Working Paper

A Jury of Her Peers

The Impact of the First Female Jurors on Criminal Convictions

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RAND Justice, Infrastructure, and Environment

WR-1146
March 2016

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A Jury of *Her* Peers: The Impact of the First Female Jurors on Criminal Convictions[*]

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Abstract

This paper uses an original data set of more than 3000 cases from 1918 to 1926 in the Central Criminal Courts of London to study the effect of the Sex Disqualification (Removal) Act of 1919. Implemented in 1921, this Act made females eligible to serve on English juries, providing a novel setting for studying the impact of female representation on jury verdicts. Results based on a pre-post research design imply that the inclusion of females had little effect on overall conviction rates but resulted in a large and significant increase in convictions for sex offenses and on the conviction rate differential between violent crime cases with female versus male victims. The inclusion of women also increased the likelihood of juries being discharged without reaching a verdict on all charges and the average time taken to reach a verdict. A complementary analysis of cases in which the jury was carried over from a previous trial also implies that the inclusion of female jurors on the seated jury sharply increased conviction rates for violent crimes against women versus men.

[*] This paper would not have been possible without the tremendous efforts of our Research Assistant Michael Bekele in transcribing the original court records and without the financial support of Foundation for Economic Research in West Sweden, and Vetenskapsrådet, The Swedish Research Council, Grants for Distinguished Young Researchers. Corresponding author: Randi Hjalmarsson, Department of Economics, University of Gothenburg, Email: randi.hjalmarsson@economics.gu.se

1. Introduction

Measured against the long span of human history, the inclusion of women in civic and political life has been remarkably recent and remains far from complete. In the United States, women have had the right to vote for fewer than 100 years and female representation in Congress has never topped 20 percent. A woman has not yet served as U.S. President. Beyond the elementary issues of fairness and representation, proponents of greater female participation in government and politics suggest that such inclusion alters the broad focus of policy, the collective functioning of legislative bodies, and executive decision-making.[1]

How the inclusion of women affects outcomes in any aspect of civic and political life is a stubbornly difficult question to answer well. The main challenge is that the selection of individuals for most positions – elected officials or political appointments – is far from random. This makes it difficult to separate the causal effect of female officials from the circumstances that lead to their appointments. That the majority of female members of the U.S. Congress represent progressive/liberal districts is an obvious reflection of this kind of selection across jurisdictions. Perhaps more subtly, this same selection problem also hampers the identification of gender differences using variation within jurisdictions. In this case, the positions of those appointed (regardless of gender) must be broadly compatible with the person or constituency doing the selecting. This tends to diminish gender differences among those selected relative to the larger population of qualified candidates. Strikingly, even the apparent gold standard of the random assignment of court cases to judges does not avoid this fundamental selection problem, as both female and male judges must have views and judicial approaches that permitted them to be appointed in the same district in the first place.[2]

In countries that follow the tradition of English common law, juries for criminal and civil trials constitute perhaps the most broad-based deliberative bodies in formal civic life. The right to a trial by a jury of one's peers is one of three laws (among 63 original clauses) to survive more or less intact over the eight centuries since the English Magna Carta was first written in 1215. Of course, the definition of 'peers' has changed tremendously over time, as

[1] A large body of correlational evidence suggests that male and female politicans differ systematically in basic policy positions, active policy focus, parliamentary approach and legislative effectiveness, implying that gender composition affects a range of substantive outcomes. See, for example, Lijphart (1999), Pearson and Dancey (2011), Anzia and Berry (2011), Volden, Wiseman, and Wittmer (2013), and Wittmer and Bouche (2013).

[2] The findings concerning female judges in the criminal courts are quite mixed. Even in the same jurisdiction, contradictory results have been found; Johnson (2014) finds female judges in Pennsylvania are more lenient than male judges while Steffensmeier and Hebert (1999) find that they impose harsher sentences. More generally, George (2001) concludes that the empirical literature has failed to find any broad and consistent gender based differences in judging. In fact, many researchers have concluded that judge characteristics, of which gender is just one example, simply do not explain much of the variation in judicial decisions (see, for instance, Ashenfelter et al., 1995), which is entirely consistent with our characterization of the fundamental selection problem above.

eligibility criteria for jury service have become increasingly inclusive along the dimensions of age, wealth, race and gender. Conditional on eligibility, however, citizens are often drawn at random for jury duty, which provides a basis for addressing the fundamental selection issue described above.

In this paper, we study one of the most substantial changes in eligibility criteria – the introduction of female jurors. Women became eligible to serve on English juries with the passing of the Sex Disqualification (Removal) Act of 1919. At the same time, similar movements were taking off around the United States; Utah became the first state in 1898 to authorize women to participate on juries but three states (Alabama, Mississippi, and South Carolina) banned female jurors as late as 1962.

We examine the impact of the first female jurors on convictions in criminal cases in London using an original data set that we have created from hand-written court records of the First and Second Courts of the Old Bailey from 1918 to 1926. From these records, we have extracted detailed information about the case and case outcomes (defendant names, charges, victim names, pleas or jury trials, verdicts and sentences), as well as the names of all individuals seated on the jury. The final data set consists of more than 3,000 observations.

Our focus on the historical period surrounding the introduction of the first female jurors in England is motivated by several important (related) considerations.[3] First, even though eligible citizens may be randomly called for jury duty, the pre-trial jury selection process (*voir dire*) provides the attorneys and judge considerable discretion over which members of the jury pool are actually seated for the trial, re-introducing a version of the fundamental selection issue described above. Second, variation in the composition of modern juries is typically centered at a point of approximate gender balance. Thus, even if research designs exploit random variation in the composition of the jury pool to deal with pre-trial selection, the results are generally only informative about variation near this point of gender balance – e.g., identifying the difference in outcomes for 12-person juries with six versus seven female members. In contrast, our analysis estimates the impact of including female jurors on what had been all-male juries, providing a sharper and more meaningful comparison for assessing the broad impact of female representation.

The question of whether female representation on juries affects verdicts has been raised by the popular press both today and at the time of the 'great experiment' - the term used by

[3] We study the introduction of female jurors in England (rather than the United States, for example) because the data that we have gathered provide direct measures of the impact of the reform on the extent to which females were actually seated on juries. Comparably rich US data is not available.

newspapers during the period to characterize the new eligibility of female jurors. For instance, a jury of six women decided George Zimmerman was not guilty in the shooting of an unarmed black teenager, Trayvon Martin, in 2014; subsequently, almost every media outlet in the country included a headline questioning whether the extreme gender composition of the jury affected the verdict. During the peak of the women's jury movement in the United States, Susan Glaskell wrote a short story titled *'A Jury of Her Peers*' that depicts how men and women perceive things differently in the household; though the story does not take place in a courtroom, it depicts how male and female jurors might evaluate evidence through different lenses, particularly in the case of a female defendant.[4]

The existing empirical literature has used experiments (mock juries) and post-trial juror surveys to study whether female jurors have an impact on trial outcomes.[5] The results of the mock jury studies are both inconsistent and often limited in their generalizability given the experimental simplifications, non-representative subjects, and lower stakes compared to the real world.[6] Studies using post-verdict survey data in the US generally find little or no evidence of a gender effect.[7] Few studies of UK juries exist given the 1981 Contempt of Court Act, which made it illegal to survey jurors. Baldwin and McConville (1979) conducted one of the first (and only) studies of English juries using a sample of 500 trials in Birmingham, finding no evidence that jury composition, including gender, affect verdicts. The Ministry of Justice has recently begun to address this knowledge gap concerning the impartiality of English juries, particularly with respect to race (Thomas and Balmer, 2007). Conducting mock trials with actual jurors at the Crown Courts, Thomas (2010) finds female jurors presented with a male defendant charged with bodily harming a male victim were significantly more likely to vote to convict at the start of deliberations, but also more likely to change their views during deliberations – such that there was no significant difference between male and female conviction rates in the final vote. Mills and Bohannon (1980) report similar findings for the US.

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[4] First published in 1917, '*A Jury of Her Peers*' takes place at the Iowa farm of Minnie Wright, who is charged with murdering her husband. The sheriff, prosecutor and a neighbor (all men) come to gather evidence to support the charge and two women (the wives of the sheriff and neighbor) accompany them. While the men find nothing unusual in the kitchen, the women note a disordered household, an irregular quilting pattern, and find a strangled canary. These clues lead them to discuss Minnie's abusive and authoritarian husband, and to sympathize with her desperation. In essence, these women decide she is not guilty and not to reveal the motive to the investigators.

[5] See Devine et al (2000) and Bonazzoli (1998) for brief reviews of this literature.

[6] Mock jury studies exist that find females are: (i) pro-conviction (Helgelson and Shaver, 1990; Hastie et al, 1983; Gabora et al, 1993; Fischer, 1997), (ii) pro-acquittal (Fitzgerald and Ellsworth, 1984; Thompson et al, 1984), and (iii) no different (Simon, 1967; Cowan et al, 1984; La Free et al, 1985; Goodman et al, 1998).

[7] Studies finding no effect of seated female jurors include Garvey et. al (2004), Bridgeman and Marlowe (1979), Moran and Comfort (1982), and most recently, Lehman and Smith (2013).

Our primary empirical analysis is based on a comparison of verdicts from the full set of trials before and after the introduction of the reform. As highlighted above, this reduced form approach focuses on the margin of the basic inclusion (or not) of female jurors and avoids the potential problem of pre-trial jury selection by not explicitly conditioning on the number of seated female jurors in the post-reform period.[8] The estimated effect encompasses many potential mechanisms, the most obvious of which is that eligibility leads directly to female representation on juries, thereby affecting deliberations and decisions. But the reform may also indirectly affect verdicts even in cases in which females are not seated on the jury. This could occur, for instance, if the judge makes comments to the entire jury pool about whether the nature of the case is 'sensitive' for female ears or if pre-trial selection is focused on striking potential female jurors, which could affect the resulting distribution of seated male jurors.

While the Sex Disqualification Act was passed in 1919, an examination of the data indicates that the reform was not implemented until 1921; this was because it took a year to write the rules governing the inclusion of female jurors and to update the jury books from which the pools were drawn. We find that more than half of seated juries in 1921 had at least one female juror and that more than 80 percent of cases in the following years had female jurors; there were no females seated prior to 1921. In addition, though males clearly still dominated the jury, more than 20 percent of the post-reform juries had at least three female jurors (25 percent of seated jurors). Our analysis of seated juries also indicates that in the post-reform period, there was some 'selection' of females according to case characteristics, especially for sex offenses. This kind of differential pre-trial selection raises the kinds of concerns that motivated our emphasis on pre-post reform comparisons in the first place.

Because our main results are based on comparing outcomes before and after the reform, it is important to assess whether other changes may have occurred regarding crime or the criminal justice system at the time of the reform. While we have not identified any concurrent policy changes,[9] there does appear to be a small change in the distribution of crimes for which defendants were charged following the reform, at least in the Second Court. As a result, we condition our entire analysis on a large and detailed set of controls for criminal charges and other case characteristics. A related possibility is that changes in plea behavior altered the set of cases that reached a jury trial following the reform. Conditional on the set of criminal

[8] In contrast, this margin is easier to study in modern day data in the context of race, for instance, given that many juries and jury pools do not include any black members (e.g., Anwar, Bayer, and Hjalmarsson (2012)).
[9] The Representation of the People Act of 1918 made females over age 30 eligible to vote. This right was extended to females between ages 21 and 30 by the Representation of the People (Equal Franchise) Act of 1928.

charges, however, there is no evidence of any change in plea behavior coinciding with the reform.

Our analysis yields a number of key findings, which are robust to the inclusion of a full set of controls for crime and case characteristics, the consideration of alternative post-reform windows, and alternative dependent variables. First, bundling all cases together, the reform did not have a significant impact on the likelihood of conviction. This overall result, however, masks several countervailing findings for specific crime categories. In particular, female representation on juries significantly increased conviction rates for sex offense cases by 16 percentage points and decreased those for property and violent crimes by 10 and 13 percentage points, respectively. The magnitude of these effects is substantial, especially in light of the fact that males continued to constitute a large majority (more than 80 percent) of jurors post-reform. Interestingly, the decrease in conviction rates for violent crimes as a whole also conceals opposing results based on the gender of the victim. In fact, prior to the reform the conviction rate differential between male and female victim violent crime cases was essentially zero; post-reform this conviction differential between female and male victim cases increased to 20 percentage points.

We complement these pre-post design results with a secondary analysis that exploits the fact that seated juries were frequently used for multiple trials. In particular, we analyze the impact of seated female jurors for the subset of cases in which the jury was carried over from a previous trial. We expect issues involving the non-random seating of female jurors to be muted in these cases and, in fact, the gender composition of 'carried-over' juries is not correlated with the gender of the victim in violent crime cases. This analysis reveals a pattern of results that is remarkably consistent with the initial pre-post reform analysis, indicating that adding females to the seated jury sharply increased conviction rates for violent crime cases against women.[10]

Taken as a whole, the results of our analysis imply that female representation on juries substantially affects the likelihood of conviction for a subset of cases – sexual and violent crimes – in which female jurors might have viewed the alleged behavior or its impact on the victim from a different perspective than their male counterparts. While the present study is based on data from nearly a century ago, the magnitude of the results raises concerns about

[10] These results are broadly consistent with results reported for Sweden in Anwar, Bayer, and Hjalmarsson (2015), which finds that lay judges (nämndemän) from the feminist Vänster party are significantly more likely to convict defendants when the victim of violent crime is female. Criminal trials in Sweden are decided by a judge and three nämndemän, who are nominated by political parties in proportion to the party's performance in the most recent election.

the basic fairness and effectiveness of modern criminal justice systems that exclude (or severely limit the role of) women. Such concerns are not limited to societies that completely exclude women from serving as judges and jurors but extend to courts or institutions, such as the military, where the historical overrepresentation of men in positions of authority continues to limit the role of women in judicial decision-making.[11]

The remainder of the paper proceeds as follows. Section 2 provides institutional details regarding the English jury, and the context of the female jury reforms. Section 3 describes the data and Section 4 presents an analysis of the first stage impact of the reform on the gender of seated jurors and the selection of jurors. Section 5 presents the basic pre-post design and findings, as well as robustness tests and an analysis of whether seated jurors are driving the results. Section 6 concludes.

