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CRIMINAL SENTENCING IN NINETEENTH CENTURY PENNSYLVANIA

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ABSTRACT

How law is interpreted and enforced at a particular historical moment reflects contemporary social concerns and prejudices. This paper investigates the nature of criminal sentencing in mid-nineteenth-century Pennsylvania. It finds that extralegal factors, namely place of conviction and several personal characteristics, were important determinants of sentence length. The observed disparities in the mid-nineteenth century, however, are different than modern disparities. Instead of longer sentences, African Americans and recent immigrants tended to receive shorter sentences, whereas more affluent offenders received longer sentences. The results are consistent with other interpretations of the period as the "era of the common man."

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1. Introduction

How law is interpreted and enforced at a particular historical moment reflects contemporary social concerns. The republican ideal holds that all lawbreakers regardless of status or condition stand equal before the law. Such a notion is, of course, naive. As Mosher and Hagan (1994) note, much legal research consists of efforts to illuminate the gap between ideal and practice. An important component of that research agenda focuses on the extent to which extralegal factors, such as a defendant's race, sex, ethnicity or other personal characteristics influence the dispensation of justice (Mustard 2001; Sarnikar et al. 2006).[1] The evidence on the issue is mixed, but concerns about the influences of personal characteristics on criminal sentencing led to the enactment of strict sentencing guidelines in the 1980s. The guidelines constrain judicial discretion in sentencing because crime severity and criminal history are determinative. Significant departures from the guidelines in either direction are condoned only when judges can point to compelling aggravating or mitigating circumstances (Shapiro 1992; Seymour 1992). Adherence to guideline implies that extralegal factors should have no observable influence on sentence length. A second common criticism of the criminal justice system is that serves the interests of the rich and powerful (Walker 1980, p.104). That prisons are disproportionately populated with minorities, immigrants and the poor suggest that these groups suffer the worst injustices even after the adoption of sentencing guidelines.

[1] The literature investigating the connection between personal characteristics is massive. Tractable reviews of racial disparities in criminal sentencing are provided by Wilbanks (1987), Weitzer (1996), Lauritsen and Sampson (1998) and Engen, Steen and Bridges (2002).

The former criticism seems more relevant to understanding nineteenth century sentencing practices than the latter. Nineteenth-century courts faced fewer sentencing constraints than modern courts. Under modern Pennsylvania law, for example, a first-time offender convicted of rape faces a sentence between 48 and 66 months (Pennsylvania Commission on Sentencing 2005). In the mid-nineteenth century, the statutory law provided for prison terms between 120 and 252 months. Such expansive sentencing limits afforded judges and juries substantial sentencing latitude and offered them opportunities to act on their prejudices. On the other hand, the nineteenth century was the "populist century," an period in during which the elite gave way to popular rule (Schlesinger 1945; Howe 2007). Instead of viewing the courts as a system by which the wealthy repressed or controlled the proletariat, nineteenth-century working class men of nearly every ethnicity exercised political power and bent some elements of the criminal justice system to their will. Far from holding disproportionate power, the wealthy complained about how little control they had over the criminal justice system, especially at the lower levels (Walker 1980, p.104).

Following a brief description of nineteenth century criminal procedure in the next section, the empirical methodology is discussed in the third section. Using previously unexploited nineteenth-century Pennsylvania prison records, the empirical analysis reveals that personal characteristics, such as age, race, sex and ethnicity influenced the length of prison term imposed on criminal offenders.[2] Modern social science typically associates a role for extralegal factors with inequitable and possibly discriminatory treatment. The empirical results offered below reveal notably different practices. Race played a modest role in determining sentence length. In fact, western Pennsylvanians systematically sentenced African Americans to shorter prison terms than

[2] Carson (2007) makes use of the anthropometric data included in the prison intake records, but does not study criminal sentencing patterns.

whites, holding all else constant. Blacks were not the only beneficiaries of mid-nineteenth century criminal sentencing practices. Women, as well as Irish and German immigrants, received shorter sentences, on average, than similarly situated native-born white men. And the era was, indeed, the age of the common man. The only group that consistently received longer prison sentences for a given crime were men drawn from the upper income classes. Professionals, shopkeepers and clerical workers consistently received longer sentences for nonviolent crimes than common laborers. The results are consistent with Steinberg's (1989) and Hirsch's (1992) interpretations of a criminal justice system run by and for the common man. Jacksonian democracy, it appears, influenced not only the political process but the dispensation of criminal justice. This is not to imply that criminals were not sanctioned, but juries consistently deviated from harsh statutory penalties and did so based on extralegal factors.

