UNIVERSITY OF GOTHENBURG
SCHOOL OF BUSINESS, ECONOMICS AND LAW

WORKING PAPERS IN ECONOMICS

No 674

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October 2016

ISSN 1403-2473 (print)
ISSN 1403-2465 (online)

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The Fall of Capital Punishment and the Rise of Prisons: How Punishment Severity Affects Jury Verdicts*

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This version: September 20, 2016

Abstract: This paper studies the effect of punishment severity on jury decision-making using a large archival data set from the Old Bailey Criminal Court in London from 1715 to 1900. We take advantage of three natural experiments in English history, which result in sharp decreases in punishment severity: The offense specific abolition of capital punishment in the 1800s, the temporary halt of penal transportation during the American Revolution, and the abolition of transportation in 1853. Using a difference-in-differences design to study the abolition of the death penalty and pre-post designs to study the temporary and permanent halts to transportation, we find that decreasing expected punishment (especially via the end of the death penalty), had a large and significant impact on jury behavior, generally leading to the jury being 'harsher'. Moreover, we find that the size of the effect differs with defendants' gender and criminal history. These results raise concerns about the impartiality of juries as well as the implicit assumption often made when designing and evaluating criminal justice policies today – that the chance of conviction is independent of the harshness of the penalty.

JEL Codes: H00, K14, K40, N00, N43, N93

Keywords: jury, verdict, conviction, punishment severity, expected punishment, crime, death penalty, English history

* This paper would not have been possible without the tremendous efforts of our Research Assistant Michael Bekele, the generous help with the data extraction by Florin Maican, and the financial support of Foundation for Economic Research in West Sweden, and Vetenskapsrådet, The Swedish Research Council, Grants for Distinguished Young Researchers. We thank Mikael Lindahl, Ines Helm, and seminar participants at the University of Gothenburg for helpful comments. Corresponding author: Randi Hjalmarsson, Department of Economics, University of Gothenburg, Email: randi.hjalmarsson@economics.gu.se.

1. Introduction

More than 50 years ago, President Lyndon B. Johnson declared a war on crime. Numerous policies have since been implemented that have increased expected punishment and are responsible for the dramatic – almost four-fold – growth in the US state and federal prison populations in the last 35 years.[1] A new era of reforms aiming to reverse this 'get tough on crime' attitude by decreasing sentence severity is currently being ushered in, resulting in a (slowly) falling prison population. These reforms range from abolishing or reducing mandatory minimums to abolishing the death penalty, which still exists in 31 states today.[2]

In this context, extensive empirical research has been conducted testing the basic implication of Becker's (1968) economic model of crime: put simply, does harsher punishment deter crime?[3] In the Becker model, individuals compare the expected utilities of legal versus criminal activities, where the latter is a function of expected punishment. Expected punishment, in turn, is a function of the chance of getting caught, the chance of conviction, and the severity of punishment. The existing literature typically emphasizes changes in expected punishment driven by punishment severity, but taking the chance of conviction as exogenous. We explicitly study this underlying assumption using three natural experiments in English history associated with large and sharp changes in punishment severity – the offense specific abolition of capital punishment in the 1800s, the temporary halt of transportation during the American Revolution, and the abolition of transportation in 1853. Do changes in punishment severity affect jury decision-making and the chance of conviction, i.e. is the chance of conviction endogenous? Moreover, are juries impacted by punishment severity in a way that is unequally applied across defendants?

A jury's job is to determine whether the facts of the case prove beyond reasonable doubt the defendant's guilt; the jury's evaluation of the evidence should not be affected by factors external to the case. Whether this holds in practice has been recently studied in the empirical literature with respect to the demographic characteristics of the jury or contemporaneous media coverage during a trial.[4] There is limited research, however, on the role of potential punishment and the existing research is generally unable to disentangle the effect of the severity of the

[1] Bureau of Justice Statistics, Key Statistics, Total Adult Correctional Population 1980-2014 on the Internet at www.bjs.gov (visited 06,03,2016).
[2] States recently abolishing the death penalty include Connecticut (2012), Maryland (2013) and Nebraska (2015). See http://www.deathpenaltyinfo.org/states-and-without-death-penalty
[3] See Chalfin and McCrary (forthcoming) and Nagin (2013) for recent reviews of the deterrence literature. For selected examples, see Lee and McCrary (2009), Levitt (1996), and Drago, Galbiati, and Vertova (2009).
[4] See Anwar, Bayer and Hjalmarsson (2012, 2014, 2015, and 2016), Lee (2014), and Lehmann and Blair-Smith (2013) for studies of juror demographics, including race, age, gender, and political affiliation, and Philippe and Ouss (2015) for the role of the media.

punishment from the severity of the offense; Devine's (2012) review of the so-called 'severity-leniency hypothesis' finds that no firm conclusions can be made.[5] Related to that is the concept of 'jury nullification': does the jury take the law into their own hands based on their own ethical beliefs, for instance by acquitting a defendant for whom the facts prove guilt beyond reasonable doubt? While we are unaware of empirical studies of this question, there is anecdotal evidence throughout history, including defendants charged with helping slaves escape or Vietnam War protesters.[6]

To the best of our knowledge, the current paper is the first to study the causal effect of changes in punishment severity on jury decision-making using a quasi-experimental research design. Our identification strategy is unique to this literature in that we capitalize on changes in sentencing laws that increase or decrease punishment severity for a given criminal offense. This contrasts previous research that asks whether juries are less likely to convict defendants charged with more serious offenses (e.g. robbery versus burglary), where one cannot disentangle the differential punishment severity across offenses from the differential characteristics of the case and/or evidence.

England in the 18th and 19th centuries provides a colorful context during which to study changing punishment. In the early 1700s, imprisonment was practically non-existent and the primary sanctions were transportation to the Americas and execution; in fact, there were more than 200 capital offenses by 1800, a period that became known as *The Bloody Code*. The British penal system was put into an unexpected crisis when the American Revolution abruptly eliminated the Americas as a penal colony in 1776. This led to the first, albeit temporary, mass use of prison sentences; transportation did not officially resume until the establishment of a penal colony in Australia. However, by the end of the 19th century, capital punishment had been abolished for most offenses by a series of offense-specific Acts in the mid-1800s, transportation had been (mostly) abolished in 1853, and the modern-day prison sentence was the primary form of punishment.

[5] Experimental studies of the severity-leniency hypothesis include Vidmar (1972), Kaplan and Simon (1972), Hamilton (1978) and Freedman et al (1994). However, mock jury studies typically focus on homicides, contain much lower stakes than real jury trials, and only indirectly manipulate expected punishment by altering the choice of offenses on which the jury could convict. The handful of existing non-experimental studies (i) again proxy for expected punishment with charge severity and (ii) use small samples, ranging from 79 trials in in Indiana (Devine et al 2004) to 293 trials in Baltimore (Flowers, 2008). Other archival studies include Werner et al (1985) and Myers (1979).

[6] A recent Washington Post article (2016) provides a number of historical examples of jury nullification: https://www.washingtonpost.com/news/in-theory/wp/2016/04/08/history-is-clear-juries-were-supposed-to-be-able-to-overturn-laws/ .

To study this 200-year period, we use a data set of more than 200,000 criminal cases tried at the Old Bailey Criminal Court in London between 1715 and 1900. The accounts of these cases were published in *The Proceedings of the Old Bailey*, which has in recent years been digitized and published by *The Old Bailey Proceedings Online*.[7] From this remarkable historical data set, we extracted information identifying the unique case, the session date, the defendant's name, gender and age, the offense category charged, as well as broad and detailed verdict and sentencing outcomes. In addition, we manually coded judge and jury names, which are available from 1750 to 1822, as well as criminal history from 1832 onwards.

Given the context, it is natural to question the external validity of our study and its relevance to the modern day criminal justice system. Though transportation 'beyond the seas' clearly no longer exists, capital punishment is still used and actively debated in many countries, including the United States. In fact, potential jurors in a U.S. capital case can be dismissed for cause if they oppose the death penalty due to the implicit assumption that such an individual cannot be impartial – an assumption that we can empirically test in this paper. There are two key advantages to studying the abolition of capital punishment in this historical context. First, it provides a large and unambiguous decrease in punishment severity, which simply cannot be observed today. Second, the differential timing in the abolition of capital punishment across offenses allows for a difference-in-differences design to retrieve the (causal) effect of changes in punishment severity on jury verdicts in a single jurisdiction.

Specifically, for each of the twenty-six offense categories in our data, we identify whether the offense was never, always or once capital eligible, and in the latter case the year that capital punishment was abolished, which ranges from 1813 (fraud) to 1856 (arson). Intuitively, our research design compares the change in the chance of conviction in the years surrounding the abolition of capital punishment for 'treated' offenses – i.e. those for which the capital status changed – to that for 'control' offenses – i.e. those which were never or always capital eligible. Such a design controls for other changes occurring during this period in both the criminal justice system, including the introduction of the Metropolitan Police force and the right to a defense attorney, and society more generally (e.g. the industrial revolution). A similar within jurisdiction identification strategy would be less feasible today, given a small number of capital

[7] To the best of our knowledge, this data set has been used in just two large scale empirical studies. Voth (1998) used witness accounts from the Old Bailey data to reconstruct historical time use budgets (prior to the digitization of the Proceedings); given the time intensive nature of this kind of coding, he evaluated a little less than 8000 cases. Vickers and Zieberth (forthcoming) use the Old Bailey online data from 1835 to 1913 to study changing demographic patterns in crime, finding that convicted defendants got older during this time period. It is important to note, however, that the Proceedings only reliably report age for those who are convicted.

eligible offenses. Rather, one would have to study the abolition of capital punishment across jurisdictions, e.g. US states, which would raise serious concerns about omitted variables even if such trial data could be obtained.

Our empirical analyses find that the decrease in expected punishment arising from the abolition of the death penalty significantly increased the chance of conviction overall (7.6 percentage points), and especially for offense categories classified as violent and sex offenses or fraud offenses (22 and 34.5 percentage points, respectively). This was accompanied by a significant decrease in jury recommendations for mercy – as mercy was no longer needed. For property offenses, there is just a small increase in the chance of conviction (1.5 percentage points); however, conditional on conviction, there is a large and significant reduction in the chance of being convicted of a lesser charge (20.3 percentage points). That is, juries were able to circumvent death sentences prior to the reforms for property offenses by convicting defendants of lesser charges that were not death eligible, e.g. for a theft of a lower value than the original charge. Heterogeneity analyses indicate that a jury's reluctance to convict on a capital charge is not equal across defendants: juries were more reluctant to convict females than males of a capital offense. Though less precise than the gender effect, we also find suggestive evidence of a similar reluctance to convict first time offenders than repeat offenders. To interpret these results as causal, we make a number of identifying assumptions (see the discussion in Section 4.2), including parallel trends and that the timing of the offense specific abolition was random. Perhaps more importantly, we implicitly assume that the quality of evidence presented to the jury did not change with the reforms, since an increase in the quality of evidence could also feasibly yield the same pattern of results. However, we provide direct empirical evidence that there is no significant increase in the quality of evidence, which we measure using keyword searches for police, evidence, and witness on *The Old Bailey Proceedings Online*.

Almost all offense categories were 'treated' contemporaneously to both the temporary halt and permanent abolition of transportation, which unfortunately limits us to simple pre-post research designs in these contexts. Our baseline specifications include offense, month, and judge fixed effects as well as a vector of defendant and jury controls.[8] As a result of the American Revolution, the share of sentences to transportation decreased from 75% to 0% in 1776, and resulted in an increase in sentences to prison and manual labor in the hulks of ships.

[8] Judge and jury controls are only available for the temporary halt of transportation while criminal history is only available upon its abolition.

This sharp and unexpected change in expected punishment is exogenous to the criminal justice system and a unique feature of this natural experiment concerning penal transportation. Our empirical analyses find that defendants charged with transportation eligible *non-capital* offenses, for which punishment severity unambiguously decreased during the war, were about five percentage points more likely to be convicted as a result of the halt of transportation. A comparable effect is not seen for transportation eligible *capital* cases, for which the change in sanction severity is ambiguous given that both death sentences and prison hulks were substitute punishments for transportation. Our analysis of the abolition of transportation in 1853 (at which point about 25% of cases were sentenced to transportation) does not find a significant effect on conviction rates. This non-effect is seen in both a simple pre-post design and a difference-in-differences specification using offenses with a relatively low share of transportation offenses prior to the reform as the control group. The obvious weakness in the simple pre-post identification strategies – namely the inability to conclusively separate the effect of the reform from other things changing at the same time – limits the extent to which these transportation results can be interpreted causally; we discuss the specific potential confounders in more detail, particularly with respect to the American Revolution, in Section 5.1. Nevertheless, these 'experiments' add to a complete picture of the role of (various degrees) of punishment severity in jury decision-making.

This paper provides empirical evidence that punishment severity, and in particular capital punishment, may impact the ability of a jury to be impartial. The fact that abolishing capital punishment has such large impacts on jury behavior during a time in history when capital punishment still had a fairly high acceptance rate in society is striking. It is certainly suggestive that punishment severity – namely the chance of a death sentence – may significantly impact jury behavior today, a period with much less societal acceptance of the death penalty. It also suggests that the behavior of jurors who are not fundamentally opposed to the death penalty may still be impacted by the potential sanction – that is, challenging jurors who are opposed to the death penalty may still not result in an entirely impartial jury. Furthermore, this lack of impartiality may be applied unequally across defendant characteristics.[9]

Aside from the question of whether punishment severity affects a jury's ability to be impartial, this paper also suggests that policy makers may be missing an important channel

[9] See Iyengar (2011) for a study of judge and jury death penalty decisions in the US. She finds that not only are juries more likely to apply the death penalty than judges, but also that juries are more influenced by demographic characteristics (such as age and race) of the offender and the victim. These findings support the hypothesis that juries may fail to be impartial, and that this impartiality may not be equally applied across defendants.

when evaluating the potential impact of a change in punishment severity. The first order question is whether such a change affects criminal behavior. Yet, this paper demonstrates that other agents in the criminal justice system may be affected, too, and importantly in a way that affects the chance of conviction. This makes it less clear how changing punishment severity impacts expected punishment as perceived by the potential criminal: if the abolition of mandatory minimums, for instance, results in an increase in convictions, then does expected punishment actually decrease? Should evaluations of such sentencing changes take the chance of conviction as exogenous?

The remainder of the paper proceeds as follows. Section 2 provides institutional details on the criminal justice system and changing sentencing regimes in the 18th and 19th centuries. Section 3 describes the data and definition of the treatment and control groups for each experiment. Section 4 presents the results concerning the impact of the offense specific abolition of capital punishment on jury verdicts, while Section 5 presents the results concerning transportation. Section 6 concludes.

2. Institutional Background

2.1. The Rise and Fall of Capital Punishment, Transportation, and Incarceration

The years from 1715 to 1900 in England represent a period of dynamic change in the criminal laws governing sentencing, providing a unique natural experiment to study how changing expected punishment affects the behavior of various agents in the criminal justice system. This section provides a broad overview of the colorful history surrounding these dramatic changes in punishment regimes, and is based on *The Old Bailey Proceedings Online*, original Acts obtained from the Parliamentary Archives, and a number of books summarizing these Acts (Cook and Keith, 1975; Hitchock and Shoemaker, 2015).

In 1688, there were approximately 50 capital offenses. The number of offenses classified as capital began to rise with the *Waltham Black Act* of 1723, which introduced the death penalty for over fifty more offenses.[10] In the following years, numerous parliamentary acts, in large part motivated by a desire to protect the property of the land-owning classes, increased the number of capital offenses to 160 in 1765 and more than 200 in the early 1800s.[11] This period became known as *The Bloody Code*, both because of the high number of capital offenses and the public

[10] The following offenses were listed in the Black Act and are included in the current study: fraud, perverting justice, animal theft, and arson. The Black Act was named after a group of poachers who blackened their faces in a series of poaching raids prior to 1723.

[11] Some offense categories are subdivided; for instance, there are different offenses for each type of animal theft.

and/or bloody spectacle made of executions. At the turn of the 19th century, even crimes that are viewed today as petty crimes (e.g. pickpocketing and shoplifting) were capital offences.