2. Institutional Background

2.1. The English Jury

The origins of the English jury date back to the Magna Carta in 1215, which provided the first set of rules to apply throughout the land. It states "No free man shall be seized or imprisoned or stripped of his rights or possessions, or outlawed or exiled, or deprived of his standing in any other way ... except by the lawful judgment of his equals or by the law of the land." Clearly, the implementation of this law has changed significantly with the passage of time.[12]

The cases analyzed in the current study are drawn from London and the surrounding suburbs in the 1920s. The rules in place during the 1920s differ significantly from those in place today due, in particular, to the Juries Act of 1974, which sets out the rules governing the modern English jury system. Most notably, the 1974 Act changed the eligibility requirements to serve as a juror from ones that were based on wealth to being a registered voter.

The following is a brief overview of the jury system during our study period, which was based on the Juries Act of 1825. To qualify for jury service one had to be: male, between the age of 21 and 60, residing in England, and a holder of land or wealth of various thresholds.[13]

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[11] The male-dominated command structure of the US military, for example, has long been viewed as a major factor in the extraordinarily low rate of successful prosecutions of sexual assaults. The Department of Defense (2013, 2014) estimates that approximately 5,000 of 25,000 incidents of sexual assaults were reported in 2013 and, of these, only 375 (less than 2 percent of all sexual assaults and 8 percent of those reported) were successfully prosecuted.

[12] See Cockburn and Green (1988) for a detailed history of the criminal trial jury in England dating back to the year 1200 and Bentley (1998) for a more focused discussion of the English criminal justice system in the nineteenth century.

[13] More specifically, a juror should: (i) possess an income of 10 pounds per year from real estate or rent charge, or (ii) 20 pounds per year from a leasehold of not less than 21 years, or (iii) should be a householder living in

Explicitly disqualified were foreign aliens and justices of the peace, and Westminster inhabitants could not be jurors for cases from Middlesex (the suburbs of London).

A master list of qualified men was to be created annually in each parish by the Churchwardens and Overseers of the parish. The list indicated each individual's name as well as some additional information (e.g. Title, Business, and Nature of Qualification) and was to be posted on church doors for the first three Sundays of September in each year to give parish members the chance to object. The final list was kept by the Clerk of Peace and copied into a Book to be delivered to the Sheriff, titled "The Jurors Book for the Year XXXX". This master list was brought into use on January 1 of the following year, and used for one year.[14]

During the time period studied, the London courts held monthly sessions, which typically lasted for two to three weeks. Jurors received a summons ten days before the required day of attendance. Unfortunately, little is known about how the pool of summoned jurors was selected from the jury book (Langbein, 1987). But, the Jury Act does include guidelines on how an empaneled jury is selected; the first 12 randomly drawn men who have appeared and are not struck for cause form the jury. An individual is struck for cause if there is a clear reason they cannot be impartial, such as knowing the defendant or victim, or if they were not qualified to be in the jury book in the first place. The 1825 Jury Act even describes how to implement randomness. 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."

If the original jury does not reach a verdict, the guidelines indicate that a new jury should be drawn, without returning the original juror cards to the box. Interestingly, the same jury, if not objected to, could try several issues (cases) in succession without being redrawn. In practice, this seems to have occurred quite often.[15] Repeated use of the same jury would potentially limit the degree to which seated jury and case characteristics are related, a problem prevalent in archival analyses of modern day data, particularly in the United States. Similarly,

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premises rated no less than 20 pounds per year (30 pounds in London and Middlesex), or (iv) should occupy a house with no fewer than 15 windows.

[14] Unfortunately, to the best of our knowledge, these juror books have not survived.

[15] This is observed both in our data from the 1920s and even more so in the Proceedings of the Old Bailey, which includes information on the juries trying cases at the Old Bailey from approximately 1750 to 1850.

if there were any men on the original jury who both parties consented to withdraw, or who was justly challenged by the court, these names could be set aside and the case tried with the residual of the original jury and new jurors were drawn from the box.

Finally, the Sheriff maintained records of who had served on juries. Depending on the court and jurisdiction, individuals should not have had to serve again within specified time periods. In practice, however, it is common to see the same men repeatedly serving as jurors.

2.2. The History of Female Jurors

Historically, only men in England and the United States (whose jury system has its origins in English common law) were qualified to be jurors.[16] To justify the exclusion of females from jury service in the 18th and 19th centuries, it was argued that (i) most women worked domestically and lacked the worldly experience necessary to make informed decisions as jurors and (ii) women were too 'delicate' and would be made impure by jury service (Fowler, 2005). As women's rights movements gained momentum, arguments in favor of female jurors suggested that female jurors would bring their 'domestic virtue' into the courtroom to have a positive influence, increase the representativeness of the community on the jury, and bring a unique perspective into deliberations (Fowler, 2005). Faced with their first case on the exclusion of female jurors, the 1946 Supreme Court decision in *Ballard v. United States* conveyed the idea that each sex contributes something unique to jury deliberations.[17]

In England, the Sex Disqualification (Removal) Act 1919, which was passed into law on December 23, 1919, enabled women to enter both the legal profession and the civil service and to become jurors. In a broad opening statement it specified that:

> "A person shall not be disqualified by sex or marriage from the exercise of any public function, or from being appointed to or holding any civil or judicial office or post, or from entering or assuming or carrying on any civil profession or vocation, or for admission to any incorporated society,…, and a person shall not be exempted by sex or marriage from the liability to serve as a juror".

In practice, however, females did not begin serving on until January 1921. Correspondence preserved in the National Archives highlights the many practical issues related to

[16] An exception to this occurred when female defendants sentenced to death pled the belly; in this case, a jury of matrons would examine the women to assess the validity of her claim.

[17] The decision states "The truth is that the two sexes are not fungible; a community made up exclusively of one is different than a community composed of both; the subtle interplay of influence one on the other is among the imponderables. To insulate the courtroom from either may not in a given case make an iota of difference. Yet a flavor, a distinct quality is lost if either sex is excluded. The exclusion of one may indeed make the jury less representative of the community than would be true if an economic or racial group were excluded."

implementing this Act, including which females should be in the jury pool, which females could apply for exemption, how the jury would be selected, and whether defendants can complain about the gender of the jury. The statutory rules governing female jurors were passed in October 1920, and had the following general provisions[18]: (i) juror books should be updated to include eligible women, (ii) jury summons should be independent of gender, (iii) the number of women on any panel of jurors should be in (approximately) the same proportion as the number in the jurors' book from which the panel is drawn, (iv) a summoned female can apply for exemption based on pregnancy or other feminine condition, (v) the Under-sheriff has the discretion to exempt any summoned woman with appropriate evidence of pregnancy or other feminine conditions, and finally, (vi) the possibility of all female or all male juries was preserved. The judge could at his discretion (or after a request by the prosecution or defense) require that a jury be all male, all female, or exempt females from service based on the sensitivity of the case.

Newspapers articles shed some light on the attitudes towards this dramatic change. According to a 1920 New York Times article, the prosecutor congratulated the women for "at last taking their proper place in the administration of justice in England."[19] From the viewpoint of the female jurors, The Manchester Guardian (1921) quotes one as saying "that the presence of women on juries would result in greater care being taken in cases where women were concerned. Some cases would be very unpleasant, but men had not shirked their duties and women must equally show a public spirit." The article also notes that the London jurywomen "seemed to have been drawn from all classes, and they varied widely in age."

Debate regarding both the suitability and potential impact of female jurors continued over the following years. Termed the "great experiment" in some newspaper headlines, opinions regarding female jurors were greatly varied.[20] A 1926 Daily Express opinion column asked: "Is the woman juror inclined to confuse the issues by the introduction of sympathetic or antipathetic considerations? … Can woman, in short, suddenly divorce herself from temperamental inconsequence and from the compassionate and emotional instincts on which she often acts, and become in a moment a hard, matter-of-fact analytical administrator of the strict letter of the law?" In response to such comments, Sir Robert Wallace, is quoted as

[18] These correspondence are preserved in National Archive document LCO2/559, "Rules of the Supreme Court (Women Jurors) 1920 made in pursuance of the Sex Disqualification Act 1919".
[19] According to a New York Times article, the first cases in England to be heard by female jurors occurred on July 28, 1920, when six women formed part of the jury in the Bristol Quarter Sessions. The article goes on to describe how the women heard six cases tried, but at the close of the proceedings, two of the women asked to be excused from further service to care for their children. Two other women replaced them on the jury.
[20] These newspaper articles, collected by the Home Office, are at the National Archives under HO 45/13321.

saying that he "found women excellent as jurors, and has no complaints against them." In terms of the impact of female jurors on case outcomes, an early assessment in April 1921 by the counsel to the Treasury at the Central Criminal Court found that mixed gender juries return the same verdict as a jury of all men (Times, 1921). Five years later, opinions may have begun to change. One 1926 newspaper article quotes a court Recorder as saying the female jurors are harsher than men: "I venture to think that a man is safer in the hands of the Bench than he would be in the hands of a present-day jury. One finds the fair sex far more severe than the old-fashioned jury."

At the same time that females qualified to be jurors in England, similar movements were taking off around the United States. (See McCammon, 2012 and Fowler, 2005 for detailed discussions of the US women's jury movements.) Utah became the first state in 1898 to authorize women to participate on juries[21], but by the time of the Second World War, twenty-one states still prohibited female jurors. And three states (Alabama, Mississippi, and South Carolina) banned female jurors as late as 1962. Appendix Figure 1 demonstrates the evolvement of female jury laws in the United States. While there is much variation in the timing of such laws across states, there is limited data in the US that measures the appropriate outcome (ideally jury convictions) during this time period or that accurately measure the extent to which (and how quickly) these laws were put into effect.

3. Data

3.1. Data Description

We collected the data for this project from the Central Criminal Court: Court Books, which are housed in England's National Archives. These books are original hand-written documents by court reporters that have summary details for each trial, including the date(s), judge name, number of charges, the main offences, plea, verdict, sentence, juror names, defendant names, and typically victim names. We obtained photocopies of the complete Court Books for the First Court (from October 1917 to October 1926) and the Second Court (from January 1919 to January 1925); this amounted to about 3500 pages of case summaries from 193 court sessions. See Appendix Figure 2 for a photograph of an example of a jury trial in Court 1. We manually transcribed these records into a defendant by case level data set, including all cases for which there was a seated jury. For the first court, we also coded cases in which the defendant confessed and there was no seated jury. For cases with multiple defendants, we keep track of

[21] However, according to McCammon (2012), women in Utah rarely served until the 1930s.

both the case number and the defendants' order; generally, the charges associated with the first defendant are more serious than those associated with the last. This resulted in a data set of 3,335 defendant-case observations (1080 jury trials and 1187 confessions in the First Court and 1068 jury trials in the Second Court).

From this raw data, we create a number of variables to characterize the defendant and case, as well as the jury. With respect to gender, the focus of this paper, we identify juror gender, victim gender, and defendant gender on the basis of names. We focus on first names, but in instances when the gender is ambiguous, we rely on middle names, which are almost always provided. For victim gender, when there is no name provided, we code it as unknown or none (meaning no person), but it could be the government or a firm that was the victim.

We then use the gender-classified juror names to identify the number of females seated on each twelve-person jury; unfortunately, the only thing we know about seated jurors is their names. However, we do create a jury identifier to keep track of whether the same jury tried multiple cases: the court documents often say "same jury sworn". In fact, we observe 1,047 unique juries in the data, but more than 2000 defendant-trial observations.

We identify the total number of charges and classify each offense (of which there were more than 100 detailed offense types listed) into one of 15 broad categories: killing (manslaughter, murder, and infanticide), aggravated assault, arson, robbery, burglary, rape, theft, bigamy (that is, the act of marrying while already married), deception, offenses against the king, other offenses against persons, other property offenses, other sex offenses, conspiracy, and all other offenses.[22] Attempts (and assisting) to commit these offenses are included in the same categories. We total the number of (known) charges in each broad category, and create dummies indicating whether the defendant is charged with at least one offense in each category. While the court records almost always indicate the total number of charges, and the types of offenses for the first charge, the specific offenses of the later (typically less serious) charges are not always listed. We also identify whether the offense was against a child, and whether the defendant was charged with having a criminal history.

