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

Anna Bindler
University of Gothenburg

Randi Hjalmarsson
University of Gothenburg and CEPR

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*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 two natural experiments in English history, which result in sharp decreases in punishment severity: the offense specific abolition of capital punishment in the 1800s and the temporary and unexpected halt of penal transportation during the American Revolution. Using a difference-in-differences design to study the former and a pre-post design to study the latter, we find that decreasing expected punishment (especially via the end of the death penalty), had a large, significant and permanent 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.

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## 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 two natural experiments in English history associated with large and sharp changes in punishment severity – the offense specific abolition of capital punishment in the 1800s and the temporary and unexpected halt of transportation during the American Revolution. 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

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[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.

severity of the 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] A related concept is that 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 with respect to jury behavior, there is anecdotal evidence of jury nullification throughout history, including defendants charged with helping slaves escape or Vietnam War protesters[6], and even with respect to capital punishment in 19th century England (Tonry, 1992).

Our research also contributes to a growing empirical literature that studies how expected punishment may impact the behavior of other criminal justice agents, including parole boards, judges, and especially prosecutors. For instance, Bjerk (2005) finds that prosecutors react to 3-strikes laws by lowering the charge to a misdemeanor when conviction of the original felony would lead to a third strike.[7] Tonry (1992) reviews the impact of mandatory sentencing on lawyer and judge actions to avoid (nullify) the impact of these laws. Yet, no study has looked at this final and arguably most important stage of the justice process – the conviction.

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. Two additional distinctions between our work and the existing non-jury literature are worth highlighting. First, juries are composed of non-professional decision makers as opposed to trained professionals. Second, most (if not all) of

[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/ .

[7] Other papers that look at how sentences may affect discretionary behavior include Bushway, Owens, and Piehl (2012) with respect to judges, Starr and Rehavi (2013) and Kurlychek et. al (2007) with respect to prosecutors, and LaCasse and Payne (1999) with respect to plea bargaining.

the existing literature looks at how *increased* sentences affect discretionary behavior. In contrast, we focus on a sharp reduction in expected punishment.

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 known as *The Bloody Code*. The British penal system was put into 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 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.

Our analysis is based on 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*.[8] 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

[8] To the best of our knowledge, these data have been used in just three large scale empirical studies, only one of which is related to the economics of crime. Voth (1998) used witness accounts from the Old Bailey to reconstruct historical time use budgets (prior to the digitization of the Proceedings) for a little less than 8000 cases. Kelly and Ó Gráda (2016) study the decrease in watch prices using reported values of stolen watches in criminal trials at the Old Bailey in the 18th century. Finally, Vickers and Zieberth (2016) 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. See Bindler and Hjalmarsson (2017) for an extension of this analysis to earlier years and potential explanations.

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 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 violent and sex offenses and 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 minimal evidence of an increase in the chance of conviction; 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. Finally, we demonstrate that the effects are persistent over time and remain significant 20 years and more after the reform.

A causal interpretation of these results relies on the identifying assumptions that trends are parallel and, relatedly, that the timing of the offense specific abolition was random with no anticipatory effects. We empirically support these assumptions by demonstrating insignificant effects of the reform in the years leading up to the reform. In addition, and perhaps more importantly, we implicitly assume that the composition of cases, and corresponding quality of evidence, presented to the jury did not change with the reforms. This could occur, for instance, if a change in plea behavior impacts the types of cases presented to the jury. In other words, an increase in the quality of evidence could feasibly yield the same pattern of results (i.e. and increase in conviction rates) and confound the effect of interest. However, we provide direct empirical evidence that (i) the results are robust to including all pleas as guilty jury verdicts, (ii) there is no significant increase in the chance of pleading guilty, and (iii) 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*.

Since almost all offense categories were 'treated' contemporaneously with the temporary halt of transportation, i.e. almost all offenses were transportation eligible, we are unfortunately limited to using a simple pre-post research designs in this context. 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. Notably, this sharp and unexpected change in expected punishment is exogenous to the criminal justice system. In particular, defendants charged with transportation eligible *non-capital* offenses faced an unambiguous decrease in punishment severity during the war, as imprisonment was substituted for transportation. The temporary halt of transportation increased the chance of conviction by about five percentage points for these individuals. The obvious weakness in the simple pre-post identification strategy – namely the inability to conclusively separate the effect of the reform from other things changing with the war – limits the causal interpretation of the transportation results. Nevertheless, we believe this 'experiment' adds to a complete picture of the role of punishment severity in jury verdicts.

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 the chance of a death sentence may significantly impact jury behavior today, when the death penalty is much less socially accepted. 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 not result in an entirely impartial jury. Furthermore, this lack of impartiality may be applied unequally across defendant characteristics.[9]

Finally, this paper also suggests that policy makers may be missing an important channel 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 an unstudied agent in the justice system – the jury – may be affected in a way that affects the chance of conviction, making it less clear how changing punishment severity impacts expected punishment as perceived by the criminal. If abolishing mandatory minimums, for instance, causes 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 defines the treatment and control groups for each experiment. Sections 4 and 5 present the results concerning the abolition of capital punishment and halt of transportation, respectively. 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 this history 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] Numerous parliamentary acts, largely motivated by a desire to protect the property of the land-owning classes, subsequently increased the

[9] Iyengar (2011) finds that are juries more likely to apply the death penalty than judges, and that juries are more influenced by demographic characteristics (such as age and race) of the offender and victim. These findings support the hypothesis that juries may fail to be impartial, and that this may be unequal across defendants.

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

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* because of the high number of capital offenses and the public and/or bloody spectacle made of executions. At the turn of the 19th century, even crimes viewed today as petty crimes (e.g. pickpocketing and shoplifting) were capital.

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). The death penalty was abolished for the remaining capital offenses of murder in 1965, arson on the docks in 1971, espionage in 1981, and piracy and high treason in 1998. The public spectacle of executions ended in 1868. Figure 1 demonstrates that more than 75 percent of the 26 offenses observed used in our analysis were capital eligible between 1715 and 1820. This sharply decreased in the mid-1800s to about 15 percent, held steady until the early 1860s and then sharply decreased again. Appendix Table 1 lists the offenses underlying this figure and corresponding acts that 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] Faced with a penal crisis, the *Hulks Act* (1776) was passed, which allowed 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 contributed to

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

[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).

the eruption of *The Gordon Riots* on June 2, 1780 (Hitchcock and Shoemaker, 2015). During the weeklong riots, many prisons were attacked, and prisoners escaped or were released. Military intervention ended the riots, leaving a temporary military presence on the streets 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, 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 replaced capital punishment as the maximum sentence. Transportation was abolished through the *Penal Servitude Acts* of 1853 and 1857, with an increased perception that it was inhumane and did not deter.[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. 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, 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, the use of imprisonment became the primary sanction. The Millbank Penitentiary opened in 1821, with 860 separate cells. Pentonville opened in 1842, with capacity for 520 prisoners to spend up to 18 months in solitary confinement; many more prisons were subsequently built on this model.

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

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[14] Four transport ship 'experiments' sent to the African coast, America and Honduras in 1782 and 1785 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.

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] The abolition of transportation is seen in the 1850s and is briefly considered as a third natural experiment in Section 5.

### *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. The catchment area at the Old Bailey, however, changed a bit over time, with an expansion in the 1830s to include Essex and because other courts trialed less serious crimes. 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, which defined men between ages 21 and 60, who resided in England and had land/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

[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, this was done with the assistance of a clerk, 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.

eligible jurors, which was delivered to the sheriff. 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 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 previously. 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 and expanded to a 15-mile radius to 1839.[23]

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

[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.).

lines and the underground facilitated commuting within cities and contributed to the growth in city size. During that period, London's population increased considerably from around 630,000 in 1715 to over one million in 1801, the year of the first census, and again tripled by 1860. By 1815, London was the largest city in the world. This stark population growth 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 subsidize 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.

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. Though the text of the Proceedings describes all charges, only the main charge is tagged in the Proceedings Online. In other words, if a defendant is charged with multiple offenses, we observe the most serious one. The broad verdict data indicate whether the jury found the defendant guilty, while the detailed verdict data indicate whether the defendant was found guilty of a lesser offense than charged or 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.

[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.

The Proceedings also contain a wealth of data that are not tagged in the xml files, including judge, jury and juror names for most cases between 1750 and 1822. Since these data had to be manually transcribed, we coded the judge and jury names 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. However, because the variation in punishment severity occurs over time, one can only look across and not within juries; we use jury name to control for jurisdiction 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 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 offenses that involve an unusually large number of defendants (conspiracy and riot), (iii) 186 cases for offenses redefined during the sample period and for which the redefinition cannot be clearly distinguished from changes in the punishment laws (kidnapping), and (iv) 4,698 cases for which no 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; the omitted observations (just 5% of all data in the proceedings) simply cannot be analyzed in any meaningful way. Figure 3 displays the annual number of cases in each broad offense category.