A movement to reform the criminal justice system, led by Sir Robert Peel, began in the 1820s with the passage of the *Judgment of Death Act* of 1823. This Act made the death penalty discretionary for almost all then capital crimes except murder and treason. Though judges still had to officially enter a death sentence (as seen in the data), this sentence could later be reduced at the judge's discretion. Additional acts reduced the number of offenses even eligible for the death penalty in subsequent years: 1832 (animal theft, coinage, and forgery), 1833 (housebreaking), 1835 (mail theft), and 1837 (wounding, burglary, and robbery). Finally, in 1861, the death penalty was abolished for wounding with the intent to kill (i.e. attempted murder). At this point, the only remaining capital offenses were murder (abolished in 1965), arson on the docks (abolished in 1971), espionage (abolished in 1981), and piracy and high treason (abolished in 1998). The public spectacle of executions ended in 1868. Figure 1 demonstrates that more than 75 percent of the 26 offenses observed in the *Old Bailey Proceedings* and used in our analysis were capital eligible between 1715 and 1820. This share sharply decreases in the mid-1800s to about 15 percent, holds steady until the early 1860s and then sharply decreases again. Appendix Table 1 lists the offenses underlying this figure, and the corresponding acts which abolished capital punishment.

In the early 1700s, a not insignificant share of offenders could escape capital punishment by invoking the "benefit of clergy".[12] Transportation provided an alternative that individuals could not escape on these grounds. The first *Transportation Act* (1718) allowed individuals convicted of a clergyable offense to be transported to America for seven years; returning from transportation, however, was a capital offense. Transportation was unexpectedly halted in 1776 due to the American Revolution.[13] In the face of the resulting penal crisis, the *Hulks Act* (1776) was passed, allowing male convicts to be put to hard labor (dredging the river Thames) and held in the hulks of ships. Poor conditions on the hulks (as evidenced by the frequent escape attempts and high risk of death due to overcrowding, poor nutrition, and illness) and growing resentment towards the over-crowded prison system culminated in the eruption of *The Gordon Riots* on June 2, 1780 (Hitchcock and Shoemaker, 2015). During the weeklong riots, many

[12] Since the middle ages, a criminal could be handed over to his church for clergyable offenses. To prevent too many criminals from getting off, many offenses were re-classified as non-clergyable, including, for instance, murder, rape, robbery, burglary, and pickpocketing in the 1500s, and housebreaking, theft from a dwelling and shoplifting (of more than 40 and 5 shillings, respectively), and sheep/cattle theft in the 16-1700s (Beattie, 1986).
[13] Judges were already losing faith in transportation as a deterrent: prisoners no longer feared the Americas, nor did they fear returning, as death sentences were often pardoned (Hitchcock and Shoemaker, 2015).

prisons were attacked, and prisoners escaped or were released. Military intervention ended the riots, leaving a temporary military presence on the streets of London and a distrust of the lower classes by 'respectable Londoners' (Hitchcock and Shoemaker, 2015).

To combat the growing unrest among the people, the courts resumed transportation sentences in October 1781, despite the lack of a viable new penal colony; those receiving such sentences were imprisoned. A new penal colony was finally established in 1786 in Botany Bay Australia, to which the First Fleet (eleven ships with more than 700 convicts) set sail in May 1787.[14] Being "transported for life beyond the Seas" to Australia was seen as a worse punishment than transportation to the Americas. The voyage commonly took four to six months, during which time many became ill or died. Upon arrival in the colonies, the convicts were often put to hard labor in gangs developing infrastructure. Discipline was harsh – lashes, chain gangs, or being sent to the most remote penal colonies in Australia. Transportation rose throughout the 1820s and 30s, as it often replaced capital punishment as the maximum sentence. As transportation to Australia began to be perceived as inhumane, and to not deter despite its harshness, it was abolished through the *Penal Servitude Acts* of 1853 and 1857.[15] The former replaced transportation for seven years with four-years penal servitude, retaining transportation for only long term cases. The 1857 Act abolished transportation for these remaining cases. However, it was not until October 1867 that the last convict ship set sail for western Australia. It is believed that about 20% of Australians today descend from the more than 160,000 convicts transported between 1787 and 1868.

The idea of imprisonment as a mainstream sentencing model dates back to the American Revolution, when a substitute was needed for transportation. Newgate prison, which was the main prison in London in the 1700s, was largely used to hold individuals awaiting trial or execution. With the abolition of capital punishment and transportation in the mid-1800s, the use of imprisonment became the primary sanction. The Millbank Penitentiary opened in 1821, with 860 separate cells. In 1842, the Pentonville modern prison opened, with the capacity for 520 prisoners to spend up to 18 months in solitary confinement; many more prisons were built on this model in the 1840s and 1850s.

Our identification strategy capitalizes on the sharp changes in punishment severity resulting from the three natural experiments in this historical context. Figure 2 illustrates these

[14] Four transport ships had been sent to the African coast, America and Honduras in 1782 and 1785; these `experiments' were deemed failures due to the 'mutinous spirit of the convicts' and 'rejection by the destination populations' (Hitchcock and Shoemaker, 2015).
[15] See the Old Bailey website: http://www.oldbaileyonline.org/static/Punishment.jsp#transportation.

by presenting the share of convicted offenders at the Old Bailey (as recorded in the Proceedings) who were sentenced to death (black line), transportation (dark grey line), and imprisonment (light grey line) during this almost 200-year period. First, the share of death sentences declines from around 25% to almost zero in the mid-1800s due to the offense specific abolition of capital punishment. Second, the temporary halt of transportation during the American Revolution results in a drop in transportation sentences from around 75% in the first half of the 1700s to 0% during the War, with a corresponding temporary increase in prison sentences.[16] Third, the abolition of transportation is seen in the 1850s.

2.2. London and The English Jury System in the 18th and 19th Century

The data for this study come from trials at the *Old Bailey*, which was the central criminal court for the City of London and the surrounding County of Middlesex; it was responsible for trying the most serious crimes, including all felonies. Criminal cases were tried by a jury upon a Grand Jury's decision that there was sufficient evidence to proceed.[17] The legal system at the beginning of this period was largely designed to protect the property of the upper classes, with little attention given to the rights of the defendant. This began to change in the 1800s, as the burden of proof shifted from the defendant to the prosecution with the presumption of innocence (1827) and the entitlement to defense attorneys for felony indictments (1836). Jury deliberations also changed in ways that likely increased the chances of a fair trial; in particular, until 1858, juries were not allowed fire, food or drink until a verdict was reached.[18]

For most of the period studied in this paper, the jury selection process was governed by the *Juries Act* of 1825. This Act defined men aged between 21 and 60, who resided in England and had land or wealth of an appropriate threshold as eligible for jury service.[19] [20] To be geographically representative, juries were separately selected for the London and Middlesex cases. Each year, the Churchwardens and Overseers of the parish created a master list of eligible

[16] The high prevalence of transportation (despite the large number of capital offenses) is driven by the most common offense category of larceny, which is generally non-capital throughout the period. Note that share of sentences to prison and capital punishment do not completely offset the decrease in transportation sentences; other sentences (not shown), especially corporal punishment, were also used increasingly during the Revolution.
[17] After 1838, a clerk assisted in the decision making at the Grand Jury sessions, resulting in fewer dropped cases.
[18] Source: Old Bailey website online.
[19] Females became eligible for jury service with The Sex Disqualification (Removal) Act of 1919. See Anwar, Bayer, and Hjalmarsson (2016) for an empirical analysis of the impact of adding females to the jury pool.
[20] According to the 1825 Act, a man must: (i) possess an income of 10 pounds per year from real estate or rent charge, or (ii) possess 20 pounds per year from a leasehold of not less than 21 years, or (iii) be a householder living in premises rated no less than 20 pounds per year (30 pounds in London and Middlesex), or (iv) occupy a house with no fewer than 15 windows. In addition, foreigners and justices of the peace were disqualified from service. See Bentley (1998) for a summary of both this act and the English criminal justice system in the 1800s.

jurors, which was delivered to the sheriff in a book to be used in the following year. Individuals to be in the jury pool were selected from this master list and received a summons ten days prior to the beginning of each session. Though little is known about how the pool was selected (Langbein, 1987), the 1825 Act does detail how to seat a jury of the first 12 randomly drawn men not struck for cause (including ineligibility to be in the pool in the first place).[21]

An underlying assumption of this paper is that the jury knew (or at least had an expectation of) the punishment associated with handing down a guilty verdict for various offenses. There are a number of reasons to believe this to be the case. First, a unique feature of this historical period compared to today is that the same jury tried many cases during a session. This is explicitly seen in the data, and in fact remained common practice until the *Juries Act* of 1974 on which current law is based. From the 1840s on, the judge handed down the sentence immediately after the verdict was announced; that is, the jury observed the sentence for each case before hearing the next (Bentley, 1998). Prior to the 1840s, however, sentences were given to all convicted defendants on the last day of the hearings/session; thus, the jury did not have the chance to learn about sentencing over the course of a single session. On the other hand, jurors (both before and after 1840) likely formed expectations about sentencing, e.g. the chance of a death sentence, by (i) regularly reading the Proceedings themselves, which were published for public consumption, and (ii) having sat on juries in previous sessions. In fact, according to the Old Bailey Online, "jurors tended to serve on more than one occasion, which meant that almost every jury included experienced members who were familiar with court procedure."[22]

Not surprisingly, other aspects of the criminal justice system also changed during this 200-year period. Perhaps the most notable institutional change is the introduction of the Metropolitan Police in 1829 in a 10-mile radius around Charing Cross (central London) but excluding the City of London. It consisted of about 3,000 uniformed men tasked with patrolling the streets to deter crime. The catchment area expanded to a 15-mile radius to 1839, with an increase in the size of the force to about 4,300 men.[23]

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[21] According to the 1825 Act, all summoned names "shall be written on a distinct Piece of Parchment or Card, such Pieces of Parchment or Card being all as nearly as may be of equal Size, ….and shall … be put together in a Box to be provided for that Purpose, and when any Issue shall be brought on to be tried, such Associate or Prothonotary shall in open Court draw out Twelve of the said Parchments or Cards one after another, and if any of the Men whose Names shall be so drawn shall not appear, or shall be challenged and set aside, the such further Number, until Twelve Men be drawn, who shall appear, and after all just Causes of Challenge allowed, shall remain as fair and indifferent."

[22] https://www.oldbaileyonline.org/static/Judges-and-juries.jsp#searchingforjurors

[23] Prior to 1829, policing was done by a local watch, which was generally decentralized through a number of institutions (constables, thief-takers, bow-runners, etc.). A more detailed discussion of the history of policing is provided on the Old Bailey online website.

The late-seventeen and early-eighteen hundreds in England were characterized by the industrial revolution that led to agglomeration and urbanization. The introduction of train lines and the underground facilitated commuting within cities and contributed to the growth in city size. During that period, London's population increased considerably. In 1715, the population amounted to around 630,000. By 1801, the year of the first census, it had grown to over one million, followed by a threefold growth to over three million inhabitants in 1860. By 1815, London was the largest city in the world. The stark population growth, as shown in Figure 3, was a result of a decrease in child and adult mortality, an increase in fertility, and an increase in migrants from both other parts of England as well as Europe and the rest of the world. [24]

3. Data

3.1. The Proceedings of the Old Bailey

The Proceedings of the Old Bailey were first published in 1674, although cases were not consistently recorded until 1715; the final issue was published 239 years later in 1913. After each monthly session, The Proceedings published an account of the criminal cases trialed at the Old Bailey, though the details recorded varied over time and across cases. As described on the Old Bailey Online, The Proceedings initially provided entertainment for the population, with detailed transcripts of the most colorful cases. By 1787, the Proceedings had a quasi-official status, as the City of London had to pay a subsidy to the publishers and, from 1778, "demanded that the Proceedings should provide a 'true, fair, and perfect narrative' of all the trials", leading to approximately equal coverage of all trials. It is important to keep in mind that the composition of cases tried at the Old Bailey may have changed over time as the catchment area changed (e.g. expanded in the 1830s to include Essex) and because there are other courts trying less serious crimes.

The records from the Proceedings have been digitized and published by *The Old Bailey Proceedings Online*.[25] We obtained The Proceedings for each of the 2000 court sessions in xml files and extracted information identifying the unique case, session date, defendant's name, gender and age, offense category, and broad and detailed verdict and sentencing outcomes. The broad verdict data indicates whether the jury found the defendant guilty, while the detailed verdict data indicates whether the defendant was found guilty of a lesser offense than charged

[24] The discussion of population growth is sourced from the Old Bailey Online website on September 14, 2016: https://www.oldbaileyonline.org/static/Population-history-of-london.jsp.
[25] The http://www.oldbaileyonline.org/ website, maintained by HRI Online Publications, provides a tremendous amount of information about the history of The Proceedings, the digitization process, as well as a search engine.

or whether he pled guilty. The broad punishment variable indicates the primary sentence issued by the judge – death, transportation, imprisonment, corporal punishment, miscellaneous or no punishment. Note that the actual sentence issued by the judge is reported in The Proceedings, and not whether the sentence was pardoned. We do not use the detailed punishment data, which indicate, for instance, how the death sentence would be carried out.

The Proceedings contain a wealth of data, however, that are not tagged in the xml files, including the judge and jury names for most cases between 1750 and 1822. In fact, The Proceedings even list each individual juror name for most of these years. Since these data had to be manually transcribed, we coded the judge name, the jury name but not that of the jurors themselves. Each session has at least two juries – one each for Middlesex and London cases. As the number of cases brought to trial increase over time, so do the number of juries. Note, however, that because the variation in punishment severity occurs over time, one can only look across juries and not within juries; we can use the jury name to control for London versus Middlesex cases in some sub-samples.

Finally, we manually transcribed information on the criminal history of the offender, which is available from the 1830s onwards and contains information on whether the defendant had been in custody once before (from 1832), more than once (from 1839) or whether they were known associates of bad character (from 1835). Previously, criminal history was largely irrelevant, since most known criminals were sentenced to death or transported.

As some cases have multiple defendants, the final data set is created at the case by defendant level; each observation refers to a unique defendant. From 1715 to 1900, there are 217,939 defendant-case observations. We exclude the 2,057 observations from 1790 to 1792, when The Proceedings selectively reported only guilty verdicts. Further, we exclude 751 observations with obvious misreporting or missing values in crucial variables. The raw data provide a high level of detail with respect to the charged offenses. As indicated in Table 1, we classify the offenses into the broad categories of: property, violent, sex, fraud and other. However, we exclude: (i) 2,649 cases with charged offenses for which the overall number of trials is too low to conduct meaningful analyses,[26] (ii) 865 cases with charged offenses that involve an unusually large number of defendants (conspiracy and riot), (iii) 186 cases for offenses that are redefined during the sample period and for which the redefinition cannot be

[26] Specific offense categories dropped and the associated number of cases from 1715 to 1900 are: Bankruptcy (404), barratry (4), concealing a birth (474), extortion (323), game law offenses (47), illegal abortion (90), infanticide (328), keeping a brothel (88), petty treason (14), piracy (7), religious offenses (17), return from transportation (378), seditious libel (45), seditious words (35), seducing allegiance (20), tax offenses (189), threatening behavior (145), treason (39), vagabond (2).

clearly distinguished from changes in the punishment laws (kidnapping), and (iv) 4,698 cases for which no particular offense is given in the data (NA, other). These restrictions result in an analysis sample of 206,733 defendant-case observations from 1715 to 1900. Figure 4 displays the annual number of cases in each broad offense category over time.

3.2. Coding Treatment Offenses and Years for Each Experiment

A crucial step in our analysis is the coding of the treated offense categories and treatment years for each experiment. We do so using a two-step approach. First, we identify discontinuities in the share of death and transportation sentences in our data. Second, we compare the timing of the observed discontinuity to that of the historical events or changes in laws, obtained from historical sources whenever possible. We follow this procedure because the long time horizon (200 years) and complicated nature of these historical laws makes it practically impossible to track and find all offense-specific relevant laws. For instance, these laws often targeted very specific offenses within our offense categories, e.g. cow versus horse theft as opposed to animal theft. Some laws described a wide range of sentencing changes for multiple offenses in the same law, where the offense we were searching for is not clearly indicated in the title of the law. In addition, the date of the law often referred to the reign of the monarch rather than the Gregorian calendar. See Appendix Figure 1 for an example of an original law text.

