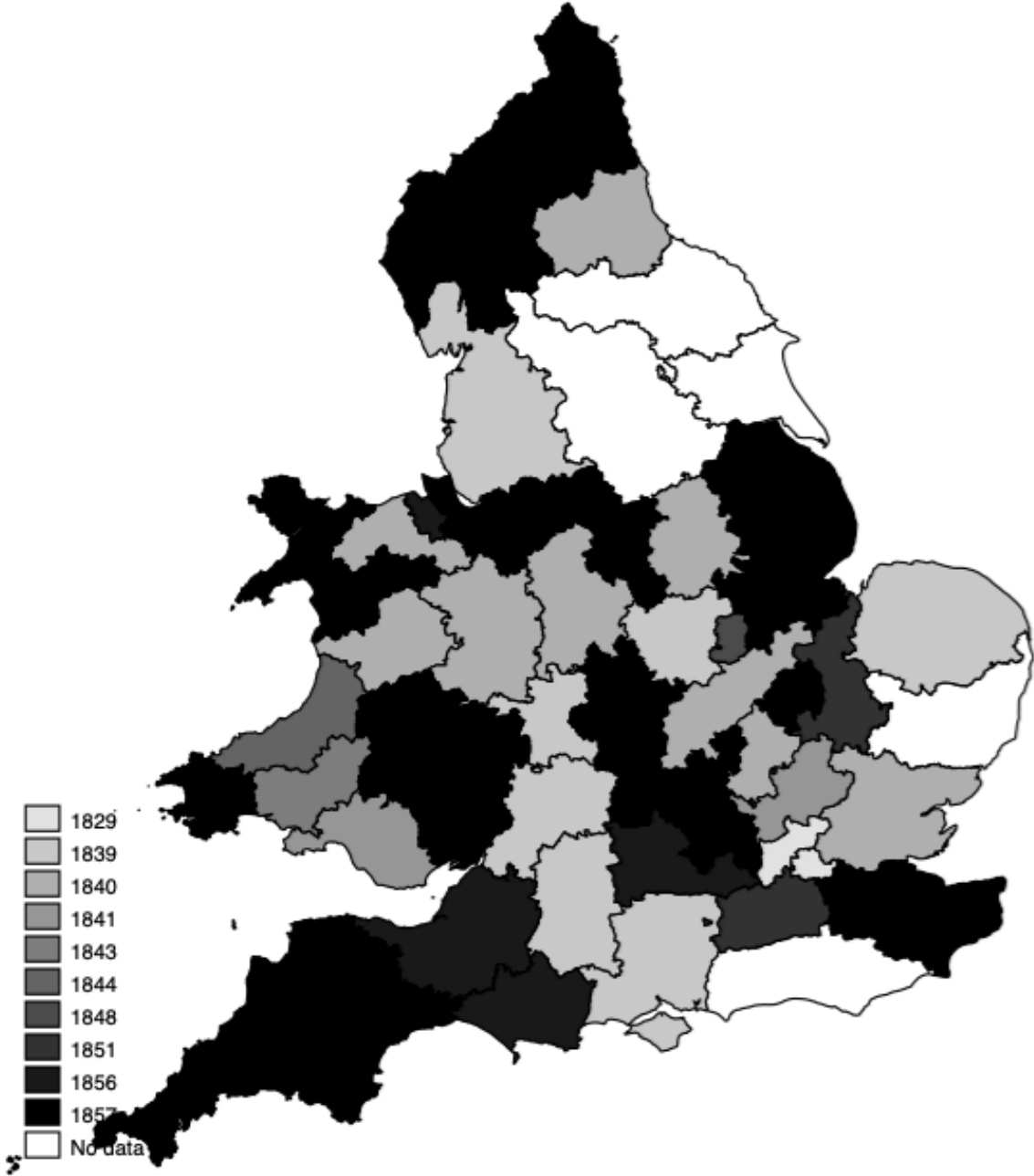


**Panel D. Weekly Number of Property Stolen Incidents**



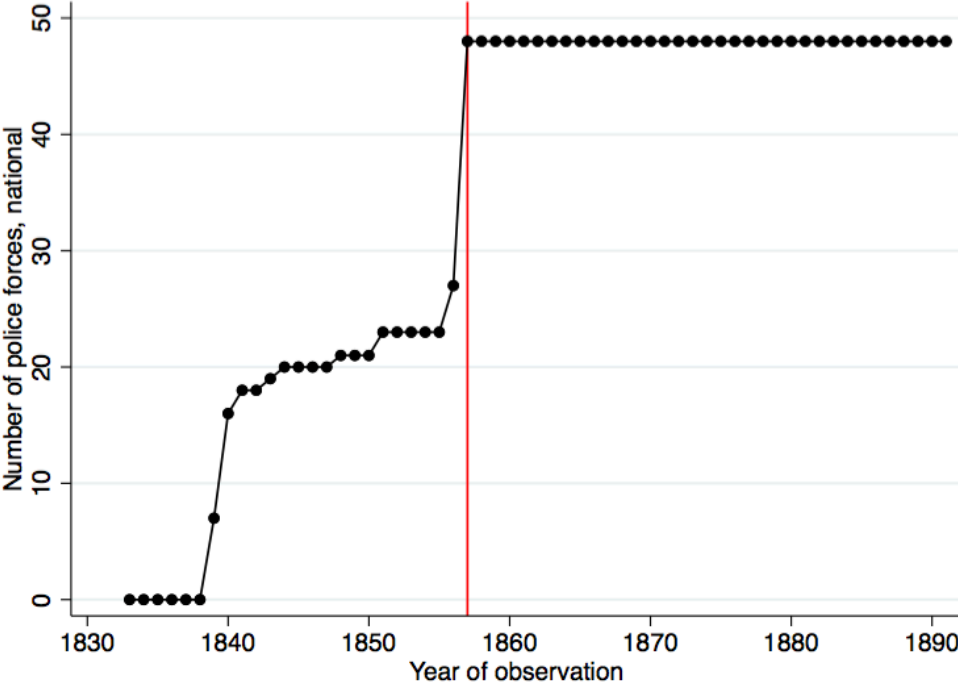
NOTES – Panel A shows the weekly number of all incidents (the sum of informations, charges and property stolen incidents) for property (black line), violent (blue line) and other (green line) for all London police offices (except Thames and the Metropolitan Police, see Section 3.1 for details). Violent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

**Figure 9. Map of Police Force Start Years for English and Welsh Counties**



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

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

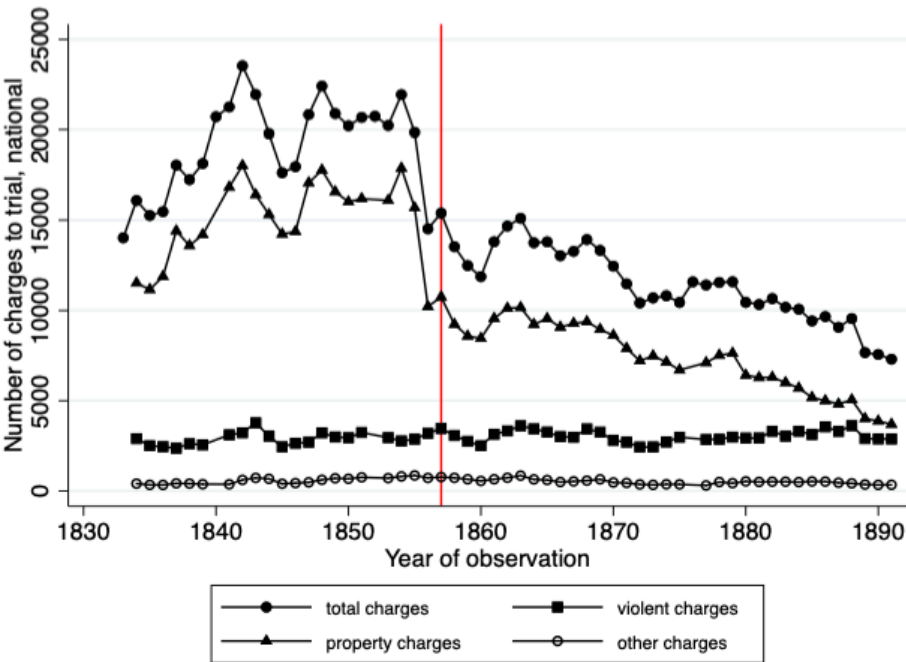


NOTES – This figure shows the number of county police forces in each year for our analysis sample of 48 counties, i.e. excluding Middlesex, York, Suffolk, and Sussex. The red vertical line marks 1857, the year when the creation of a county police force became mandatory. See Section 4.1 and 4.2 for details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