[22] Some offense categories include multiple sub-categories. Killing includes Murder, manslaughter, child murder, and infanticide. Rape includes charges of rape as well as indecent assault. Burglary includes burglary as well as having the tools for burglary. Theft includes autotheft, theft, postal theft, larceny, blackmail, embezzlement, bribery, shoplifting, and extortion. The main 'other property offense' is receiving stolen goods. 'Other offenses against persons' include assault, threatening behavior, kidnapping, and libel. Other_sex_offenses include buggery, pornography, possession of indecent print, sending indecent cards, indecent exposure, gross indecency, incest, procurement, and prostitution. Deception primarily includes forgery, fraud, and perjury. Offenses against the king include coining, tax fraud, seditious speech, treason, trading with the enemy, blasphemous libel, communicating secret information, conspiracy against the military, and uttering. Finally, the all other offense category includes offenses such as abortion, child neglect, suicide, concealing birth, and firearms.

We also code the verdict for each offense: whether the defendant confessed (in which case there is no jury verdict) or whether the case was *put* to the jury. In most jury trials, the jury returned a verdict of guilty or not guilty. However, we also note those cases in which a jury was seated but never delivered a verdict, either because it was a hung jury, a directed verdict, or a confession/plea in the middle of trial. For cases with a jury verdict, we identify whether the first offense (generally the most serious offense) resulted in conviction as well as the share of total charges facing the jury that resulted in conviction.

Additional variables that we code are the length of the trial in days, which is suggestive of the quality of evidence and the seriousness of the case, and whether the defendant was undefended. Attorney names were typically listed in the margins, but in most instances, they were too hard to read to confidently code; 'undefended' was clearly indicated however. Though juries only decided verdicts, we also code the sentences associated with each case. The most common sentences include prison (with or without hard labor), penal servitude (which was also a form of imprisonment with hard labor), and death.

A close reading of these historical court records also provides some data that can be used in a more qualitative manner to understand the jury system at the time and the role of female jurors. At the beginning of many court sessions, for instance, a list of jurors who are fined (for not showing up at court) was provided, including the juror name and profession. Females do not show up in these lists very often (just 17 instances): 75% of those that do are characterized as married, widowed, a spinster, a gentlewoman, and a householder. The remaining are an artist, a boarding house keeper, a mathematical coach, and a shipping agent. In contrast, male professions range from professional (accountants, stockbrokers, engineers), to retailers, manufacturers, manual laborers, and artists; more than 10 percent, however, are classified as gentlemen. In addition, for a handful of cases (11), there was a note in the documents that the judge requested an all-male jury; it is possible, however, that such requests were not always noted in the records. Finally, we also see notes in the margins by the court reporter indicating if the jury was provided certificates for lunch or tea, and even hotel lodging, as well as whether the jury was exempted from future jury service beyond the normal amount. While we have no way of knowing whether this information was always noted by the court reporter, we code it as a way of possibly controlling for the most serious of cases.

3.2. Summary Statistics

Table 1 presents summary statistics for jury trials overall, and broken down by court. The statistics for offense categories reveal that 20 percent of defendants in jury trials have at least

one charge of killing, the most serious of offense categories. Five percent of defendants are charged with robbery while eight percent are charged with aggravated assault and eight percent with another offense against a person (i.e. that is violent in nature). Among property crimes, 10 percent are charged with burglary, 14 percent with theft, and 15 percent some other property crime. For sex offenses, six percent are charged with rape and nine percent with another sex offense. Finally, among those offenses that do not easily fall into the categories of violent, property, and sex offenses, three percent of defendants are charged with bigamy, 16 percent with deception, three percent with an offense against the king, and six percent with another offense. It is also worth noting that 14 percent are identified as having a criminal history and the average number of charges in total is 3.2.

The next panel of Table 1 reports statistics for defendant, victim and other case characteristics. 13 percent of defendants are female and there are, on average, 1.9 defendants per case. 25 percent of the cases are identified as having a female victim and 40 percent as having a male victim and 36 percent of the cases are classified as having no victim.[23] Four percent of the cases have a child victim and 21 percent of defendants are undefended. The average trial length is 1.9 days, although this does not mean that the trial actually took two complete days but rather that the court was in session, on average, on two dates.

Turning to case outcomes, of the 2,148 trials for which a jury was seated, three percent resulted in a hung jury on at least one offense and eight percent were dismissed by the judge for any reason (including a hung jury) before the jury made a verdict (it is unclear at what stage in the trial some of these cases were dismissed). For the cases in which the jury returned a verdict on at least one charge, 60 percent of defendants are found guilty on at least one charge, 45 percent of defendants are found guilty of all charges, and 49 percent are found guilty on the first (typically most serious) charge. The average share of charges on which a guilty verdict is returned is 53 percent.[24] For those found guilty, we ale report judge sentences: 63 percent are sentenced to prison, 20 percent to penal servitude, 4 percent to death, and 12 percent to some other punishment such as a fine or corporal punishment.

Table 1 also clearly demonstrates the differences between the First and Second Court. Broadly speaking, cases in the first court are more serious and can be eligible for capital

[23] This classification is difficult from a modern viewpoint, when considering offenses like abortion or buggery. Also note that there are some missing observations for victim characteristics due to no names being listed in the court summaries or difficulties reading the names (or assigning gender).

[24] There is some variation in sample sizes across outcome variables depending on which charges were put to (and decided by) the jury and whether any information was omitted from the records. In addition, cases can fall into multiple classifications. At the most extreme, consider a hypothetical defendant charged with five offenses, to which he confesses to two. Three charges are put to the jury – the jury reaches a verdict for two. The jury may be dismissed from deciding the final charge (hung, are directed by the judge, or the defendant confesses.)

punishment. 39 percent of the cases in the First Court have at least one killing charge, compared to two percent in the Second Court. Likewise, rape and aggravated assault are more prevalent in the First Court, while the Second Court largely sees various types of property crimes. An examination of the sentencing data also indicates that the crimes in the First Court are more serious, and eligible for harsher punishment. Seven percent of the defendants receive a death sentence in the First Court compared to none in the Second Court. A higher share of defendants in the Second Court are sentenced to prison than the First Court (77% versus 53%) while a higher share in the First are sentenced to penal servitude, which is harsher than prison.

Conviction rates also differ across the two courts. For instance, 54 percent of defendants in the First Court are found guilty of all charges compared to 37 percent in the Second; likewise, 68 percent in the First are guilty of at least one charge compared to 53 percent in the Second. These differences in conviction rates likely reflect differing offense distributions, and the quality and nature of evidence for various offenses. Other differences across courts include a higher share of undefended cases in the less serious Second Court, and differing defendant/victim characteristics. The latter is unsurprising, given the different offenses.

4. The 'First Stage' Impact of the Reform

4.1. The Effect of the Reform on the Presence of Females on the Jury

This section characterizes the implementation of the reform. We begin by examining when females were actually seated on juries in Figure 1, which presents annual data for all cases in which a jury was seated. The top panel considers all cases and presents the extensive margin (whether there were any females seated) on the left and the intensive margin (the number of females seated) on the right. Consistent with the historical documentation described in Section 2, no females were seated on London juries prior to 1921. But, in 1921, more than 60 percent of juries had at least one female and the average number of females seated was about 1.7 (note that this includes cases with zero females). The presence of females on the jury continued to increase over subsequent years so that by 1923 about 90 percent of cases had a female juror and the average number of female jurors was almost 2.5.

Table 2 quantifies the first stage impact of the reform, presenting regressions of whether any females were seated and the number of females seated on dummies for each post reform year, from 1921 to 1925; the 1925 dummy also includes a small sample of cases tried in 1926. The estimated impact of the reform on female representation on juries is independent of controlling for a large set of offense and case characteristics, as can be seen by comparing the even and odd numbered columns in Table 2. In the first year of the reform, the likelihood of

having a female juror increases by 64 percentage points and there is an average of 1.6 female jurors seated. It appears to have taken about three years to reach full implementation and a steady state level of female representation on jurors.

The second panel of Figure 1 assesses whether these 'first stage' effects of the reform on female representation were heterogeneous across the First and Second Courts. At both the extensive and intensive margins, the effects of the reform are comparable in the first year (1921) across courts. Approximately 60 percent of cases have at least one female seated and on average, there are about 1.6 females seated (including all male juries). While a secondary increase in the use of female jurors in the next two years is observable in both courts, it appears to occur faster in the Second Court. The presence of female jurors on the First Court holds about constant from 1921 to 1922 at both margins. In the Second Court, the share of cases with female jurors increases already to almost 80 percent in 1922 and the average number of seated jurors increases to 2.3 in 1922 and 2.6 in 1923.

The previous figures have only displayed the average number of seated females on the jury. An examination of the distribution of seated females (not shown) shows that more than 30 percent of cases in the first two post-reform years have no seated females. Almost 15 percent have one female, more than 20 percent have two females, and about 15 percent have three females. More than 10 percent of juries contain four or five seated females. Just a handful had more than five females, and there were never more than seven in any year. In the following years, there is a sharp increase in the share of juries with two females (30%), three females (almost 25%) and four females (about 12%).

We have thus far identified a number of key facts about the implementation of the reform. First, the reform was implemented in 1921 (despite the original act passing in 1919), and a female was seated on more than half of the juries in the first post-reform year. Second, the reform gained further traction in the two subsequent years, increasing the presence of female jurors. Finally, though males clearly still dominated the jury, more than 20 percent of the post-reform juries had at least three female jurors (25 percent of seated jurors).

4.2. Post-reform Selection of Female Jurors

The previous section demonstrated that the reform did indeed lead to females on juries. This section assesses whether females were potentially deselected on the basis of case characteristics, such as offense type or victim gender. This would be consistent with anecdotes from newspaper coverage at the time. A 1921 *Times* article describes a challenge by the defense against two female jurors on an indictment for an offense against a girl aged 11

and three jurywomen on another case of a similar nature. Another *Times* article highlights the 'practice of challenging women on juries in cases of a sexual character, in which young girls were concerned' while another quotes a judge who gave the female jurors the option of leaving (at any attorney request) on a case involving relations between two men.[25]

To begin to get at these questions, the third panel of Figure 1 breaks out cases according to whether they had at least one female victim, male victim or no easily identifiable victims. The same general pattern in the use of female jurors is observed across each subcategory. However, in every year, and at both margins, we see a stronger presence of female jurors for male victim cases compared to female victim cases and for female victim cases compared to no victim cases. These gaps are quite small, however, and not necessarily significant. Decomposing all post-reform cases by victim gender reveals that female jurors are seated more often for cases involving male versus female victims (84 versus 79 percent) – see Appendix Table 1.[26]

Finally, the last panel of Figure 1 classifies the cases according to whether the first charge is a violent offense, property crime, sex offense, or other offense. Though the same general pattern is again seen across all cases, it also appears that a female presence on the jury was strongest for property crimes and weakest for sex offenses. In fact, for sex offenses, less than 20 percent of the cases had a female juror in the first year. When looking across crime categories over the entire post-reform period in Appendix Table 1, we see that at least 75 percent of trials have female jury representation, with an average number of females seated of 1.8 or more. The only exceptions are sex offenses: for cases with at least one rape charge, just 56 percent of trials have a female juror, and the average number seated is 1.6. The presence of females is even lower for the other sex offense category, which includes offenses such as buggery, incest, pornography, indecent exposure: just 27 percent of cases have a female juror and the average number of female jurors is 0.7. This is clear evidence that females are deselected from sex offense cases. However, as can be seen in Figure 1, by 1923, the presence of females on sex offense juries was comparable to that for other offense types.

Appendix Table 2 presents regressions of whether any females are seated on a court dummy, offense characteristics, victim and defendant characteristics, and other case

[25] An article titled "Women Jurors in Unpleasant Cases" quoted the judge: "The question at issue in this case involves relations between two men. It will involve sexual points of the gravest indelicacy – questions which even men would hesitate to discuss amongst themselves. If the ladies of the jury, or any one of them, desires to remain in that body so be it, but if the ladies prefer that their places be taken by men I will assent to their request." One of three female jurors left the court.

[26] Likewise, Appendix Table 1 demonstrates females are more often seated on cases with a female versus male defendant (85 versus 77 percent).

characteristics. Even in this multivariate regression framework, there is significant selection of females based on offense type. Not surprisingly, females are less likely to be seated on rape and other sex offense cases; the rape relationship is only seen during the first three years. Having controlled for case characteristics (namely offense type), however, female jurors are not seated differentially on female versus male victim cases. However, there is some evidence that females are significantly less likely to be seated on 'no victim' cases. Again, it is important to note that 'no victim' cases are quite heterogeneous in nature, and can include offenses against firms or the government rather than people, or offenses such as buggery which in many instances would be seen as victimless in modern definitions. There is no significant effect of defendant gender on the female presence on the jury.

The results in this section strongly imply that, post-reform, the cases in which female jurors were seated on are likely different in unobservable ways from the cases in which they were not seated on. This suggests that a research design that exploits the post-reform variation in female seated jurors is likely to yield biased results. This motivates our use of a reduced form pre-post research design which is described in detail in the following section.

5. Estimating the Effect of the Reform on Case Outcomes

5.1. Methodology/Identification Strategy

This section presents the results from a pre-post design that compares conviction rates by juries before and after The Sex (Disqualification) Act of 1919 in England, which changed the gender composition of the jury pool. Specifically, our research design will compare conviction rates prior to the implementation of the law in 1921 (when there were no female jurors in the pool) to conviction rates from 1921 on, when female jurors were regularly seen in the pool and seated. The research setting we exploit allows us to study the role of female jurors using real world data in a way that overcomes two fundamental challenges that has arisen in prior research that identified this effect by examining the impact the variation in the gender composition of the seated jury had on trial outcomes.