2. Nineteenth century criminal procedure

Historians describe nineteenth century criminal courtrooms that little resemble the popular modern image. Steinberg (1989) describes the early nineteenth century Philadelphia courtroom as almost circus-like. Court day was a popular form of lower class entertainment as people crowded into small courtrooms to jeer and cheer the procession of vagrants, drunks, prostitutes and more dangerous felons brought before the court. Certain groups, such as blacks or Irish immigrants, sometimes attended in unusually large numbers when one of their own appeared. Some came to witness the drama; others came to offer support or to influence the outcome.

Not only were courtrooms crowded and unruly, judges conducted business at a pace that would astonish the modern observer. Defendants were shuttled in and out. Pleading and testifying

were minimal. Most cases, even those involving serious felonies, lasted a few minutes.[3] Bailiffs were in constant motion, moving defendants in and out, ushering attorneys to and fro, and quieting noisy spectators. On a typical day in 1848, one Philadelphia justice of the peace presided over six assault cases, three larceny cases, three breach of ordinance (disorderly conduct) cases, one firecracker case, and a fast driving case. In addition, the JP examined a man for life insurance eligibility, conducted a marriage ceremony, committed three boys to the orphanage, issued two warrants of eviction and eight summonses to appear (Steinberg 1989, p. 17).

In nineteenth-century Pennsylvania most criminal prosecutions were private or citizen prosecutions (Steinberg 1986; Steinberg 1989).[4] That is, citizens charged their fellows with some crime, such as assault or larceny, and then acted as their own prosecuting attorney. When the stakes were high enough, however, victims or their surviving families hired private prosecutors and Walker (1980, p. 110) argues that the "wildly democratic" nature of the legal profession in America meant that almost everyone could afford a qualified attorney to represent them.[5] The historical evidence on the use of private prosecutors is mixed. Steinberg (1989) finds that the use of private prosecutors was limited in nineteenth century Philadelphia, but Ireland (1995) suggests that many victims preferred a private to a public prosecutor because the latter was typically underpaid, overworked and

[3] Langbein (1978) reports similarly speedy trials at London's Old Bailey. The typical trial lasted only a few minutes. A typical quarter session at the Bailey sat for two to five days and heard between 50 and 100 criminal cases.

[4] Langbein (1979, pp. 266-67) also discusses private prosecution, mostly in the English context.

[5] American courts were more accustomed to private prosecutors than their English predecessors and Langbein (1979, p. 267) argues that when public prosecutors eventually took control they acquired the discretion long afforded the private prosecutor.

either young and inexperienced or old and incompetent.[6] As late as 1866, the Pennsylvania legislature amended its law allowing aggrieved citizens prosecuting their cases to dismiss the state district attorneys in favor of private prosecutors who, once recognized by the court, assumed the prosecutorial powers afforded district attorneys.[7]

Citizen prosecution remained the principal form of criminal prosecution into the 1870s and beyond because police forces remained rudimentary and prosecutors's offices poorly staffed.[8] In 1833, for example, Philadelphia was divided into four wards, each with just 6 daytime policemen, 30 night watchmen, one watch captain and three inspectors, all under the control of the mayor's office. Because such a police force was incapable of "acting as a powerful instrument of state enforcement ... police prosecution consisted of little more than the apprehension of drunks and vagrants" (Steinberg 1989, p. 121). The force was expanded and rationalized in 1854, but it would be several decades before anything resembling a modern police force emerged.

Given the small role played by police and the centrality of citizen prosecutions, local justices of the peace (rural Pennsylvania) or aldermen (Philadelphia) acted as the court of first hearing in all criminal proceedings. Under Pennsylvania law, justices of the peace held the power of summary judgment over minor offenses, such as public drunkenness, vagrancy and disorderly conduct. If the JP found an individual guilty of a minor offense, he might impose a fine, commit the offender to jail, or demand a peace bond to be surrendered if the defendant later appeared before the court for the

[6] Little (1991) does not mention private prosecutions in rural nineteenth-century Pennsylvania. It remains unclear how often victims used private prosecutors to pursue their claims.

[7] Pennsylvania General Assembly (1866) Chapter 72.

[8] Monkkonen (1981) argues that it was not until the Progressive Era that city police forces shifted focus from public disorder offenses to more serious offenses against persons and property.

same infraction. In instances of serious crime, the JP heard the complainant's testimony or private prosecutor's arguments and, if convinced that a crime had been committed, turned the case over to a grand jury. In property crime suits – larceny, robbery or burglary – even if the amounts involved were quite small, justices of the peace did not possess summary judgment power. Their choice was to hold the defendant for trial or dismiss. If the case was reported to the grand jury, the JP could either establish bail or commit the offender to jail to await the hearing. Because justices were paid based on the number of cases reported out to the grand jury, they faced obvious incentives to report even when the injury was small and the complainant could offer little evidence, a practice that bedeviled and frustrated grand juries throughout the nineteenth century.