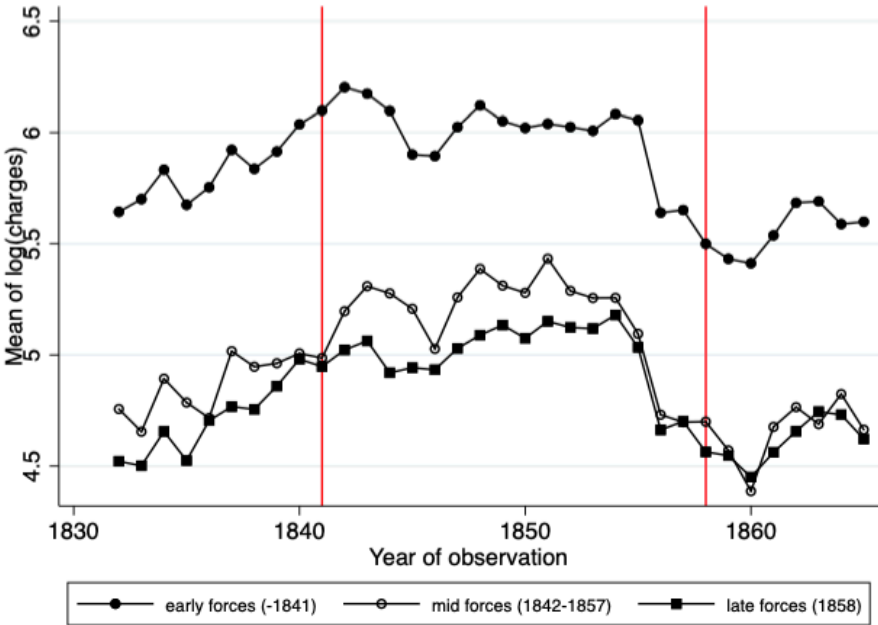


**Figure 11. County-Level Data on Charges Brought to Trial**

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



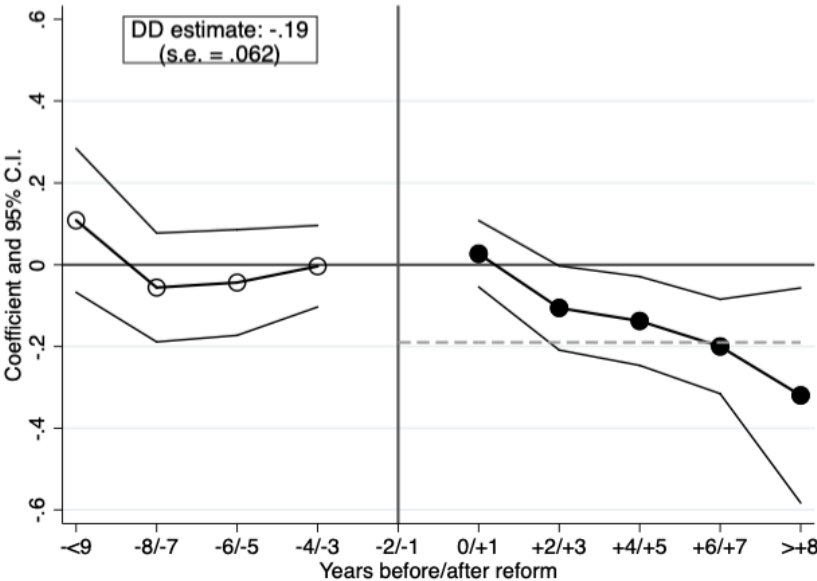
*Panel B. Average Log Charges for Early, Mid and Late Reforming Counties*



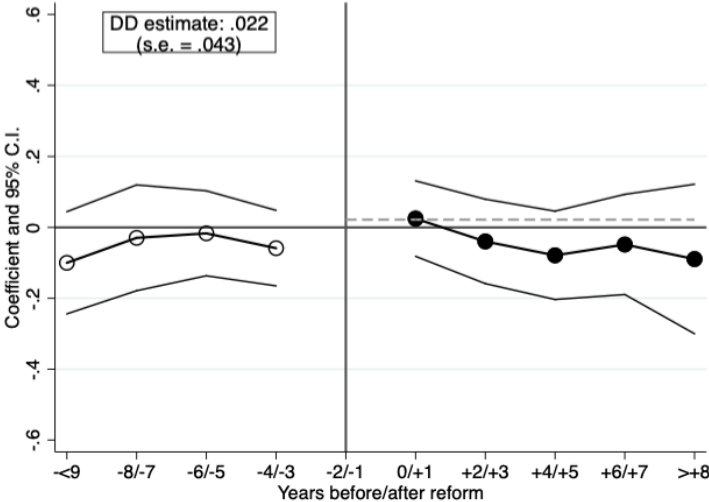
NOTES – Panel A shows the annual number of charges brought to trial in England and Wales, overall and by crime type and for all counties included in the analysis sample, i.e. excluding Middlesex, York, Suffolk, and Sussex. The red vertical line marks 1857, the year when the creation of a county police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

**Figure 12. Event-Study: Sufficiently/Insufficiently Large County Police Forces on Crime**

**Panel A: Sufficiently Large Police Forces, Log-Level Specification, All Charges**



**Panel B: Insufficiently Large Police Forces, Log-Level Specification, All Charges**



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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Any police witness		Any "new" police witness		Any "old" police witness		Police at crime scene	
Sample:	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832
<b><i>Panel A. Pre-Post Analysis</i></b>								
<i>Treated (&lt;4 miles)</i>								
Post Met	0.025 (0.018)	-0.018 (0.032)	0.574*** (0.019)	0.540*** (0.034)	-0.487*** (0.021)	-0.485*** (0.044)	0.082*** (0.021)	0.007 (0.045)
Observations	1,586	427	1,586	427	1,586	427	1,586	427
<i>Uncertain (4-7 miles)</i>								
Post Met	0.034 (0.064)	-0.071 (0.145)	0.455*** (0.057)	0.357*** (0.117)	-0.254*** (0.082)	-0.133 (0.211)	0.074 (0.057)	-0.092 (0.187)
Observations	135	31	135	31	135	31	135	31
<i>Control (&gt;7 miles)</i>								
Post Met	0.014 (0.052)	-0.007 (0.083)	0.155*** (0.033)	0.088** (0.038)	-0.056 (0.063)	0.059 (0.096)	-0.005 (0.038)	-0.052 (0.077)
Observations	230	89	230	89	230	89	230	89
<i>City of London</i>								
Post Met	-0.062 (0.039)	-0.096 (0.059)	0.268*** (0.042)	0.113 (0.081)	-0.259*** (0.056)	-0.201* (0.103)	-0.031 (0.052)	-0.156 (0.103)
Observations	314	100	314	100	314	100	314	100

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

**Table 2. Differences in Means in the Old Bailey Proceedings**

	Total			Burglary			Robbery			Homicide						
	Before	After	Δ	Before	After	Δ	Before	After	Δ	Before	After	Δ				
<i>Panel A. 1821-1837, Y = Number of crimes per month/area</i>																
Treated	8.24	7.29	-0.95	**	4.81	4.90	0.09		2.59	1.46	-1.13	***	0.85	0.93	0.08	
Uncertain	0.52	0.81	0.29	**	0.36	0.57	0.21	**	0.11	0.14	0.03		0.06	0.10	0.04	
Control	0.98	1.28	0.30	**	0.68	0.78	0.10		0.18	0.35	0.17	**	0.12	0.15	0.03	
City of London	1.46	1.62	0.16		0.79	1.14	0.35	**	0.57	0.25	-0.32	***	0.11	0.23	0.12	**
All	2.80	2.75	-0.05		1.66	1.85	0.19		0.86	0.55	-0.31	***	0.28	0.35	0.07	
<i>Panel B. 1828-1832, Y = Number of crimes per month/area</i>																
Treated	8.20	6.57	-1.63	*	4.70	4.40	-0.30		2.80	1.33	-1.48	***	0.70	0.85	0.15	
Uncertain	0.50	0.52	0.02		0.40	0.40	0.00		0.05	0.10	0.05		0.05	0.03	-0.03	
Control	1.70	1.38	-0.33		1.15	1.03	-0.13		0.30	0.28	-0.03		0.25	0.08	-0.17	
City of London	1.60	1.70	0.10		0.95	1.05	0.10		0.50	0.35	-0.15		0.15	0.30	0.15	
All	3.00	2.54	-0.46		1.80	1.72	-0.08		0.91	0.51	-0.40	**	0.29	0.31	0.03	

NOTES – The table shows the average number of monthly trials for crimes that took place before and after the introduction of the Metropolitan Police (and their difference), for all as well as each offense separately, as well as by area (separately and all areas together). The treated area includes trials for crimes located within 4 miles from Charing Cross, the uncertain area those located between 4 and 7 miles from Charing Cross, the control area those located more than 7 miles from Charing Cross and City of London those located in the City of London. Panel A shows the results for 1821-1837, Panel B for 1828-1832. The numbers are based on data from the *Old Bailey Proceedings Online* and own transcriptions/calculations; the sample includes trials for robbery, burglary and homicide. See the text for details. Statistical significance of the difference is based on corresponding before-after regressions. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample:	1821-1837	1825-1835	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1825-1835
Radius:	<i>Four miles from Charing Cross</i>								
Specification:	City of London = Control			City of London = Treated from 02 April 1832		City of London = Uncertain		City of London = Excluded	
<i>Panel A. Total crime (by month/area)</i>									
Post Met x Treatment Area	-1.146** (0.457)	-2.157*** (0.602)	-1.472* (0.797)	-0.880*** (0.286)	-1.045* (0.548)	-1.177** (0.468)	-1.328 (0.805)	-1.190** (0.468)	-1.234 (0.810)
Post Met x Uncertainty Area	0.095 (0.176)	0.074 (0.228)	0.178 (0.353)	-0.021 (0.202)	0.213 (0.370)	-0.002 (0.195)	0.360 (0.359)	0.051 (0.220)	0.416 (0.438)
<i>Panel B. Burglary (by month/area)</i>									
Post Met x Treatment Area	-0.106 (0.370)	-0.611 (0.473)	-0.294 (0.660)	0.007 (0.241)	-0.285 (0.474)	0.020 (0.382)	-0.234 (0.686)	0.047 (0.383)	-0.120 (0.701)
Post Met x Uncertainty Area	0.016 (0.149)	-0.073 (0.187)	0.006 (0.288)	0.053 (0.170)	-0.013 (0.298)	0.211 (0.163)	0.116 (0.319)	0.169 (0.186)	0.180 (0.377)
<i>Panel C. Robbery (by month/area)</i>									
Post Met x Treatment Area	-1.032*** (0.219)	-1.345*** (0.284)	-1.297*** (0.428)	-0.828*** (0.132)	-0.832*** (0.299)	-1.216*** (0.224)	-1.281*** (0.433)	-1.261*** (0.224)	-1.336*** (0.438)
Post Met x Uncertainty Area	0.129 (0.088)	0.190 (0.119)	0.228 (0.162)	0.005 (0.098)	0.288* (0.170)	-0.231** (0.104)	0.144 (0.189)	-0.100 (0.110)	0.189 (0.208)
<i>Panel D. Homicide (by month/area)</i>									
Post Met x Treatment Area	-0.008 (0.139)	-0.200 (0.186)	0.120 (0.251)	-0.060 (0.093)	0.072 (0.181)	0.019 (0.145)	0.188 (0.262)	0.024 (0.145)	0.222 (0.266)
Post Met x Uncertainty Area	-0.049 (0.057)	-0.043 (0.074)	-0.055 (0.115)	-0.079 (0.068)	-0.062 (0.119)	0.019 (0.066)	0.100 (0.133)	-0.018 (0.072)	0.047 (0.143)
Observations	816	528	240	816	240	816	240	612	180
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to equation (1). Panel A shows the results for all offenses, Panel B for burglary, Panel C for robbery and Panel D for homicide. The dependent variable is the number of crimes (that are brought to trial) per month and area. In columns (1) to (3), the treated area includes crimes located within 4 miles from Charing Cross, the uncertain area those located between 4 and 7 miles from Charing Cross, the control area those located more than 7 miles from Charing Cross and City of London those located in the City of London. In columns (4) to (5), the City of London is alternatively assigned to the treatment group after establishing their own police (1832), in columns (6) to (7) to the uncertainty group and in columns (8) to (9) the City of London is excluded from the analysis. Regressions are based on manually geocoded data from the *Old Bailey Proceedings Online* and own transcriptions/calculations; see the text for details. Robust standard errors are shown in parentheses below the coefficient. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