[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).

### *3.2. Coding Treatment Offenses and Years for Each Experiment*

A crucial step in our analysis is coding 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 the natural experiment concerning the halt of transportation, we identify the set of transportation eligible offenses immediately prior to the American Revolution in 1776. Only transportation eligible offenses were actually *treated* by this event. Thus, we assign offenses with a positive share of transportation sentences to the treatment group and the remaining offenses, i.e. those with a zero share of transportation sentences, to the control group. These assignments 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 never capital and assigned to the control group. Figure 4 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 marks the timing of the transportation experiments).[28] For offenses with a discontinuity, we use the observed year of

[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 attributable to cases with multiple charges, both of which were capital. For instance, a handful of burglaries were sentenced to death after abolishing 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 still capital eligible. We recoded such cases as the appropriately defined more severe offense.

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

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. For all but three offenses (receiving, fraud, and perverting justice), we could identify the specific law pertaining to the offense-specific abolition of the death penalty. For these three offenses, we rely solely on the year of discontinuity in the data. For one offense (arson), there are two changes in law (1837 and 1856), however the discontinuity in the data is seen for the later year only and we therefore code that year as the year of reform.

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'. 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.[29] 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 indicates the number of observations for each offense in the treatment and control groups (or dropped) in 1772 - 1789 for the transportation experiment and +/- 10 years around the offense specific year that capital punishment was abolished. Note that the 20-year window for capital punishment is only used for descriptive purposes here and in the figures; all empirical specifications are based on the years 1803-1871. For offenses in the capital punishment control group, 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 or that the offense-specific abolition of capital punishment falls into the same time window as the abolition of transportation (wounding). In the transportation experiment, just 53 observations

[29] 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.

– corresponding to eight offense categories – are dropped. Second, in the years surrounding the transportation experiment, the treatment group (14,624 observations) is substantially larger than the control group (just 780, i.e. 5% of 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 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: 1803-1871 for capital punishment and 1772 – 1789 for transportation. 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 groups, 73% of cases are property offenses while the remaining are classified as violent (10.1%), sex (1.8%), fraud (13.3%) and other (2%); however, property offenses comprise almost 88% of all cases during the Revolution. In addition, each category is represented in both the treatment and control groups for the death penalty experiment, though the control group consists of a larger 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 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 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 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 period is 67.5%, but just 58.4% and nearly 70%, respectively, of cases are found guilty during the 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.

## *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 considers changes in punishment due to the temporary halt and subsequent reinstatement 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 5). 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 5, 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 parallel.[30]

[30] In addition to the graphical evidence, we estimate a specification analogous to equation (1) and conditional on the convicted subsample for sentencing outcomes. The results indeed confirm that there is a substantial and significant positive effect of abolishing capital punishment on the likelihood of transportation and prison sentences. Including leads in these specifications provides formal evidence of parallel trends in sentencing outcomes for the treatment and control offenses. Regression results are available upon request.

### *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. 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.[31] When considering the secondary outcomes, we estimate equation (1) for the sample of individuals found guilty. We use this conditional sample, as recommendations to mercy or conviction of a lesser charge can only be observed conditional on there being a guilty verdict in the first place. Another way to think about it is that the jury has three verdict options of increasing severity: acquit, convict of lesser charge, and convict of original charge. If one were to use the unconditional sample, including acquittals, then it would be impossible to interpret any observed effects on the likelihood of a lesser conviction – is this effect driven by a shift from the more or less serious alternative?

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, 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.[32]

The baseline difference-in-differences specification includes: (i) offense fixed effects ($\alpha_o$) to control for baseline differences in case characteristics and conviction rates across offenses, (ii) year fixed effects ($\alpha_t$) to capture other criminal justice reforms that affected all offenses such as the introduction of the police, (iii) month fixed effects ($\alpha_m$) to capture

[31] 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.

[32] 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.

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 by year level, thus allowing for within offense and year correlation in the error term. As discussed in Bertrand et al. (2004), that level of clustering would lead to invalid inference in the presence of serial correlation. The latter is the case for example in state by year difference-in-differences models used to study economic outcome variables that are correlated over time such as wages or employment. We are less concerned about such year to year correlation given the quite flat conviction rates (our outcome variable) seen in the years leading up to the reforms (see Figure 6). However, following Angrist and Pischke (2008), we demonstrate the robustness of our results by clustering and block bootstrapping at the offense level. As offense-level clustering results in a relatively low number of clusters, our preferred specification clusters on the offense by year level.

Intuitively, our research design compares how conviction rates changed for the treatment group relative to that for the comparison group, the difference reflecting the effect of abolishing capital punishment. For $\beta_1$ 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 in the absence of the death penalty reforms. Figure 6 presents the share of jury decisions resulting in conviction for the treatment and control offenses and is suggestive of 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. As these figures simply assign the median year to the control offenses, we more formally test for parallel trends by including leads in equation (1). Figure 6 also provides the first suggestive evidence that the abolition of capital punishment increased conviction rates for treatment offenses relative to control offenses. In interpreting the figure, it is important to keep in mind that the overall sample disproportionately consists of property offenses.

A causal interpretation of the effect of abolishing capital punishment on conviction rates relies on three 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.[33] Jurors and defendants did not know which offense would be reformed next, nor the year that the reform would occur. The absence of a change in conviction (Figure 6) or sentencing (Figure 5) behavior in the years immediately preceding the reforms supports the validity of this assumption. Once again, we will formally demonstrate the lack of anticipatory effects by considering leads in equation (1).

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, and the potential channels through which it might be violated such as changing prosecutor charges or defendant plea behavior, in further detail in Section 4.4 and provide empirical tests of whether the quality of evidence changed.

Finally, a causal interpretation relies on our ability to disentangle the effects of abolishing capital punishment from anything else happening at the time. We believe there are a number of reasons to justify this claim. First, other criminal justice reforms – the introduction of the police in 1829, the shifting burden of proof to the prosecution in 1827, or the entitlement to defense attorneys for felonies in 1836 – applied to all offenses at the Old Bailey (which included primarily felonies). Thus, simply including year effects controls for any reforms that affect all offense categories. To the extent one does not believe this to be the case – though we have no anecdotal evidence to suggest this – much of our analyses are conducted at the broad offense category level (e.g. property offenses or violent and sex offenses). It is reasonable to argue that all sub-offense categories within these broad categories are equally treated by these reforms. Finally, we note that the abolition of capital punishment did not actually occur for any offense in the same years (1827, 1829, 1836) as the above mentioned reforms. In addition, Appendix Figure 2 demonstrates that the results are robust to excluding each offense category.

### *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 the following broad offense categories: property, violent and sex, and fraud. Panel A presents the results for the main dependent variable indicating whether the jury

[33] 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.

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 and sex offense cases and 35 percentage points (47.5%) for fraud offenses. In contrast, the effect for property crimes – by far the largest 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 offense and recommendation to mercy, respectively. Conditional on being found guilty, the chance of conviction of a lesser charge *on average* 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 estimates are *average* effects; importantly, the average rates of conviction of a lesser offense and recommendation to mercy vary substantially between offenses.[34] 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.[35] 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.

[34] 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*.

[35] 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 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 minimal change in the chance of conviction and mercy; however, there is a large 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 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 robustness to excluding sodomy and wounding, which are the two offense categories for which capital punishment was abolished in stages.[36] Finally, 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 estimate (columns (7) and (8), respectively).[37]

In addition, we test the robustness of our results to alternative specifications of the standard errors as discussed above. The results are shown in Appendix Table 2 and suggest that the baseline results are indeed robust to clustering at a higher level and/or block bootstrapping. The only change in significance is that the previously observed marginally significant effect on conviction and mercy for property crimes becomes insignificant; however, this does not change our interpretation of the results.

[36] In fact, Appendix Figure 2 demonstrates the robustness of our results to excluding each offense category at a time. The results are not being driven by any single category.

[37] Additional specification checks (not shown, but available upon request) demonstrate robustness to excluding multiple defendants and 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).

### 4.4. *Empirical Tests of Identifying Assumptions*

The above analysis makes three key identifying assumptions that we empirically test in this section: (i) there were parallel pre-reform trends in conviction rates for treatment and control offenses, (ii) the timing of the reforms was as good as random and therefore there are no anticipatory effects, and (iii) there were no confounding effects, i.e. it is only the expected punishment for the charged offense that changes the jury's behavior and not any other aspects of cases, in particular with respect to the quality of evidence.

We begin by testing the assumptions of parallel trends and no anticipatory effects by estimating our baseline model with five leads for the years prior to the reform. If there were anticipatory effects, then one would expect to see an effect before the reform was put in place. Moreover, if there are no significant differences between conviction rates for treated and control offenses in the years prior to reforms, then this is evidence of parallel pre-reform trends. Table 6 demonstrates this to be the case; that is, there is no differential conviction behavior for treated versus control offenses in the years leading up to the reforms.