The third level of criminal jurisdiction, beyond the justice of the peace and the grand jury, in nineteenth century Philadelphia was the Mayor's Court (abolished in 1836) and the County Court of Quarter Sessions. Originally the Quarter Sessions, following the common law tradition, sat four times each year, but by mid-century the courts were so busy that they sat almost continuously. The Mayor's Court and the Quarter Sessions tried cases involving more serious crimes originally pled before a local justice and held over for trial. If the grand jury found it probable that a crime had been committed, it issued an indictment and turned the case over to the deputy attorney general for prosecution unless the victim asked that a private prosecutor handle the case.

What about the defendants? The nature of legal training and of the profession itself in nineteenth-century America – an apprenticeship system with little formal accreditation – made legal services affordable and available to nearly everyone. In fact, many attorneys lived comfortably from the fees generated in representing low-income defendants. "The experienced criminal-defense lawyer," Walker (1980, p. 110) writes, "master of the intricate technicalities of criminal procedure,

was one of the major resources that low-income groups could rely on in the face of the law." But few cases actually came to arguing fine points of the law. Despite the availability of defense attorneys, many defendants appeared without counsel. Some produced a witness or two. Most offered little in the way of consequential defense, which explains why the court could resolve cases with dispatch.

Other features that distinguished nineteenth from twentieth century criminal procedure were the central role of the jury and the virtual absence of plea bargaining. Alleged larcenists were the most likely to plead guilty, but even in these instances only about ten percent of defendants did so. Incentives favored a guilty plea in larceny cases because conviction rates were high and those who plead guilty received significantly shorter sentences. Steinberg (1989, p. 75) finds that the confession advantage disappeared after the 1850s, but the guilty plea rate remained largely unchanged for about a decade thereafter. Plea bargaining in the modern sense had not yet appeared (Friedman 1979; Langbein 1979; Haller 1979). Those who pleaded guilty did so seeking a lesser sentence for a given crime rather than a lesser sentence for a lesser charge.

With low confession rates and no plea bargaining the judge and jury, rather than defense counsel and the state prosecutor, played a central role in assigning blame and determining the punishment. Early Americans considered the jury trial an essential bulwark against governmental power and oppression. Contemporary faith in the jury trial reflected the era's faith in the common man (Note 1964, p. 172). Nearly every felony case was heard by a jury, even if perfunctorily. But the importance of nineteenth-century juries went beyond their ubiquity. Nineteenth-century American juries decided not only the facts of the case, but typically applied the law as well. Judicial instruction was viewed as so much meddling with jury independence and was little tolerated in some

jurisdictions.[9] When few attorneys appeared on either side and few arcane technicalities were introduced, judicial instruction was unnecessary in most instances. As courtrooms became increasingly professional after mid-century, accepted practice evolved toward the judge's right to instruct the jury on the law, but limited his ability to comment on the facts (Note 1964, p. 188). It was only in the last third of the century, as enthusiasm for juries waned, that the judge supplanted the jury as the principal finder of fact and law.

Because juries were not as ideologically committed to legal technicalities as trained attorneys, its seems likely that defendants would be subject to more capricious applications of the law under a jury-dominated than under a lawyer-dominated system. We might then expect to observe a pronounced role for extralegal factors in nineteenth-century sentencing. Before turning to an analysis of nineteenth criminal sentencing in Section 4, the next section discusses the empirical approach and addresses several econometric issues that have arisen in the literature.

3. Theoretical and empirical issues in criminal sentencing research

Rationalist Anglo commentators since at least the time of John Locke have argued that the objective of the criminal justice system is to provide deterrence, incapacitation and proportional retribution.[10] The alternative utilitarian approach to criminal sentencing emphasizes the extent of the harm, including all relevant externalities, and achieving efficient deterrence in a cost-effective

[9] Gross (1998, p. 117) writes that in the antebellum era, "the realms of 'law' and 'fact' were far from distinct; although the nineteenth-century trend was towards greater power for the judge, vigorous popular efforts to limit judicial power, and strong customary traditions of juries deciding 'law' questions meant that the battle [for judicial determination of law] was far from won." Juries often received instruction on the law, but such instructions rarely settled the question.