First, seated juries in the US and UK (though to a lesser extent) result from a de-selection process that strikes potential jurors on the basis of observable (and unobservable) characteristics. Even though one can no longer legally strike jurors on the basis of gender, there may be other justifications that are in fact correlated with gender. Thus, seated juries are not random and the characteristics of the jury can conceivably be related to unobserved case factors such as the amount of evidence present. As a result, a causal interpretation cannot reliably be attached to the results of studies that only consider the variation in seated jury

composition on trial outcomes. In contrast, our research design exploits exogenous variation in the gender composition of the seated jury that came about due to a law change.[27]

A second issue that arises in prior research is that these studies focus solely on modern civilian criminal trial settings in the U.S., where juries are approximately fifty percent female. As a result, the variation that is observed in these studies amount to shifts in female representation in a reasonably narrow range around 50 percent. Endogeneity concerns aside, it is conceivable that one of the reasons the previous literature discussed in Section 1 found no real impact of female jurors is that increasing female jurors from 40% to 60% may not be the margin where female representation has much impact on trial outcomes. In particular, one might expect female representation to have a bigger impact when going from no females to some females, as just the presence of one female can potentially change the nature of the deliberations. The latter margin is extremely policy relevant as there are many current institutions, such as the U.S. military, where the jury selection procedures can severely limit the inclusion of female jurors. Our research setting is unique in that exploiting the implementation of this law allows us to identify the impact of female jurors precisely at this inclusion margin, and as such can identify a more policy relevant effect.[28]

The caveat to our research design is that it identifies the causal impact of the reform, which is not necessarily the impact of adding females to the seated jury. To see this, note that the reform can have two types of impacts on case outcomes. The first, which we term the direct effect, comes about through females being seated on juries. This effect can encompass the impact of a female's vote on the final conviction decision, as well as whether the presence of a female on the jury changes the nature of the deliberations (and thus impacts the votes of male jurors). However, the reform can also have an impact on cases where no females are seated on the jury. This indirect effect can arise for several reasons. First, the results in Section 4 imply there is some selection of female jurors on the basis of case characteristics, by attorneys and/or judges. If attention is focused on 'striking' female jurors, this could affect the distribution of male jurors actually seated. Second, judges might request an all-male jury because of the 'sensitive' nature of the case; such a signal about the case could potentially influence the deliberations and behavior of the seated male jurors. Finally, media coverage of the 'women juror experiment' could affect the behavior of males, even when women are not

[27] Another way prior literature has dealt with this endogeneity issue is to take advantage of the exogenous day to day variation in the jury pool (see Anwar, Bayer, and Hjalmarsson, 2012 and 2014). This approach is not used here as we do not observe the composition of the jury pool.

[28] To our knowledge, there is no data available that would allow one to study a modern setting (such as the U.S. military) where the key variation of women in the jury pool is none to a few.

present on the jury. For all of these reasons, it is important to keep in mind that our primary analysis estimates the reduced form effect of the policy rather than the causal effect of seated female jurors on case outcomes per se.

Because we will be identifying the causal impact of the reform using a simple pre-post design (without control groups), it is important to consider the time windows that make this design the most accurate. The further observations get from the date of implementation, the less clean the design becomes, as other things change in society, the economy, and the justice system. On the other hand, the further away we get from implementation, the more observations that are available, potentially increasing the precision of the analysis. In an effort to balance these two issues, we focus on a 3-year window around the reform in our baseline specifications, thereby comparing outcomes in the first three years after the reform (1921 to 1923) to outcomes three years before the reform (1918 to 1920). However, to increase the precision of the other parameter estimates, sample observations for all years (1918 to 1926) are included in the analysis, as well as a dummy variable to capture this additional post-reform period. We chose a 3-year window as our baseline because the reform is fully implemented by this time, and it is symmetric with the amount of pre-reform data available. We demonstrate, in addition, that the main results are not sensitive to our choice of window.[29]

The equation below presents the baseline specification, where the dependent variable is some measure of whether defendant *i* on case *j* tried on date *t* is convicted by the jury:

$$(1)\quad Guilty_{ijt} = \propto + \beta_0 PostRef3_{ijt} + \beta_1 PostRef4plus_{ijt} + Court2_{jt} + Offense_{ijt}\pi + X_{ijt}\delta + \epsilon_{ijt}$$

The primary dependent variable is whether the defendant is convicted of at least one offense put to the jury; robustness checks consider the share of convictions, convictions on the first offense, and whether convicted on all charges. *PostRef3* is a dummy for the first three years after the reform, while *PostRef4plus* captures all additional post reform years. β_0 provides the cleanest estimates of the reduced form effect of the reform. Given that the types of cases are different across courts, all specifications include a court dummy. *Offense* is a vector of 15 dummy variables characterizing the broad offense categories of the charges. *X* is a vector of

[29] The main findings are also robust to starting the data in 1919, after the end of the First World War. During the war, anecdotal evidence suggests that the jury pool was different and that selection into the pool did not strictly adhere to regulations.

case characteristics, including victim gender, defendant gender, number of charges, number of defendants, defendant ranking order, and child victim.

5.2. Identification Concerns

Given that we are estimating the pre-post impact of the reform on conviction rates, it is important to ask whether anything else changed at the time of the reform, which would contaminate the pre-post design. Table 3 takes a simple first look at this issue by comparing the sample characteristics for jury trials before and after the reform. In the First Court, we see little change in terms of the offense distribution and defendant/case characteristics. In the Second Court, there are a number of statistically significant differences in case characteristics, although these changes are generally small in magnitude. In the regressions that follow, we control directly for all observable offense and case characteristics but it is important to point out that we cannot rule out the possibility that some omitted case characteristics changed at the time of the reform.

While the differences in the types of offenses seen after the reform (in the Second Court) could be due to a change in the types of offenses committed (or emphasized by the justice system) at this time, it could also be that a change in plea behavior results in a different composition of cases reaching a jury trial. If attorneys have perceptions about female jurors' likelihood to convict, then defendants may be more or less likely to plea after the reform. This could result in a change in the composition of the cases actually brought to trial, thereby affecting the average likelihood of conviction. To assess whether this is a concern, we examine the First Court jury trial and plea data.[30]

Table 4 considers whether there is a change in plea behavior by regressing whether the defendant pled guilty to all charges on our three-year treatment window dummies. When not controlling for anything else, column (1) demonstrates that defendants are 12 percentage points less likely to plea after the reform. But when just controlling for a vector of offense types in column (2), the post-reform plea effect goes to zero, suggesting that conditional on the type of offense charged, there is no change in the likelihood to plea as a result of the reform. In columns (3) – (6), we also see that there is no significant effect on the likelihood of pleading when decomposing the data into broad offense categories: violent, property, sex, and other. Results not shown also indicate that there is no change in the likelihood of pleading

[30] Generally, plea cases are more minor; defendants tend to be charged with property rather than violent offenses, they do not/cannot result in a death sentence, and there are fewer total charges. A larger share of pleas are made by defendants that are undefended (57 percent) compared to trial defendants (16 percent).

after the reform for female victim or female defendant cases. The fact that a defendant's decision to go to trial did not change in response to females being added to the jury pool suggests that one can look at conviction rates to identify the true impact of the reform, rather than accounting for decisions at earlier stages of the judicial process.

5.3. Pre-Post Reduced Form Results

The above discussions indicate the importance of conditioning the analysis on offense type. Thus, our baseline specification includes the vector of offense categories, as well as a court indicator (which to some extent also captures the type of offense). Table 5 begins in column (1) by looking at the effect of the reform on the likelihood of being convicted of at least one offense; the baseline controls are included. There is no observable impact of the reform on the likelihood of conviction when aggregating all cases together.[31] Columns (2) – (5) of Table 5 look at the effect of the reform by broad offense categories, with the baseline set of controls. The full set of controls, including victim and defendant characteristics and proxies for the quality of evidence and case severity, are included in columns (6) – (10). While there is no effect of the reform on overall conviction rates, there is evidence that the reform had a significant positive impact on the likelihood of conviction for sex offense cases as estimated in specifications with and without the full set of controls. Sex offense cases (i.e. cases where at least one charge is a rape or other sex offense) tried after the reform are about 17 percentage points more likely to result in conviction; given that just 46 percent of such cases result in conviction, exposure to the reform increases the likelihood of conviction in a sex offense case by 37 percent. The effect of the reform on conviction for other offense categories is, if anything, negative. When case characteristic controls are included in the specification, the reductions in the chance of conviction for violent and property offenses are statistically significant and sizeable in magnitude, 10 and 13 percentage points, respectively.

Table 6 demonstrates the robustness of the sex offense findings to alternative dependent variables (the share of charges resulting in conviction, conviction of all charges, and conviction on the first, most serious charge) and post-reform treatment windows (two, four, and five years). The effect decreases in size with alternative windows, but is still quite large: depending on the window, exposure to the reform increases the chance of conviction of a sex offense by 12-17 percentage points. Despite reduced sample sizes, this robust finding for sex

[31] Appendix Table 3 demonstrates that these results (or lack thereof) are seen in both our baseline three-year window and when defining the window as two or four years, as well as in both courts. These results are also not sensitive to including month, defendant and victim characteristics, number of charges, and proxies for quality of evidence and severity of case – trial length, trial breaks, and undefended.

offenses is still seen (at standard significance levels) when looking separately at the first versus second courts, and rape versus other sex offense cases. However, similar tables (not shown) for property offenses do not tell as robust a story. The reduction in the conviction rate for property offenses appears to be driven by the third year after the reform, and is only visible with the full set of controls. The driving factor responsible for the observed reduction in conviction rates for violent crimes is discussed in detail below.

Given that previous work has shown that the impact of jurors can depend on victim gender (Anwar, Bayer, and Hjalmarsson, 2015), Table 7 looks directly at whether there was a differential effect of the reform on convictions for cases with at least one female victim. We restrict this analysis to violent offenses, as the classification of victim gender is the most straightforward for these cases: almost all violent offenses have a male or female victim. We present the results of estimating equation (1) with a three-year post reform window, and include an interaction of whether there is a female victim with all post-reform dummies. The first five columns present the three-year window results for alternative dependent variables and controls.[32] Columns (6)-(8) demonstrate robustness to alternative post-reform windows. Regardless of the specification, the impact of the reform on the conviction rate differential between male and female victim cases is significant and large in magnitude. The specification in column (2) implies that prior to the reform there was essentially no difference in the conviction rates between violent crime cases with female victims and those with male victims. Post reform, this conviction rate differential dramatically increased such that cases with female victims were now 20 percentage points more likely to be convicted than those with male victims. Thus the reform had a significant impact on the way male versus female victim cases were treated.[33,34]

Is it really the female jury reform that is underlying the sex offense effects and the male/female victim conviction differential effects? As the analysis relies on a simple pre-post

[32] All specifications control, however, for offense types and basic offense characteristics; this is important given the variation in offense type across violent offenses with male versus female victims. A substantially higher share of female victim violent offenses are for killing than for male victims (75 versus 46 percent) while males are more likely to be a victim of a robbery (18 versus 7 percent of cases). The point estimates are also fairly robust, albeit a little less precise, to including interactions between the full set of controls and the post reform dummy; it is also worth noting that these additional interactions are generally not significant and small relative to the female victim interaction.

[33] Note that the increase in the conviction differential between male and female victim is primarily achieved by conviction rates for male victim cases decreasing after the reform. This explains why the overall impact on violent crime conviction rates (regardless of victim gender) shown in Table 5 is negative.

[34] Similar results (not shown) are also seen when looking separately by court. In addition, these results are driven by violent offenses with male defendants; there are simply not enough female defendants – violent crime cases to conclude anything substantial (there are only 90 trials over the entire period). Similar data limitations are seen when considering female defendants overall or in other crime categories: the results are too noisy to draw any definitive conclusions one way or the other.

design, we could potentially find the same effects, even if the reform has no impact, if society in general is becoming more and more sympathetic to female rights.[35] To rule out this explanation of the results, we examine the pre-reform trends in conviction rates for sex offenses and violent offenses in Figure 2. The upper panel present the average annual share of charges convicted for cases with any violent offenses, broken down by victim gender. Annual violent offense conviction rates are not trending one way or the other before the reform. More importantly, prior to the reform, conviction rates for violent offenses with male and female victims are moving together, while they diverge after the reform. Likewise, the bottom panel of Figure 2 shows that there is no substantive upward trend in sex offense convictions prior to the reform; in fact, sex offense conviction rates are quite flat. Consistent with these figures, falsification tests, which use just the pre-reform sample and impose a hypothetical reform date in July 1919, do not find significant evidence of the 'hypothetical' reform on conviction rates; thus it appears to be the actual reform driving the results and not underlying societal trends.