We now turn to the assumption of no confounding effects with regards to the quality of evidence. 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, making it easier for a jury to convict the defendant. Clearly, it is possible that abolishing the death penalty impacts the behavior of other agents in the justice system, including potential criminals, defendants, police, and attorneys. But, does it do so in a way that changes the composition of cases facing the jury and, more importantly, 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.[38]

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

[38] 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.

there are no *long-term* deterrent effects of executions on homicide. Panel A of Figure 7 presents the number of cases within the 'treated' offenses for each broad crime category seen in the Old Bailey 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.[39] 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, though modern day empirical studies demonstrate that prosecutors respond to expected punishment (e.g. by reducing charges when confronted with three-strikes laws; Bjerk, 2005), it is not clear that this is a significant concern in this context, since (i) punishments became more lenient and not harsher and (ii) lawyers were a less formalized institution and it was not too long ago that just the victim served as prosecutor. It is thus not surprising that Tonry's (1992) anecdotal review of the means by which capital punishment was avoided prior to its abolition highlights the role of the jury (and judges issuing pardons, which is a stage further in the judicial process than we study here), but does not mention lawyers.

Finally, one could imagine that changes in expected punishment affect a defendant's decision to plead guilty. If this affects the type of cases 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) are more likely to

[39] 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.

plead, which would again lower the average 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 7 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).

Given the potential importance of changing plea behavior as a result of the reforms, however, we conduct two additional robustness checks. First, we check that our results are not driven by a change in case composition due to a change in plea behavior by re-estimating our baseline specification when including all cases in which a defendant pled guilty and treating them as a guilty verdict by the jury. Given an overall conviction rate of just 70%, assuming that all pleas would result in a guilty verdict is a strong assumption. These results are presented overall and by broad offense category in Panel A of Table 7. Although the overall effect (which remains significant) and the property crime effect (which becomes insignificant) decrease compared to the baseline, we still find that the abolition of capital punishment significantly increases conviction rates for violent and sex offenses as well as for fraud offenses. In fact, the violent and sex offense effect does not change at all when including plea cases as guilty verdicts; this is in part due to the fact that pleas were much less common for these types of offenses. Indeed, given that some of the largest effects of abolishing capital punishment are seen for offense categories with few pleas, it is hard to justify the argument that changing plea behavior is driving our results.

Second, we directly test whether there is a significant change in plea behavior for treated offenses relative to control offenses with the abolition of capital punishment. That is, we re-estimate the baseline empirical specification when the dependent variable is a dummy equal to one if the defendant pled guilty. These results are presented in Panel B of Table 7 and indicate that, if anything, there is actually a decrease in the likelihood of pleading overall and for property and fraud offenses. There is a small increase in the chance of pleas for violent offenses, but this has already been shown to not impact the results. The estimated reduction in the chance of pleas is consistent with the results described above in Panel B of Figure 7. Plea behavior is simply becoming more common at this time for all offenses, and slightly more so for control offenses.

Finally, 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).[40] Table 8 presents the results of estimating equation (1) for a 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.

### *4.5. Capital Punishment: Heterogeneity and Dynamics*

Before turning to the transportation experiment, we consider two potential sources of heterogeneity – across case characteristics and over time (i.e. dynamics). Panel A of Table 9 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 9). 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.

[40] 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.

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.

Table 10 considers the dynamics of the estimated effects: are they temporary or permanent? To do this, we estimate the baseline specifications when breaking up the post-reform period into five periods: 0-4 years after treatment, 5-9 years after, 10-14 years after, 15-19 years after, and 20 or more years after. We find that the results are very persistent over time: this is true for both the increased chance of conviction overall and for violent/sex and fraud offenses as well as the decreased chance of a lesser guilty verdict for property offenses. Such permanent effects imply that the baseline effect is not driven by a transitory shock to juries in the year of the reform, but indeed captures a persistent change to juries' conviction behavior.

## 5. *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 8 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 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 8).

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.[41] 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. Unfortunately, we lack a sufficiently large control group to estimate a difference-in-differences specification to isolate the causal effect of the shift in punishment from anything else changing during the war. That is, as seen in Table 2, only about 5% of the trials in the years surrounding the American Revolution are ineligible for transportation. We are thus limited to using a simple reduced-form pre-post design to provide suggestive evidence regarding the impact of the unexpected (and large) shift in expected punishment in the years during and after the Revolution. 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), and focuses on the years 1772 to 1779, i.e. the four years surrounding the start of the war and prior to the riots.

[41] 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.

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

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 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). [42]

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), we divide the transportation eligible offenses into two sub-treatment groups, capital versus non-capital eligible offenses. We emphasize the latter, for which the halt of transportation unambiguously decreases punishment severity, as the only substitute is prison/hulks.

The results of estimating equation (2) are presented in Table 11. 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

[42] 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.

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 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.[43]

In summary, our analysis of the temporary halt of transportation finds a reduction in the chance of conviction for non-capital transportation eligible offenses during the war, when imprisonment in the hulks of ships was the primary substitute sentence. While the simple pre-post design limits the causal interpretation, additional findings point in that direction. First, we did not find an effect for capital cases, which suggests that the results are not just driven by a change in attitudes towards all offenders due to the war. Second, we see the reverse relationship (though not significant) when considering the reinstatement of transportation.

[43] 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.

Lastly, it is important to note that any questions regarding defendant plea behavior are irrelevant in this context, as a plea is recorded for just 0.2% of the sample observations.

Transportation was primarily abolished as a sentencing option in the United Kingdom in 1853 (and completely in 1857). At this time, however, expected punishment was already at the lowest level since (at least) the early 1700s, with the abolition of capital punishment almost completed and less than 25% of offenses being sentenced to transportation; the primary sentencing option was already imprisonment. Estimating a similar pre-post specification for the years surrounding the abolition of transportation or a difference-in-differences specification that classifies treatment and control offenses as those with relatively high and low shares of transportation sentences, we find no evidence that this change in expected punishment impacted conviction rates (see Bindler and Hjalmarsson, 2016, for an earlier version including these results). Potential explanations include that either the extent to which changes in punishment severity affect jury behavior depends on the size of the change, or that jurors had in recent years already substantially increased conviction rates with the abolition of capital punishment.

## *6. Discussion and Conclusion*

Using two 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, significant and persistent 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.

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).[44] That is, our results are consistent with an

[44] See for example Katz et al. (2003), Donohue and Wolfers (2006), Hjalmarsson (2009), or Cohen-Cole et al. (2009).

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.[45] 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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[45] See for example Abrams et al. (2012) or Alesina and Ferrara (2014).