**Table 4. Robustness Checks – Alternative Aggregation Levels**

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	1821-1837	1825-1835	1828-1832	1821-1837	1825-1835	1828-1832
Radius:	<i>Four miles from Charing Cross</i>			<i>Four miles from Charing Cross</i>		
Specification:	City of London = Control			City of London = Control		
	Extensive margin (1/0): By Week/area			Extensive margin (1/0): By Month/distance bands		
<i>Panel A. Total crime</i>						
Post Met x Treatment Area	-0.048 (0.032)	-0.079** (0.038)	-0.022 (0.060)	-0.039 (0.033)	-0.082** (0.040)	-0.034 (0.066)
Post Met x Uncertainty Area	0.012 (0.030)	-0.006 (0.038)	-0.033 (0.056)	0.040 (0.035)	-0.009 (0.045)	-0.013 (0.066)
<i>Panel B. Burglary</i>						
Post Met x Treatment Area	-0.069* (0.036)	-0.091** (0.045)	-0.062 (0.069)	-0.032 (0.036)	-0.065 (0.045)	-0.064 (0.072)
Post Met x Uncertainty Area	-0.006 (0.026)	-0.026 (0.033)	-0.029 (0.049)	0.031 (0.031)	-0.010 (0.039)	-0.028 (0.059)
<i>Panel C. Robbery</i>						
Post Met x Treatment Area	-0.115*** (0.033)	-0.159*** (0.042)	-0.127* (0.065)	-0.116*** (0.034)	-0.161*** (0.043)	-0.168** (0.069)
Post Met x Uncertainty Area	0.020 (0.016)	0.029 (0.022)	-0.002 (0.031)	0.018 (0.018)	0.022 (0.025)	0.027 (0.030)
<i>Panel D. Homicide</i>						
Post Met x Treatment Area	0.026 (0.027)	0.000 (0.034)	0.064 (0.047)	0.000 (0.028)	-0.040 (0.036)	0.044 (0.051)
Post Met x Uncertainty Area	-0.005 (0.012)	-0.005 (0.016)	-0.007 (0.024)	0.002 (0.015)	-0.004 (0.019)	-0.008 (0.024)
Observations	3,604	2,332	1,060	3,672	2,376	1,080
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	No	No	No	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	No	No	No
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

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

**Table 5. Summary Statistics – Daily Crime Reports**

Variable	All			Before: 1828			After: 1830			After: 1830-1832		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
<i>All incidents: Informations + charges + property stolen</i>												
Number of incidents: All	3,232	7.299	3.801	800	6.685	3.569	816	7.025	3.672	2,432	7.501	3.854
Number of incidents: Property	3,232	5.748	3.264	800	5.189	3.139	816	5.592	3.138	2,432	5.932	3.283
Number of incidents: Violent	3,232	0.203	0.492	800	0.295	0.599	816	0.178	0.445	2,432	0.173	0.447
Number of incidents: Other	3,232	1.348	1.373	800	1.201	1.320	816	1.255	1.322	2,432	1.396	1.386
<i>Informations</i>												
Number of informations: All	3,232	0.513	0.982	800	0.791	1.154	816	0.489	0.875	2,432	0.421	0.899
Number of informations: Property	3,230	0.401	0.806	800	0.566	0.922	814	0.396	0.743	2,430	0.346	0.757
Number of informations: Violent	3,230	0.048	0.242	800	0.101	0.362	814	0.036	0.192	2,430	0.030	0.182
Number of informations: Other	3,230	0.065	0.282	800	0.124	0.386	814	0.059	0.274	2,430	0.045	0.236
Any informations: All	3,232	0.311	0.463	800	0.465	0.499	816	0.316	0.465	2,432	0.260	0.439
Any informations: Property	3,232	0.269	0.444	800	0.371	0.483	816	0.279	0.449	2,432	0.236	0.424
Any informations: Violent	3,232	0.043	0.204	800	0.086	0.281	816	0.037	0.188	2,432	0.029	0.168
Any informations: Other	3,232	0.057	0.232	800	0.107	0.310	816	0.053	0.224	2,432	0.040	0.197
<i>Charges</i>												
Number of charges: All	3,232	6.382	3.590	800	5.281	3.154	816	6.161	3.419	2,432	6.744	3.651
Number of charges: Property	3,230	4.946	3.064	800	4.010	2.746	814	4.834	2.878	2,430	5.254	3.101
Number of charges: Violent	3,230	0.155	0.421	800	0.194	0.479	814	0.143	0.402	2,430	0.143	0.399
Number of charges: Other	3,230	1.284	1.355	800	1.077	1.271	814	1.199	1.306	2,430	1.352	1.375
Any charges: All	3,232	0.991	0.094	800	0.983	0.131	816	0.990	0.0986	2,432	0.994	0.0783
Any charges: Property	3,232	0.976	0.153	800	0.949	0.221	816	0.979	0.143	2,432	0.985	0.121
Any charges: Violent	3,232	0.136	0.343	800	0.164	0.370	816	0.127	0.334	2,432	0.127	0.333
Any charges: Other	3,232	0.660	0.474	800	0.598	0.491	816	0.627	0.484	2,432	0.681	0.466
<i>Property stolen</i>												
Number of incidents	3,230	0.405	0.750	800	0.613	0.951	814	0.376	0.655	2,430	0.337	0.656
Any incident	3,232	0.292	0.455	800	0.394	0.489	816	0.295	0.456	2,432	0.258	0.438

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

**Table 6. Daily Crime Reports – Baseline Results**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	1828-1830	1828-1830	1828-1830	1828-1830	1828-1832	1828-1830	1828-1830	1828-1830
Crime type:	total	total	total	total	total	prop	viol	other
<i>Panel A. Number of all incidents per day/station</i>								
Post Met Police	0.340*	0.340**	0.337**	0.347**	0.821***	0.406***	-0.115***	0.056
	(0.180)	(0.161)	(0.160)	(0.154)	(0.134)	(0.135)	(0.026)	(0.063)
<i>Panel B. Any informations per day/station</i>								
Post Met Police	-0.149***	-0.149***	-0.148***	-0.148***	-0.206***	-0.090***	-0.049***	-0.055***
	(0.024)	(0.022)	(0.022)	(0.022)	(0.019)	(0.022)	(0.012)	(0.013)
<i>Panel C. Number of informations per day/station</i>								
Post Met Police	-0.302***	-0.302***	-0.302***	-0.301***	-0.371***	-0.170***	-0.064***	-0.065***
	(0.051)	(0.046)	(0.046)	(0.046)	(0.041)	(0.039)	(0.014)	(0.016)
<i>Panel D. Any 'stolen property' per day/station</i>								
Post Met Police	-0.098***	-0.098***	-0.099***	-0.099***	-0.137***	na	na	na
	(0.024)	(0.023)	(0.023)	(0.023)	(0.019)			
<i>Panel E. Number of charges per day/station</i>								
Post Met Police	0.879***	0.879***	0.881***	0.890***	1.471***	0.827***	-0.050**	0.126**
	(0.164)	(0.146)	(0.144)	(0.140)	(0.120)	(0.121)	(0.022)	(0.061)
Observations	1,616	1,616	1,616	1,616	3,232	1,616	1,616	1,616
Office FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar week FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Day of week FE	No	No	No	Yes	Yes	Yes	Yes	Yes

NOTES – The table shows the regression results corresponding to equation (2). For a description of the underlying data, see Section 3.1. The dependent variable in Panel A is the number of all incidents (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 top of each column indicates the years included in the sample and where appropriate the crime category. Robust standard errors are shown in parentheses below the estimated coefficients. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample:	1828-1832			1828-1832			1828-1832			1828-1832
Y:	<i>Number of all incidents</i>			<i>Number of charges</i>			<i>Any informations</i>			<i>Any 'stolen property'</i>
Crime type:	total	prop.	viol.	total	prop.	viol.	total	prop.	viol.	total
Post Met: 1830, Jan.	-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, > Jan.	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)
<i>p-value</i>	<i>0.000</i>	<i>0.000</i>	<i>0.853</i>	<i>0.000</i>	<i>0.000</i>	<i>0.483</i>	<i>0.000</i>	<i>0.000</i>	<i>0.079</i>	<i>0.000</i>
Observations	3,232	3,232	3,232	3,232	3,232	3,232	3,232	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 equation (2) but allowing for separate coefficients by time after the introduction of the Met (note that the second wave of hiring, mainly in the outer divisions, occurred in February 1830). For a description of the underlying data, see Section 3.1. The dependent variable in columns (1) to (3) is the total number of any 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 8. Summary Statistics for County-Level Analysis**

	All Counties: 1832-1865			Early Reformers: N= 16			Mid-Reformers: N= 9			Late Reformers: N = 23		
	N	mean	SD	N	mean	SD	N	mean	SD	N	mean	SD
Fiscal Start Year	1,632	1850	8	544	1840	0	306	1848	6	782	1857	0
Force existence all year	1,632	0.45	0.50	544	0.74	0.44	306	0.50	0.50	782	0.24	0.42
Charges	1,632	367	505	544	569	731	306	304	321	782	251	276
Violent charges	1,440	62	86	480	94	128	270	51	53	690	43	45
Property charges	1,440	279	392	480	434	567	270	228	247	690	190	218
Other charges	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
<i>Snap Shot Variables</i>												
England	48	0.75	0.44	16	0.88	0.34	9	0.67	0.50	23	0.70	0.47
Wales	48	0.25	0.44	16	0.13	0.34	9	0.33	0.50	23	0.30	0.47
Number parishes	48	190	156	16	236	176	9	154	127	23	173	151
Acres	48	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
<i>1851 Census Variables</i>												
Farmer (share)	48	0.15	0.09	16	0.12	0.08	9	0.16	0.10	23	0.18	0.09
Male (share)	48	0.48	0.01	16	0.48	0.01	9	0.48	0.02	23	0.49	0.01
Married (share)	48	0.33	0.01	16	0.34	0.01	9	0.33	0.01	23	0.33	0.02
Native (share)	48	0.98	0.02	16	0.98	0.02	9	0.99	0.01	23	0.98	0.02
Employed (share)	48	0.67	0.03	16	0.69	0.03	9	0.67	0.02	23	0.67	0.03
Out of labor force (share)	48	0.33	0.03	16	0.31	0.03	9	0.33	0.02	23	0.33	0.03
Age 0-15 (share)	48	0.38	0.01	16	0.38	0.01	9	0.38	0.01	23	0.38	0.01
Age 16-25 (share)	48	0.18	0.01	16	0.18	0.01	9	0.18	0.01	23	0.18	0.01
Age 26-35 (share)	48	0.14	0.01	16	0.14	0.01	9	0.14	0.01	23	0.14	0.01
Age 36-45 (share)	48	0.11	0.00	16	0.11	0.00	9	0.11	0.01	23	0.11	0.00
Age 46-55 (share)	48	0.08	0.00	16	0.08	0.00	9	0.08	0.01	23	0.08	0.00
Age 56-65 (share)	48	0.06	0.01	16	0.06	0.01	9	0.06	0.01	23	0.06	0.01
Age 66 plus (share)	48	0.05	0.01	16	0.05	0.01	9	0.05	0.01	23	0.05	0.01