Finally, Table 8 considers whether there is an effect on outcomes other than convictions. The previous conviction analyses were based on the sample of trials for which the jury reached a verdict. Table 8 examines the sample of all cases for which a jury was seated, and tests whether there is a post reform effect on the likelihood of having a jury dismissed for any reason, including a hung jury, discharged on at least one charge (typically convicted of other charges), and dismissed on select charges when defendant pleas to other charges after jury selection. Column (1) demonstrates that, overall, there is an increase in the likelihood of a jury being dismissed for any reason by 2.7 percentage points; given that only eight percent of seated juries are dismissed on at least one charge, this is a substantial increase. The effect is seen regardless of the post-reform window used, though it appears to get substantially larger four to five years after the reform (results available upon request). The overall effect in the three-year window appears to be driven by property offenses and other offenses, though as the window is expanded, the same effect is also seen for violent offenses. Such an effect is never seen for sex offenses. Columns (2) – (4) decompose the dependent variable according to the reason for dismissal: hung, discharged on some charges (but reach verdict on other charges), and dismissed due to confession on other charges. Generally, there is no significant effect on the likelihood of a hung jury. It should be noted, however, that the point estimate for sex offenses is quite large (though imprecise); sex offense cases are more than three percentage points less likely to result in a hung jury after the reform. Most of the

[35] At this time, there was increased acceptance of the female rights movement. For instance, females over age 30 (with property qualifications) were given the right to vote in 1918; this was extended to women over 21 in 1928.

overall effect appears to be driven by juries being discharged on at least one charge (while typically reaching a verdict (usually guilty) on other charges). A significant effect is seen in column (3) overall and for property offenses, sex offenses, and other offenses, while the point estimate for violent offenses is large but not quite significant.

One possible explanation is that having reached a verdict on some charges, the other charges are dismissed to avoid a lengthy deliberation. Consistent with such an explanation, we find that trials became significantly longer after the reform. As seen in Appendix Table 4, trials increased in length (including deliberation) by approximately 0.15 to 0.3 days (or 8-15 percent, compared to the average length of 1.9 days), depending on the specification. This could have occurred, for instance, if females were less familiar with the laws or more careful in evaluating evidence, or increased the diversity of perspectives within juries.

5.4. The Effect of Seated Female Jurors

As discussed in detail above, our focus on the pre-post reform analysis is driven by concerns that the pre-trial process of seating juries is not random and may lead, therefore, to spurious correlation between the gender composition of the jury and conviction rates. As shown in the first column of Appendix Table 1, concerns about jury selection appear to be especially warranted for some crime categories (e.g., rape and other sex offenses) where the propensity for female jurors to be seated is substantially lower than for other crime categories. While avoiding issues related to non-random jury selection, a disadvantage of the pre-post design is that the identified effect includes both the direct effect of seating females on the jury and any indirect effects associated with how pre-trial selection is conducted in the presence of female jurors. Perhaps more importantly, the pre-post design also does not allow us to say anything about the effect of each additional seated female on the jury.

This section seeks to look directly at the relationship between the number of seated female jurors and verdicts. To this end, we step away from our pre-post analysis and focus instead on just the post-reform period; specifically, we estimate regressions of the case outcome (e.g. share of charges convicted) on the number of females seated on the jury. This analysis has the advantage of utilizing case-by-case variation in the gender composition of the jury (due to day to day variation in the composition of the pool), rather than limiting the variation to that due to the pre-post change in the master jury books.

The primary concern with exploiting variation in the seated jury is that females might be seated on cases that are distinctly different from the cases where they are not seated (but are in the pool). We take two steps to limit such concerns, and facilitate a causal interpretation of

our results. First, we focus on the violent crimes – female victim findings, as female victims do not predict whether females are seated on the jury overall and for the subsample of violent crimes – see Appendix Table 1 – and purposely do not focus on crimes, such as sex offenses, where there is much stronger evidence of gender differences in jury selection. Second, we restrict our sample to cases in which the jury was seated for more than one trial; arguably, there should be less selection on specific case characteristics when the same jury is carried over from a previous trial. This restriction results in a sample of 235 violent crime trials (130 unique juries) with complete verdict and victim gender information; 14% of these cases have zero females seated, 20% have one female, 26% have two females, and the remainder has three or more.

Results are presented in Table 9. Columns (1)-(4) of Table 9 examine the effect of the number of seated female jurors on various measures of conviction (any, share, 1st offense), and includes an interaction between the number of seated females and whether the victim is female. The results are quite robust across alternative dependent variables, and the inclusion of controls (see column (2)). We find that each additional female seated significantly increases the likelihood of conviction on female victim offenses by more than 10 percentage points on average, whereas the number of seated females do not directly affect the conviction rate for male victim cases. These effects are quite large given that 60 percent of defendants are convicted of at least one charge. Columns (5) – (8) assess whether the observed relationship is indeed linear or whether there is a differential effect of adding two or more female jurors. Relative to seated juries with zero females, adding just one female to the jury increases the chance of conviction on female victim cases by slightly more than 10 percentage points; though this estimate is very imprecisely measured, it is in line with the estimates from the linear specifications. However, adding two or more females to the seated jury significantly increases female victim conviction rates by 30-40 percentage points. These results suggest that it is not just the presence of any females that affect the verdicts, but that there is added influence of additional female voices during deliberations.[36]

6. Conclusion

This paper takes advantage of a novel data set and a historical jury reform to study the effect of female jurors on real-world jury verdicts – a question that is difficult to study today given that seated juries are not random and that jury pools and seated juries are typically about 50

[36] Specifications (not shown) that look separately at one, two, and three or more seated females find similar patterns; larger point estimates on the interactions for two versus one and three versus two seated females.

percent female. Though there was no overall effect of the reform on conviction rates, our findings suggest that adding females to the jury pool significantly impacted conviction rates on cases with characteristics that may be particularly salient for females – namely sex offenses and violent offenses with female versus male victims. The magnitudes of these results are substantial, especially considering that an average of two females serve on each twelve-person jury in the post-reform period.

The findings of the pre-post reform analysis can be driven by both direct and indirect mechanisms. With respect to the former, complementary regression analyses suggest that the seating of female jurors increased the conviction rate differential between violent offense cases with female versus male victims. Given the strong evidence that females are (de)selected from sex offense cases, especially in the early years, we are limited in the extent to which we can look at the direct channel in this instance. On the other hand, the large estimated effects of the reform for sex offenses, despite the limited seating of female jurors in these cases, suggest that indirect channels – such as the effects of judge comments about the nature of the case on the seated jury (regardless of the gender composition of that jury) – may also play an important role.

Taken together, the results of the pre-post reform and seated jury regression analyses paint a very consistent picture, implying that the addition of female jurors substantially increased the conviction differential for violent crime cases between female versus male victim cases. One potential explanation is that all else equal (i.e. holding the characteristics of the case and quality of evidence constant), all-male juries prior to the reform did not deem the alleged violence against women to be illegal in a subset of cases (e.g., domestic violence). An alternative explanation is that female jurors are not impartial, perhaps because they overly sympathize with the female victim. Of course, these findings could also be generated by mechanisms that are more benign in nature. For instance, the quality of evidence may not be constant across cases if female jurors are simply better at assessing the testimony of a female victim than a male victim. In general, without knowing more about the exact nature of the evidence, it is impossible to distinguish among these explanations. An important consideration in weighing these possibilities, however, is that juries were still overwhelmingly male after the reform, requiring female jurors to affect the votes of a large number of male colleagues in order to impact trial outcomes.

While the nature of any historical study raises obvious issues about the generalizability of the results to modern times, it is worth setting these concerns alongside the advantages of studying such an important moment in history. In the context of examining the impact of

female jurors, these advantages include the ability to deal with the serious endogeneity issues related to the pre-trial selection process and to study the broad effects of female representation as opposed to variation around balanced gender composition. Our study also contributes to the much larger literature on the effects of female representation in political, civic, social and economic life. In this broader context, the study of juries avoids a fundamental selection bias that hampers efforts to reach causal conclusions in the vast majority of these settings. In particular, elected or appointed individuals must naturally satisfy the preferences of the underlying constituency or individual making the appointment, as is the case for judicial appointments, elected officials, or positions in corporate leadership. This ubiquitous selection problem generally tends to diminish gender differences among selected individuals, making it exceedingly difficult to identify causal effects of female representation in most aspects of political or civic life.

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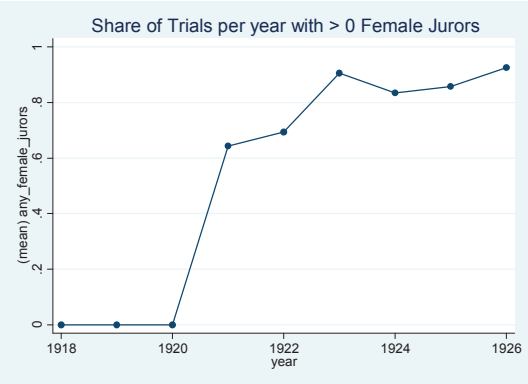
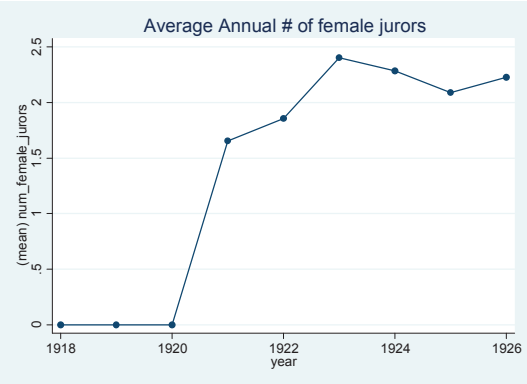
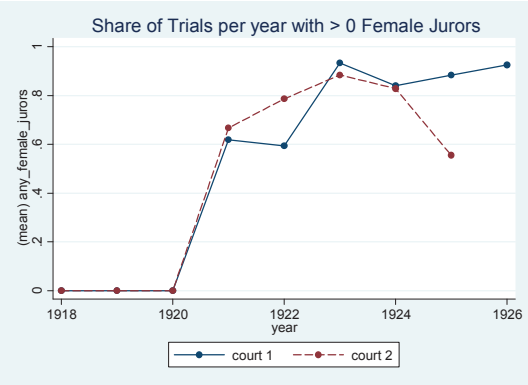
Figure 1.The Treatment: Share of Cases with Female Jurors and Average Number of Seated Jurors

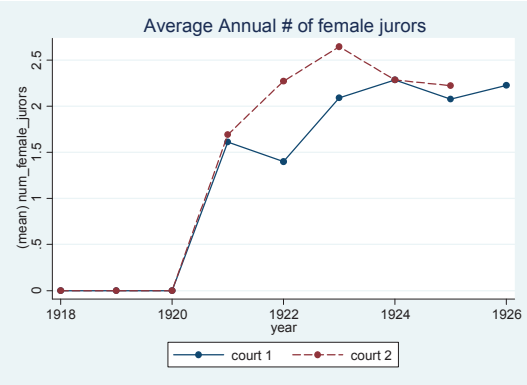
Panel 1: The Overall Treatment



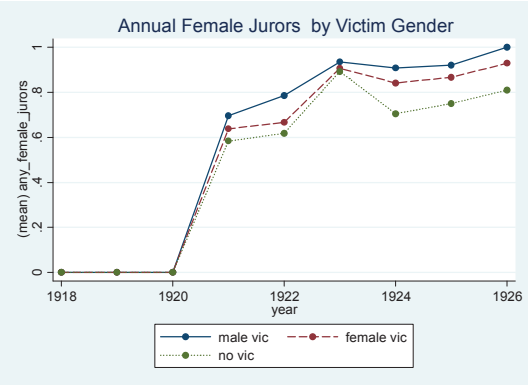


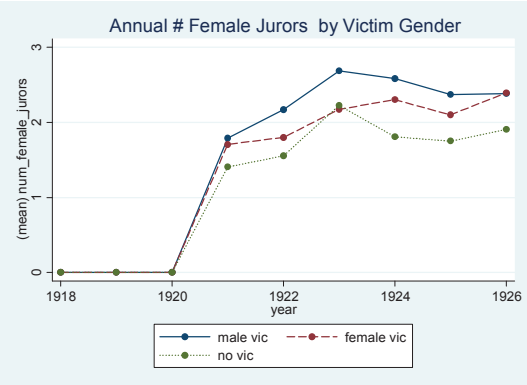
Panel 2: The Treatment by Court



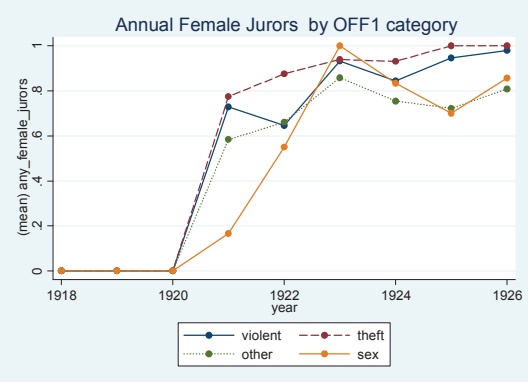


Panel 3: The Treatment by Victim Gender





Panel 4: The Treatment by First Offense Category



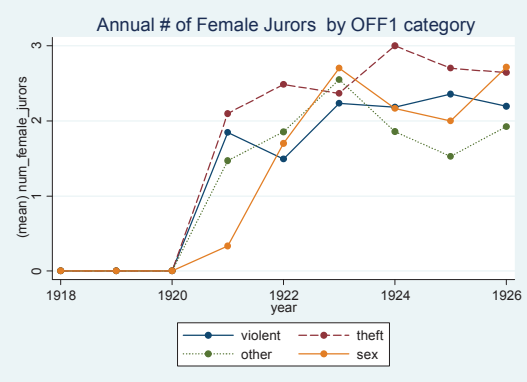
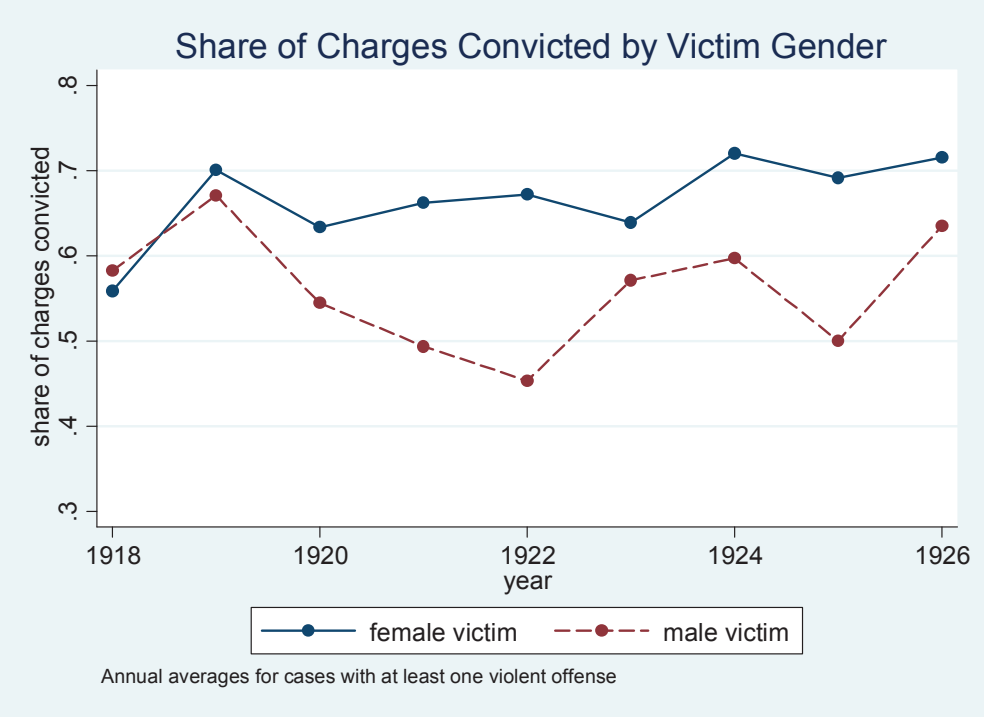


Figure 2. Could Underlying Trends in Attitudes Towards Females Explain the Main Results?