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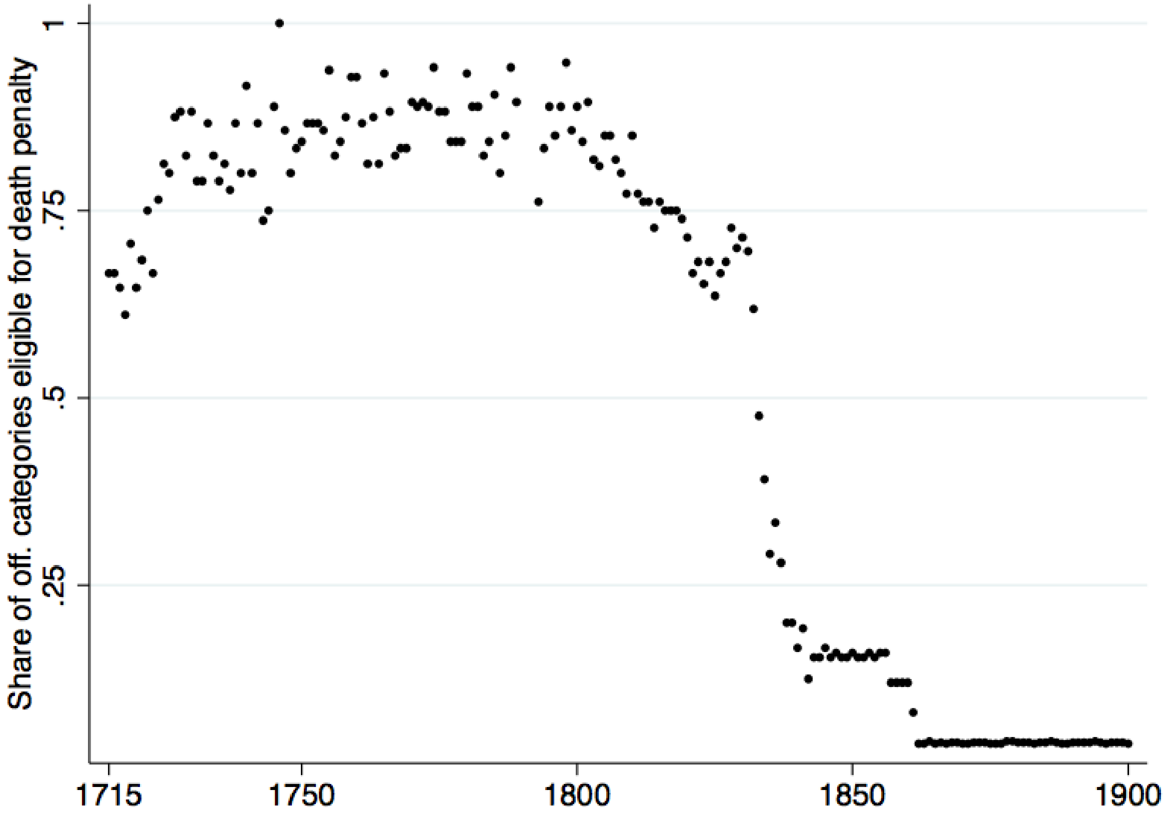
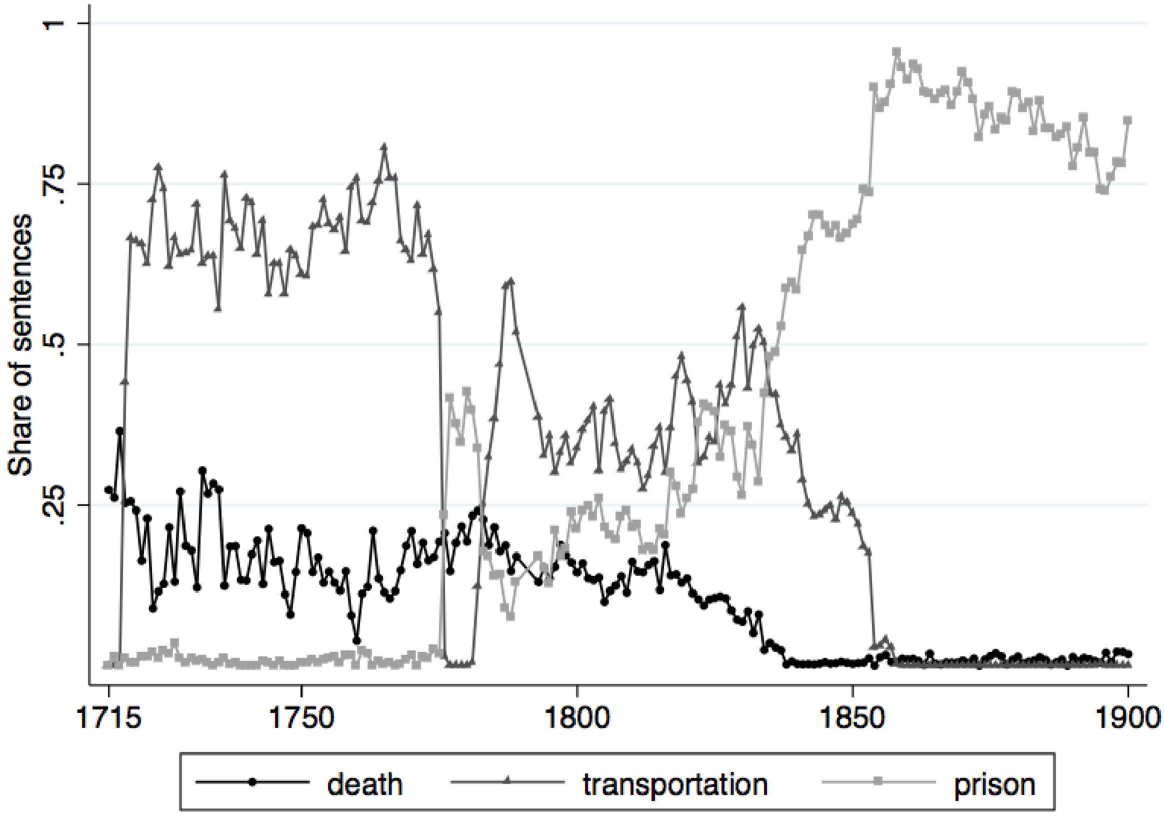
**Figure 1. Share of capital eligible offenses (1715-1900)**



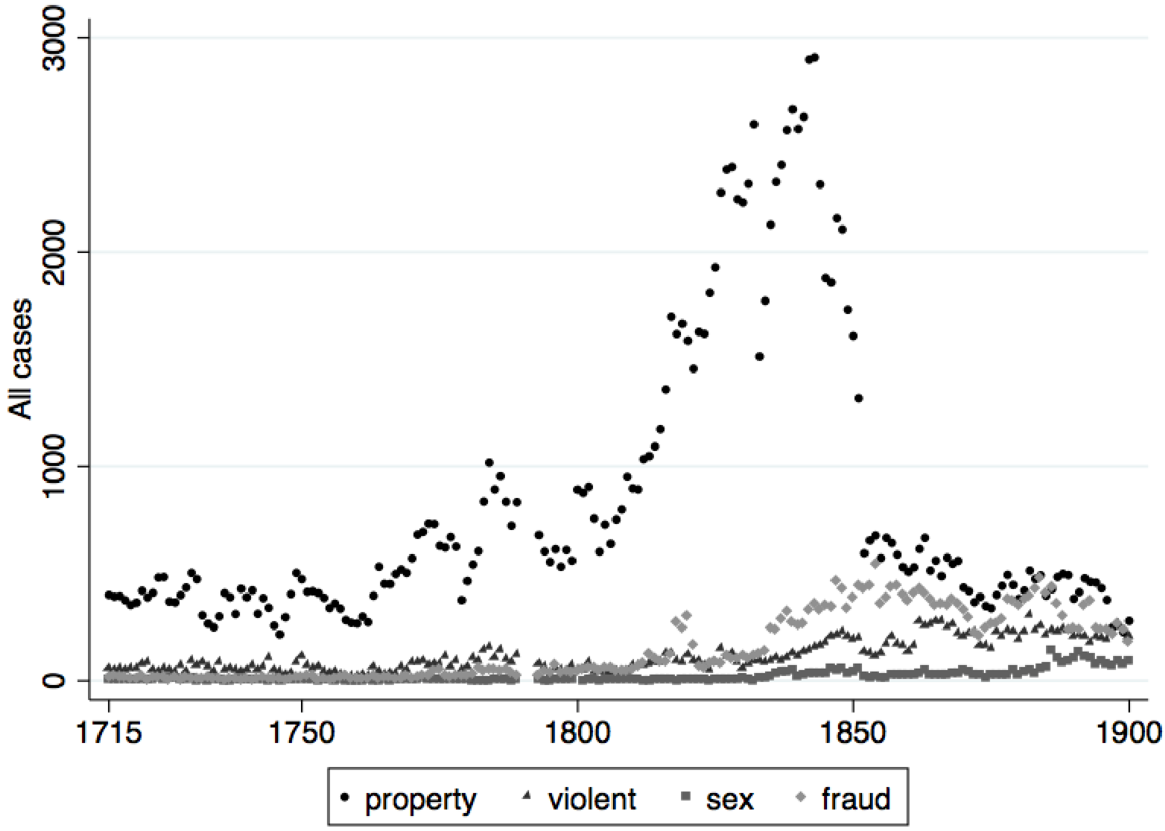

NOTE- The figure shows the share of offense categories in the sample that are eligible for capital punishment between 1715 and 1900. SOURCE- *The Old Bailey Proceedings Online* and own calculations.

**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.

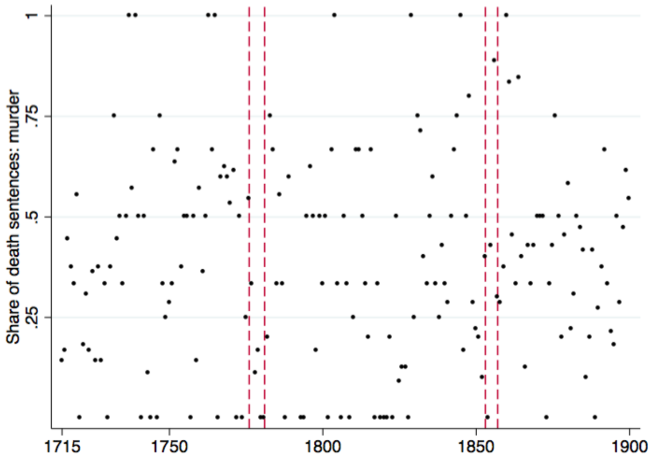
**Figure 3. Number of cases by broad offense category, analysis sample**



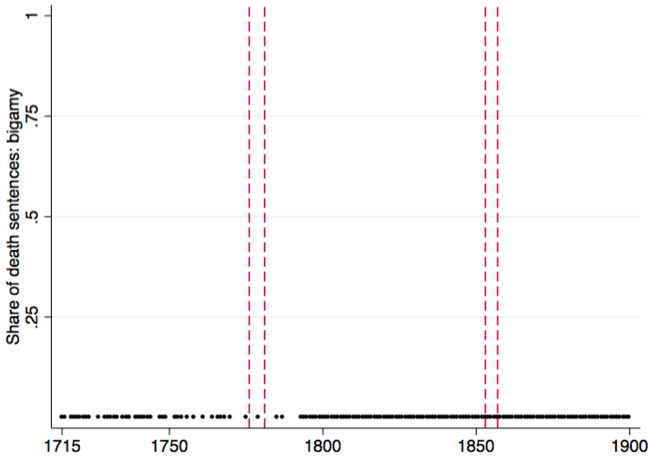

NOTE- The figure shows the annual number of cases in the sample (tried at the Old Bailey) between 1715 and 1900 and by broad offense category (property offenses, violent offenses, sex offenses and fraud offenses). SOURCE- *The Old Bailey Proceedings Online* and own calculations.