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

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

	(1)	(2)	(3)	(4)
	Dep. Variable: Log (Number of Charges)		Dep. Variable: Log (Charge Rate)	
	First Treated Year Defined as Police Force Existed for:			
	Any of year	All of year	Any of year	All of year
<i>Panel A: All Charges</i>				
Force	0.008 [0.037]	-0.024 [0.033]	0.015 [0.038]	-0.018 [0.034]
Observations	1,632	1,632	1,632	1,632
R-squared	0.959	0.959	0.891	0.891
<i>Panel B: Violent Charges</i>				
Force	-0.018 [0.053]	-0.031 [0.050]	-0.006 [0.054]	-0.020 [0.051]
Observations	1,431	1,431	1,431	1,431
R-squared	0.894	0.894	0.712	0.712
<i>Panel C: Property Charges</i>				
Force	0.029 [0.044]	0.017 [0.042]	0.041 [0.043]	0.027 [0.042]
Observations	1,440	1,440	1,440	1,440
R-squared	0.958	0.958	0.896	0.896
<i>Panel D: Other Charges</i>				
Force	0.054 [0.080]	0.002 [0.078]	0.067 [0.081]	0.013 [0.079]
Observations	1,267	1,267	1,267	1,267
R-squared	0.771	0.772	0.509	0.511

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

**Table 10. Police Force in Name Only? Heterogeneity by Police Force Size**

	(1)	(2)	(3)	(4)	(5)
	Dep. Variable: Log (Number of Charges)				
	Police Force Size Defined According to the Below Thresholds in the # of <i>People Per Policeman</i> (upon police force creation)				
	1,500	1,750	2,000	2,250	2,500
<i>Panel A: All Charges</i>					
Force Sufficiently Large	-0.190*** [0.062]	-0.127** [0.058]	-0.118** [0.055]	-0.109** [0.049]	-0.079 [0.048]
Force Insufficiently Large	0.022 [0.043]	0.040 [0.051]	0.053 [0.057]	0.088 [0.068]	0.066 [0.075]
<i>Panel B: Violent Charges</i>					
Force Sufficiently Large	-0.183* [0.104]	-0.134 [0.080]	-0.129* [0.070]	-0.130** [0.062]	-0.093 [0.062]
Force Insufficiently Large	-0.002 [0.058]	0.020 [0.068]	0.034 [0.078]	0.081 [0.094]	0.048 [0.104]
<i>Panel C: Property Charges</i>					
Force Sufficiently Large	-0.143** [0.065]	-0.073 [0.069]	-0.063 [0.066]	-0.050 [0.058]	-0.028 [0.057]
Force Insufficiently Large	0.064 [0.050]	0.080 [0.055]	0.090 [0.060]	0.113 [0.073]	0.101 [0.083]
<i>Panel D: Other Charges</i>					
Force Sufficiently Large	-0.181 [0.120]	-0.079 [0.099]	-0.055 [0.097]	-0.071 [0.090]	-0.049 [0.089]
Force Insufficiently Large	0.027 [0.077]	0.022 [0.088]	0.009 [0.092]	0.055 [0.099]	0.030 [0.107]

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

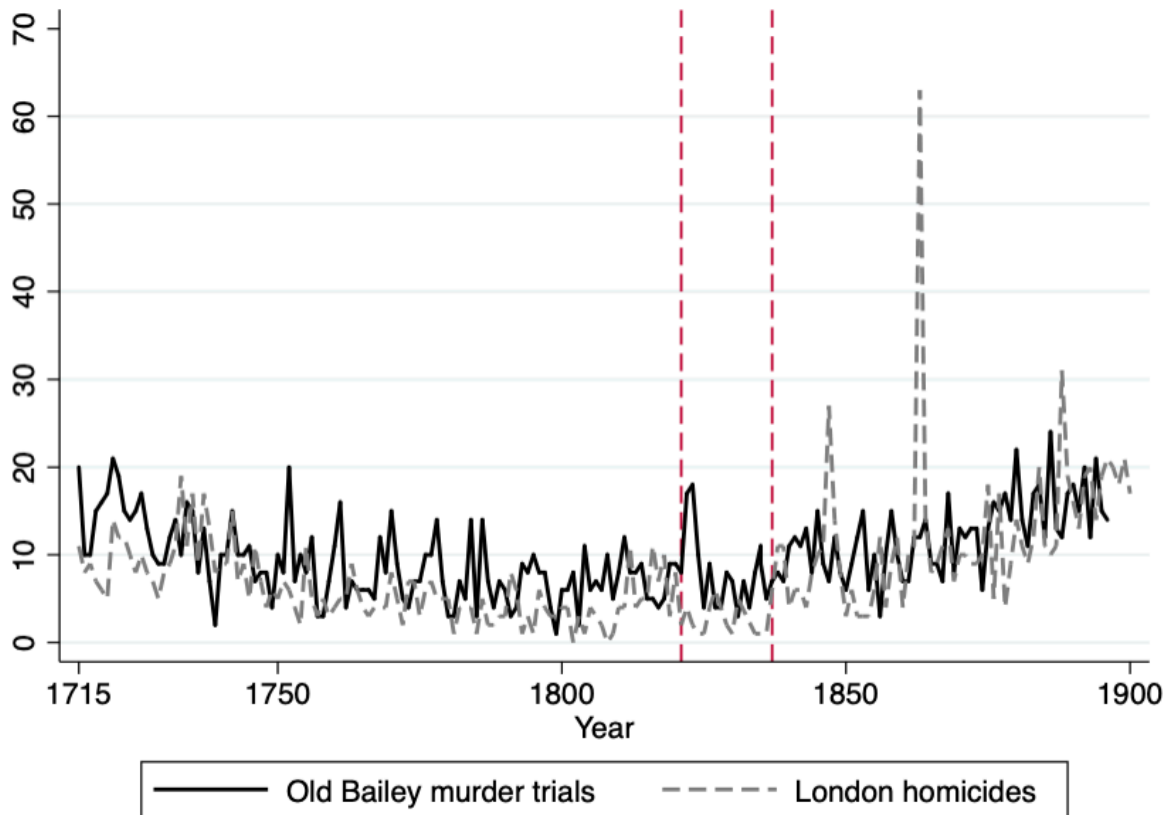
**Table 11. Spillover Effects of Neighboring Police Forces**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Log (Number charges)			Log (Number violent charges)			Log (Number property charges)			Log (Number other charges)		
Own Force Sufficiently Large	-0.190*** [0.062]	-0.190*** [0.069]	-0.184*** [0.060]	-0.183* [0.104]	-0.183* [0.108]	-0.182* [0.101]	-0.143** [0.065]	-0.143** [0.068]	-0.140** [0.063]	-0.181 [0.120]	-0.180 [0.117]	-0.188 [0.117]
Own Force Insufficiently Large	0.022 [0.043]	0.021 [0.044]	-0.006 [0.045]	-0.002 [0.058]	-0.003 [0.059]	-0.030 [0.059]	0.064 [0.050]	0.062 [0.049]	0.040 [0.052]	0.027 [0.077]	0.027 [0.075]	0.001 [0.074]
Any Neighboring Force		0.189*** [0.059]			0.109 [0.112]			0.253*** [0.055]			-0.168** [0.072]	
Any Neighboring Sufficiently Large Force			-0.158** [0.060]			-0.192** [0.074]			-0.149** [0.068]			-0.195** [0.080]
Any Neighboring Insufficiently Large Force			0.131** [0.055]			0.064 [0.137]			0.199*** [0.054]			-0.010 [0.109]
Observations	1,632	1,632	1,632	1,431	1,431	1,431	1,440	1,440	1,440	1,267	1,267	1,267
R-squared	0.960	0.961	0.961	0.894	0.895	0.896	0.959	0.960	0.960	0.775	0.776	0.777

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

Appendix A: Additional Figures and Tables

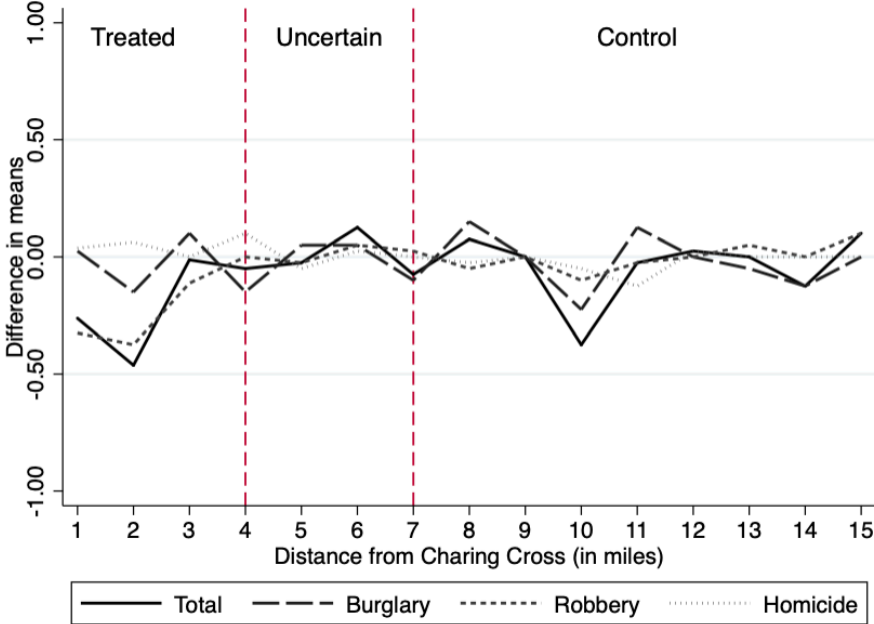
Appendix Figure A1. London Murders and Homicides