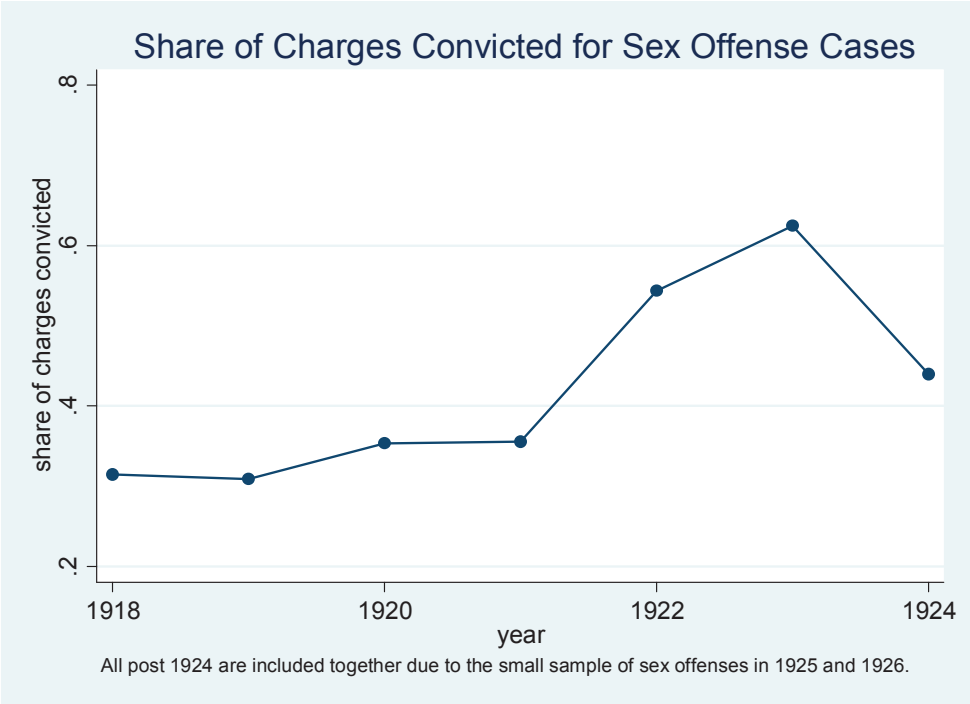


Table 1. Summary Statistics for Cases with a Seated Jury: Overall and By Court

Variable	Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD
	Both Courts			*Court 1*			*Court 2*		
post_reform (post 1921)	2148	0.63	0.48	1080	0.67	0.47	1068	0.59	0.49
any_female jurors	2147	0.49	0.50	1079	0.52	0.50	1068	0.46	0.50
# female jurors	2147	1.29	1.58	1079	1.28	1.46	1068	1.30	1.69
Offense Characteristics									
# of counts	2148	3.22	5.39	1080	3.20	4.64	1068	3.25	6.07
violent									
any_killing	2148	0.20	0.40	1080	0.39	0.49	1068	0.02	0.13
any_aggassault	2148	0.08	0.27	1080	0.10	0.30	1068	0.06	0.24
any_arson	2148	0.02	0.13	1080	0.03	0.16	1068	0.01	0.09
any_robbery	2148	0.05	0.21	1080	0.02	0.15	1068	0.07	0.26
any other off. vs person	2148	0.08	0.27	1080	0.09	0.29	1068	0.06	0.24
property									
any_theft	2148	0.14	0.35	1080	0.05	0.21	1068	0.23	0.42
any_burglary	2148	0.10	0.30	1080	0.06	0.23	1068	0.15	0.35
any_other_property	2148	0.15	0.35	1080	0.04	0.20	1068	0.25	0.43
Sex offense									
any_other_sex offense	2148	0.09	0.28	1080	0.05	0.22	1068	0.13	0.33
any_rape	2148	0.06	0.24	1080	0.08	0.27	1068	0.05	0.22
Other offense									
any_deception	2148	0.16	0.36	1080	0.13	0.34	1068	0.18	0.38
any_bigamy	2148	0.03	0.17	1080	0.01	0.12	1068	0.05	0.21
any_offense_against_king	2148	0.03	0.18	1080	0.04	0.20	1068	0.03	0.16
any_other	2148	0.06	0.24	1080	0.10	0.31	1068	0.01	0.11
charged_with crim history	2148	0.14	0.35	1080	0.08	0.28	1068	0.21	0.40
Defendant and Victim Characteristics									
def_female	2131	0.13	0.33	1070	0.17	0.37	1061	0.08	0.28
# defendants	2148	1.86	1.48	1080	1.89	1.70	1068	1.84	1.23
female_victim	2051	0.25	0.43	1011	0.34	0.48	1040	0.15	0.36
male_victim	2051	0.40	0.49	1011	0.39	0.49	1040	0.42	0.49
no_victim	2051	0.36	0.48	1011	0.28	0.45	1040	0.43	0.50
child_victim	2148	0.04	0.20	1080	0.07	0.25	1068	0.02	0.14
trial_length (days)	2142	1.91	2.10	1079	1.87	1.80	1063	1.94	2.37
Undefended	2148	0.21	0.41	1080	0.16	0.36	1068	0.27	0.44
Case Outcomes									
Guilty_any offense (1/0)	2027	0.60	0.49	998	0.68	0.47	1029	0.53	0.50
guilty_share offenses	2021	0.53	0.46	992	0.61	0.46	1029	0.46	0.46
guilty all offenses (1/0)	2021	0.45	0.50	992	0.54	0.50	1029	0.37	0.48
guilty 1st offense (1/0)	1972	0.49	0.50	968	0.57	0.50	1004	0.42	0.49
jury_hung	2148	0.03	0.16	1080	0.02	0.15	1068	0.03	0.17
jury_dismissed or hung	2148	0.08	0.28	1080	0.08	0.27	1068	0.09	0.28
Prison	1286	0.63	0.48	744	0.53	0.50	542	0.77	0.42
penal servitude	1286	0.20	0.40	744	0.25	0.43	542	0.14	0.35
Death	1286	0.04	0.20	744	0.07	0.25	542	0.00	0.04
other_sentence	1286	0.12	0.33	744	0.15	0.36	542	0.08	0.27

Sample sizes vary due to: (i) difficulty identifying names (or associated gender), (ii) incomplete information in the original case summaries, and (iii) different subsamples. Sentencing variables are only defined for cases that resulted in at least one guilty verdict. Jury hung and dismissed variables are based on cases that had a jury seated, regardless of whether that jury reached a verdict. The guilty variables are based on the sample of cases for which at least one charge had a verdict decided by the jury; but, even this can vary across outcome variables since for instance, the first charge may not have been put to the jury.

Table 2. 'First Stage' Effect of Reform on Likelihood of Female Juror.

	(1)	(2)	(3)	(4)
	Dependent Variable:			
	any_female_jurors		num_female_jurors	
year_1921	0.644***	0.627***	1.654***	1.618***
	(0.0213)	(0.0216)	(0.0815)	(0.0837)
year_1922	0.694***	0.696***	1.855***	1.847***
	(0.0213)	(0.0211)	(0.0817)	(0.0816)
year_1923	0.905***	0.894***	2.404***	2.362***
	(0.0221)	(0.0221)	(0.0847)	(0.0856)
year_1924	0.834***	0.803***	2.286***	2.169***
	(0.0226)	(0.0226)	(0.0866)	(0.0873)
year_1925	0.888***	0.885***	2.151***	2.245***
	(0.0248)	(0.0259)	(0.0949)	(0.100)
_cons	8.72e-15	0.0649*	1.49e-14	0.263*
	(0.0112)	(0.0355)	(0.0429)	(0.137)
Court	no	yes	no	yes
Offense categories	no	yes	no	yes
Other Case char1	no	yes	no	yes
Month Dummies	no	yes	no	yes
Other Case char2	no	yes	no	yes

Estimated with OLS. Other case characteristics 1 include victim characteristics (male, female, no, and child), defendant gender, number of defendants, codefendnat id, # charges, and charged with criminal history. Other Case Char 2 includes trial length, undefended, and jury breaks.

Table 3. Comparison of Pre- and Post- Case Characteristics By Court (for jury trials)

	Court 1 Jury Trials			Court 2 Jury Trials		
	Pre-1921 N =355	Post-1921 N=725		Pre-1921 N=443	Post-1921 N= 625	
undefended	0.15	0.16		0.25	0.28	
def_female	0.15	0.17		0.08	0.08	
# defendants	1.80	1.93		1.94	1.77	*
female_victim	0.38	0.33		0.12	0.18	*
male_victim	0.38	0.39		0.41	0.43	
no_victim	0.26	0.29		0.47	0.40	*
child_victim	0.06	0.07		0.02	0.02	
# of counts	3.00	3.30		2.48	3.79	*
any_killing	0.42	0.38		0.00	0.02	*
any_aggassault	0.08	0.10		0.05	0.07	
any_arson	0.02	0.03		0.00	0.01	*
any_robbery	0.03	0.02		0.06	0.08	
any_other_off. Vs. person	0.09	0.10		0.04	0.08	*
any_burglary	0.06	0.06		0.17	0.13	*
any_theft	0.04	0.05		0.26	0.22	
any_other_property	0.03	0.04		0.29	0.23	*
any_other_sexoffense	0.06	0.05		0.16	0.10	*
any_rape	0.08	0.07		0.04	0.05	
any_deception	0.14	0.13		0.13	0.21	*
any_offense_against_king	0.06	0.03	*	0.02	0.03	
any_bigamy	0.01	0.01		0.05	0.05	
any_other	0.09	0.11		0.00	0.02	*
charged_with crim history	0.06	0.09		0.17	0.23	*

* indicates pre-post difference is significant at 5%.

Table 4. Does Plea Behavior Change After the Reform?

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable = Plea to All Charges					
	all cases		Violent	property	sex	other
Post-reform 3 years	-0.115***	0.00171	0.0104	-0.00920	0.0592	-0.0359
	(0.0249)	(0.0200)	(0.0310)	(0.0537)	(0.0769)	(0.0297)
_cons	0.526***	0.557***	0.345***	0.986***	0.561***	0.415***
	(0.0171)	(0.0293)	(0.0500)	(0.0928)	(0.137)	(0.0799)
Offense type dummies	no	Yes	Yes	yes	yes	yes
Other Case Char	no	No	No	No	no	no
N	2267	2267	704	468	201	952
R-sq	0.009	0.397	0.089	0.223	0.109	0.402

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. Note that these specifications are restricted to the First Court, where the plea and trial data have been recorded. Post-reform 3 years is a dummy equal to one for the first three years after the reform: 1921, 1922, 1923. All specifications also control for a dummy after the treatment period: 1924-1926.