**Figure 4. Identifying the time of treatment – Abolition of capital punishment**

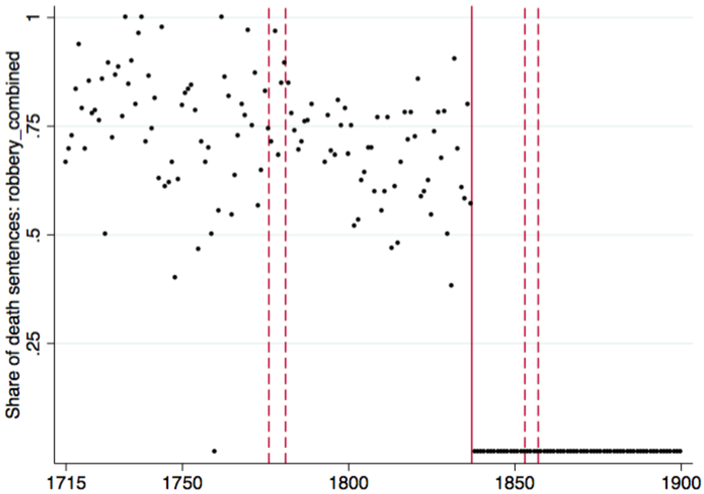
***Panel A – Always capital punishment eligible***




***Panel B – Never capital punishment eligible***



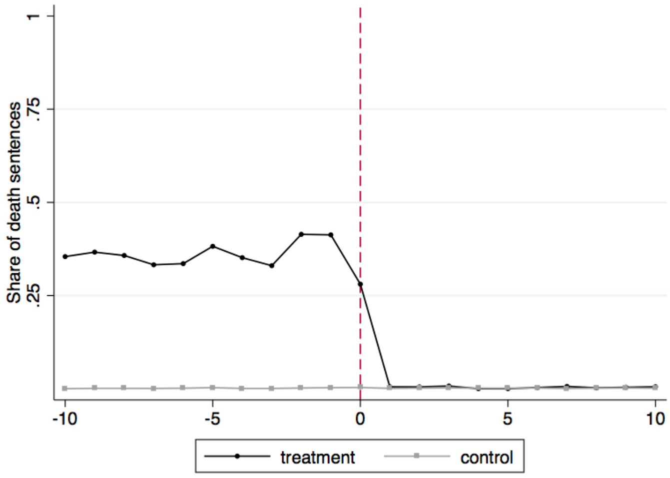

***Panel C – Change in capital punishment eligibility***



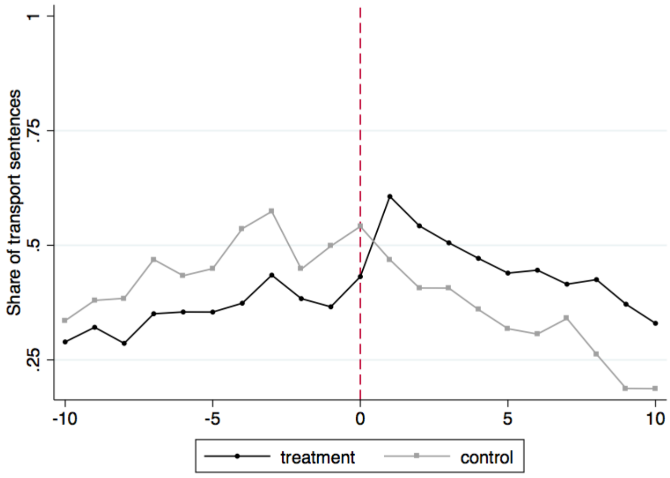

NOTE- The figure shows the annual share of convicted cases in the sample that were sentenced to death for murder (Panel A, always capital punishable), bigamy (Panel B, never capital punishable) and robbery (Panel C, change in capital punishment eligibility). The dashed vertical red lines mark the years 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 5. Sentencing and the abolition of capital punishment**

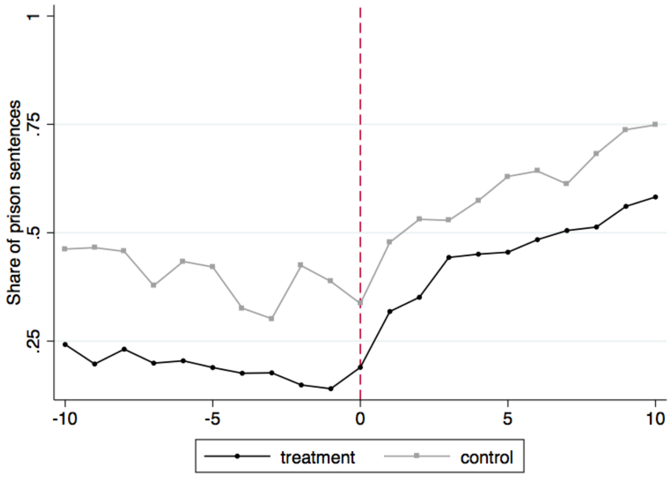
***Panel A – Treatment and control group, death penalty***




***Panel B – Treatment and control group, transportation***

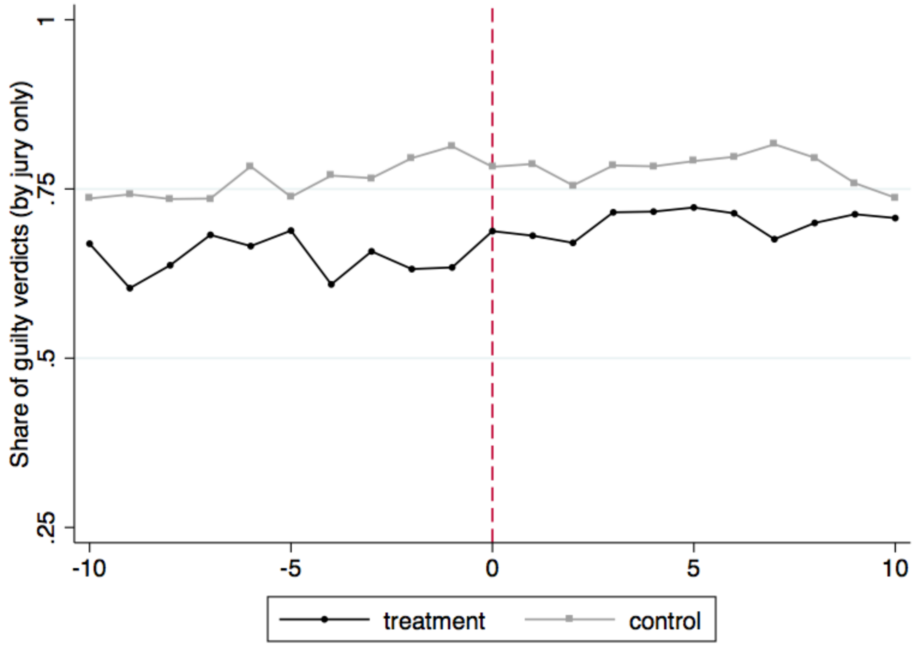



***Panel C – Treatment and control group, prison***




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.