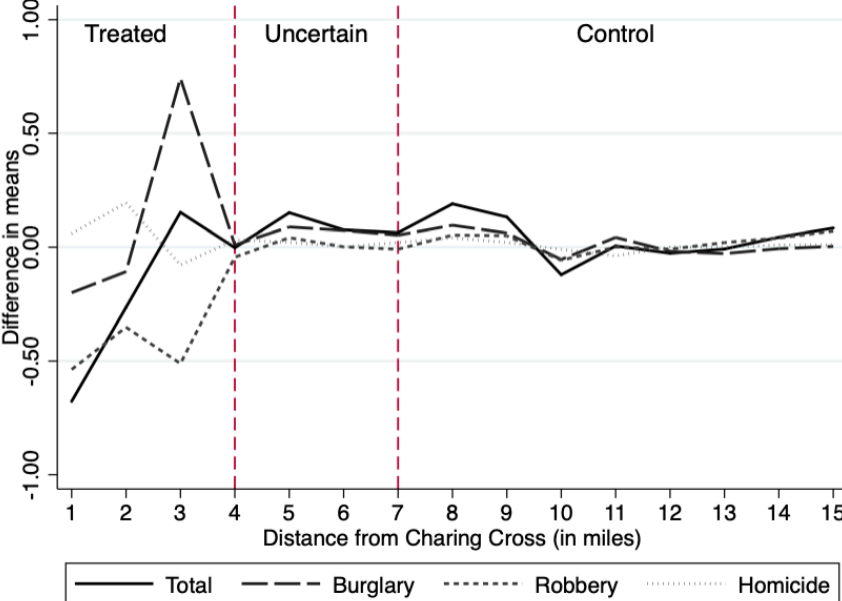
NOTES – The figure compares two measures of homicides in London between 1715 and 1900. The black solid line shows the number of trials for murder at the Old Bailey, retrieved from *The Old Bailey Proceedings Online*. The grey dashed line shows the number of London homicides compiled in “Homicides in 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. Differences in Means by Distance Band (Old Bailey)**

*Panel A. 1828-1832*

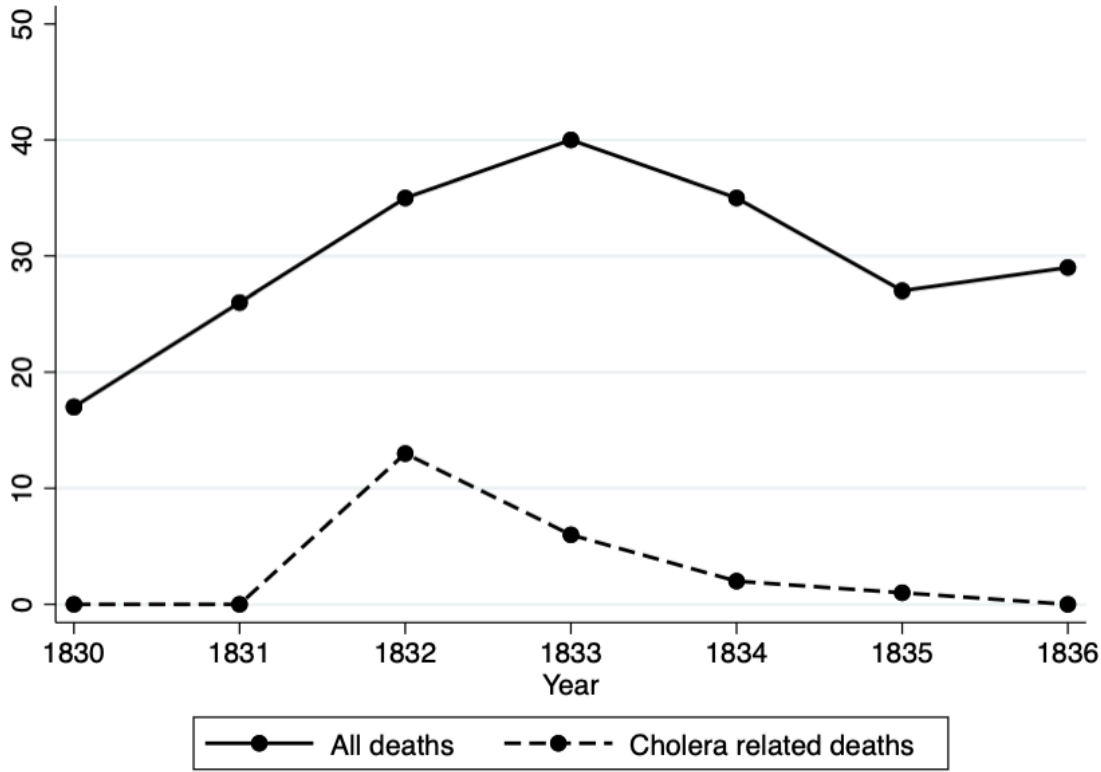


*Panel B. 1821-1837*



NOTES – The figures plot the differences in means (of the number of trials per month and 1-mile distance band) before and after the introduction of the Metropolitan Police, for all trials in the sample (solid line) as well as separately 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 Figure A3. Deaths of Metropolitan Police Officers

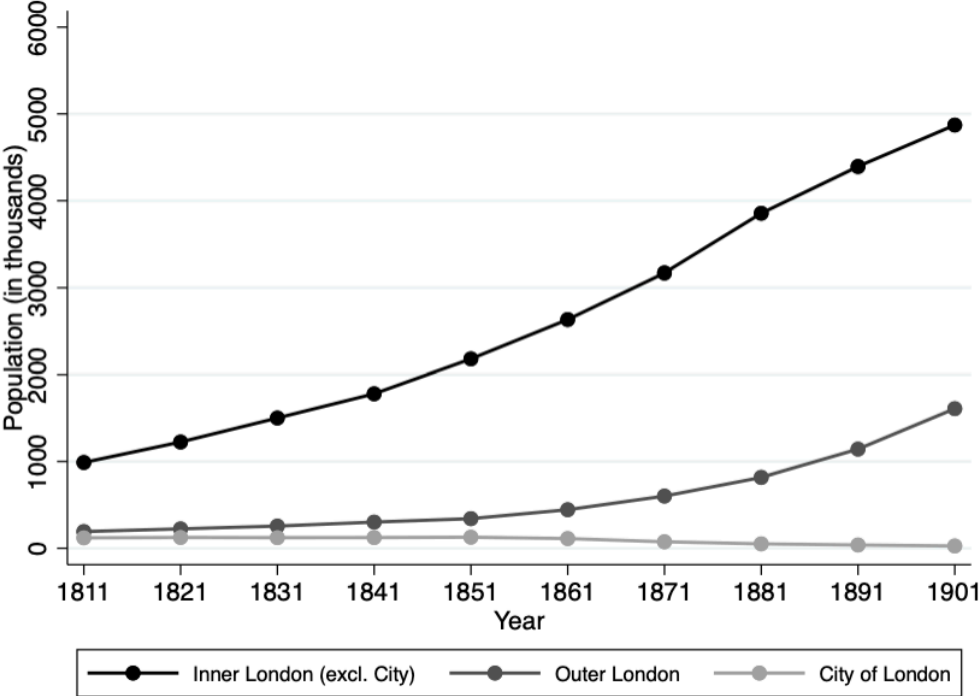


NOTES –The figure shows the number of deaths of Metropolitan Police Officers between 1830 and 1836, overall (solid black line) and due to Cholera (dashed black line). The figures as based on manually transcribed data from the *Returns to Death 1829-1889* sourced from the National Archives (MEPO 4/2).