More specifically, for both natural experiments concerning transportation, we identify the set of transportation eligible offenses immediately prior to the American Revolution in 1776 and the Penal Servitude Act of 1853, respectively. Only transportation eligible offenses were actually *treated* by these two events. Thus, we assign offenses with a positive share of transportation sentences to the treatment groups and the remaining offenses, i.e. those with a zero share of transportation sentences, to the control groups. Note that treatment offenses can be different in the two experiments, depending on whether the offense was transportation eligible at that time. For instance, murder and rape were not transportation eligible in 1776, but by 1853, they were transportation eligible. These assignments – for both experiments – are indicated in Table 2.

For the natural experiment concerning the death penalty, we capitalize on the offense specific variation by coding a unique treatment period for each offense again using a data-driven approach. That is, we code the first treatment year as the year when the share of death sentences drops to zero for that offense.[27] Offenses with no such discontinuity are classified as always or

[27] Note that there were 37 cases, which appeared to be anomalies, in that occasional death sentences occurred after the drop to zero. But, a close reading of the transcripts from the Proceedings made it clear that these were

never capital and assigned to the control group. Figure 5 provides examples – murder (always capital), bigamy (never capital) and robbery (reformed in 1837) – of the type of graphical evidence used to identify the discontinuities in the data for the capital punishment experiment. Each graph shows the share of death sentences over time; the solid vertical line marks the time of the discontinuity (the dashed vertical line mark the timing of the transportation experiments).[28] For offenses with a discontinuity, we use the observed year of treatment to identify the corresponding offense specific historical Acts from the House of Lords Parliamentary Archives and additional online sources. Table 2 indicates treatment and control offenses for the capital punishment experiment, and the offense specific first years of treatment; Appendix Table 1 lists the original Acts abolishing capital punishment.[29]

It is important to highlight that our two-stage approach to identifying treatment and control offenses capitalizes on discontinuities observable in sentencing variables, which can be directly linked to a change in the law, and not discontinuities observable in verdicts. That is, we do not look for discontinuities in the outcome, but rather discontinuities in a measure of the 'treatment'.[30] One offense category for which this may be a potential concern, however, is larceny. As seen in Table 1, we combine the offenses of grand larceny (theft of more than one shilling), petty larceny (theft of less than one shilling), simple larceny, and pocket picking into a single larceny offense.[31] Capital punishment did exist for larceny if the stolen goods were of a value greater than a specific threshold, which changed over time; for instance, theft over one shilling was capital until 1827. In practice, however, death sentences are almost never seen for larceny, because the juries had the ability to convict the defendant of a lesser charge, which was not capital. For this reason, we demonstrate that both our overall results and property crime results are robust to excluding larceny.

Table 2 also indicates the number of observations for each offense in the treatment and control groups (or dropped) in the years surrounding each experiment: 1772 - 1789 for the first

attributable to the cases with multiple charges. For instance, a handful of burglaries were sentenced to death after the abolition of capital punishment for burglary; but, in every one of these cases, the person was charged with felonious wounding, stabbing, beating and/or striking, which was indeed a still capital eligible offense. We recoded such cases as the appropriately defined more severe offense.

[28] Such figures are available for each offense upon request for each experiment.

[29] We are unable to identify the specific law pertaining to the abolition of the death penalty for receiving, fraud, and perverting justice, and hence rely solely on the year of discontinuity in the data. Yet, given the high rate at which our data driven and archival law analyses correspond, we believe that the timing of the observed discontinuity in death sentences is a reliable measure for the time of treatment.

[30] In an ideal world, we would have identified the statute that abolished capital punishment for each offense, and then looked at the sentencing data (e.g. death penalty rate) to confirm that there was indeed a treatment or policy change. We are simply doing this in the reverse order for reasons of practicality.

[31] We combine these offenses because of how offense definitions changed over time; this allows us to continuously define a 'larceny' variable over the entire sample period which otherwise is not feasible.

transportation experiment (American Revolution), 1848 – 1857 for the second transportation experiment (the Penal Servitude Act of 1853), and +/- 10 years around the offense specific year that capital punishment was abolished. For offenses in the control group for the capital punishment experiment, we report the number of observations in a window around the median reform year of 1833. A number of facts stand out. First, dropped offenses are those that cannot be reliably studied given either the rarity with which they are observed in the Proceedings or that the offense-specific abolition of capital punishment falls into the same time window as the abolition of transportation (wounding). The most observations (277 disregarding wounding) are dropped in the second transportation experiment, whereas in the first transportation experiment just 53 observations – corresponding to eight offense categories - are dropped. Second, in the years surrounding both transportation experiments, the treatment groups (14,624 and 15,808 observations, respectively) are substantially larger than the control groups (just 780 and 291 observations). Third, there are 16 offenses for which capital punishment was abolished (15,576 observations) and nine control offenses (39,676 observations). There is substantial variation in the abolition year; the earliest year is 1813 for fraud while the latest is 1856 for arson. Finally, the largest crime category in all three experiments is larceny; however, though more than 80% of the control observations in the case of capital punishment are larcenies, we again demonstrate that the results are robust to excluding larceny.

3.3. Summary Statistics

Table 3 presents summary statistics for the whole sample (1715 – 1900) as well as the sub-samples corresponding to each experiment. These descriptives provide an indication of how the criminal justice system is changing over this two hundred year period and a comparison between the treatment and control group of the capital punishment experiment.

From 1715 to 1900, there were 1,748 sessions at the Old Bailey, more than 900 of which are included in our analysis periods. In terms of the broad offense categories, 73% of cases are property offenses while the remaining are classified as violent (10.1%), sex offenses (1.8%), fraud (13.3%) and other (2%); however, property offenses comprise almost 88% of all cases during the American Revolution. In addition, each of these categories are represented in both the treatment and control groups for the death penalty experiment, although the control group consists of a relatively large share of property offenses and the treatment group has relatively more violent and fraud offenses.

Because defendant age is inconsistently reported in The Proceedings – it is missing for 99% of observations in the first transportation experiment and primarily reported for just guilty

defendants in later years – we do not include it as a baseline control. More than 21% of defendants are female, with a larger share during the first transportation experiment (27%). Finally, slightly more than 10% of defendants in both the treatment and control groups for the death penalty experiment have some criminal history (in the years after such data was recorded). From 1750 to 1822, we see, on average, three juries per session, though one should note that the number of juries is increasing over time. 24% of the juries were for London (versus Middlesex) cases. There are 104 judges observed during this time, 30 of whom are seen during the first transportation experiment from 1772 to 1789.

The primary outcome of interest is whether the jury found the defendant guilty. The jury conviction rate over the entire sample period is 67.5%, but just 58.4% of cases are found guilty during the first transportation experiment and nearly 70% during the second transportation and death penalty abolishment samples. It is also clear that the practice of pleading guilty changed over time; it was hardly used during the 1700s (0.2%), but had increased to more than 14% of charged cases from 1803 to 1871; note that it is also possible that the reporting of pleas in the Proceedings changed over time.

4. The Impact of Abolishing Capital Punishment on Jury Decision-Making

The main goal of this paper is to identify the effect of changes in punishment severity on jury decision-making, i.e. the likelihood of handing down a guilty verdict. In this section, we look at changes in punishment severity attributed to the offense specific abolition of capital punishment throughout the 1800s. Section 5 will consider changes in punishment due to the temporary halt and then permanent abolition of transportation.

4.1. Graphical Evidence of the Treatment – Capital Punishment

We begin by demonstrating the impact of abolishing capital punishment on sentences to death, transportation and prison for both the treatment and control groups (Panel A of Figure 6). The figure shows the share of each sentence in the ten years before and after the crime-specific year of reform as represented by the vertical line for the treatment group and the median year of reform (1833) for the control group, respectively. The share of death sentences is fairly steady in the treatment group (around 35%) in the years leading up to its abolition. In the first complete year after the reform, the share sentenced to death drops to zero. In the control group, the share of death sentences is just over 0% in both the years before and after the reform; it is not equal to zero as murder, which is always capital, is included in the control group. Panel B demonstrates the substitution from capital punishment to transportation for cases in the

treatment group in the year immediately after the reform. Despite a parallel pattern in the use of transportation prior to the reform in the treatment and control groups, this post-reform increase in transportation is only observed in the treatment group. Finally, as seen in Panel C of Figure 6, incarceration in both the treatment and control groups is decreasing slightly in the years leading up to the reforms, and increasing afterwards. This is consistent with anecdotal evidence on the timing of the rise of imprisonment as a preferred sanction. Though there is a difference in the level of imprisonment across treatment and control groups (both before and after the reform), the trends in the share incarcerated appear to be parallel throughout the time period.[32]

4.2. Empirical Methodology – Capital Punishment

Motivated by the variation across offenses in the timing of the abolition of capital punishment as well as a not insubstantial share of offenses for which the capital punishment status does not change, we adopt a difference-in-differences design to estimate the effect of the decrease in punishment severity occurring upon the abolition of capital punishment on the chance of conviction. Such a design helps to isolate the effect of capital punishment reforms from other changes occurring in the criminal justice system in the early and mid-1800s, including the introduction of a police force and defense counsel. We estimate the baseline specification, presented in equation (1), for the sample of observations from 1803 to 1871.

$$(1)\ GV_{ijogt} = \propto + \beta_1 noncapital_{ot} + \alpha_o + \alpha_t + \alpha_m + X_{ijogt}\delta + \epsilon_{ijogt}$$

The primary dependent variable is whether defendant *i* charged with offense category *o* (in offense group *g*) in year *t* is found guilty by jury *j*. Secondary outcomes include whether the jury convicts on a lesser charge and whether the jury makes a recommendation to mercy, each conditional on the guilty sample.[33] The primary variable of interest, *noncapital*, is an indicator equal to one for offense-year combinations for which the offense is not capital eligible. That is,

[32] In addition to the graphical evidence, we estimated the effect of the abolition of capital punishment on the probability of being sentenced to transportation or prison in a difference-in-differences regression framework analogous to estimating equation (1) and conditional on the convicted subsample. The results indeed confirm what can be seen in the graphs: There is a substantial and significant positive effect of the abolition of capital punishment both on the probability of being sentenced to transportation and to prison. We do not include the regression results in this paper; all results are available upon request.

[33] The original variable indicating the detailed verdict contains separate information on whether the verdict was guilty of a lesser offense, manslaughter (different from the genuine offense category manslaughter) or guilty for a theft under a certain value below the value originally charged. For our analysis, we construct a broad variable "guilty of lesser charge" by combining the three.

the treatment indicator *noncapital* turns on upon the abolition of capital punishment for treatment group offenses; for control group offenses, *noncapital* does not change over time and equals one (zero) for always (never) capital offenses. The offense-specific treatment years are reported in Table 2.[34]

The baseline difference-in-differences specification includes: (i) offense fixed effects (α_o) to control for baseline differences in case characteristics and conviction rates across offenses, (ii) year fixed effects (α_t) to capture other criminal justice reforms that affected all offenses such as the introduction of the police, (iii) month fixed effects (α_m) to capture seasonality in criminal behavior and even jury behavior (given the absence of heat, it is certainly feasible that deliberations were different in the summer and winter), and (iv) a vector of controls (X) including the defendant's gender, the number of defendants, and the defendant's criminal history in subsample analyses. Standard errors are clustered on the specific offense type by year level.[35]

Intuitively, this design compares how conviction rates changed for the treatment group to that for the comparison group, the difference reflecting the effect of abolishing capital punishment. For β_1 to represent the causal effect of the abolition of capital punishment on conviction rates, however, we clearly make the usual parallel trends assumption – namely that the change in conviction rates for treatment group offenses would have been the same as that for comparison group offenses in the absence of the death penalty reforms. A visual inspection of Figure 6 provides the first evidence that this assumption is satisfied; as discussed above, the pre-reform trends in sentencing (death, transportation, and prison) are comparable for the treatment and control groups prior to the reform year. Panel A of Figure 7 presents comparable graphs for the main outcome – the share of jury decisions resulting in conviction; visual inspection once again suggests parallel pre-reform trends. In fact, pre-reform conviction rates for both the treatment and control groups are fairly flat in the years leading up to the reforms. This figure also provides the first suggestive evidence that the abolition of capital punishment increased conviction rates for treatment offenses relative to control offenses. Panel B of Figure 7 demonstrates parallel pre-reform trends in conviction rates by crime category, and suggests that the increase in conviction rates after the death penalty abolition is driven by violent offenses in particular.

[34] Capital punishment for two treatment group offenses – sodomy and wounding – was abolished in stages; our baseline uses the first year of change as the reform year.

[35] We are less concerned about year to year autocorrelation given the quite flat conviction rates seen in the years leading up to the reforms, especially for property and violent offenses (this can be seen in Figure 7). The baseline results are robust, however, to clustering just by offense type rather than offense-year cell.

A causal interpretation of the effect of abolishing capital punishment on conviction rates relies on two additional assumptions. First, although the abolition of capital punishment was doubtfully a 'random' policy given the criminal justice reform movement at the time, our identification strategy relies on the assumption that the *timing* of the offense-specific abolition was random. It took more than 40 years for capital punishment to be abolished for the whole of our treatment sample; there were no crime-specific movements determining the year that each offense was reformed.[36] Jurors and defendants did not know which offense would be reformed next, nor did they know the year that the reform would occur. The absence of a change in conviction (Figure 7) or sentencing (Figure 6) behavior in the years immediately preceding the reforms supports the validity of this assumption.

Second, our identification strategy relies on the assumption that the quality of evidence presented to the jury did not change after the reform; if it did, then it would be unclear whether jury decisions changed in response to changes in punishment severity or in the type of case. We will discuss this assumption in further detail in Section 4.5 and provide empirical tests of whether the quality of evidence changed.

4.3. Capital Punishment: Main Results and Robustness Checks

Table 4 presents the results of estimating equation (1) for all offenses (with and without controls) and for the following broad offense categories: property, violent and sex, and fraud offenses. Panel A presents the results for the main dependent variable indicating whether the jury convicts the defendant. Including our full set of controls (column 2), we find that the abolition of capital punishment significantly increases the chance of conviction by 7.6 percentage points (10.6% relevant to the mean). However, these estimates are quite heterogeneous across crime categories. Abolishing capital punishment increased the chance of conviction by 22 percentage points (37.0%) for violent crime and sex offense cases and 35 percentage points (47.5%) for fraud offenses. In contrast, the effect for property crimes – by far the largest crime category – is much smaller (1.5 percentage points or 2%) and only significant at the 10 percent level.

Panels B and C of Table 4 present the results for our secondary outcomes for the sample of guilty verdicts – convictions of a lesser offence and recommendation to mercy, respectively. Conditional on being found guilty, the chance of conviction of a lesser charge *on average*

[36] One exception that we are aware of is forgery. According to Hans and Vidmar (1986), English bankers requested the abolition of the death penalty for forgery. We present results overall and broad crime category. While the estimates are large for fraud offenses, they are just as large for violent and sex offenses.

decreases by more than 15 percentage points relative to a (pooled) mean of 0.07 while the chance of a jury recommendation for mercy *on average* decreases by six percentage points relative to a (pooled) mean of 0.11. These estimate are *average* effects; importantly, the average rates of conviction of a lesser offence and recommendation to mercy vary substantially between offenses.[37] One prominent example is larceny: excluding larceny cases from the regression yields a 10 percentage point (instead of 15) decrease in convictions for a lesser offense relative to a mean of 0.133 (instead of 0.07). While the regressions take care of that by including offense specific fixed effects, the interpretation at the mean has to be done with caution.[38] In terms of the sign and relative magnitudes of the coefficients across offenses, these findings are in line with economic intuition. Before the abolition of capital punishment, the jury had to find a means of lessening the sentence; as capital punishment is abolished, they no longer have to do this. When looking at the broad offense categories, we find that the lesser charge effect is completely driven by property crimes. For instance, the jury can convict an individual of theft of less than 5 shillings to make the offense not eligible for capital punishment (for that point in history when 5 shillings was the threshold). There are fewer violent and sex offenses with corresponding 'lesser' offenses; one exception is murder and manslaughter. The 'recommendation to mercy' results, however, are driven by violent and sex offenses as well as fraud.