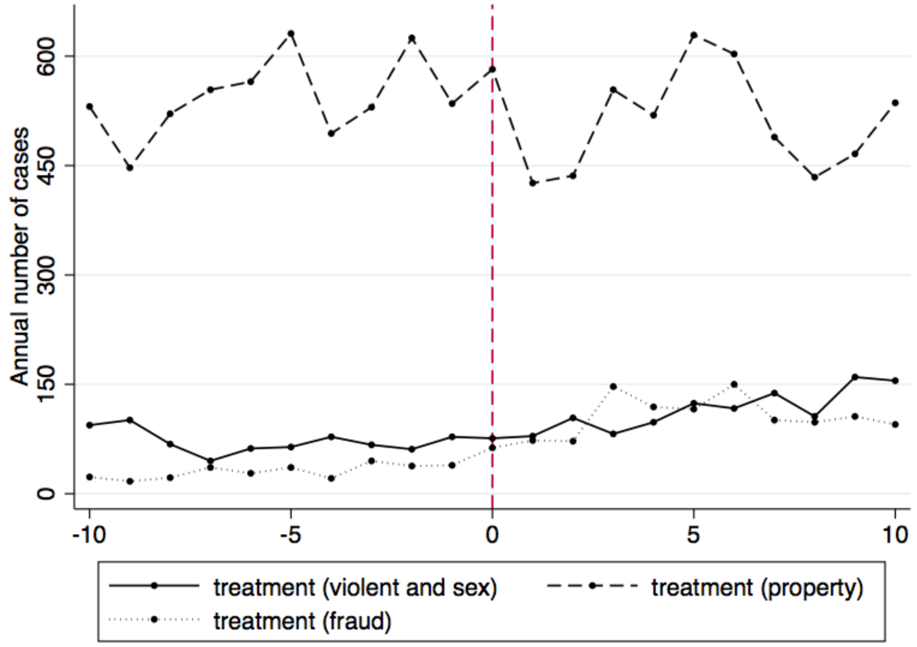
**Figure 6. Conviction rates and the abolition of capital punishment**



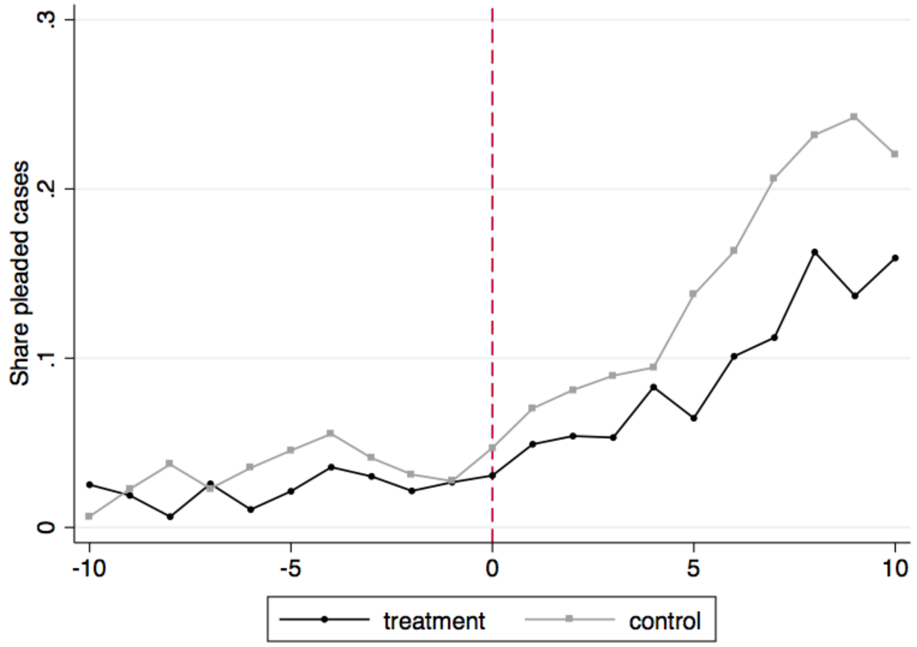

NOTE- The figure shows the annual share of guilty verdicts (cases convicted by jury) in the treatment (black) and control (grey) group for all offenses in the sample in the 10 years before and after the assigned treatment year and relative to all cases tried by jury. The vertical line marks the offense 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 7. Changes in behavior and the abolition of capital punishment**

***Panel A - Changes in criminal behavior***



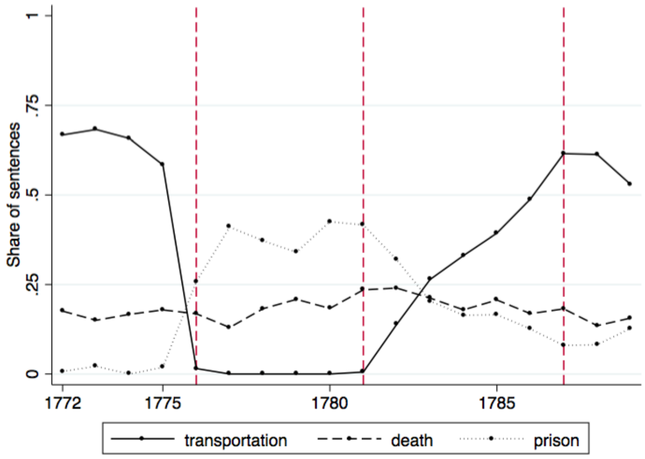

***Panel B - Changes in plea behavior***




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.

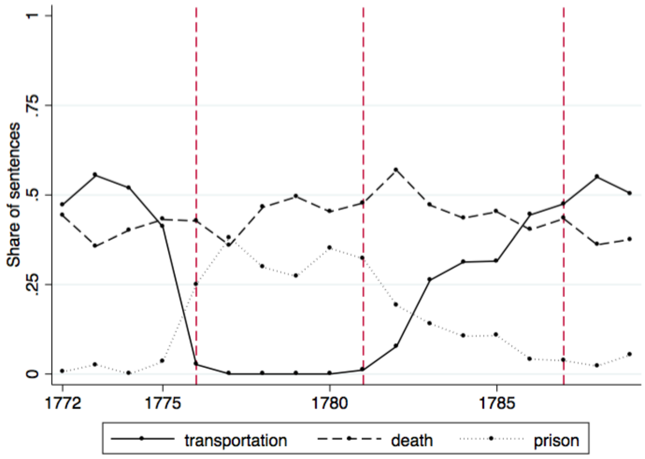
**Figure 8. Sentencing and the American Revolution**

***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.

**Table 1. Offense categories**

| Category | Subcategory | Offenses | Combined Offenses |
|---|---|---|---|
| Property | Theft | Animal theft, mail, stealing from master, theft from place, shoplifting<br><br>*Excluded from sample:*<br>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<br><br>*Excluded from sample:*<br>Breaking into place | |
| Violent | Killing | Manslaughter, murder<br><br>*Excluded from sample:*<br>Infanticide, petty treason | |
| | Other | Assault, wounding<br><br>*Excluded from sample:*<br>Kidnapping, riot | Robbery: Highway robbery, robbery |
| Sex | Violent | Rape | Sexual assault: Assault with intent, indecent assault |
| | Other | *Excluded from sample:*<br>Keeping a brothel | Sodomy: Assault with sodomitical intent, sodomy |
| Fraud | Fraud | Coining offenses, embezzlement, forgery, fraud<br><br>*Excluded from sample:*<br>Treason | |
| Other | Other | Bigamy, libel, perjury, perverting justice<br><br>*Excluded from sample:*<br>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 | |
|---|---|---|---|---|---|
| | *Offence specific laws* | | | *American Revolution* | |
| | +/-10 years around treatment year | | | 1772 - 1789 | |
| **Offense** | **Treatment** | **Year** | **#Cases** | **Treatment** | **#Cases** |
| ***Property offenses*** | | | | | |
| Animal theft | T | 1832 | 1168 | T | 435 |
| Arson | T | 1856 | 111 | D | 19 |
| Burglary | T | 1837 | 1081 | T | 1323 |
| Housebreaking | T | 1833 | 1396 | T | 164 |
| Larceny | C (never) | Median | 32278 | T | 8181 |
| Mail | T | 1834 | 74 | D | 5 |
| Receiving | T | 1837 | 3567 | T | 686 |
| Shoplifting | T | 1820 | 763 | T | 441 |
| Stealing from master | C (never) | Median | 4696 | D | 0 |
| Theft from place | T | 1832 | 3706 | T | 1537 |
| ***Violent and sex offenses*** | | | | | |
| Assault | C (never) | Median | 185 | D | 5 |
| Manslaughter | C (never) | Median | 295 | C | 14 |
| Murder | C (always) | Median | 222 | C | 161 |
| Robbery | T | 1837 | 859 | T | 1529 |
| Rape | T | 1841 | 228 | C | 63 |
| Sexual assault | D | - | 0 | D | 0 |
| Sodomy | T | 1832 (1860) | 81 | C | 15 |
| Wounding | T | 1837 (1861) | 825 | C | 35 |
| ***Fraud offenses*** | | | | | |
| Coining offenses | T | 1832 | 893 | C | 337 |
| Embezzlement | C (never) | Median | 1650 | D | 3 |
| Forgery | T | 1832 | 581 | C | 155 |
| Fraud | T | 1813 | 160 | T | 151 |
| ***Other offenses*** | | | | | |
| Bigamy | C (never) | Median | 225 | D | 20 |
| Libel | C (never) | Median | 23 | D | 1 |
| Perjury | C (never) | Median | 102 | T | 100 |
| Perverting justice | T | 1831 | 83 | T | 77 |
| ***Total*** | **26** | | **55 252** | **26** | **15 457** |
| *Treatment* | *16* | **Median: 1833** | *15 576* | *11* | *14 624* |
| *Control* | *9* | | *39 676* | *7* | *780* |
| *Dropped* | *1* | | *0* | *8* | *53* |