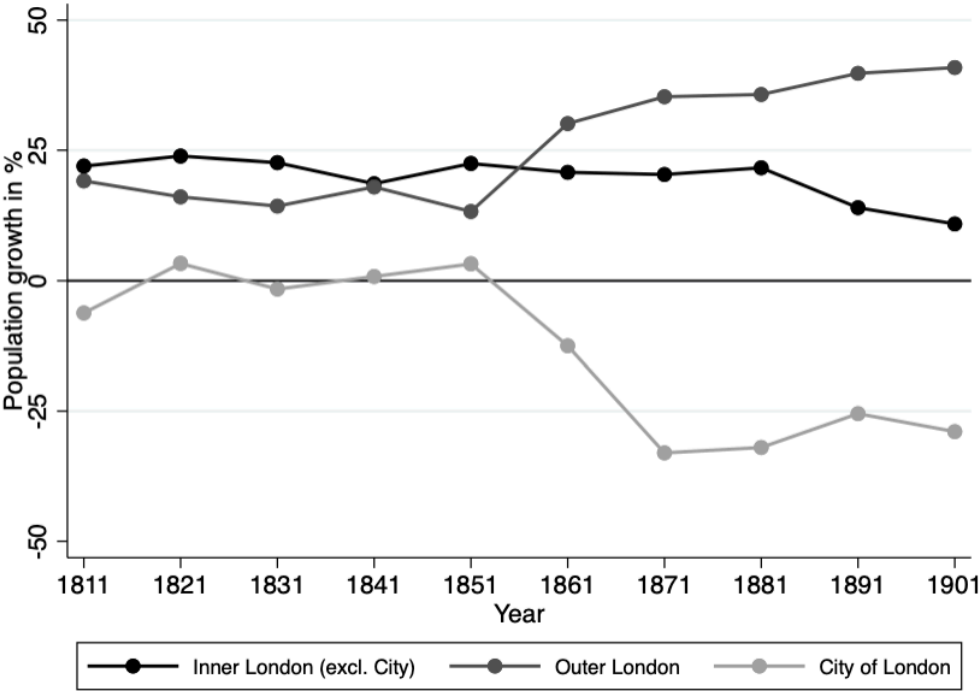


**Appendix Figure A4. Historical Population Estimates by Region of London**

*Panel A. Population Levels*

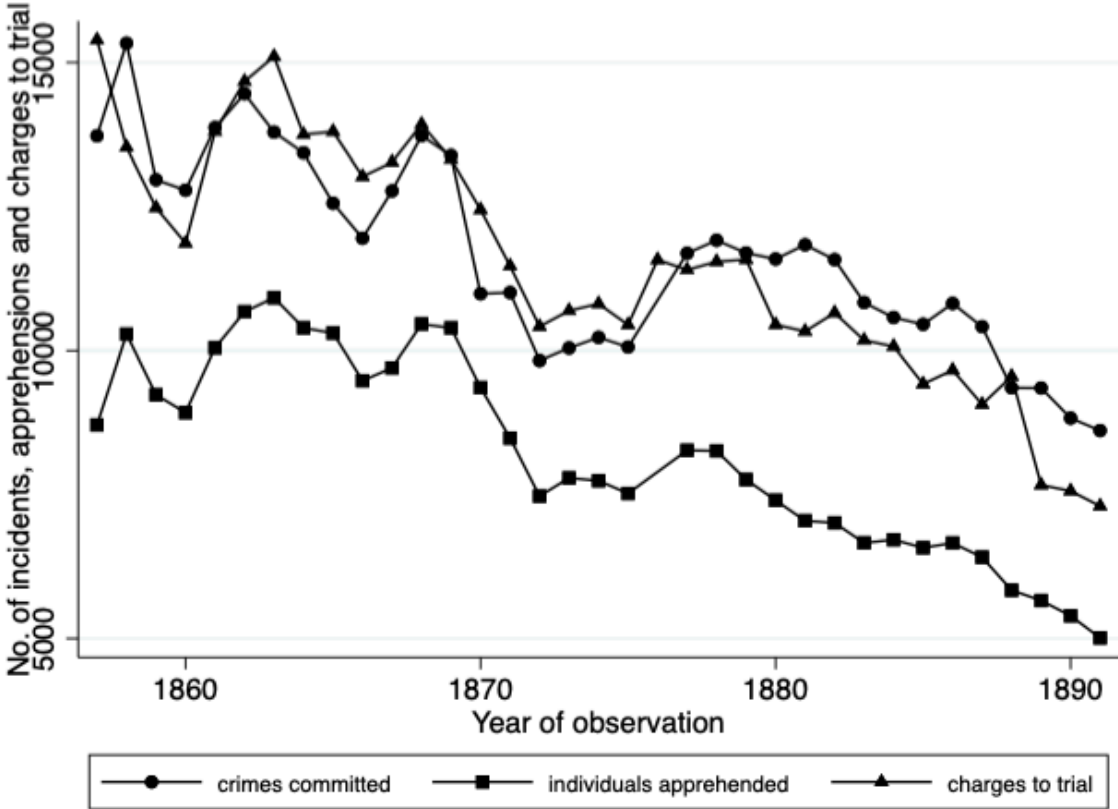


*Panel B. Population Growth*



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 was retrieved from the Historical Census Population provided by the Office for National Statistics.

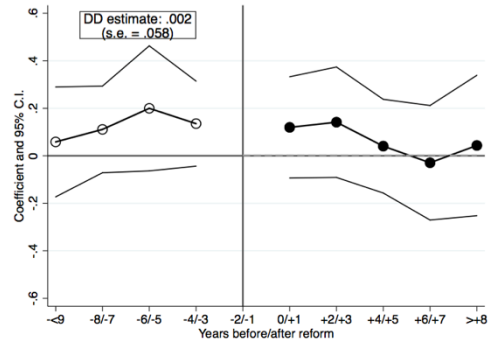
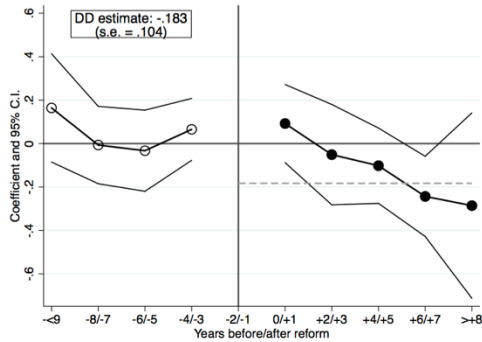
**Appendix Figure A5. Charges Brought to Trial as a Crime Proxy (County Analysis)**



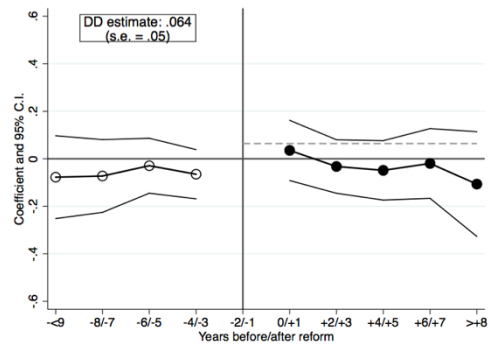
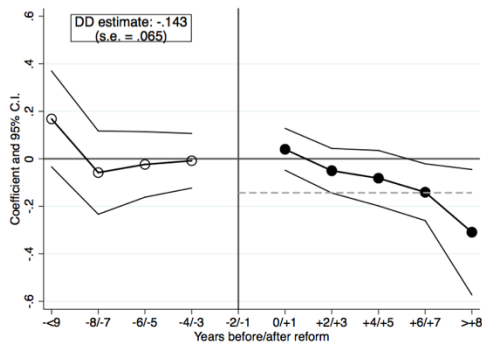
NOTES – The figure presents the national annual number of crimes committed, charges brought to trial, and individuals apprehended in all England and Wales counties, excluding Middlesex, York, Suffolk, and Sussex from 1857 to 1891. The number of charges to trial is the main outcome variable used in the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

**Appendix Figure A6. Event-Study: Sufficiently/Insufficiently Large County Forces, Crime Type**

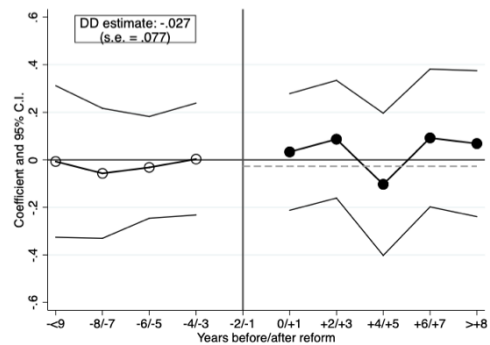
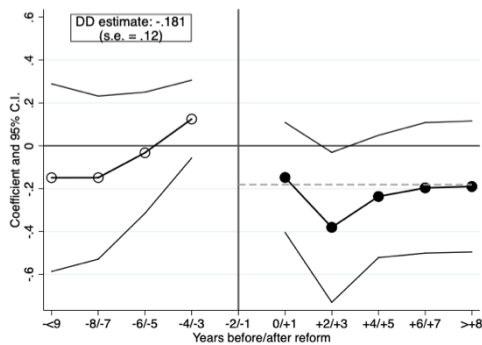
**Panel A: Violent Charges**



**Panel B: Property Charges**



**Panel C: Other Charges**



NOTES – The figures shows the results from event-study specifications for the county-level analysis separately by crime type. See Figure 12 for details.

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

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

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

**Appendix Table A2. Descriptive Statistics - Old Bailey Proceedings Data**

Variable	within 7-miles radius				City of London				outside 7-miles radius			
	1821-1829	1829-1837	1825-1835	1828-1832	1821-1829	1829-1837	1825-1835	1828-1832	1821-1829	1829-1837	1825-1835	1828-1832
<i>Crime</i>												
Number of crime incidents	915	806	1161	458	153	161	228	100	102	128	148	89
Burglary	540	544	736	294	82	114	153	61	71	78	99	64
Manslaughter	35	73	76	30	8	16	20	11	3	7	6	5
Murder	59	30	61	20	4	6	5	4	9	8	10	3
Robbery	281	159	288	114	59	25	50	24	19	35	33	17
Distance to Charing Cross (miles)	1.935	2.208	2.110	2.058	1.659	1.637	1.633	1.624	10.371	10.244	10.289	10.631
Distance to Charing Cross (in km)	3.114	3.553	3.395	3.311	2.670	2.635	2.628	2.612	16.687	16.483	16.555	17.104
Number of co-defendants	1.414	1.297	1.373	1.376	1.320	1.261	1.303	1.260	1.578	1.359	1.507	1.562
Days crime to session start	33.547	29.211	32.944	34.590	67.283	30.416	55.537	33.545	60.657	79.570	51.939	49.584
<i>Police</i>												
Any police witness (1/0)	0.830	0.839	0.839	0.852	0.869	0.783	0.846	0.870	0.794	0.805	0.804	0.831
# of police first 5 witnesses who are:	1.403	1.371	1.430	1.504	1.490	1.385	1.425	1.450	1.186	1.281	1.243	1.337
Constable												
Policeman	0.309	0.315	0.298	0.293	0.301	0.261	0.294	0.260	0.559	0.633	0.662	0.809
Watchman	0.001	0.733	0.384	0.395	0.000	0.342	0.171	0.180	0.000	0.172	0.095	0.079
Other (pre-Met type)	0.363	0.032	0.215	0.242	0.386	0.230	0.285	0.250	0.108	0.102	0.101	0.079
Other (post-Met type)	0.573	0.145	0.385	0.410	0.706	0.342	0.526	0.600	0.373	0.281	0.277	0.258
Missing	0.023	0.127	0.083	0.116	0.039	0.124	0.075	0.090	0.000	0.070	0.047	0.056
Police at crime scene (1/0)	3.715	3.644	3.622	3.517	3.562	3.696	3.645	3.620	3.951	3.734	3.804	3.708