To summarize the main results presented in Table 4, we find that the decrease in punishment severity arising from the abolition of capital punishment significantly increased the chance of conviction overall, and especially for violent and sex as well as fraud cases. This was accompanied by a large decrease in the chance of a recommendation for mercy in these crime categories - as mercy was no longer needed to spare someone death. Finally, for property offenses, there is a small increase in the chance of conviction and an accompanying reduction in the chance of mercy; however, the main channel through which property crime is affected is a reduction in the chance of being convicted of a lesser charge.

Table 5 presents a series of robustness and sensitivity analyses for conviction by the jury (Panel A) and conviction of a lesser offense (Panel B). For comparison purposes, the baseline result for all offenses is presented in column (1). Columns (2) and (3) demonstrate robustness

[37] For example, the mean rate of conviction for a lesser offense is 0.03 for *manslaughter* but 0.54 for *murder*, while the mean rate of recommendation for mercy is 0.46 for *manslaughter* but only 0.11 for *murder*. Similarly, the mean rate of conviction for a lesser offense is 0.004 for *stealing from master* but 0.22 for *burglary*, while the mean rate of recommendation for mercy is 0.325 for *stealing from master* but only 0.08 for *burglary*.

[38] An alternative explanation for finding point estimates that exceed the mean is a misspecification of the linear model in the case of low probability outcomes. In order to rule that out, we run probit models for the secondary outcomes and find robust results: the marginal effects at the mean are still negative and statistically significant, and allow the same economic interpretation. As for the magnitudes, the nonlinear estimations yield smaller coefficients but suffer from the usual concern of biased results in nonlinear fixed effects estimation.

to controlling for offense group by year fixed effects and an offense group specific linear time trend. These very demanding specifications do not change the qualitative nature of the results, though the effect size decreases somewhat for the main outcome in Panel A. Columns (4) and (5) present the overall and property crime results when excluding larceny. Larceny is the largest crime category, but also the 'messiest': it was redefined a number of times during our sample period and furthermore is not a perfectly clean 'control' offense, as the laws did – theoretically - allow for death sentences at various points in history for thefts over a certain threshold. The results show that, if anything, excluding larceny increases our estimates of the effect of the abolition of capital punishment on the chance of conviction. Column (6) demonstrates the robustness of the results to excluding sodomy and wounding, which are the two offense categories for which capital punishment was abolished in stages. Finally, we demonstrate that restricting the sample to 1850 and earlier (i.e. before the abolition of transportation) and to after 1820 yields the same general pattern of results with only a small decrease in the magnitude of the point (columns (7) and (8), respectively).[39]

4.4. Capital Punishment: Heterogeneity Analysis

Panel A of Table 6 turns to the question of whether the abolition of capital punishment has heterogeneous effects on the chance of conviction for different types of defendants. Two dimensions that we can consider are the defendant's gender and criminal history. Columns (1) to (3) consider whether the abolition of capital punishment had differential effects for male versus female defendants overall (column (1)) as well as for property and violent offenses (columns (2) and (3)). Sex offenses are excluded here given the lack of female sex offenders. Overall, the abolition of capital punishment increases the chance of conviction more than seven percentage points, with no differential effects by gender. When zooming in on violent crimes, however, we see that the abolition of capital punishment increases the chance of conviction by 30 percentage points for females and just 18 percentage points for males; this suggests that juries were more reluctant to convict females than males prior to the abolition of the death penalty. Consistent with these findings, we also find that the abolition of capital punishment results in a significantly larger reduction in the chance of being convicted of a lesser charge for females than males (Panel B of Table 6). That is, prior to the reform, females were more likely to be treated favorably by the jury – they were less likely to be convicted and if convicted, they were more likely to be convicted of a lesser offense.

[39] Additional specification checks (not shown) demonstrate robustness to using just the treatment group offenses and a more uniform control group (i.e. just those that are always capital or just those that are never capital).

Because criminal history is only recorded after 1832, column (4) begins by presenting our baseline specification for this restricted sample period; the abolition of capital punishment increases the chance of conviction by ten percentage points. Controlling for criminal history in column (5) has minimal impact on this finding, despite the fact that having a criminal history itself significantly increases the chance of conviction by 28 percentage points. Finally, column (6) suggests that juries had less of a problem imposing a death sentence prior to the reform on individuals of known 'bad character', since the increase in conviction rates caused by the abolition of capital punishment is only observed for those individuals without a criminal history. Note, however, that this is suggestive and the coefficient on the interaction is somewhat imprecisely estimated.

4.5. Threat to Identification: Changing Quality of Evidence

The above empirical analysis estimates the effect of changing punishment severity on jury verdicts. The implicit identifying assumption is that it is only the expected punishment for the charged offense that changes from the jury's perspective, while all other aspects of the case, and in particular the quality of evidence, are held constant. However, an alternative explanation for our finding that abolishing capital punishment increases conviction rates could be that it was accompanied by an *increase* in the quality of evidence presented to the jury. Clearly, it is possible that the abolition of capital punishment impacts the behavior of other agents in the criminal justice system, including potential criminals, defendants, police, and attorneys. But, does it do so in a way that affects (in particular, increases) the quality of evidence? We discuss each channel in turn and empirically test whether there is a change in the quality of evidence.[40]

The economic model of crime (Becker, 1968) predicts that abolishing the death penalty (i.e. decreasing expected punishment) should increase the number of crimes. The extensive empirical research regarding this question, however, does not find strong evidence that this is the case (Donohue and Wolfers, 2006). Perhaps most relevant in the current context is a study by Phillips (1980) of the deterrent effect of publicized London executions in the latter half of the 19th century. He finds that homicides are significantly lower in the two-weeks immediately after an execution (with larger effects for more publicized executions) but that there are no *long-term* deterrent effects of executions on homicide. Panel A of Figure 8 presents the number of cases within the 'treated' offenses for each broad crime category seen in the Old Bailey

[40] Note that we focus on an *increase* in the quality of evidence as a threat to identification. A *decrease* in the quality of evidence would result in a downward bias in the estimates meaning that our estimates would be a *lower* bound of the true effect.

Proceedings in the ten years before and after the respective reform years. Similar patterns are seen when considering crime rates normalized by linearly interpolated population estimates from the decennial census. A deterrent effect (in this case an *increase* in crime) is not apparent. It should be noted, however, that any observed change in the number of cases can reflect either a change in criminal behavior, or a change in the reporting behavior of the victim or witnesses.[41] Nevertheless, even if the crime rate did not change, it could still be the case that there was a change in the nature of the crime (e.g. how 'sloppily' it was committed) and the resulting quality of evidence (e.g. witnesses). We return to this shortly.

Alternatively, does the change in punishment severity affect policing behavior or the prosecutors' decision to bring a case to trial? For this to be a concern to the validity of our analysis, however, it is not enough that the prosecutors decided to bring more cases to trial; it must also be that they were bringing cases forward with a differential standard of evidence in order to affect the jury's decision to convict. Yet, punishment severity decreases with the abolition of capital punishment, which means that the stakes are decreasing. Thus, one may expect that prosecutors bring more cases with a *lower* quality of evidence to court rather than more cases with a *higher* quality of evidence. This would imply a downward bias in our baseline findings and that we estimate a *lower* bound of the true effect, but it would not undermine the validity of our results.

Finally, one could imagine that changes in expected punishment affect a defendant's decision to plead guilty. If this affects the type of case faced by the jury, then this would raise similar concerns; however, the most likely scenario would be that defendants faced with the greatest chance of losing (i.e. the strongest evidence against them) would be more likely to plead, which would again lower the quality of evidence of the remaining cases that are faced by the jury. It is important to note, though, that pleas did not yet play a large role in the criminal justice system during this period: until 1836, just three percent of all cases are recorded as pleas; after 1836 (contemporaneous with the introduction of defense attorneys for felony indictments), pleading becomes a more common feature of the criminal justice system. Panel B of Figure 8 demonstrates that the share of cases that are pled are trending up in the years surrounding the abolition of capital punishment, and that this occurs for both the treatment (centered on treatment year) and control offenses (again centered on 1833). As a further robustness check that our results are not driven by a change in case composition due to a change in plea behavior, we re-estimate our baseline specification when including all cases in which a defendant pled

[41] In addition, the Proceedings are likely to be a noisy measure of crime at this time, given changing catchment areas and the presence of other courts.

guilty and assume these cases would have resulted in a guilty verdict by the jury (see column (9) of Table 5). Although this decreases the magnitude of our estimated effect, we still find that the abolition of capital punishment significantly increases conviction rates.

To more directly assess the bottom line concern presented in this section – namely that there might be an increase in the quality of evidence with the abolition of capital punishment – we use the Old Bailey online search function to create proxies for the quality of evidence. Specifically, we conduct keyword searches for *witness*, *police*, and *evidence* by year and offense category, and then normalize by the number of charges in that year (i.e. we look at the hit rate).[42] Table 7 presents the results of estimating equation (1) using the resulting panel of offense category (26 offenses) by year data, including offense and year fixed effects. When looking at all offenses, there is a significant reduction in the hit rate for the keywords 'evidence' and 'witness' after the abolition of capital punishment, and a marginally significant reduction in hits on 'police'. Similar patterns – though less significant – are seen when looking at violent and sex offenses or property offenses. Thus, we find that, if anything, there is a decrease in the quality of evidence, but certainly not an increase in the quality of evidence.

5. The Impact of Halting and Abolishing Transportation on Jury Decision-Making

5.1. The American Revolution and Temporary Halt of Transportation

This section assesses the impact of the temporary halt of transportation during and following the American Revolution on conviction rates. We begin by graphically assessing how punishment changed during the war. The vertical lines in Figure 9 correspond to the years 1776 when transportation was first suspended due to the Revolution, 1781 when judges began issuing transportation sentences despite the lack of a penal colony, and 1787 when a new penal colony in Australia was officially established. Conditioning on the sample of guilty cases, Panel A presents the share of sentences to transportation, death, and prison for the transportation eligible offenses (i.e. treated offenses as specified in Table 2). Almost 75% of sentences in each year leading up to the war were sentenced to transportation and 0% in the years 1776 to 1781. The share of sentences to transportation began to increase again in 1781, until the pre-war levels were nearly reached in 1787. Panel A also demonstrates that imprisonment was the primary substitute for transportation, as the share of prison sentences rose from around 0% to almost

[42] Specifically, the three searches include the following terms: (i) evidence; (ii) witness(es); and (iii) policeman, police, constable, watchman, watch-man, watchmen, runner, thief taker, bobby, bobbies, peeler, peelers.

50% during the war while the share of death sentences only rose by 5-10 percentage points. After the war, imprisonment rates decreased again, though not back to zero.

The fact that in some cases transportation was substituted by capital punishment – which is clearly a harsher punishment than incarceration – makes it hard to say whether punishment severity (i.e. the jury's expectation regarding punishment) actually increased or decreased. Yet, it becomes clearer (at least for a subset of offenses) upon decomposing the treatment offenses into those that were and those that were not capital eligible. For non-capital offenses (larceny and perjury) the temporary halt of transportation sharply decreased expected punishment with an increase in prison sentences, if one assumes that a sentence to prison (despite the horrible prison conditions) was perceived as better than transportation to the Americas. For capital offenses, however, both death and imprisonment were substitutes, leaving the change in expected punishment ambiguous (see Panels B and C of Figure 9).

Our baseline specification to estimate the effect of the unexpected change in punishment severity upon the temporary halt of transportation in 1776 is presented in equation (2). Since, as seen in Table 2, we lack a sufficiently large control group (i.e. offenses not eligible for transportation at this time), we use a simple reduced-form pre-post design. A distinguishing feature of this experiment is that the change in punishment severity is driven by a shock – the war – that is exogenous to the justice system. The flip side is that this reduced form experiment captures not just the first order effect of the American Revolution on transportation, which we already demonstrated to be both sharp and large, but also any other channels through which the war may affect conviction rates.[43] One may be particularly concerned about the immediate aftermath of the war, when the release of military personnel shocked the skilled and unskilled labor markets and when there was tremendous unrest in London following the Gordon Riots in 1780. Thus, our baseline specification focuses on the years 1772 to 1779, i.e. the four years surrounding the start of the war and prior to the riots.

$$(2)\ \boldsymbol{GV_{ijot}^{cap,noncap}} = \propto + \boldsymbol{\beta_1 Pre1776_t} + \boldsymbol{\alpha_o} + \boldsymbol{\alpha_m} + \boldsymbol{\alpha_{judge}} + \boldsymbol{X_{ijot}\delta} + \boldsymbol{\epsilon_{ijot}}$$

The dependent variable is whether the jury returned a guilty verdict (GV) for defendant *i* facing jury *j* charged with offense category *o* in year *t.* The primary variable of interest, *Pre1776,* is a dummy indicating the four years prior to the war; that is, it is an indicator for the period during

[43] See King (2000) for a discussion of the relationship between wars and crime during the 1700s, including the use of the military instead of sanctions and the impact of service on post-service crime, potentially attributable to poor labor market opportunities.

which transportation existed. Defining the specification with the omitted time period having the *changed* expected punishment (no transportation) allows us to expand the same specification to assess the impact of re-introducing transportation. All specifications again control for offense and month fixed effects; in this case, more detailed data also allow us to control for judge fixed effects. We control for a vector *X* of case specific characteristics, including defendant gender, number of defendants, and whether the jury (and therefore case) was a London jury (case). The latter is a particularly important control during this time period as the Middlesex judges had limited access to the hulks as a potential sentence compared to the London judges (Hitchcock and Shoemaker, 2015). [44]

A number of additional points are worth making about our choice of baseline specification. First, we do not include time trends given that sentencing patterns (share transported and share sentenced to death) were relatively constant in the years leading up to the war. Second, we focus on just the pre-war period. We believe this to be the cleanest natural experiment, because (i) in contrast to its reinstatement, the halt of transportation was unexpected, (ii) our reduced form framework would make it difficult to disentangle the effect of reintroducing transportation from the general discontent with the criminal justice system in part due to the overcrowded prisons and hulks, and (iii) it is difficult to characterize what happened to *expected* punishment severity in the post-war period, as transportation was re-instated in name only until 1786. Finally, as denoted in the superscript in equation (2), much of our empirical analysis divides the transportation eligible offenses into two sub-treatment groups, capital versus non-capital eligible offenses, and emphasizes the latter. As demonstrated above, halting transportation differentially affects punishment severity for these two groups. For the non-capital offenses, the halt of transportation unambiguously decreases punishment severity, as the only substitute is prison/hulks, whereas the impact on punishment severity is less clear for capital offenses.