Table 5.Is there a post reform effect on conviction by broad offense category?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent Variable = guilty_anyoff									
	All cases	Any Violent	Any Property	Any Sex	Any Other	All cases	Any Violent	Any Property	Any Sex	Any Other
Post-reform 3 years	0.00123	-0.0659	-0.0468	0.175***	-0.0437	-0.0194	-0.0973**	-0.126***	0.161**	-0.0471
	(0.0245)	(0.0423)	(0.0471)	(0.0629)	(0.0501)	(0.0247)	(0.0442)	(0.0465)	(0.0665)	(0.0529)
_cons	0.592***	0.620***	0.469***	0.284**	0.759***	0.428***	0.339***	0.200*	0.419*	0.656***
	(0.0359)	(0.0663)	(0.0854)	(0.125)	(0.119)	(0.0568)	(0.0995)	(0.120)	(0.251)	(0.160)
N	2027	724	555	289	516	1911	658	534	285	489
R-sq	0.049	0.029	0.104	0.141	0.054	0.111	0.134	0.244	0.267	0.132
Mean Dep Variable	0.60	0.67	0.59	0.46	0.62	0.60	0.67	0.59	0.46	0.62
Court Dummy	yes	yes	yes	yes	yes	yes	yes	yes	Yes	yes
Offense Types	yes	yes	yes	yes	yes	yes	yes	yes	Yes	yes
Case Char 1	no	no	no	no	No	yes	yes	yes	Yes	yes
Month Dummies	no	no	no	no	No	yes	yes	yes	Yes	yes
Case Char 2	no	no	no	no	No	yes	yes	yes	Yes	yes

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. Post-reform 3 years is a dummy equal to one for the first three years after the reform: 1921, 1922, 1923. All specifications also control for a dummy after the treatment period: 1924-1926. Other case characteristics 1 include victim characteristics (male, female, no, and child), defendant gender, number of defendants, co-defendant id, # charges, and charged with criminal history. Other Case Char 2 includes trial length, undefended, and jury breaks

Table 6. Robustness of Sex Offense Results to Alternative Dependent Variables and Treatment Period Windows

	(1)	(2)	(3)	(4)
	Dependent Variable:			
	guilty_anyoff	guilty_shareoff	guilty_alloff	guilty_1stoff
	Restricted to the Sample with At Least One Sex Offense			
Post-reform 3 years	0.175***	0.169***	0.155**	0.173***
	(0.0629)	(0.0600)	(0.0614)	(0.0620)
_cons	0.284**	0.338***	0.453***	0.403***
	(0.125)	(0.125)	(0.141)	(0.147)
N	289	289	289	285
R-sq	0.141	0.136	0.135	0.122
Post-reform 2 years	0.116*	0.107	0.103	0.0998
	(0.0680)	(0.0649)	(0.0665)	(0.0669)
Post-reform 4 years	0.129**	0.124**	0.112*	0.127**
	(0.0592)	(0.0563)	(0.0573)	(0.0577)
Post-reform 5 years	0.141**	0.134**	0.118**	0.140**
	(0.0586)	(0.0557)	(0.0565)	(0.0572)

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. The upper panel presents the baseline treatment period of 3 years, and controls for all years after that. The lower panels redefine the treatment period to be two years (1921-1922), four years (1921-1924), or five years (1921-1925) and include a dummy capturing all additional years (post-treatment period). All specifications control for court and the basic set of offense characteristics.

Table 7. Heterogeneous Effect on Female Victim -Violent Crime Cases

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable:							
	guilty_any offense		guilty_ shareoff	guilty_ alloff	guilty_ 1stoff	guilty_any offense		
			post reform 3 years			*2 years*	*4 years*	*5 years*
Post reform 'Y' years	-0.108*	-0.174***	-0.152***	-0.153***	-0.156***	-0.211***	-0.128**	-0.123**
	(0.0575)	(0.0556)	(0.0524)	(0.0546)	(0.0562)	(0.0596)	(0.0537)	(0.0523)
Post reform 'Y' years * female victim	0.146*	0.200**	0.191**	0.207**	0.208**	0.256***	0.162*	0.161**
	(0.0878)	(0.0869)	(0.0831)	(0.0892)	(0.0896)	(0.0983)	(0.0836)	(0.0817)
female_victim	0.0580	-0.000361	-0.00962	-0.0407	-0.0208	-0.00275	0.00384	0.00245
	(0.0669)	(0.0682)	(0.0650)	(0.0692)	(0.0695)	(0.0679)	(0.0681)	(0.0681)
_cons	0.579***	0.372***	0.501***	0.644***	0.618***	0.404***	0.409***	0.409***
	(0.0846)	(0.100)	(0.0934)	(0.100)	(0.105)	(0.101)	(0.101)	(0.100)
Mean Dependent Variable	0.67	0.67	0.59	0.51	0.54	0.67	0.67	0.67
Court Dummy	yes	yes	yes	yes	Yes	yes	yes	Yes
Offense Categories	yes	yes	yes	yes	Yes	yes	yes	Yes
Case Characteristics 1	yes	yes	yes	yes	Yes	yes	yes	Yes
Month Dummies	no	yes	yes	yes	Yes	yes	yes	Yes
Case Characteristics 2	no	yes	yes	yes	Yes	yes	yes	Yes
N	661	658	658	658	644	658	658	658
R-sq	0.069	0.141	0.159	0.232	0.205	0.143	0.132	0.135

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. Post-reform 3 years is a dummy equal to one for the first three years after the reform: 1921, 1922, 1923. All specifications also control for a dummy after the treatment period: 1924-1926, and an interaction of that post – treatment period with female victims. Case characteristics 1 include victim characteristics (male, female, no, and child), defendant gender, number of defendants, codefendnat id, # charges, and charged with criminal history. Case Characteristics 2 includes trial length, undefended, and jury breaks

Table 8. Is there an effect of the reform on hung and dismissed juries?

	(1) Jury Dismissed for Any Reason	(2) Hung Jury	(3) Jury Discharged on at least one charge	(4) Jury dismissed due to defendant confession
All Cases				
Post reform 3 years	0.0273**	-0.00322	0.0229***	0.0110
	(0.0123)	(0.00746)	(0.00627)	(0.00824)
Mean Dep Variable	0.083	0.025	0.029	0.031
N	2148	2148	2148	2148
R-sq	0.028	0.015	0.063	0.022
Violent Cases				
Post reform 3 years	0.0324	0.00353	0.0150	0.0172
	(0.0212)	(0.0121)	(0.00934)	(0.0155)
Mean Dep Variable	0.076	0.021	0.022	0.037
N	767	767	767	767
R-sq	0.050	0.040	0.102	0.047
Property Cases				
Post reform 3 years	0.0417*	0.00319	0.0275*	0.0143
	(0.0247)	(0.0143)	(0.0162)	(0.0138)
Mean Dep Variable	0.09	0.023	0.045	0.026
N	577	577	577	577
R-sq	0.109	0.016	0.163	0.057
Sex Cases				
Post reform 3 years	0.00878	-0.0327	0.0285*	0.0130
	(0.0314)	(0.0260)	(0.0147)	(0.0106)
Mean Dep Variable	0.062	0.039	0.016	0.007
N	305	305	305	305
R-sq	0.021	0.019	0.030	0.064
Other Cases				
Post reform 3 years	0.0655**	0.00696	0.0346**	0.0316
	(0.0259)	(0.0120)	(0.0148)	(0.0193)
Mean Dep Variable	0.107	0.02	0.043	0.047
N	559	559	559	559
R-sq	0.079	0.019	0.125	0.041

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. All specifications control for court and the basic set of offense characteristics, as well as for whether the case is after the main post-reform period.

Table 9. The Direct Effect of Seated Female Jurors on Violent Crime Convictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable:							
	share of guilty offenses		guilty of any offense	guilty of 1st offense	share of guilty offenses		guilty of any offense	guilty of 1st offense
# female jurors	0.000730	-0.00403	-0.00950	0.000868				
	(0.0274)	(0.0274)	(0.0297)	(0.0275)				
# female jurors * female victim	0.106**	0.114**	0.102*	0.119**				
	(0.0453)	(0.0464)	(0.0515)	(0.0469)				
one female juror					0.0218	0.0378	0.00428	0.0504
					(0.152)	(0.158)	(0.170)	(0.167)
one female juror*female victim					0.116	0.0620	0.117	0.0498
					(0.227)	(0.230)	(0.250)	(0.230)
two female jurors					0.0404	0.0287	0.00563	0.0554
					(0.125)	(0.122)	(0.132)	(0.128)
two female jurors * female victim					0.349*	0.370**	0.333*	0.398**
					(0.189)	(0.185)	(0.199)	(0.186)
female victim	-0.132	-0.175	-0.153	-0.193	-0.157	-0.188	-0.179	-0.211
	(0.123)	(0.122)	(0.134)	(0.124)	(0.177)	(0.171)	(0.186)	(0.169)
_cons	0.604***	0.651***	0.476***	0.847***	0.595***	0.628***	0.462***	0.812***
	(0.127)	(0.151)	(0.161)	(0.155)	(0.147)	(0.160)	(0.174)	(0.164)
offense categories and court	yes	yes	yes	yes	yes	yes	yes	yes
other offense characteristics	no	yes	yes	yes	no	yes	yes	yes
Just Repeat Juries?	yes	yes	yes	yes	yes	yes	yes	yes
N	235	235	235	231	235	235	235	231
R-sq	0.123	0.174	0.167	0.220	0.128	0.182	0.169	0.234

Robust standard errors,* 10%, ** 5% *** 1%. This analysis is restricted to cases with at least one violent offense in the post-reform period, as well as those trials in which the jury was a repeat jury, i.e. sat on more than one trial. Other offense characteristics include defendant gender, number of defendants, defendant order, victim child, criminal history, and number of charges.

Appendix

Data Appendix

Original data for this project was obtained from the Central Criminal Court: Court Books, which are held at the National Archives in London. The following six books were used:

Central Criminal Court: Court Books, CRIM 6/25 Oct 1917 – September 1920 First Court, National Archives.

Central Criminal Court: Court Books, CRIM 6/26 Oct 1920 – July 1923 First Court, National Archives.

Central Criminal Court: Court Books, CRIM 6/27 Sept 1923 – Oct 1926 First Court, National Archives.

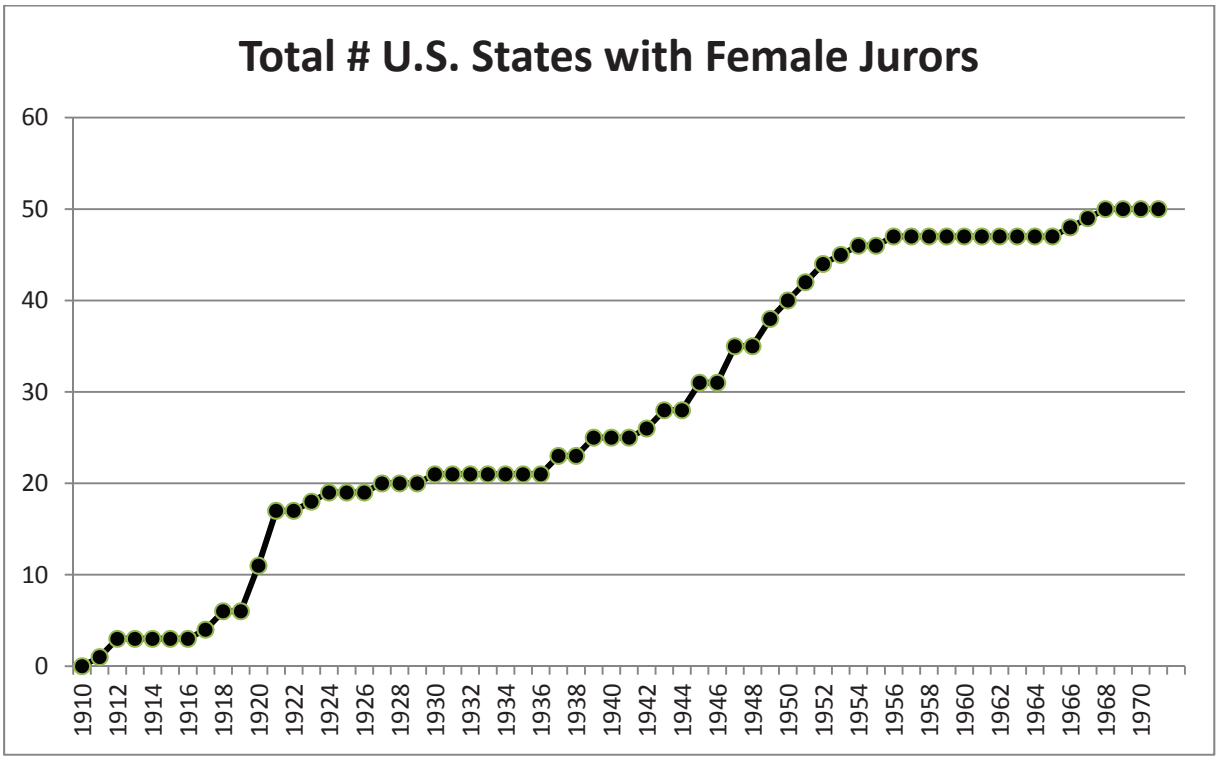
Central Criminal Court: Court Books, CRIM 6/25 Jan 1918 – September 1920 Second Court, National Archives.

Central Criminal Court: Court Books, CRIM 6/25 Oct 1920 – December 1922 Second Court, National Archives.

Central Criminal Court: Court Books, CRIM 6/25 Jan 1923 – January 1925 Second Court, National Archives.