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	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	
<i>Panel A. Total crime</i>								
Post Met x Treatment Area	-2.641*** (0.864)	-1.411 (1.255)	-1.206*** (0.411)	-1.688** (0.759)	-0.452 (0.339)	-1.160** (0.525)	-1.168** (0.456)	-1.459* (0.787)
Post Met x Uncertainty Area	0.042 (0.345)	0.450 (0.568)	0.093 (0.170)	0.162 (0.339)	-0.116 (0.143)	-0.085 (0.260)	0.094 (0.176)	0.166 (0.351)
<i>Panel B. Burglary</i>								
Post Met x Treatment Area	-0.913 (0.716)	0.257 (1.156)	-0.371 (0.339)	-0.462 (0.630)	-0.003 (0.262)	-0.831* (0.453)	-0.106 (0.370)	-0.294 (0.660)
Post Met x Uncertainty Area	-0.155 (0.281)	-0.132 (0.418)	0.028 (0.144)	0.088 (0.274)	-0.041 (0.118)	-0.156 (0.213)	0.016 (0.149)	0.006 (0.288)
<i>Panel C. Robbery</i>								
Post Met x Treatment Area	-1.408*** (0.410)	-1.449*** (0.476)	-0.803*** (0.190)	-1.222*** (0.385)	-0.563*** (0.161)	-0.521* (0.304)	-1.032*** (0.219)	-1.297*** (0.428)
Post Met x Uncertainty Area	0.304* (0.177)	0.565* (0.300)	0.096 (0.081)	0.128 (0.143)	-0.016 (0.069)	0.129 (0.114)	0.129 (0.088)	0.228 (0.162)
<i>Panel D. Homicide</i>								
Post Met x Treatment Area	-0.320 (0.271)	-0.219 (0.373)	-0.033 (0.130)	-0.003 (0.227)	0.115 (0.102)	0.192 (0.181)	-0.030 (0.138)	0.132 (0.254)
Post Met x Uncertainty Area	-0.107 (0.103)	0.017 (0.157)	-0.031 (0.056)	-0.053 (0.113)	-0.059 (0.046)	-0.058 (0.100)	-0.050 (0.057)	-0.068 (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) to (3) of Table 3 (see notes in that table for details on the baseline specification). The estimation windows are shown at the top of each column. Columns (1) to (2) add an area-specific annual trend; columns (3) and (4) exclude locations that were identified as “long streets” only (and potentially misclassified as treated); columns (5) and (6) exclude locations for which we had to refer to historical maps; columns (7) and (8) exclude observations for which the date of the actual crime is missing in the data and proxied by the session start date instead in the baseline estimation. Robust standard errors are shown in parentheses below the coefficient. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix Table A4. Alternative Standard Errors (Old Bailey Data)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample:	1821-1837	1825-1835	1828-1832	1821-1837	1828-1832	1821-1837	1828-1832	1821-1837	1825-1835
Radius:	<i>Four miles from Charing Cross</i>								
Specification:	City of London = Control			City of London = Treated from 02 April 1832		City of London = Uncertain		City of London = Excluded	
<i>Panel A. Total crime (by month/area)</i>									
Post Met x Treatment Area	-1.146*** <i>0.002</i>	-2.157*** <i>0.002</i>	-1.472*** <i>0.002</i>	-0.880 <i>0.494</i>	-1.045*** <i>0.002</i>	-1.177*** <i>0.002</i>	-1.328*** <i>0.002</i>	-1.190*** <i>0.002</i>	-1.234*** <i>0.002</i>
Post Met x Uncertainty Area	0.095 <i>0.286</i>	0.074 <i>0.536</i>	0.178 <i>0.492</i>	-0.021 <i>0.756</i>	0.213 <i>0.492</i>	-0.002 <i>0.862</i>	0.360 <i>0.096</i>	0.051 <i>0.536</i>	0.416 <i>0.536</i>
<i>Panel B. Burglary (by month/area)</i>									
Post Met x Treatment Area	-0.106 <i>0.496</i>	-0.611*** <i>0.002</i>	-0.294 <i>0.108</i>	0.007 <i>0.890</i>	-0.285*** <i>0.002</i>	0.020 <i>0.906</i>	-0.234 <i>0.254</i>	0.047 <i>0.536</i>	-0.120 <i>0.748</i>
Post Met x Uncertainty Area	0.016 <i>0.880</i>	-0.073 <i>0.470</i>	0.006 <i>0.890</i>	0.053 <i>0.598</i>	-0.013 <i>0.850</i>	0.211*** <i>0.000</i>	0.116 <i>0.742</i>	0.169*** <i>0.000</i>	0.180 <i>0.536</i>
<i>Panel C. Robbery (by month/area)</i>									
Post Met x Treatment Area	-1.032 <i>0.504</i>	-1.345*** <i>0.002</i>	-1.297*** <i>0.002</i>	-0.828 <i>0.494</i>	-0.832 <i>0.244</i>	-1.216*** <i>0.002</i>	-1.281*** <i>0.002</i>	-1.261*** <i>0.002</i>	-1.336*** <i>0.002</i>
Post Met x Uncertainty Area	0.129 <i>0.494</i>	0.190 <i>0.494</i>	0.228 <i>0.154</i>	0.005 <i>0.864</i>	0.288 <i>0.308</i>	-0.231 <i>0.348</i>	0.144 <i>0.762</i>	-0.100 <i>0.494</i>	0.189 <i>0.762</i>
<i>Panel D. Homicide (by month/area)</i>									
Post Met x Treatment Area	-0.008 <i>0.746</i>	-0.200 <i>0.375</i>	0.120 <i>0.455</i>	-0.060 <i>0.496</i>	0.072 <i>0.492</i>	0.019 <i>0.758</i>	0.188 <i>0.387</i>	0.024 <i>0.758</i>	0.222 <i>1.000</i>
Post Met x Uncertainty Area	-0.049 <i>0.496</i>	-0.043 <i>0.468</i>	-0.055 <i>0.508</i>	-0.079 <i>0.496</i>	-0.062 <i>0.680</i>	0.019 <i>0.870</i>	0.100 <i>0.491</i>	-0.018 <i>0.764</i>	0.047 <i>1.000</i>
Observations	816	528	240	816	240	816	240	612	180
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES – See notes of Old Bailey baseline table (Table 3). Clustered standard errors are based on a Wild Cluster Bootstrap (1,000 repetitions); p-values are shown in italics below the coefficient. Standard errors are clustered by area (treated, uncertain, control, city).

**Appendix Table A5. Robustness Checks for Pre-Post Estimation (Daily Crime Reports)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Sample:	1828-32	1828-32	1828-32	1828-32	1828-32	1828-32	1828-32	1828-32	1828-32	1828-32	1828-32
Crime type:	total	total	total	total	total	total	total	total	total	total	total
Specification:	Drop one office at the time:									Weekly Comple e	Log outcome
	Bow Street	Hatton Garden	Lambeth Street	Marlye- bone	Marlborough Street	Queen Square	Union Hall	Worship Street	All weeks		
<i>Panel A. Number of all incidents</i>											
Post Met Police	0.17 (0.164)	0.29* (0.165)	0.35** (0.167)	0.37** (0.168)	0.59*** (0.162)	0.20 (0.169)	0.42** (0.165)	0.39** (0.159)	5.09*** (1.090)	0.67 (1.411)	0.13*** (0.044)
<i>Panel B. Any informations</i>											
Post Met Police	-0.18*** (0.024)	-0.15*** (0.023)	-0.14*** (0.023)	-0.13*** (0.024)	-0.15*** (0.024)	-0.15*** (0.024)	-0.12*** (0.023)	-0.17*** (0.023)	-0.22*** (0.034)	-0.13*** (0.047)	-
<i>Panel C. Any 'stolen property'</i>											
Post Met Police	-0.09*** (0.024)	-0.10*** (0.025)	-0.12*** (0.024)	-0.11*** (0.025)	-0.1*** (0.025)	-0.09*** (0.025)	-0.09*** (0.025)	-0.10*** (0.025)	-0.73*** (0.119)	-0.71*** (0.165)	-
<i>Panel D. Number of charges</i>											
Post Met Police	0.72*** (0.150)	0.87*** (0.148)	0.89*** (0.152)	0.91*** (0.153)	1.14*** (0.147)	0.76*** (0.152)	0.88*** (0.150)	0.96*** (0.142)	8.63*** (1.003)	3.98*** (1.333)	0.28*** (0.045)
Observations	1,414	1,414	1,414	1,414	1,414	1,414	1,414	1,414	576	240	576
Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cal. week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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



**Appendix Table A6. Dates of County Police Force Formation and Size of Initial Force**

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

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

## Appendix Table A7. 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, Harboursing and Aiding the Escape of Felons, Riot, Sedition, Breach of the Peace, Refusing to Aid Peace Officers, Keeping Disorderly Houses, Indecently Exposing the Person, Felonies Not Included Above, Misdemeanors Not Included Above

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

**Appendix Table A8. Sensitivity Analysis for Sufficiently and Insufficiently Large County Police Force Results**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample Years:	1832-1865	1832-1865	1832-1865	1832-1865	1832-1865	1835-1862	1832-1849	1850-1865	1832-1865
Sample Counties	all	all	all	all	all	all	all	all	English
Force Sufficiently Large	-0.190*** [0.062]	-0.207*** [0.062]	-0.202*** [0.062]	-0.207*** [0.070]	-0.190*** [0.062]	-0.153** [0.062]	-0.088** [0.035]	-0.074 [0.072]	-0.099* [0.056]
Force Insufficiently Large	0.022 [0.043]	0.043 [0.046]	0.051 [0.046]	0.034 [0.043]	0.022 [0.043]	0.020 [0.044]	0.008 [0.061]	0.005 [0.073]	-0.024 [0.046]
Force Sufficiently Large	-0.183* [0.104]	-0.199** [0.086]	-0.184** [0.086]	-0.220** [0.103]	-0.183* [0.104]	-0.134 [0.119]	-0.049 [0.194]	-0.094 [0.101]	-0.114 [0.104]
Force Insufficiently Large	-0.002 [0.058]	0.040 [0.058]	0.055 [0.056]	0.013 [0.058]	-0.002 [0.058]	0.005 [0.060]	0.068 [0.089]	-0.103 [0.108]	-0.058 [0.054]
Force Sufficiently Large	-0.143** [0.065]	-0.152** [0.067]	-0.148** [0.069]	-0.147* [0.077]	-0.143** [0.065]	-0.124** [0.061]	-0.025 [0.064]	-0.026 [0.069]	-0.046 [0.054]
Force Insufficiently Large	0.064 [0.050]	0.087* [0.050]	0.095* [0.051]	0.071 [0.049]	0.064 [0.050]	0.059 [0.050]	0.050 [0.069]	0.038 [0.071]	0.008 [0.055]
Force Sufficiently Large	-0.181 [0.120]	-0.183 [0.113]	-0.170 [0.113]	-0.115 [0.124]	-0.181 [0.120]	-0.195 [0.130]	-0.186 [0.254]	-0.197* [0.101]	-0.188 [0.125]
Force Insufficiently Large	0.027 [0.077]	0.049 [0.077]	0.072 [0.075]	0.046 [0.077]	0.027 [0.077]	0.021 [0.082]	0.117 [0.123]	-0.004 [0.094]	0.046 [0.082]
+ population	no	yes	yes	no	no	no	no	no	no
+ Eng. and region dummies	no	no	yes	no	no	no	no	no	no
+ region specific trend	no	no	no	yes	no	no	no	no	no
+ > median acre trend	no	no	no	no	yes	no	no	no	no