The results of estimating equation (2) are presented in Table 8. When considering all transportation eligible cases and including the full set of controls, as shown in column (2), we find that defendants are about 3 percentage points less likely to be convicted (5.5% relative to the mean conviction rate of 56.4%) in the pre-war period when transportation is a possible sentence; without controlling for the jurisdiction, we find a slightly negative, but insignificant effect. Columns (3) and (4) decompose these offenses into those that are non-capital and capital, respectively. The overall effect is, in fact, being driven by the non-capital cases, for which

[44] Note that judge fixed effects can be included since we observe 30 judges who try cases in multiple periods; jury fixed effects on other hand cannot be included given that each jury is only observed in one period.

punishment severity is unequivocally higher before the halt of transportation; these defendants were almost five percentage points less likely to be convicted (8% relative to the mean) when transportation was on the table compared to the war period. No effect, however, is seen for capital offenses. Columns (5) and (6) look separately at London and Middlesex cases; the effect of transportation on verdicts in non-capital cases is larger in Middlesex (about 6 percentage points) compared to London (about 2 percentage points). Column (7) includes an offense group specific linear time trend; though the coefficient decreases somewhat (and precision is lost), the same qualitative results are seen. Finally, column (8) tests whether the same pattern of results is seen upon the reintroduction of transportation. To do this, we expand the sample to include years through to 1789 (we stop here as the data is missing between 1790 and 1792) and include dummy variables for two additional periods: *1780-1786*, which includes the Gordon Riots, its aftermath, and the presence of transportation in name only, and *post1786* when a new Australian penal colony is established. We focus on the latter period for two reasons: (i) to avoid confounding our estimates with the other channels through which the immediate aftermath of the war may affect crime and conviction rates and (ii) because punishment severity has unambiguously changed again, whereas it is unclear what people perceived punishment to be during the 1780-1786 period, given the lack of a penal colony. We find that reinstating transportation decreases the chance of conviction by about 2.5 percentage points, though this effect is not significantly different from zero.[45]

5.2. The Abolition of Transportation in 1853

Finally, we consider the impact of abolishing transportation in 1853. While there was a second Penal Servitude Act in 1857, we focus on the impact of the first Penal Servitude Act in 1853 that abolished transportation for almost all cases (see Figure 2) and a sample period from 1848 to 1857. We choose these years to limit the chance of confounding the effect of the abolition of transportation with anything else happening contemporaneously – namely the recent abolition of capital punishment in the 1830s and an 1858 jury reform that allowed juries food, drink and heat during deliberations.

[45] The risk of confounding the causal estimates with the effects of the immediate aftermath of the war is arguably larger when we estimate the effects on the secondary outcomes (lesser charge, recommendation for mercy), which allow for more discretion. When we estimate these effects, we find that the probability of being charged of a lesser offense for capital offenses in London (but neither for capital offenses in Middlesex nor for non-capital offenses) is higher in the pre-war period. We do not find any effect on recommendations for mercy. One possible explanation is that the lack of a substitute punishment for capital cases led to fewer verdicts of a lesser offense. The results are not reported in the paper but available upon request.

We again begin with graphical evidence of the treatment. The vertical line in Figure 10 corresponds to the year 1853, the year in which the first and main law abolishing transportation was passed. The share of sentences to transportation decreased from 20-25% to almost 0% with the first reform in 1853, with a corresponding substitution from transportation to imprisonment. There is thus a *decrease* in expected punishment, but it should be noted that the overall level of punishment severity is already much lower in the 1850s than during the American Revolution.

We use a similar pre-post research design as in the analysis of the halt of transportation to study the reduction in punishment severity upon the abolition of transportation. Since the sharpest decrease in transportation sentences occurred after the first reform, our baseline empirical specification, presented in equation (3), includes a dummy for whether the trial was in 1853 or later.

$$(3)\ \boldsymbol{GV_{ijot} = \propto + \beta_1 Post1852_t + \alpha_o + \alpha_m + X_{ijot}\delta + year_t + \epsilon_{ijot}}$$

Punishment severity was trending down in the years leading up to the reform (easiest to see in Figure 2), with the gradual abolition of capital punishment and growth of the prison system. Thus, our baseline empirical specification includes a linear time trend (*year)* in addition to the usual controls (offense fixed effects, month fixed effects, defendant gender, number of defendants, and in this case, criminal history). During this time period, we no longer observe judge and jury characteristics. Thus, our baseline analysis estimates equation (3) for the treatment group offense categories as listed in Table 2. Columns (1) to (3) of Table 9 present the results; using the full set of controls as shown in column (3), the decrease in punishment severity does not significantly affect conviction rates. Note that when we do not include a linear annual trend, we find significant negative coefficients; however, these appear to reflect a general trend rather than the impact of the abolition of transportation, as they are absorbed once we include the linear trend variable.

Finally, in an attempt to control for changes in other factors that affect conviction rates, we break the set of offenses into two groups: those which had relatively high and low transportation shares prior to the abolition of transportation. We calculate the share of transportation sentences in the years just before the reform and define the high transportation share group as offenses with a more than 25% share of transportation sentences.[46] We then

[46] Note that we use different windows in order to calculate the share of transportation sentences and to define the treatment. Our treatment definition is robust to that except for the two offenses murder and receiving. Hence, we exclude these two offenses from the difference-in-differences analysis.

estimate a difference-in-differences specification with the high share transportation offenses as the treatment group, based on the notion that the change in punishment severity upon the abolition of transportation is larger for these offenses. The corresponding results are presented in columns (4) and (5) of Table 9; we find no evidence that the decrease in punishment severity results in an increase in conviction rates in the high compared to the low transportation share offenses, confirming the results from the pre-post design.

Why does the decrease in punishment severity occurring with the abolition of transportation have no impact on conviction rates? One potential explanation is that the extent to which changes in punishment severity affect jury behavior depends on the size of the change: the largest change in punishment clearly occurred with the abolition of capital punishment. Likewise, the temporary halt of transportation during the war likely represents a larger reduction in sanction severity than the final abolition of transportation, since transportation was so much more common at the time of the first experiment than later. An additional explanation may be that the abolition of transportation had no effect on conviction rates because it came so soon after the abolition of capital punishment. That is, in the years prior to 1853, jurors had already experienced a large decrease in expected punishment, and reacted by substantially increasing conviction rates, leaving less room for future such increases. Finally, it may be that changes in expected punishment only affect jury behavior if they surpass some threshold – i.e. a level of punishment that a juror's moral code prevents them from imposing.[47]

6. Discussion and Conclusion

Using three natural experiments from English history, this paper studies how changes in punishment severity affect jury decision-making. We find that the decrease in punishment severity resulting from the abolition of the death penalty had a large and significant impact on jury behavior, generally leading to the jury being 'harsher'. Similarly, the unexpected decrease in punishment severity at the time of the American Revolution resulted in a significant increase in convictions, albeit one that is smaller than that in the death penalty context. Finally, the abolition of transportation in the 1850s, which followed the abolition of the death penalty, had no impact on conviction rates.

[47] Similar to the death penalty analysis, we also estimated the effects on convictions separately by crime type as well as the effects on the secondary outcomes (charged of lesser offense, recommendation for mercy). We do not find any heterogeneity across crime types neither do we find significant effects for the secondary outcomes. The results are not reported in the paper but available upon request.

Despite the historical context, these findings have important potential implications to today's criminal justice system. First, we show that punishment severity does affect jury behavior, especially in the typically highly controversial capital cases. An unexpected consequence of abolishing the death penalty may be an increase in convictions. Conversely, that implies lower conviction rates when the death penalty is in place. This is one potential explanation for the finding in much of the recent literature that there is no or only little evidence of a deterrence effect of capital punishment (whether because there is no deterrence effect or a lack of statistical power to detect it).[48] That is, our results are consistent with an explanation that a potential deterrence effect of capital punishment is (at least to some extent) offset by an inverse deterrence effect of lower conviction rates, which decrease the expected punishment for the offender.

Second, our heterogeneity analyses indicate that, at least for certain crime categories, juries were differentially affected by the reforms depending on the defendant's gender and criminal history. This (perhaps unintentional) unequal application of justice raises questions about the fairness of the criminal justice system with respect to observable characteristics of the defendant. Such questions are clearly still topical today, especially in the context of defendant and victim race, and continue to be discussed in the literature.[49] Third, although juries in today's criminal justice system decide only a small share of cases, this research certainly raises the question of whether punishment severity impacts the behavior of other agents in the criminal justice system, such as judges. Finally, our findings may be relevant in the evaluations of many other contexts in which an individual's actions are potentially affected by the expected consequences of his actions, such as reporting cheating students or reporting households to welfare agencies.

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[48] See for example Katz et al. (2003), Donohue and Wolfers (2006), Hjalmarsson (2009), or Cohen-Cole et al. (2009).
[49] See for example Abrams et al. (2012) or Alesina and Ferrara (2014).

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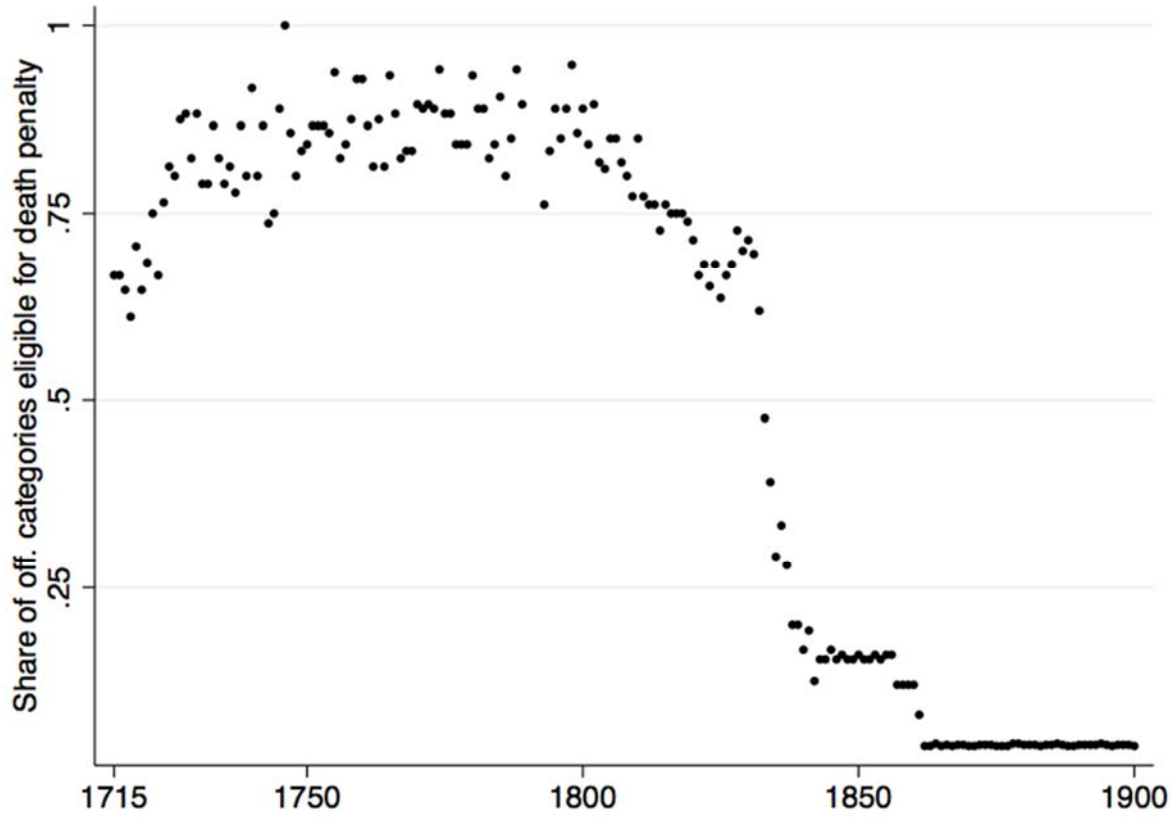
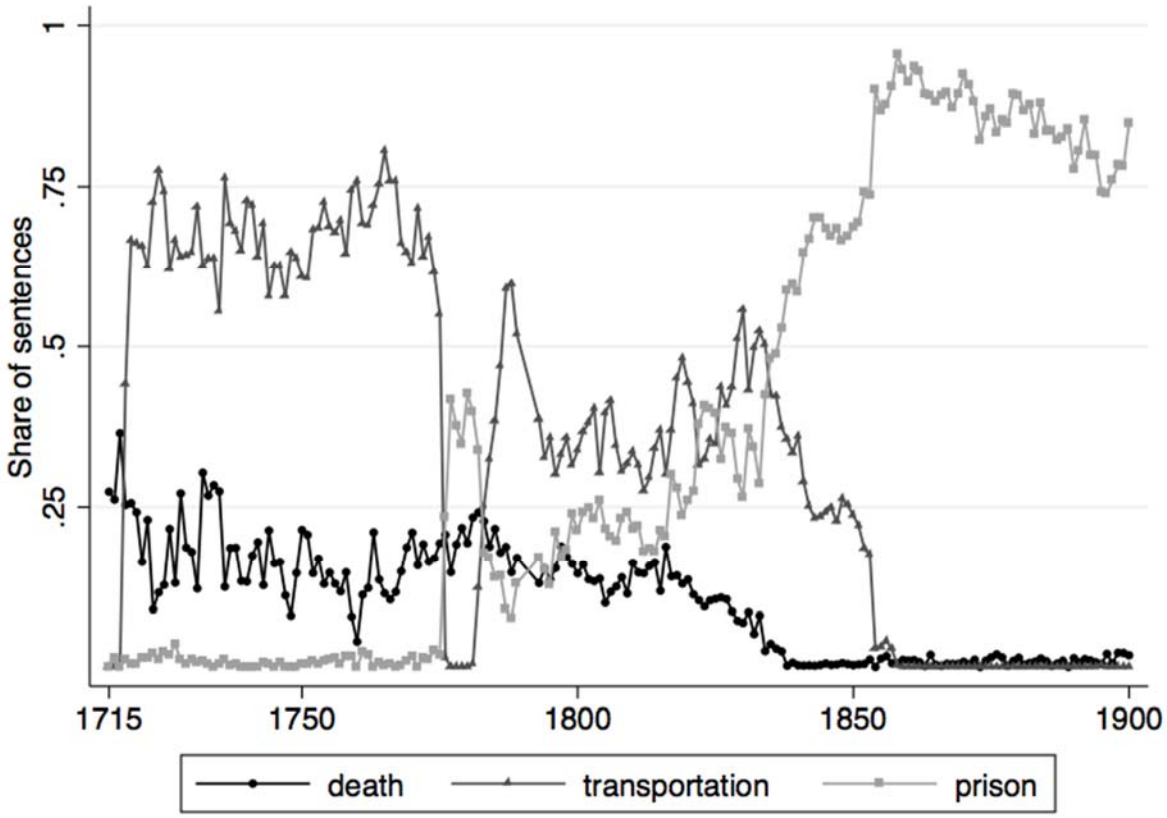
Figure 1. Share of capital eligible offenses (1715-1900)



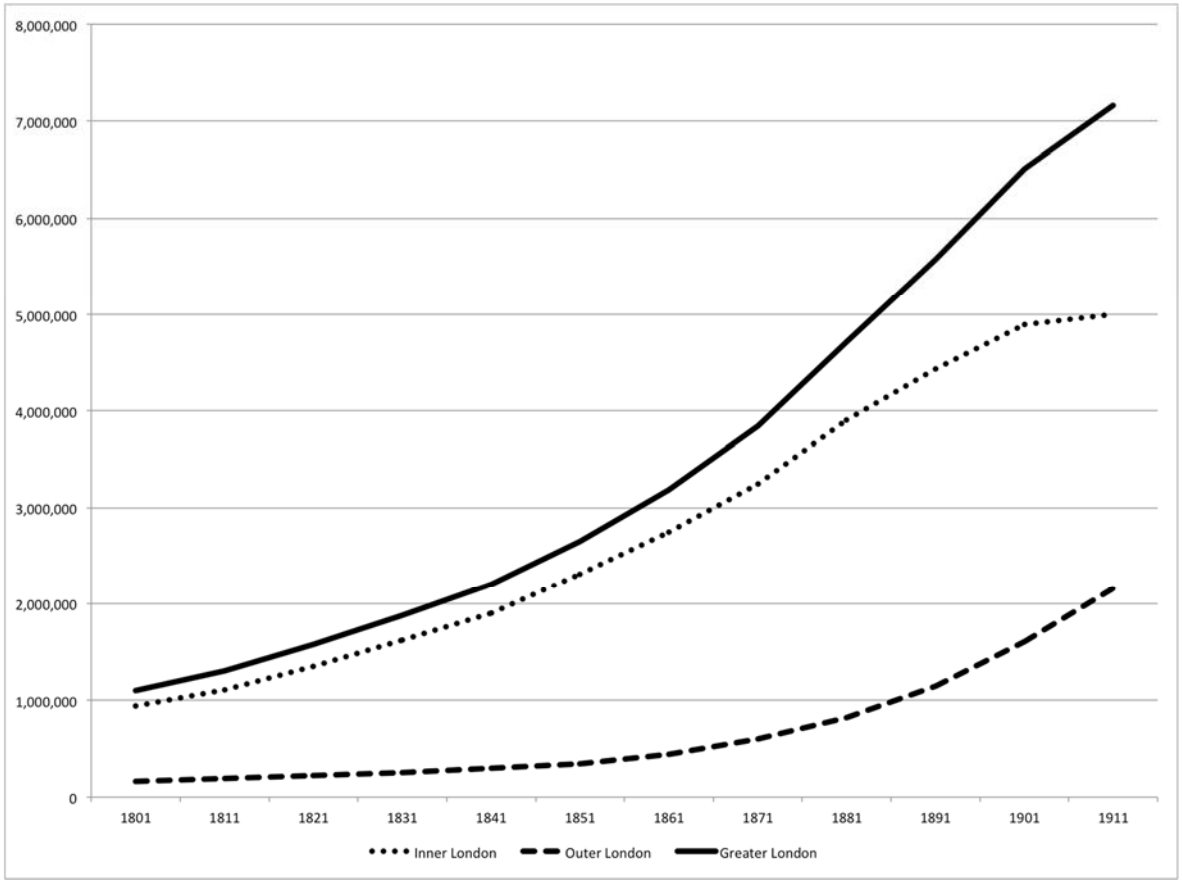
NOTE- The figure shows the share of offense categories in the sample that are eligible for capital punishment between 1715 and 1900. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 2. Share of sentences - Death, transportation and prison (1715-1900)