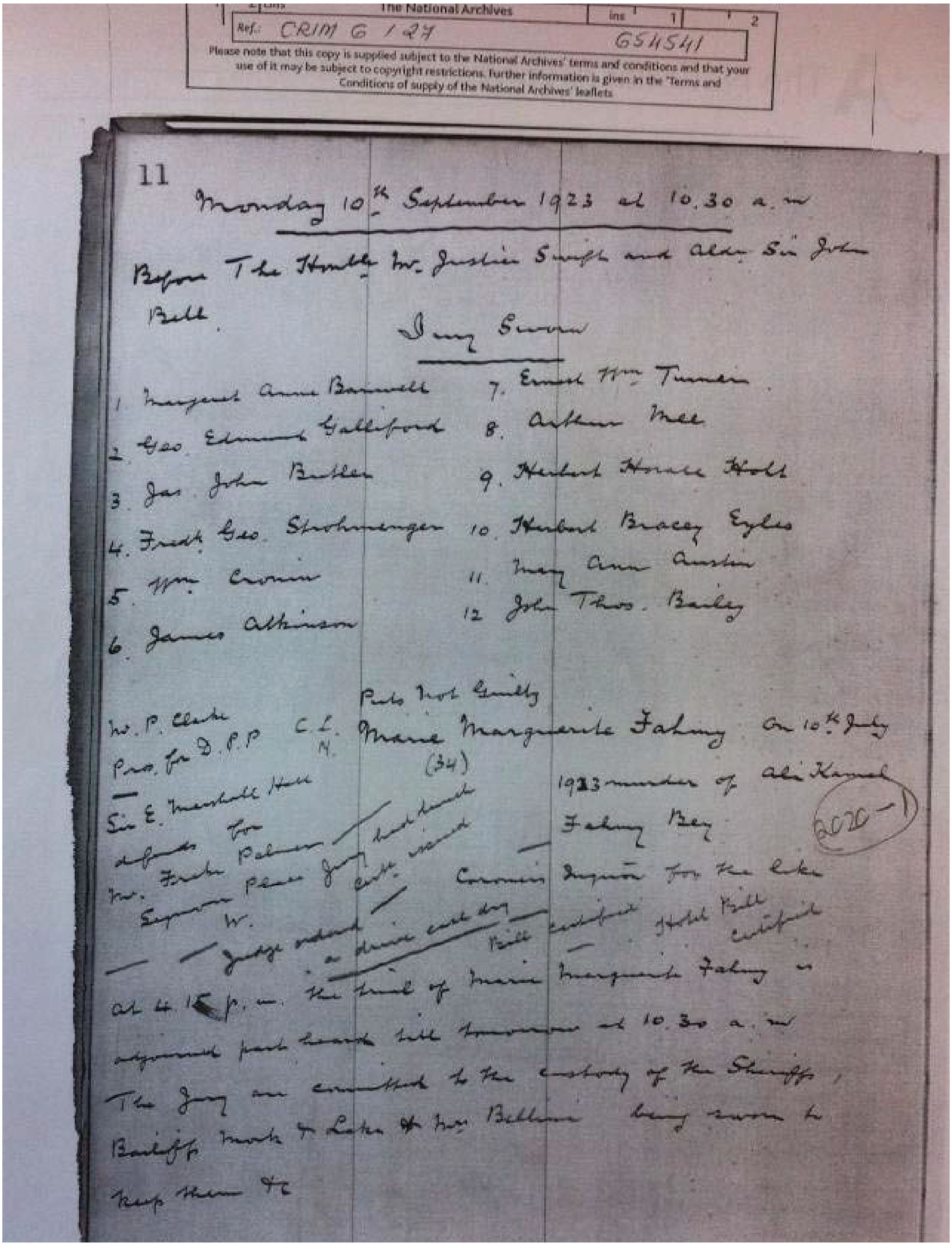
A photocopy of one page of the data source is provided in Appendix Figure 2.

Appendix Figure 1. Total # U.S. States with Female Jurors by Year



Note – Authors calculations based on data provided in McCammon (2012), page 38.

Appendix Figure 2 – Photograph of Jury Trial Raw Data from the First Court



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Monday 10th September 1923 at 10.30 a.m

Before The Honble Mr Justice Swift and Alder Sir John Bell.

Jury Sworn

1 Margaret Anne Barwell
2 Geo. Edmund Galliford
3 Jas. John Butler
4. Fredk Geo. Strohmenger
5. Wm Cronin
6 James Atkinson
7. Ernest Wm Turner
8. Arthur Mee
9. Herbert Horace Holt
10. Herbert Bracey Eyles
11. Mary Ann Austin
12 John Thos. Bailey

Mr P. Clarke Pros. for D.P.P
Sir E. Marshall Hall defends for
Mr Frank Palmer

C.C. M. Marie Marguerite Fahmy (34) Pleads Not Guilty. On 10th July 1923 murder of Ali Kamel Fahmy Bey

Coroner's Inquisition for the like [illegible]

at 4.15 p.m. the trial of Marie Marguerite Fahmy is adjourned part heard till tomorrow at 10.30 a.m. The Jury are committed to the custody of the Sheriffs, Bailiffs Mark & Lake & Mr Bellman being sworn to keep them &c

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Appendix Table 1. Average Female Juror Variables by Offense Category After the Reform (1921-1926)

	N	any female jurors	# female jurors
By offense type			
any_killing	287	0.81	1.86
any_aggass	122	0.87	2.28
any_arson	31	0.97	2.52
any_robbery	64	0.81	2.05
Any other off. Vs. person	118	0.75	1.92
any_burglary	122	0.94	2.56
any_theft	176	0.87	2.54
any_other_property	173	0.89	2.39
any_othersex	99	0.27	0.71
any_rape	87	0.56	1.57
any_deception	230	0.79	2.03
any_off_ag_king	40	0.85	2.23
any_bigamy	39	0.97	3.05
any_other	94	0.79	1.88
By Victim Gender			
male victim	519	0.84	2.28
female victim	329	0.79	2.04
no victim	434	0.71	1.74
By Defendant Gender			
male defendant	1165	0.77	2.05
female defendant	177	0.85	2.04

Appendix Table 2. Are female jurors selected based on case characteristics?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	any_female_jurors			# female jurors			2 or more female jurors?		
years	1921-26	1921-23	1924-26	1921-26	1921-26	1921-26	1921-26	1921-26	1921-26
offense types	all	all	all	violent	violent	violent	violent	violent	violent
Repeat juries only?	no	no	no	no	yes	no	yes	no	yes
N	1267	832	435	473	252	473	252	474	252
Court 2	0.00156	0.0774**	-0.100***	0.153***	0.156**	0.722***	0.210	0.101	0.0319
	(0.0261)	(0.0354)	(0.0385)	(0.0443)	(0.0624)	(0.215)	(0.293)	(0.0652)	(0.103)
any_killing	-0.0586	-0.0403	0.0590	-0.000301	0.0264	-0.236	-0.267	-0.0741	0.0872
	(0.0396)	(0.0531)	(0.0517)	(0.0588)	(0.0825)	(0.243)	(0.365)	(0.0771)	(0.117)
any_agg_assault	0.0652*	0.0104	0.179***	0.0568	-0.00680	0.102	0.167	0.0906	0.136
	(0.0389)	(0.0561)	(0.0370)	(0.0499)	(0.0675)	(0.181)	(0.252)	(0.0607)	(0.0863)
any_other_offense_against_person	-0.0610	-0.0196	-0.00408	-0.0505	0.0255	-0.283	-0.0452	-0.0466	0.106
	(0.0481)	(0.0613)	(0.0645)	(0.0536)	(0.0716)	(0.218)	(0.316)	(0.0707)	(0.107)
any_robbery	-0.0352	-0.101	0.182***	-0.0902	-0.238**	-0.502*	-0.563	0.0548	0.159
	(0.0561)	(0.0738)	(0.0512)	(0.0731)	(0.112)	(0.286)	(0.448)	(0.0888)	(0.132)
any_rape	-0.234***	-0.296***	-0.0297	0.0786	0.0104	-0.452	-0.901**	-0.225	-0.167
	(0.0791)	(0.0862)	(0.159)	(0.0635)	(0.0705)	(0.516)	(0.451)	(0.265)	(0.367)
any_other_sex	-0.489***	-0.495***	-0.375***	-0.141	-0.241	-0.788	0.0347	0.0294	0.677*
	(0.0595)	(0.0715)	(0.101)	(0.352)	(0.241)	(0.953)	(1.001)	(0.368)	(0.353)
any_arson	0.145**	0.168**	0.173	0.246**	0.170	-0.462	1.687*	-0.168	0.478***
	(0.0637)	(0.0756)	(0.121)	(0.118)	(0.103)	(0.875)	(0.912)	(0.170)	(0.160)
any_burglary	0.0980**	0.0496	0.201***	0.134**	0.176**	0.329	0.0462	-0.197	-0.0407
	(0.0410)	(0.0534)	(0.0629)	(0.0526)	(0.0797)	(0.668)	(0.572)	(0.200)	(0.212)
any_theft	0.0452	0.00709	0.201***	0.137**	0.0292	0.428	0.388	0.0992	0.0750
	(0.0361)	(0.0437)	(0.0662)	(0.0646)	(0.102)	(0.447)	(0.671)	(0.164)	(0.250)
any_other_property	0.0123	0.0516	-0.00964	0.193***	0.256***	-0.0221	1.000**	0.297**	0.460***
	(0.0359)	(0.0481)	(0.0630)	(0.0663)	(0.0941)	(0.430)	(0.476)	(0.133)	(0.119)
any_off_against_king	0.0480	0.000762	0.209***	0.277***		-0.563***		-0.476***	
	(0.0618)	(0.0845)	(0.0490)	(0.0640)		(0.210)		(0.0704)	
female_victim	0.00902	0.0109	-0.00215	-0.0294	-0.00639	-0.213	-0.111	-0.0332	-0.0467
	(0.0311)	(0.0414)	(0.0458)	(0.0393)	(0.0513)	(0.144)	(0.201)	(0.0499)	(0.0670)
no_victim	-0.0519*	-0.0104	-0.13***	-0.0638	0.0377	0.852	-1.757**	0.140	-0.0544
	(0.0296)	(0.0398)	(0.0431)	(0.124)	(0.0555)	(1.028)	(0.859)	(0.192)	(0.120)
child_victim	0.0670	0.0690	0.0882	0.138**	0.0405	1.219*	1.260	0.356	0.223
	(0.101)	(0.120)	(0.175)	(0.0537)	(0.0726)	(0.655)	(0.869)	(0.244)	(0.463)
Defendant female	0.0350	0.0640	-0.0219	0.0732	0.00806	0.0145	-0.345	-0.0209	-0.136
	(0.0354)	(0.0457)	(0.0580)	(0.0494)	(0.0660)	(0.192)	(0.252)	(0.0677)	(0.0896)
# of counts	-0.00370	-0.007**	-0.00021	-0.0168	0.0255	-0.0890	-0.0380	-0.0324*	-0.0321
	(0.00291)	(0.00302)	(0.00181)	(0.0169)	(0.0217)	(0.0558)	(0.0895)	(0.0177)	(0.0314)
_cons	0.845***	0.778***	0.780***	0.870***	0.776***	2.435***	2.593***	0.798***	0.762***
	(0.0455)	(0.0605)	(0.0640)	(0.0750)	(0.102)	(0.278)	(0.411)	(0.0854)	(0.134)
R-sq	0.165	0.170	0.276	0.102	0.112	0.121	0.128	0.104	0.149

Robust standard errors, *10%, **5%, *** 1%. Not shown coefficients number of defendants, codefendant id, bigamy, conspiracy, deception, and charged with criminal history.

Appendix Table 3. Is there a post reform effect on the likelihood of conviction, overall and by court

	(1)	(2)	(3)
	Dep. Variable = guilty any offense		
	all	court 1	court 2
Panel A: 3 year window			
Post-reform 3 years	0.00123	-0.00811	-0.00907
	(0.0245)	(0.0352)	(0.0338)
_cons	0.592***	0.694***	0.374***
	(0.0359)	(0.0499)	(0.0491)
N	2027	998	1029
R-sq	0.049	0.025	0.076
Panel B: 2 year window			
Post-reform 2 years	-0.00738	-0.0130	-0.0185
	(0.0270)	(0.0393)	(0.0368)
Panel C: 4 year window			
Post-reform 4 years	0.0265	-0.00337	0.0283
	(0.0229)	(0.0333)	(0.0319)
Court Dummy and offense types	yes	yes	yes

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. In Panel A, Post-reform 3 years is a dummy equal to one for the first three years after the reform: 1921, 1922, 1923. All Panel A specifications also control for a dummy after the treatment period: 1924-1926. In Panels B and C, the treatment period is 1921-1922 and 1921 – 1924 respectively. For the ease of presentation, just the post-reform treatment effect is reported in Panels B and C.

Appendix Table 4. Is there a post- reform effect on trial length?

	(1)	(2)	(3)	(4)	(5)	(6)
			Dependent Variable = trial_length			
		3 years		*2 years*	*4 years*	*5 years*
Post Reform Y Years	0.261***	0.144*	0.164**	0.312***	0.159**	0.195***
	(0.0865)	(0.0748)	(0.0791)	(0.116)	(0.0745)	(0.0724)
_cons	1.893***	0.420*	0.404*	1.871***	1.838***	1.873***
	(0.147)	(0.215)	(0.241)	(0.146)	(0.147)	(0.149)
Mean Dependent Variable	1.9 days	1.9 days	1.9 days	1.9 days	1.9 days	1.9 days
Court and Basic Offense	yes	yes	yes	yes	Yes	yes
Other char1	no	yes	yes	no	No	no
Month and other char2	no	no	yes	yes	Yes	yes
N	2143	2032	2032	2143	2143	2143
R-sq	0.289	0.474	0.485	0.290	0.288	0.288

Robust standard errors in parentheses. * 10%, ** 5%, ***1%. All specifications control for whether the case is after the main post-reform period.