NOTES – This table presents sensitivity analyses of the baseline difference-in-differences specification (see Table 10), where the variables of interest Force 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. All specifications include county and year fixed effects. The baseline sample includes 48 counties for the years 1832-1865. The different specifications are indicated at the top and the bottom of the table, respectively. Standard errors are clustered by county and shown in brackets below the estimated coefficients. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Appendix Table A9. Crime Displacement from London to Neighboring Counties?**

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

<i>Panel B. Regressions (1828-1832)</i>		
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. Statistical significance is based on simple pre-post regressions (with limited numbers of observations; N=5 for single counties). Panel B shows regressions results for regressions of the log (total) number of charges on indicators for London/Middlesex direct and second neighbors interacted with a post-Met introduction dummy, respectively. 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 A10. Determinants of the Timing of County Police Force Formation**

Variable	(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							
	Dependent Variable = adoption (1 in year of adoption)													
Lag1: Charge rate	0.010 [0.032]	-0.012 [0.016]	-0.005 [0.022]				-0.005 [0.004]	-0.006 [0.005]	-0.003 [0.005]	-0.004 [0.005]				
Lag1: Any neighboring force			-0.106 [0.197]	0.002 [0.204]	-0.005 [0.207]	-0.010 [0.230]					-0.070* [0.040]			
Lag1: Any sufficiently large neighboring force											-0.003 [0.025]	-0.006 [0.028]	-0.005 [0.029]	-0.005 [0.030]
Lag1: Any insufficiently large neighboring force											-0.067* [0.036]	-0.094** [0.046]	-0.082 [0.050]	-0.079 [0.051]
Lag1: Violent crime charge rate				-0.862 [1.020]	-0.884 [1.024]	-0.747 [1.234]						0.108 [0.127]	0.091 [0.127]	0.120 [0.139]
Lag1: Property crime charge rate				0.260 [0.155]	0.260 [0.156]	0.219 [0.179]						-0.038 [0.028]	-0.030 [0.027]	-0.042 [0.030]
Lag1: Other crime charge rate				-4.605** [2.052]	-4.650** [2.073]	-4.007* [2.240]						0.324 [0.242]	0.313 [0.236]	0.446* [0.246]
Population (in 10,000)		0.017*** [0.005]	0.017*** [0.005]	0.011* [0.006]	0.011* [0.006]	0.011 [0.009]		0.002 [0.001]	0.001 [0.001]	0.001 [0.001]	0.001 [0.002]	0.002 [0.002]	0.002 [0.002]	
Lag1: Average charge rate, all neighbors					0.021 [0.059]	0.036 [0.069]							-0.020* [0.012]	-0.016 [0.012]
Initial pre-county police						0.000 [0.001]								0.000 [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

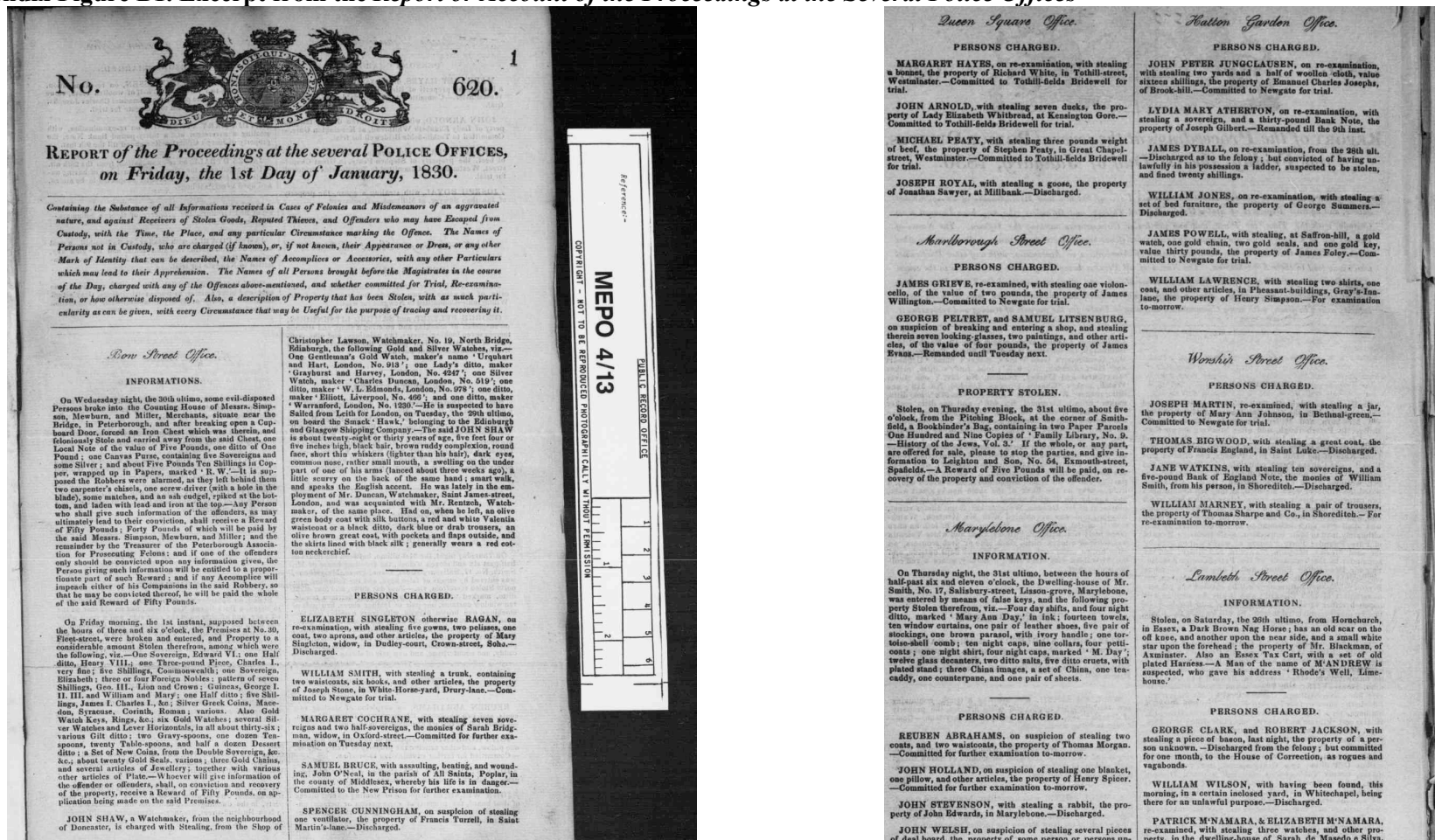
**Appendix Table A11. Determinants of Initial Force Size to Population Ratio**

	(1)	(2)	(3)	(4)	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
<i>Fixed Geographic Variables</i>						
Acres (in 10,000)	0.001 [0.004]	-27.099 [18.663]	-0.001 [0.006]	-31.207 [23.103]	-0.004 [0.005]	-34.354 [30.762]
Number of parishes (in 100)	0.010 [0.118]	49.912 [384.362]	0.051 [0.147]	25.820 [470.753]	-0.003 [0.118]	-19.884 [481.501]
Number of neighbors	0.029 [0.048]	-35.477 [239.006]	0.032 [0.046]	9.968 [223.389]	0.043 [0.040]	19.787 [224.012]
England	-0.153 [0.179]	888.542 [1,435.166]	0.035 [0.319]	1,310.160 [2,266.308]	0.161 [0.332]	1,417.821 [2,107.863]
<i>Variables measured in the 1851 census (shares)</i>						
Farmer	-0.017 [0.017]	-8.729 [112.116]	-0.010 [0.026]	-2.527 [148.072]	-0.017 [0.024]	-7.902 [163.879]
Male	-0.104** [0.048]	522.620 [388.323]	-0.103* [0.058]	396.943 [397.435]	-0.066 [0.060]	429.000 [470.022]
Married	0.050 [0.073]	267.177 [427.595]	0.065 [0.094]	90.110 [478.618]	0.026 [0.080]	57.382 [534.110]
Native	-0.049 [0.088]	-102.064 [325.609]	-0.122 [0.109]	137.937 [392.508]	-0.001 [0.094]	240.613 [655.567]
Employed	-0.007 [0.031]	-246.094 [176.390]	0.006 [0.038]	-284.430 [194.710]	-0.003 [0.035]	-292.068 [219.775]
Age 0-15	-0.131 [0.127]	-536.135 [589.462]	-0.162 [0.171]	-222.318 [767.984]	-0.297* [0.167]	-337.007 [1,005.164]
Age 16-25	0.010 [0.162]	1,363.391 [1,019.616]	-0.061 [0.212]	1,423.109 [1,448.194]	-0.170 [0.217]	1,330.720 [1,226.612]
Age 26-35	-0.219 [0.209]	23.037 [1,314.718]	-0.245 [0.272]	37.629 [1,269.969]	-0.137 [0.281]	130.006 [1,235.988]
Age 36-45	0.005 [0.457]	-1,844.465 [1,780.253]	-0.142 [0.600]	-22.887 [2,128.982]	-0.574 [0.535]	-390.638 [2,373.003]
Age 46-55	-0.263 [0.326]	3,415.117** [1,645.359]	-0.303 [0.399]	3,489.476* [1,873.414]	-0.460 [0.429]	3,355.611* [1,838.108]
<i>Variables measured in the year before force adoption</i>						
Violent crime rate			-0.453 [0.978]	2,916.493 [3,928.376]	-0.131 [0.890]	3,191.165 [4,152.820]
Property crime rate			-0.071 [0.192]	-161.968 [658.132]	-0.165 [0.195]	-242.323 [834.754]
Other crime rate			2.823 [2.640]	-11,572.963 [9,164.806]	3.467 [2.554]	-11,024.540 [9,995.851]
Any neighbors with suff. large force			-0.124 [0.220]	-560.707 [1,073.534]	-0.105 [0.184]	-544.154 [1,142.446]
Any neighbors with insuff. large force			0.069 [0.215]	-95.433 [977.603]	0.165 [0.207]	-13.718 [996.988]
<i>Population control</i>						
Population (in 10,000)					0.026*** [0.009]	22.536 [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 efficiency at the time of force formation. Census controls are measured in 1851; the omitted age category is older than 55. All census variables are measured as the share of the county population with characteristic X (value of 0-100%). Pre-formation variables are measured one year prior to county police force formation (i.e. using a different year for different counties); pre-formation crime variables are the lagged crime rates (per 1000 population), while the neighboring force variables are indicators for whether any neighbors had an efficient or inefficient force in the year prior to adoption. All regressions have a single observation per county. Robust standard errors. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Appendix B: Data Sources

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



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