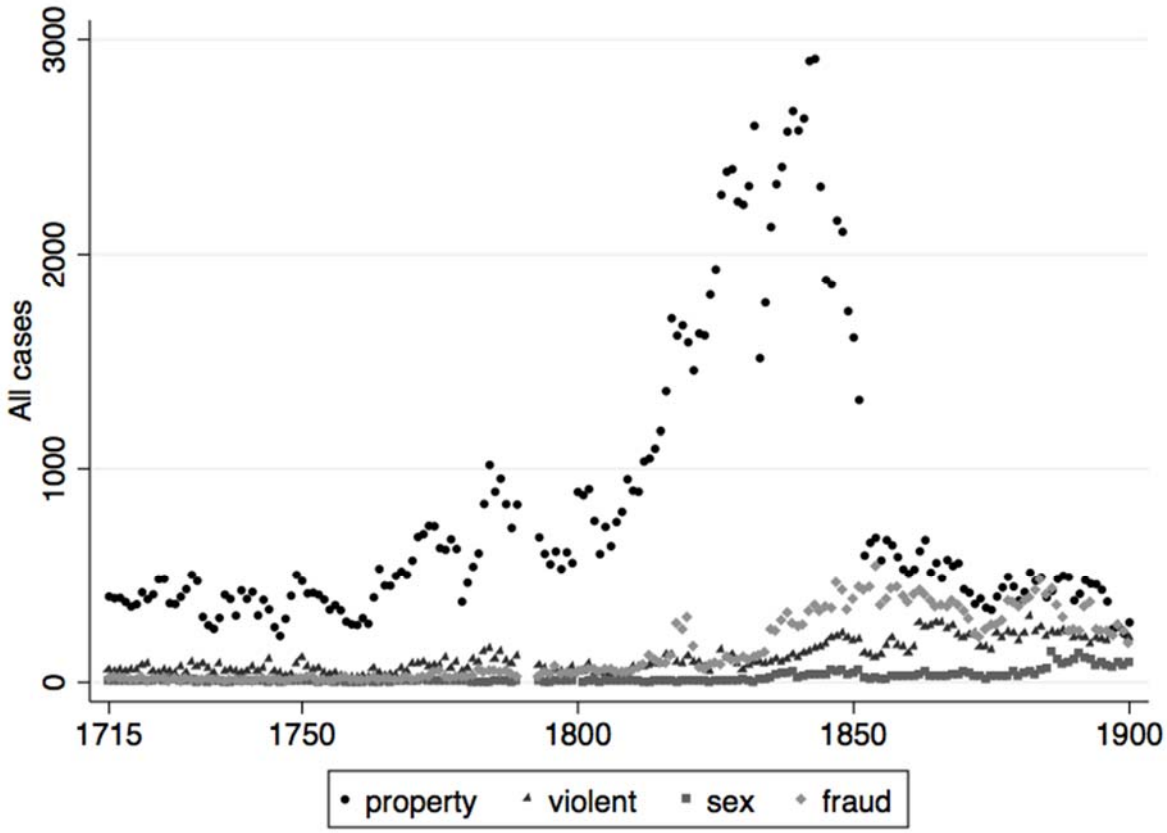
NOTE- The figure shows the share of convicted cases that result in a death penalty (black line), penal transportation (dark grey line) and prison (light grey line) in the sample between 1715 and 1900. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 3. London - Population growth (1801-1911)



NOTE- The figure shows the population in Inner London (dotted), Outer London (dashed) and Greater London (solid line) as obtained from the decennial census data between 1801 and 1911. SOURCE- Historic census UK.

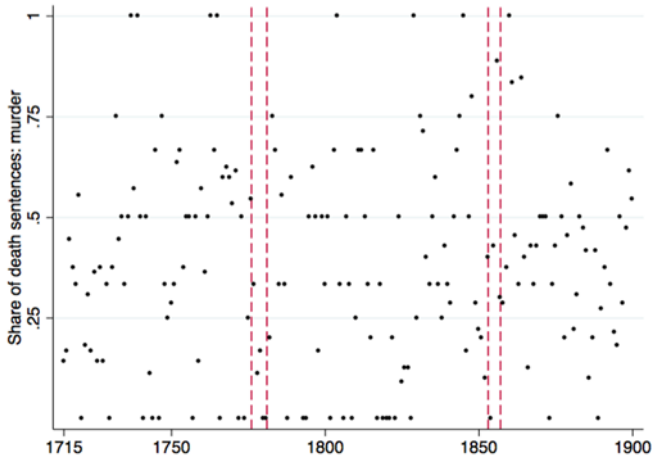
Figure 4. Number of cases by broad offense category, analysis sample



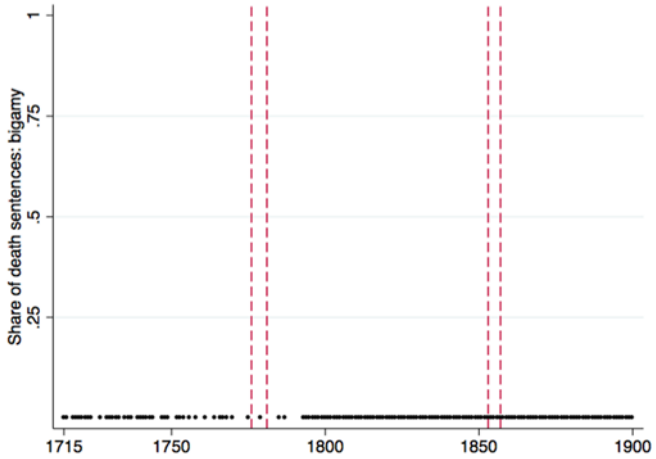
NOTE- The figure shows the annual number of cases in the sample (tried at the Old Bailey) between 1715 and 1900 and by broad offense category (property offenses, violent offenses, sex offenses and fraud offenses). SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 5. Identifying the time of treatment – Abolition of capital punishment

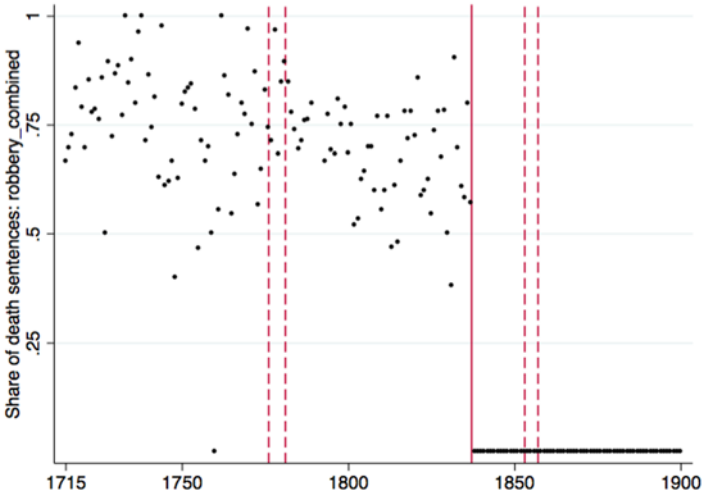
Panel A – Always capital punishment eligible



Panel B – Never capital punishment eligible



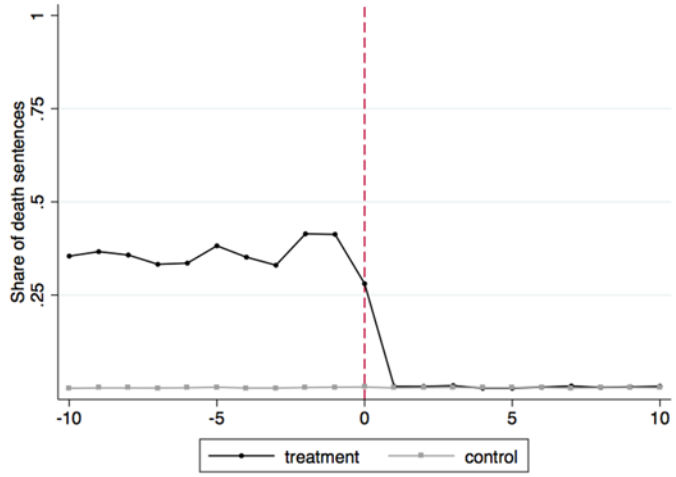
Panel C – Change in capital punishment eligibility



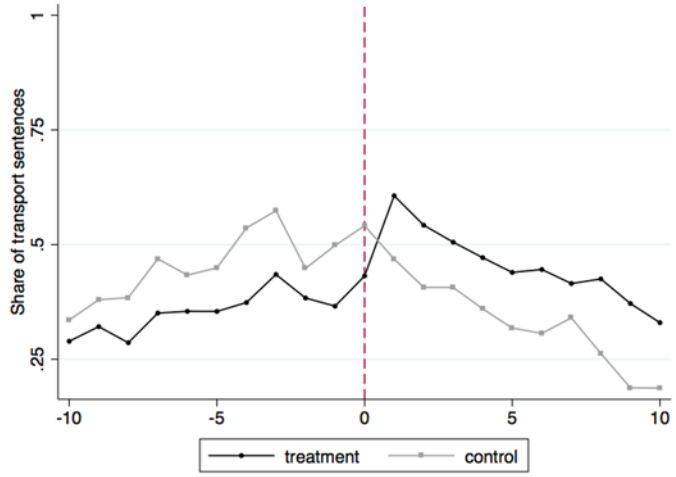
NOTE- The figure shows the annual share of convicted cases in the sample that were sentenced to death for murder (Panel A, always capital punishable), bigamy (Panel B, never capital punishable) and robbery (Panel C, change in capital punishment eligibility). The dashed vertical red lines mark the years that were affected by changes in penal transportation (American Revolution and abolition of transportation); the solid red line in Panel C marks the year of treatment, i.e. the first year in which the observed share of capital punishment is equal to zero. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 6. Sentencing and the abolition of capital punishment

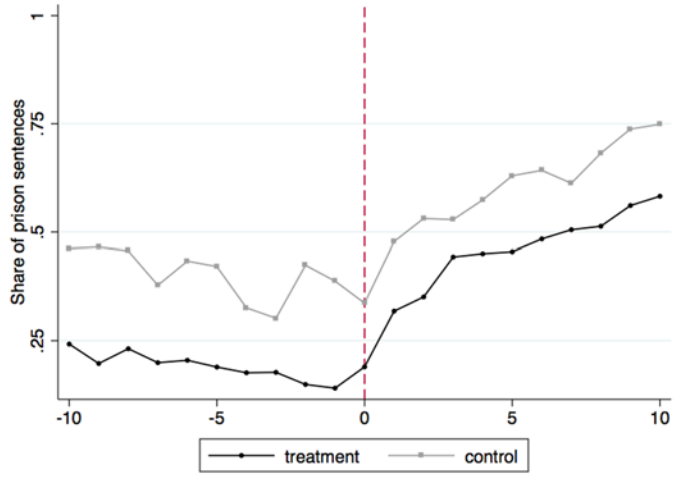
Panel A – Treatment and control group, death penalty



Panel B – Treatment and control group, transportation



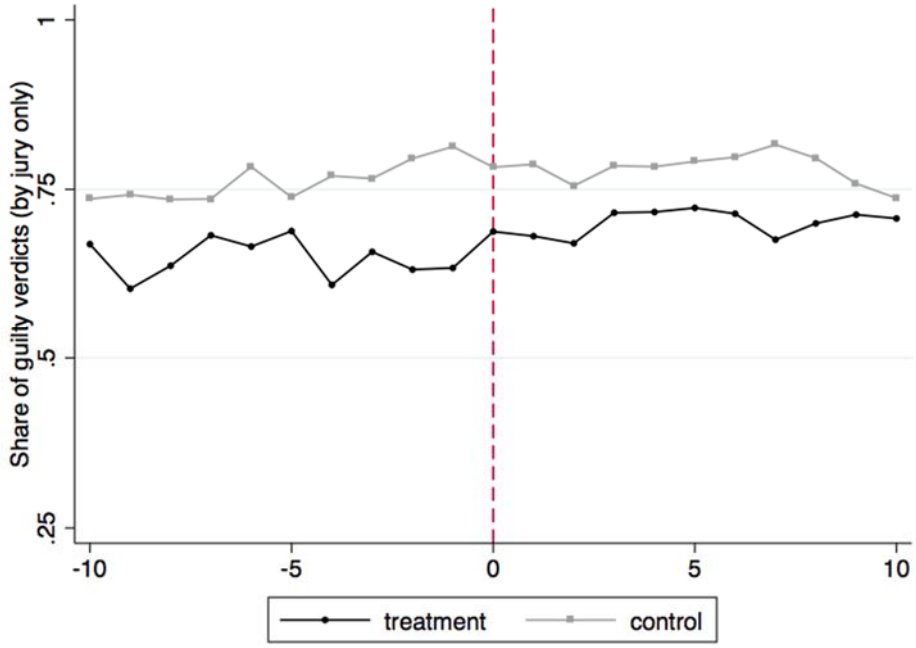
Panel C – Treatment and control group, prison



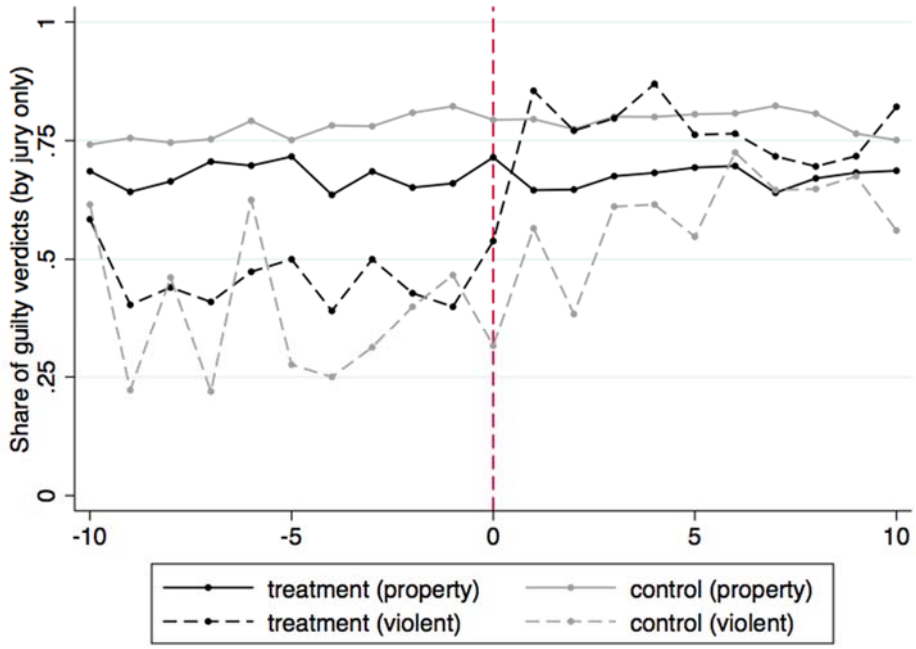
NOTE- The figure shows the annual share of convicted cases in the treatment (black) and control (grey) group that were sentenced to death (Panel A), transportation (Panel B) or prison (Panel C) in the 10 years before and after the assigned treatment year. The vertical line marks the offense specific year of abolition of capital punishment for offenses in the treatment group and the median year of abolition of capital punishment (1833) for the control group. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 7. Conviction rates and the abolition of capital punishment