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* |
| **Variable** | **1715-1900** | **1803 - 1871** | | **1772 -1789** |
| ***Sample*** | | | | |
| Number of observations (N) | 206,733 | 49,285 | 76,673 | 14,624 |
| Number of sessions (N) | 1 748 | 703 | 703 | 153 |
| Avg. number of cases per session | 150.0 | 172.8 | 211.1 | 97.64 |
| Avg. number of defendants per case | 1.483 | 1.762 | 1.265 | 1.512 |
| ***Offenses*** | | | | |
| Property off. (0/1) | 0.729 | 0.577 | 0.893 | 0.873 |
| Violent off. (0/1) | 0.101 | 0.137 | 0.032 | 0.105 |
| Sex off. (0/1) | 0.018 | 0.020 | 0.006 | |
| Fraud off. (0/1) | 0.133 | 0.262 | 0.051 | 0.010 |
| Other off. (0/1) | 0.020 | 0.005 | 0.018 | 0.012 |
| ***Defendants*** | | | | |
| Avg. age | 27.57 | 27.10 | 26.40 | 16.79 |
| Aged 18 to 21 (0/1) | 0.240 | 0.258 | 0.254 | 0.131 |
| Aged under 18 (0/1) | 0.150 | 0.129 | 0.200 | 0.779 |
| Age missing (0/1) | 0.376 | 0.237 | 0.230 | 0.985 |
| Age missing, guilty cases (0/1) | 0.161 | 0.053 | 0.045 | 0.569 |
| Age missing, non-guilty cases (0/1) | 0.214 | 0.183 | 0.185 | 0.410 |
| Male (0/1) | 0.786 | 0.813 | 0.781 | 0.728 |
| Any known criminal history, from 1832 (0/1) | 0.111 | 0.106 | 0.101 | |
| ***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 |
| Guilty by jury or plea (0/1) | 0.719 | 0.726 | 0.779 | 0.584 |
| Guilty by jury (0/1) | 0.675 | 0.681 | 0.741 | 0.583 |
| Guilty of lesser offense (0/1) | 0.047 | 0.085 | 0.009 | 0.052 |
| Recommended for mercy (0/1) | 0.061 | 0.071 | 0.093 | 0.030 |
| Not guilty by jury (0/1) | 0.324 | 0.318 | 0.259 | 0.416 |
| ***Sentences conditional on guilty by jury or plea*** | | | | |
| Capital punishment (0/1) | 0.069 | 0.123 | 0.004 | 0.187 |
| Transportation (0/1) | 0.294 | 0.259 | 0.308 | 0.381 |
| Imprisonment (0/1) | 0.522 | 0.564 | 0.578 | 0.186 |
| Corporal punishment (0/1) | 0.042 | 0.011 | 0.038 | 0.179 |
| Miscellaneous punishment (0/1) | 0.045 | 0.020 | 0.042 | 0.055 |
| No punishment (0/1) | 0.030 | 0.022 | 0.030 | 0.012 |

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**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Offense: | All | All | All | All | Property | All | All | 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 |
| ***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*** |
| | (0.0091) | (0.0079) | (0.0085) | (0.0135) | (0.0127) | (0.0089) | (0.0098) | (0.0111) |
| Mean | 0.721 | 0.721 | 0.721 | 0.673 | 0.686 | 0.722 | 0.728 | 0.739 |
| Observations | 104,670 | 104,670 | 104,670 | 52,535 | 31,855 | 101,909 | 86,637 | 66,679 |
| Cluster | | 1535 | 1535 | 1466 | 554 | 1405 | 1030 | 693 |
| R-squared | 0.067 | 0.076 | 0.070 | 0.082 | 0.064 | 0.067 | 0.072 | 0.068 |
| ***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 | 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 |
| Year f.e. | yes | yes | yes | yes | yes | yes | yes | yes |
| Month f.e. | yes | yes | yes | yes | yes | yes | yes | yes |
| Control var. | 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. Tests for Parallel Trends and Anticipatory Effects**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Offense: | all | all | property | violent and sex | fraud |
| ***Guilty by jury verdict (0/1) - Leads*** | | | | | |
| noncapital (0/1), t | 0.0573*** | 0.0508*** | 0.0006 | 0.2910*** | 0.266*** |
| | (0.0154) | (0.0157) | (0.0122) | (0.0404) | (0.0658) |
| noncapital (0/1), t+1 | -0.0361 | -0.0551 | -0.0672* | -0.0343 | -0.1830 |
| | (0.0401) | (0.0392) | (0.0397) | (0.0776) | (0.1450) |
| noncapital (0/1), t+2 | 0.0447 | 0.0496 | 0.0404 | -0.0553 | 0.4810* |
| | (0.0481) | (0.0474) | (0.0504) | (0.1140) | (0.2850) |
| noncapital (0/1), t+3 | -0.0104 | -0.0118 | -0.0099 | 0.0483 | -0.0712 |
| | (0.0344) | (0.0342) | (0.0348) | (0.1310) | (0.2690) |
| noncapital (0/1), t+4 | -0.0035 | -0.0047 | 0.0048 | -0.1700 | 0.2150 |
| | (0.0353) | (0.0354) | (0.0321) | (0.1210) | (0.1480) |
| noncapital (0/1), t+5 | 0.0400 | 0.0431 | 0.0390 | 0.1010 | -0.327*** |
| | (0.0303) | (0.0306) | (0.0277) | (0.0757) | (0.1220) |
| Observations | 100,797 | 100,585 | 82,354 | 8,785 | 8,289 |
| R-squared | 0.053 | 0.068 | 0.051 | 0.114 | 0.152 |
| Cluster | 1410 | 1410 | 573 | 435 | 192 |
| 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 robustness regressions corresponding to estimating equation (1) when allowing for 5 years of leads. The dependent variable is a dummy variable indicating a guilty jury verdict. 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. Robustness to and Tests for Changes in Plea Behavior**

| Offense: | (1) all | (2) property | (3) violent and sex | (4) fraud |
|---|---|---|---|---|
| ***Panel A. Guilty by jury verdict or plea (0/1)*** | | | | |
| noncapital (0/1) | 0.0471*** | 0.0047 | 0.2210*** | 0.2610*** |
| | (0.0080) | (0.0073) | (0.0279) | (0.0482) |
| Mean | 0.759 | 0.771 | 0.612 | 0.799 |
| Observations | 121,410 | 96,527 | 10,470 | 12,808 |
| Cluster | 1548 | 625 | 475 | 207 |
| R-squared | 0.064 | 0.053 | 0.111 | 0.078 |
| ***Panel B. Plea (0/1)*** | | | | |
| noncapital (0/1) | -0.1130*** | -0.0747*** | 0.0221** | -0.1070** |
| | (0.0080) | (0.0057) | (0.0091) | (0.0476) |
| Mean | 0.138 | 0.130 | 0.043 | 0.268 |
| Observations | 121,410 | 96,527 | 10,470 | 12,808 |
| Cluster | 1548 | 625 | 475 | 207 |
| R-squared | 0.151 | 0.174 | 0.057 | 0.073 |
| Offense f.e. | yes | yes | yes | yes |
| Year f.e. | yes | yes | yes | yes |
| Month f.e. | yes | yes | yes | yes |
| Control var. | yes | yes | yes | yes |