Panel A – Treatment and control group, all offenses



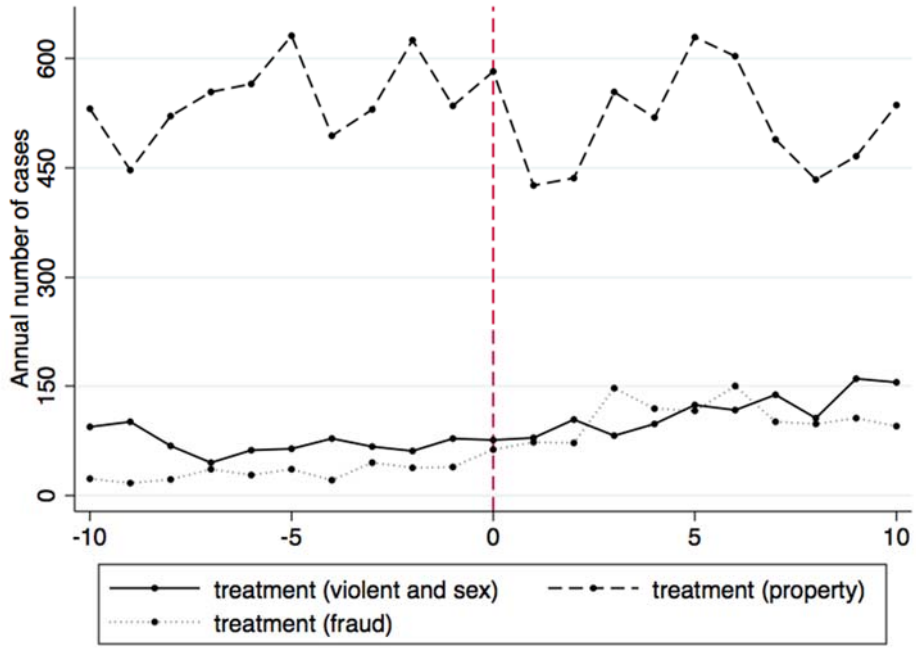
Panel B – Treatment and control group, property and violent offenses



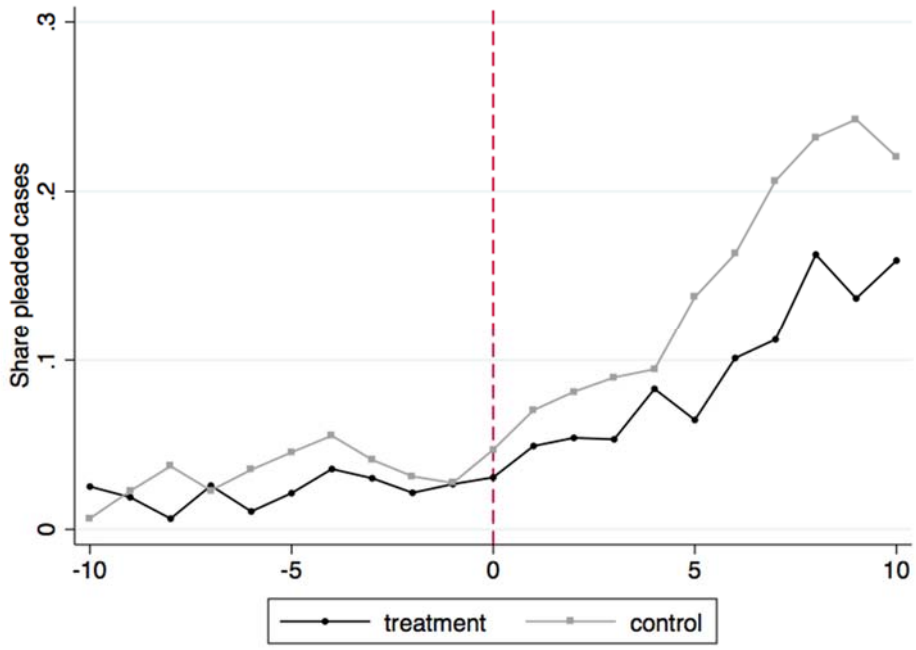
NOTE- The figure shows the annual share of guilty verdicts (cases convicted by jury) in the treatment (black) and control (grey) group for all offenses in the sample (Panel A) and separately for property and violent offenses (Panel B) in the 10 years before and after the assigned treatment year and relative to all cases tried by jury. The vertical line marks the offense specific year of abolition of capital punishment for offenses in the treatment group and the median year of abolition of capital punishment (1833) for the control group. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 8. Changes in behavior and the abolition of capital punishment

Panel A - Changes in criminal behavior



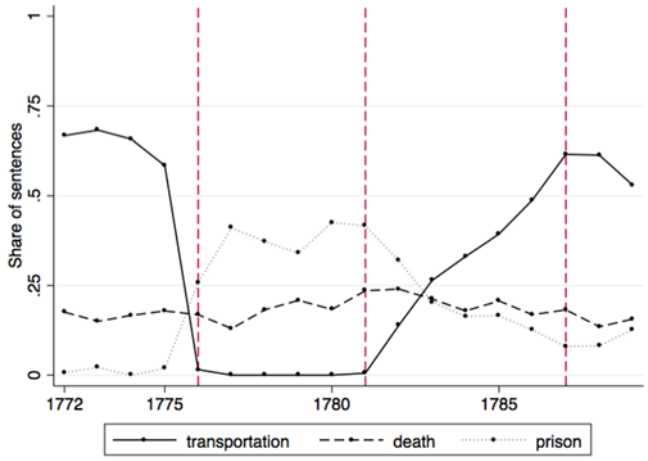
Panel B - Changes in plea behavior



NOTE- Panel A shows the annual number of cases in the sample tried at the Old Bailey in the treatment group for violent and sex offenses (solid line), property offenses (dashed) and fraud offenses (dotted) in the 10 years before and after the assigned treatment year. Panel B shows the share of pleaded cases in the treatment (black) and control (grey) group. The vertical line marks the offense specific year of abolition of capital punishment for offenses in the treatment group and the median year of abolition of capital punishment (1833) for the control group. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

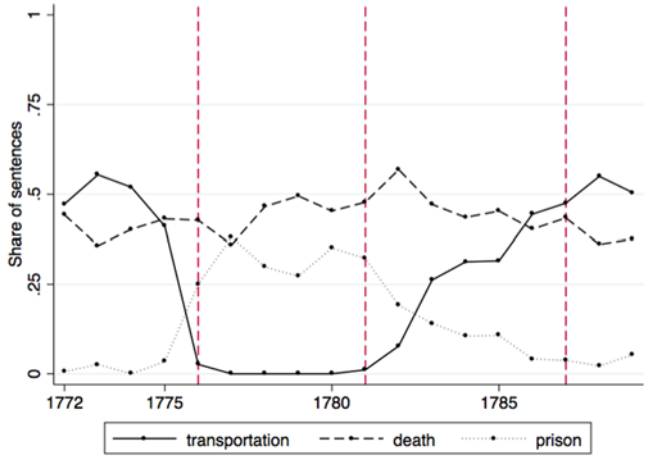
Figure 9. Sentencing and the American Revolution

Panel A - All transportation eligible offenses



Panel B - Non-capital transportation eligible offenses

Panel C - Capital transportation eligible offenses



NOTE- Panel A shows the annual share of all convicted cases in the treatment group that were sentenced to transportation (solid line), death (dashed) or prison (dotted) between 1772 and 1789. Panel B shows the equivalent numbers for non-capital eligible offenses, Panel C for capital eligible offenses. The vertical lines mark the halt of transportation in 1776, the reinstatement in transportation by name only in 1781 and the actual start of transportation to Australia in 1787. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Figure 10. Sentencing and the abolition of transportation

NOTE- The figure shows the annual share of convicted cases in the treatment group that were sentenced to transportation (solid line), death (dashed) or prison (dotted) between 1848 and 1857. The vertical line marks the abolition of transportation in 1853. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 1. Offense categories

Category	Subcategory	Offenses	Combined Offenses
Property	Theft	Animal theft, mail, stealing from master, theft from place, shoplifting *Excluded from sample:* Game law offense	Larceny: Grand larceny (more than 1 shilling), petty larceny (less than one shilling), simple larceny, pocket picking
	Other	Arson, burglary, house breaking, receiving *Excluded from sample:* Breaking into place	
Violent	Killing	Manslaughter, murder *Excluded from sample:* Infanticide, petty treason	
	Other	Assault, wounding *Excluded from sample:* Kidnapping, riot	Robbery: Highway robbery, robbery
Sex	Violent	Rape	Sexual assault: Assault with intent, indecent assault
	Other	*Excluded from sample:* Keeping a brothel	Sodomy: Assault with sodomitical intent, sodomy
Fraud	Fraud	Coining offenses, embezzlement, forgery, fraud *Excluded from sample:* Treason	
Other	Other	Bigamy, libel, perjury, perverting justice *Excluded from sample:* Barratry, concealing a birth, conspiracy, extortion, habitual criminal, illegal abortion, piracy, religious offenses, return from transportation, seditious libel, seditious words, seducing allegiance, tax offenses, threatening behavior, vagabond, bankruptcy	

NOTE- The table shows the offense categories included and excluded from the analysis sample. Where applicable, we combine offense categories into one bigger category (larceny, robbery, sexual assault, sodomy).

Table 2. Treatment/control offenses and treatment year for each experiment

	Capital Punishment			Transportation (I)		Transportation (II)	
	Offence specific laws			*American Revolution*		*Penal Servitude Acts*	
	+/-10 years around treatment year			1772 - 1789		1848 - 1857	
Offense	**Treatment**	**Year**	**#Cases**	**Treatment**	**#Cases**	**Treatment**	**#Cases**
Property offenses							
Animal theft	T	1832	1168	T	435	T	248
Arson	T	1856	111	D	19	D	46
Burglary	T	1837	1081	T	1323	T	880
Housebreaking	T	1833	1396	T	164	T	497
Larceny	C (never)	Median	32278	T	8181	T	4673
Mail	T	1834	74	D	5	T	127
Receiving	T	1837	3567	T	686	T	740
Shoplifting	T	1820	763	T	441	T	69
Stealing from master	C (never)	Median	4696	D	0	T	2223
Theft from place	T	1832	3706	T	1537	T	1072
Violent and sex offenses							
Assault	C (never)	Median	185	D	5	C	176
Manslaughter	C (never)	Median	295	C	14	T	213
Murder	C (always)	Median	222	C	161	T	108
Robbery	T	1837	859	T	1529	T	481
Rape	T	1841	228	C	63	T	116
Sexual assault	D	-	0	D	0	C	80
Sodomy	T	1832 (1860)	81	C	15	D	89
Wounding	T	1837 (1861)	825	C	35	D	710
Fraud offenses							
Coining offenses	T	1832	893	C	337	T	2138
Embezzlement	C (never)	Median	1650	D	3	T	719
Forgery	T	1832	581	C	155	T	694
Fraud	T	1813	160	T	151	T	634
Other offenses							
Bigamy	C (never)	Median	225	D	20	T	134
Libel	C (never)	Median	23	D	1	C	35
Perjury	C (never)	Median	102	T	100	D	142
Perverting justice	T	1831	83	T	77	T	42
Total	**26**		**55 252**	**26**	**15 457**	**26**	**17 086**
Treatment	*16*	**Median: 1833**	*15 576*	*11*	*14 624*	*19*	*15 808*
Control	*9*		*39 676*	*7*	*780*	*3*	*291*
Dropped	*1*		*0*	*8*	*53*	*4*	*987*

NOTE- The table shows the classification of offenses into treatment (T) and control (C) groups, the assigned treatment year as well as the number of observations for each of the analyzed natural experiments. D indicates offenses that were dropped for a given 'experiment'. For control group offenses, we assign the year of 'treatment' as the median year of observed reforms (1833). SOURCE- *The Old Bailey Proceedings Online*, various sources as specified in the text (laws) and own calculations.

Table 3. Summary statistics

	All	Capital Punishment		Transportation	
		Treatment	*Control*	*American Revolution*	*Penal Servitude Acts*
Variable		**1803 - 1871**		**1772 -1789**	**1848 - 1857**
Sample					
Number of observations (N)	206,733	49,285	76,673	14,624	15,808
Number of sessions (N)	1 748	703	703	153	120
Avg. number of cases per session	150.0	172.8	211.1	97.64	149.1
Avg. number of defendants per case	1.483	1.762	1.265	1.512	1.361
Offenses					
Property off. (0/1)	0.729	0.577	0.893	0.873	0.666
Violent off. (0/1)	0.101	0.137	0.032	0.105	0.051
Sex off. (0/1)	0.018	0.020	0.006		0.007
Fraud off. (0/1)	0.133	0.262	0.051	0.010	0.265
Other off. (0/1)	0.020	0.005	0.018	0.012	0.011
Defendants					
Avg. age	27.57	27.10	26.40	16.79	26.61
Aged 18 to 21 (0/1)	0.240	0.258	0.254	0.131	0.263
Aged under 18 (0/1)	0.150	0.129	0.200	0.779	0.140
Age missing (0/1)	0.376	0.237	0.230	0.985	0.236
Age missing, guilty cases (0/1)	0.161	0.053	0.045	0.569	0.068
Age missing, non-guilty cases (0/1)	0.214	0.183	0.185	0.410	0.167
Male (0/1)	0.786	0.813	0.781	0.728	0.812
Any known criminal history, from 1832 (0/1)	0.111	0.106	0.101		0.098
Juries and judges					
Avg. number of juries per session	3.072			3.245	
London jury (0/1)	0.243			0.277	
Number of judges	104			30	
Pleads and Verdicts					
Pleaded guilty (0/1)	0.136	0.140	0.148	0.002	0.316
Guilty by jury or plea (0/1)	0.719	0.726	0.779	0.584	0.793
Guilty by jury (0/1)	0.675	0.681	0.741	0.583	0.697
Guilty of lesser offense (0/1)	0.047	0.085	0.009	0.052	0.026
Recommended for mercy (0/1)	0.061	0.071	0.093	0.030	0.081
Not guilty by jury (0/1)	0.324	0.318	0.259	0.416	0.302
Sentences conditional on guilty by jury or plea					
Capital punishment (0/1)	0.069	0.123	0.004	0.187	0.002
Transportation (0/1)	0.294	0.259	0.308	0.381	0.156
Imprisonment (0/1)	0.522	0.564	0.578	0.186	0.808
Corporal punishment (0/1)	0.042	0.011	0.038	0.179	0.007
Miscellaneous punishment (0/1)	0.045	0.020	0.042	0.055	0.001
No punishment (0/1)	0.030	0.022	0.030	0.012	0.026

NOTE- The table shows summary statistics for the variables in the analysis sample for each of the analysis periods. Where not otherwise specified, the mean of the variable is shown. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 4. Baseline results - Abolition of capital punishment and jury decisions

	(1)	(2)	(3)	(4)	(5)
Offense:	All	All	Property	Violent and sex	Fraud
Panel A. Guilty by jury verdict (0/1)					
noncapital (0/1)	0.0917***	0.0764***	0.0153*	0.220***	0.345***
	(0.0102)	(0.0091)	(0.0080)	(0.0285)	(0.0515)
Mean	0.720	0.721	0.737	0.595	0.726
Observations	104,910	104,670	83,990	10,017	9,375
Number of clusters	1535	1535	623	475	207
R-squared	0.053	0.067	0.051	0.107	0.138
Panel B. Guilty of lesser offence conditional on guilty by jury verdict (0/1), broad definition					
noncapital (0/1)	-0.153***	-0.153***	-0.203***	0.0214	0.0017
	(0.0106)	(0.0105)	(0.0114)	(0.0397)	(0.0133)
Mean	0.069	0.069	0.053	0.280	0.032
Observations	75,571	75,422	61,919	5,961	6,806
Number of clusters	1423	1423	595	434	205
R-squared	0.256	0.258	0.239	0.221	0.140
Panel C. Recommended for mercy conditional on guilty by jury verdict (0/1)					
noncapital (0/1)	-0.0590***	-0.0602***	-0.0363***	-0.235***	-0.150***
	(0.0070)	(0.0069)	(0.0069)	(0.0273)	(0.0358)
Mean	0.112	0.112	0.117	0.103	0.076
Observations	75,571	75,422	61,919	5,961	6,806
Number of clusters	1423	1423	595	434	205
R-squared	0.062	0.066	0.065	0.142	0.054
Offense f.e.	yes	yes	yes	yes	yes
Year f.e.	yes	yes	yes	yes	yes
Month f.e.	yes	yes	yes	yes	yes
Control var.	no	yes	yes	yes	yes

NOTE- The table shows the results for the baseline regressions corresponding to estimating equation (1). The dependent variable is a dummy variable indicating a guilty jury verdict (Panel A), a verdict guilty of a lesser offense (Panel B) and a recommendation for mercy (Panel C). Standard errors clustered on year x offense are shown in parentheses below the estimated coefficient. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 5. Robustness Analyses – Abolition of capital punishment

Offense:	(1) All	(2) All	(3) All	(4) All	(5) Property	(6) All	(7) All	(8) All	(9) All
Specification:	Baseline	Off.group x year f.e.	Off.group x annual trend	Exclude larceny	Exclude larceny	Exclude sodomy and wounding	Before 1850	After 1820, before 1850	Plea = guilty
Panel A. Guilty by jury verdict (0/1)									
noncapital (0/1)	0.0764***	0.0344***	0.0552***	0.0865***	0.0431***	0.0621***	0.0607***	0.0554***	0.0471***
	(0.0091)	(0.0079)	(0.0085)	(0.0135)	(0.0127)	(0.0089)	(0.0098)	(0.0111)	(0.0080)
Mean	0.721	0.721	0.721	0.673	0.686	0.722	0.728	0.739	0.759
Observations	104,670	104,670	104,670	52,535	31,855	101,909	86,637	66,679	121,410
Cluster	1535	1535	1535	1466	554	1405	1030	693	1548
R-squared	0.067	0.076	0.070	0.082	0.064	0.067	0.072	0.068	0.064
Panel B. Guilty of lesser offence conditional on guilty by jury verdict (0/1), broad definition									
noncapital (0/1)	-0.153***	-0.186***	-0.164***	-0.104***	-0.135***	-0.167***	-0.146***	-0.0986***	
	(0.0105)	(0.0108)	(0.0106)	(0.0124)	(0.0138)	(0.0103)	(0.0123)	(0.0125)	
Mean	0.069	0.069	0.069	0.139	0.138	0.058	0.066	0.048	
Observations	75,422	75,422	75,422	35,356	21,853	73,537	63,101	49,287	
Cluster	1423	1423	1423	1354	526	1310	928	642	
R-squared	0.258	0.276	0.264	0.228	0.212	0.222	0.257	0.236	
Offense f.e.	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year f.e.	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month f.e.	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control var.	yes	yes	yes	yes	yes	yes	yes	yes	yes

NOTE- The table shows the results for the robustness analysis corresponding to estimating equation (1) and as specified. The dependent variable is a dummy variable indicating a guilty jury verdict (Panel A) and a verdict guilty of a lesser offense (Panel B). Standard errors clustered on year x offense are shown in parentheses below the estimated coefficient. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 6. Heterogeneity analyses – Abolition of capital punishment

	(1)	(2)	(3)	(4)	(5)	(6)
Offense:	All	Property	Violent	All	All	All
Specification:		***Gender***			***Criminal history***	
	Interaction	Interaction	Interaction	Baseline after 1832	Control variable	Interaction
Panel A. Guilty by jury verdict (0/1)						
noncapital (0/1)	0.0750***	0.0177	0.305***	0.100***	0.116***	0.119***
	(0.0132)	(0.0136)	(0.0414)	(0.0223)	(0.0254)	(0.0255)
male defendant (0/1)	0.0663***	0.0773***	0.124***			
	(0.0105)	(0.0118)	(0.0311)			
noncapital x male defendant	0.0017	-0.0031	-0.119***			
	(0.0116)	(0.0129)	(0.0349)			
criminal history (0/1)					0.277***	0.388***
					(0.00867)	(0.0716)
noncapital x criminal history						-0.112
						(0.0722)
Mean	0.721	0.737	0.609	0.727	0.724	0.724
Observations	104,670	83,990	8,702	59,544	57,134	57,134
Cluster	1535	623	310	949	940	940
R-squared	0.067	0.051	0.111	0.069	0.105	0.105
Panel B. Guilty of lesser offence conditional on guilty by jury verdict (0/1), broad definition						
noncapital (0/1)	-0.236***	-0.295***	-0.0646	-0.0622***	-0.0591***	-0.0564***
	(0.0159)	(0.0173)	(0.0603)	(0.0234)	(0.0213)	(0.0210)
male defendant (0/1)	-0.112***	-0.122***	-0.138***			
	(0.0132)	(0.0152)	(0.0435)			
noncapital x male defendant	0.102***	0.113***	0.108**			
	(0.0135)	(0.0154)	(0.0473)			
criminal history (0/1)					-0.0115***	0.0463
					(0.00282)	(0.0608)
noncapital x criminal history						-0.0583
						(0.0611)
Mean	0.069	0.053	0.282	0.053	0.054	0.054
Observations	75,422	61,919	5,299	43,259	41,344	41,344
Cluster	1423	595	295	919	910	910
R-squared	0.261	0.244	0.248	0.283	0.289	0.289
Offense f.e.	yes	yes	yes	yes	yes	yes
Year f.e.	yes	yes	yes	yes	yes	yes
Month f.e.	yes	yes	yes	yes	yes	yes
Control var.	yes	yes	yes	yes	yes	yes

NOTE- The table shows the results for the heterogeneity analysis (by criminal history and gender) corresponding to estimating equation (1). The dependent variable is a dummy variable indicating a guilty jury verdict (Panel A) and a verdict guilty of a lesser offense (Panel B). Standard errors clustered on year x offense are shown in parentheses below the estimated coefficient. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 7. Identification test - Abolition of capital punishment and quality of evidence

	(1)	(2)	(3)
Offense:	All	Violent and sex	Property
Dependent variable:		***Hit rate (key words)***	
Panel A. 'Evidence'			
noncapital (0/1)	-0.104***	-0.186***	-0.0369
	(0.0182)	(0.0253)	(0.032)
Observations	1444	438	557
R-squared	0.386	0.621	0.303
Panel B. 'Police'			
noncapital (0/1)	-0.0375*	-0.0548	0.0137
	(0.0218)	(0.0344)	(0.0342)
Observations	1444	438	557
R-squared	0.569	0.823	0.42
Panel C. 'Witness'			
noncapital (0/1)	-0.0688***	-0.059	-0.0436
	(0.0217)	(0.0363)	(0.0444)
Observations	1444	438	557
R-squared	0.37	0.597	0.406
Offense f.e.	yes	yes	yes
Year f.e.	yes	yes	yes

NOTE- The table shows the results for the identification test of estimating equation (1). The dependent variable is the hit rate corresponding to the key words evidence (Panel A), police (Panel B) and Witness (Panel C) – see the text for further details on the construction of the variable. Robust standard errors are shown in parentheses below the estimated coefficient. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 8. Baseline results – American Revolution, halt of transportation and convictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offense:	All	All	Non-capital	Capital	Non-capital	Non-capital	Non-capital	Non-capital
Jurisdiction:			All cases			London	Middlesex	All cases
pre1776 (0/1)	-0.0040	-0.0308**	-0.0504***	-0.0046	-0.0234	-0.0615**	-0.0272	-0.0483***
	(0.0143)	(0.0147)	(0.0190)	(0.0233)	(0.0297)	(0.0260)	(0.0371)	(0.0183)
1780-1786 (0/1)								0.0100
								(0.0221)
post1786 (0/1)								-0.0249
								(0.0263)
Mean	0.546	0.564	0.604	0.511	0.703	0.539	0.604	0.631
Observations	5,702	5,420	3,095	2,325	1,227	1,868	3,095	7,794
R-squared	0.062	0.076	0.082	0.067	0.061	0.072	0.084	0.067
Offense f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Month f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Judge f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Control var. (incl. jury)	no	yes	yes	yes	yes	yes	yes	yes
Off. group specific linear trends	no	no	no	no	no	no	yes	no

NOTE- The table shows the results for the baseline regressions corresponding to estimating equation (2). The dependent variable is a dummy variable indicating a guilty jury verdict (conviction). Robust standard errors are shown in parentheses below the estimated coefficient. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Table 9. Baseline results –Abolition of transportation and convictions

Offense:	(1) All	(2) All	(3) All	(4) All	(5) All
post1852 (0/1)	-0.0225**	-0.0222**	0.0096	-0.0363***	-0.0355***
	(0.0092)	(0.0090)	(0.0186)	(0.0113)	(0.0111)
high transportation offense				0.0228	0.0050
				(0.0366)	(0.0359)
post1852 x high transportation offense				0.0249	0.0239
				(0.0204)	(0.0200)
Mean	0.696	0.696	0.696	0.710	0.710
Observations	11,026	10,972	10,972	10,323	10,272
R-squared	0.062	0.101	0.101	0.052	0.092
Offense f.e.	yes	yes	yes	yes	yes
Month f.e.	yes	yes	yes	yes	yes
Control var.	no	yes	yes	no	yes
Linear trend	no	no	yes	no	no

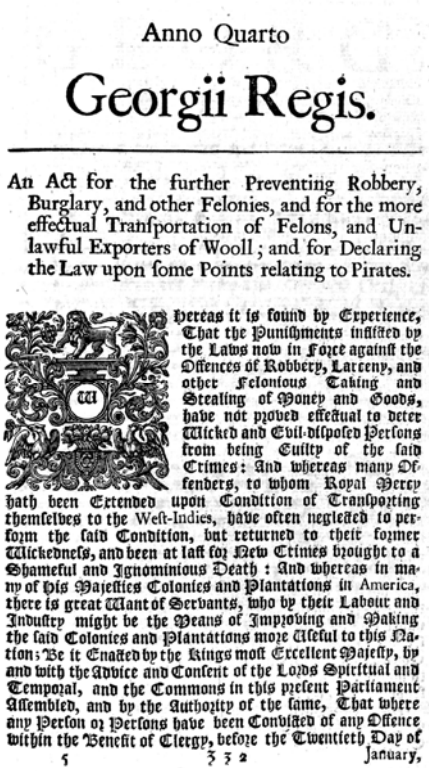
NOTE- The table shows the results for the baseline regressions corresponding to estimating equation (3). The dependent variable is a dummy variable indicating a guilty jury verdict (conviction). Robust standard errors are shown in parentheses below the estimated coefficient. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

Appendix Table 1. Capital punishment eligibility, reform years and act names

Offense	Law	Treatment years
Panel A. Property offenses		
Animal theft	An act for abolishing the Punishment of Death in certain cases, and substituting a lesser punishment in lieu thereof (1832)	1832
Arson	Burning of Buildings, etc. Act (1837)	1856
Burglary	An act to Amend the Laws realting to Burglary and Stealing in a Dwelling house (1837)	1837
Housebreaking	Criminal law act (1833)	1833
Larceny	-	practically never eligible
Mail	An act for abolishing capital punishment in cases of letter-stealing and sacrilege (1834)	1834
Receiving	-	1837
Shoplifting	Stealing in Shops Act (1820)	1820
Stealing from master	-	never eligible
Theft from place	An act for abolishing the Punishment of Death in certain cases, and substituting a lesser punishment in lieu thereof (1832)	1832
Panel B. Violent and sex offenses		
Assault	-	
Manslaughter	-	never eligible
Murder	-	always eligible
Robbery	An act to Amend the Laws relating to Robbery and Stealing from the Person (1837)	1837
Rape	Substitution of Punishments for Death Act (1841)	1841
Sexual assault	-	never eligible
Sodomy	An act to consolidate and amend the Statute Law of England and Ireland relating to Offences against the Person (1861)	1832 (1860)
Wounding	Act to Amend the Laws Relating to Offences against the Person (1837) An Act to consolidate and amend the Statute Law of England and Ireland relating to Offffences against the Person (1861)	1837 (1861)
Panel C. Fraud offenses		
Coining offenses	Coinage Offences Acts (1832)	1832
Embezzlement	-	practically never eligible
Forgery	An Act for abolishing the Punishment of Death in certain Cases of Forgery (1832)	1832
Fraud	-	1813
Panel D. Other offenses		
Bigamy	-	not eligible
Libel	-	not eligible
Perjury	-	not eligible
Perverting justice	-	1831

NOTE- The table indicates the punishment eligibility for capital punishment for each offense in the analysis sample. SOURCE- *The Old Bailey Proceedings Online*, various sources as specified in the text (laws) and own calculations.

Appendix Figure 1. Examples from original text of law



Anno Quarto

Georgii Regis.

An Act for the further Preventing Robbery, Burglary, and other Felonies, and for the more effectual Tranſportation of Felons, and Unlawful Exporters of Wooll; and for Declaring the Law upon ſome Points relating to Pirates.

Whereas it is found by Experience, That the Puniſhments inflicted by the Laws now in Force againſt the Offences of Robbery, Larceny, and other Felonious Taking and Stealing of Money and Goods, have not proved effectual to deter Wicked and Evil-diſpoſed Perſons from being Guilty of the ſaid Crimes: And whereas many Offenders, to whom Royal Mercy hath been Extended upon Condition of Tranſporting themſelves to the Weſt-Indies, have often neglected to perform the ſaid Condition, but returned to their former Wickedneſs, and been at laſt for New Crimes brought to a Shameful and Ignominious Death: And whereas in many of His Majeſties Colonies and Plantations in America, there is great Want of Servants, who by their Labour and Induſtry might be the Means of Improving and Making the ſaid Colonies and Plantations more Uſeful to this Nation; Be it Enacted by the Kings moſt Excellent Majeſty, by and with the Advice and Conſent of the Lords Spiritual and Temporal, and the Commons in this preſent Parliament Aſſembled, and by the Authority of the ſame, That where any Perſon or Perſons have been Convicted of any Offence within the Benefit of Clergy, before the Twentieth Day of

5 3 3 2 January,

ANNO SECUNDO & TERTIO

GULIELMI IV. REGIS.

****

CAP. CXXIII.

An Act for abolishing the Punishment of Death in certain Cases of Forgery. [16th *August* 1832.]

WHEREAS by an Act passed in the First Year of His present Majesty's Reign, intituled *An Act for reducing into One Act all such Forgeries as shall hereafter be punished with Death, and for otherwise amending the Laws relative to Forgery*, it was provided, that if any Person should after the Commencement of that Act be convicted of any Forgery or other Offence therein named or described, for which he would at the Time of the passing of that Act have been liable to the Punishment of Death, he should not suffer Death for the same, unless the same should be made punishable with Death by that Act: And whereas by the Law and Practice now prevailing in *Scotland* and in *Ireland* the Penalty of Death may be awarded, in certain Cases, for Forgery, for uttering counterfeit Instruments, and for false Personation: And whereas it is expedient to abolish the Punishment of Death for Offences of that Nature, except so far as relates to Wills and certain Powers of Attorney, as herein-after mentioned; be it therefore enacted by the King's most Excellent Majesty, by and with the Advice and Consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the Authority of the same, That where any Person shall after the passing of this Act be convicted of any Offence whatsoever for which the said Act enjoins or authorizes the Infliction of the Punishment of Death, or where any Person shall after the passing of this Act be convicted in *Scotland* or *Ireland* of any Offence now punishable with Death, which Offence shall consist wholly or

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Persons hereafter convicted of Crimes punishable with Death under recited Act.

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