NOTE- The table shows the results for the robustness regressions corresponding to estimating equation (1) when the dependent variable is a dummy variable indicating either a guilty jury verdict or a guilty plea (Panel A) or a dummy variable indicating a guilty plea (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 8. 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 9. 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 10. Dynamics of Changing Expected Punishment**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Offense: | all | all | property | violent and sex | fraud |
| ***Panel A. Guilty by jury verdict (0/1)*** | | | | | |
| noncapital (0/1) x 0-4 years after treatment | 0.0529*** | 0.0412*** | -0.0192 | 0.222*** | 0.308*** |
| | (0.0131) | (0.0127) | (0.0127) | (0.0376) | (0.0670) |
| noncapital (0/1) x 5-9 years after treatment | 0.0801*** | 0.0630*** | 0.0044 | 0.265*** | 0.300*** |
| | (0.0129) | (0.0121) | (0.0106) | (0.0383) | (0.0758) |
| noncapital (0/1) x 10-14 years after treatment | 0.106*** | 0.0905*** | 0.0274** | 0.245*** | 0.467*** |
| | (0.0138) | (0.0131) | (0.0114) | (0.0351) | (0.0643) |
| noncapital (0/1) x 15-19 years after treatment | 0.154*** | 0.134*** | 0.0909*** | 0.165*** | 0.568*** |
| | (0.0139) | (0.0128) | (0.0136) | (0.0365) | (0.0709) |
| noncapital (0/1) x 20+ years after treatment | 0.110*** | 0.0945*** | 0.0217 | 0.212*** | 0.529*** |
| | (0.0155) | (0.0137) | (0.0150) | (0.0331) | (0.0760) |
| Observations | 104,910 | 104,670 | 83,990 | 10,017 | 9,375 |
| Cluster | 1535 | 1535 | 623 | 475 | 207 |
| R-squared | 0.054 | 0.068 | 0.052 | 0.107 | 0.144 |
| ***Panel B. Guilty of lesser offence conditional on guilty by jury verdict (0/1), broad definition*** | | | | | |
| noncapital (0/1) x 0-4 years after treatment | -0.110*** | -0.110*** | -0.143*** | 0.0704 | 0.0119 |
| | (0.0129) | (0.0129) | (0.0152) | (0.0461) | (0.0210) |
| noncapital (0/1) x 5-9 years after treatment | -0.142*** | -0.142*** | -0.199*** | 0.0474 | -0.0017 |
| | (0.0171) | (0.0170) | (0.0180) | (0.0544) | (0.0219) |
| noncapital (0/1) x 10-14 years after treatment | -0.194*** | -0.194*** | -0.242*** | -0.00268 | -0.0266 |
| | (0.0170) | (0.0168) | (0.0200) | (0.0480) | (0.0200) |
| noncapital (0/1) x 15-19 years after treatment | -0.200*** | -0.201*** | -0.253*** | -0.0491 | -0.0272 |
| | (0.0179) | (0.0178) | (0.0214) | (0.0525) | (0.0249) |
| noncapital (0/1) x 20+ years after treatment | -0.157*** | -0.157*** | -0.221*** | 0.0142 | -0.0341 |
| | (0.0115) | (0.0114) | (0.0121) | (0.0423) | (0.0267) |
| Observations | 75,571 | 75,422 | 61,919 | 5,961 | 6,806 |
| Cluster | 1423 | 1423 | 595 | 434 | 205 |
| R-squared | 0.258 | 0.261 | 0.242 | 0.222 | 0.141 |
| 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 dynamic analysis corresponding to estimating equation (1) but allowing the coefficient to vary by time period after treatment. 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 11. 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.

## 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), Criminal Law Consolidation Acts (1856) | 1856 (1837) |
| Burglary | An act to Amend the Laws relating 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)<br>An Act to consolidate and amend the Statute Law of England and Ireland relating to Offfences 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 Table 2. Baseline Regressions with Alternative Clustering

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Offense: | All | All | Property | Violent and sex | Fraud |
| ***Panel A. Guilty by jury verdict (0/1)*** | | | | | |
| coeff.: noncapital (0/1) | 0.092 | 0.076 | 0.015 | 0.220 | 0.345 |
| s.e.: off x year cluster | (0.0102)*** | (0.0091)*** | (0.0080)* | (0.0285)*** | (0.0515)*** |
| s.e.: off cluster | {0.0401}** | {0.0365}** | | | |
| s.e.: off block bootstrap | [0.0450]** | [0.0412]* | [0.0241] | [0.0842]*** | [0.0638]*** |
| Observations | 104,910 | 104,670 | 83,990 | 10,017 | 9,375 |
| Number of cluster ( ) | 1535 | 1535 | 623 | 475 | 207 |
| Number of cluster { } and [ ] | 25 | 25 | 10 | 8 | 3 |
| ***Panel B. Guilty of lesser offence conditional on guilty by jury verdict (0/1), broad definition*** | | | | | |
| coeff.: noncapital (0/1) | -0.153 | -0.153 | -0.203 | 0.0214 | 0.0017 |
| s.e.: off x year cluster | (0.0106)*** | (0.0105)*** | (0.0114)*** | (0.0397) | (0.0133) |
| s.e.: off cluster | {0.0462}*** | {0.0456}*** | | | |
| s.e.: off block bootstrap | [0.0533]*** | [0.0529]*** | [0.0606]*** | [0.207] | [0.0477] |
| Observations | 75,571 | 75,422 | 61,919 | 5,961 | 6,806 |
| Number of cluster ( ) | 1423 | 1423 | 595 | 434 | 205 |
| Number of cluster { } and [ ] | 25 | 25 | 10 | 8 | 3 |
| ***Panel C. Recommended for mercy conditional on guilty by jury verdict (0/1)*** | | | | | |
| coeff.: noncapital (0/1) | -0.059 | -0.0602 | -0.0363 | -0.235 | -0.150 |
| s.e.: off x year cluster | (0.0070)*** | (0.0069)*** | (0.0069)*** | (0.0273)*** | (0.0358)*** |
| s.e.: off cluster | {0.0194}*** | {0.0199}*** | | | |
| s.e.: off block bootstrap | [0.0234]** | [0.0236]** | [0.0285] | [0.0719]*** | [0.0554]*** |
| Observations | 75,571 | 75,422 | 61,919 | 5,961 | 6,806 |
| Number of cluster ( ) | 1423 | 1423 | 595 | 434 | 205 |
| Number of cluster { } and [ ] | 25 | 25 | 10 | 8 | 3 |
| 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 verdict guilty with recommendation for mercy (Panel C). Standard errors clustered on year x offense are shown in ( ), standard errors clustered on offense in { } and standard errors block bootstrapped on offense (10,000 bootstrap repetitions) in [ ] 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 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.

*****************************************************

## C A P. 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

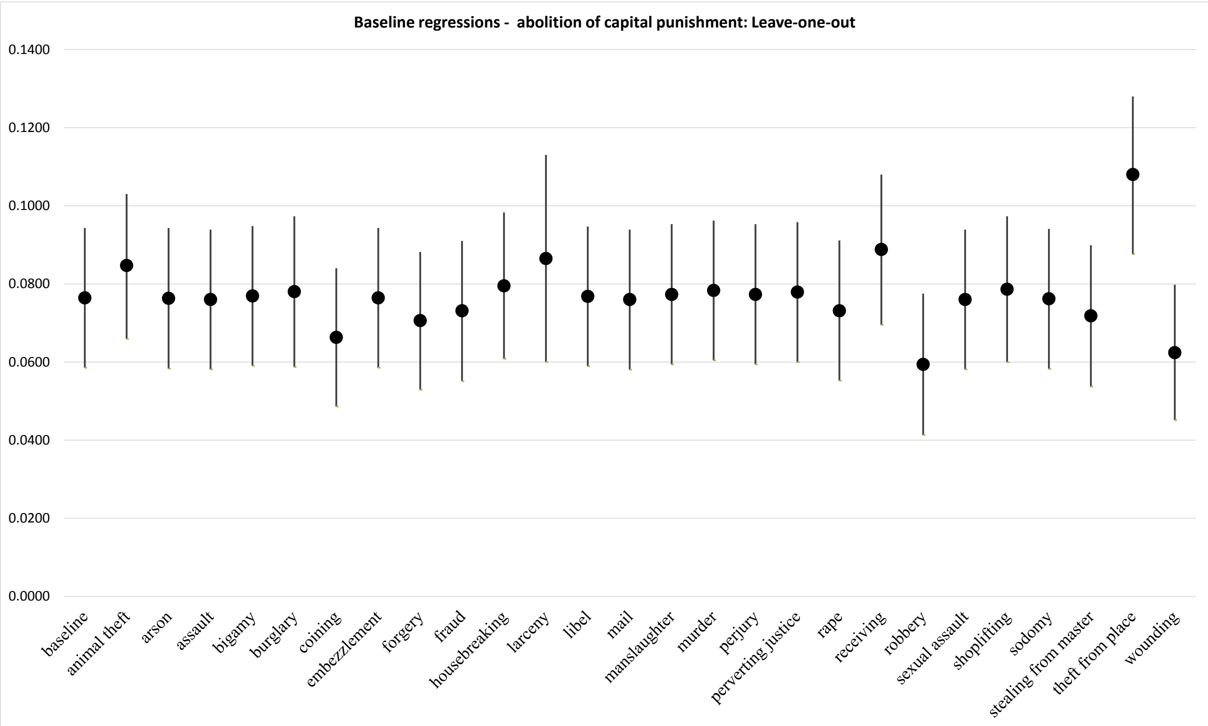
1 W. 4. c. 6

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

13 H or

SOURCE- UK Parliamentary Archives.

## Appendix Figure 2. Baseline regressions excluding one offense category at a time




NOTE- The figure shows the results for the baseline regressions corresponding to estimating equation (1) when excluding one offense category at a time. The dependent variable is a dummy variable indicating a guilty jury verdict. The dots represent the point estimate when the offense category indicated on the x-axis is excluded. The bars represent the corresponding 95% confidence intervals with standard errors clustered on year x offense. SOURCE- *The Old Bailey Proceedings Online* and own calculations.