[10] Locke wrote, "Each transgression may be *punished* to that *degree*, and with so much *Severity* as will suffice to make it an ill bargain to the Offender, give him cause to repent, and terrife [sic] others from doing the like" (quoted in Hirsch 1992, p. 81).

fashion (Waldfogel 1993). Criminal sentencing in early America was an amalgam approach that acknowledged the philosophical justifications for proportionality in addition to public desires for rehabilitation and Christian charity, so that capital and corporal punishments gave way to incarceration. By end of the first third of the nineteenth century, for example, the presumptive sentence for convicted counterfeiters was no longer the pillory followed by hanging as prescribed in a 1767 statute. After 1829 one to seven years in the state penitentiary was believed to offer a more efficient mix of deterrence, incapacitation and rehabilitation.[11] Under the utilitarian approach, which held sway by mid-century, the rehabilitative penitentiary was as much church as prison, but the rationalist approach held that a crime was a crime because it advanced the criminal's utility at another's expense. Incarceration in the penitentiary was intended to drain the criminal act of utility. Although legal commentators focused on proportionality, taxpayers insisted on a cost-effective system of retribution and incapacitation, which led the early prisons to be less like churches and more like unpleasant, malodorous workhouses (Hirsch 1992, pp. 20-23).

Under either rationalist or utilitarian approaches, the efficient price for an infraction was unrelated to extralegal factors, or personal characteristics such as sex, age, race or ethnicity. Becker's (1968) canonical model of crime concludes that the optimal sentence is negatively related to both the marginal cost of imprisonment and the probability of imprisonment because the supply of crime is inversely related to the probability of detection, conviction and incarceration, but does not easily accommodate extralegal factors. Albonetti's (1991, p. 249) statistical discrimination approach, on the other hand, incorporates the "past experience, stereotypes, prejudices, and highly

[11] Pennsylvania General Assembly, *Laws of the Commonwealth* (1810) Vol. I; *Laws of the General Assembly* (1829), chapter 204.

particularized views" of judges, which may influence sentencing outcomes. The judge's experiences, stereotypes and prejudices shape his or her beliefs about a criminal's propensity of recidivate so that, in the face of uncertainty about the connection between sentence severity and recidivism, observable characteristics believed to be correlated with recidivism, such as race, ethnicity and sex, will influence sentence length.

Although Albonetti's statistical discrimination model is judge-based, it can be adapted to the nineteenth century's jury-based system. Developing a model of group decision making in the face of uncertainty is well beyond the scope of this study, but it is not unreasonable to assume that juries, acting as a body, were as subject to prejudicial stereotyping as modern judges. Further, it is reasonable to assume that juries balanced their charges to apply the law equitably and to protect the public. If popular opinion accepted that a characteristic, such as blackness or immigrant status, served as a predictor of recidivism, it is equally reasonable to believe that juries used existing stereotypes in determining sentences.[12] Imperfect information and bounded rationality do not excuse discriminatory practices, of course, but understanding jury sentencing will shed light on the power and salience of racial and ethnic stereotypes to nineteenth century Americans. To the extent that juries reflected the public's belief in differential criminal propensities, sentencing disparities should become evident in the data. If nineteenth century juries accepted the primacy of the rule of law, statutory penalties will have had presumptive force, and sentencing variance will be statistical noise in that it will reflect the jury's desire to tailor the sanction to the specific crime and not the characteristics of the criminal (Langbein 2006, p. 46).

Several important methodological issues arise in adapting the vast literature on the modern

[12] Moehling and Piehl (2007) find that early twentieth century concerns with youthful immigrant criminality were not unfounded.

determinants of criminal sentencing to understanding historical criminal sentencing. One of the more important issues is that the same basic regression model has been used to identify both system-wide discrimination and to isolate the consequences of discretion at specific points in the process. While it is a fact that any particular sentence is influenced by individuals acting at consecutive stages of the criminal justice process, no single model can simultaneously identify system-wide discrimination as well as discriminatory acts at a specific point in the process (Bushway and Piehl 2001). Because any sentence results from decisions made by multiple actors at multiple stages, it is imperative to identify the role performed by specific actors or groups of actors and then model their behavior in a manner that yields testable hypotheses. As Sampson and Lauritsen (1997) recognize, the standard approach that seeks to determine whether deviations from sentencing norms correlated with extralegal factors are warranted or unwarranted fails to deliver in that the approach tends to focus on system-wide disparities.

An exception is the sentencing guidelines departure literature, which specifically models deviations from presumptive sentences provided by state or federal sentencing guidelines.[13] These studies correlate departures from the guidelines with various extralegal factors. Because the guidelines are constructed based on crime severity and criminal history, these factors should not enter into a judge's decision to depart from the guidelines. Although judges note that departures sometimes result from a desire to assign blame, reward contrition or protect the public, the existing literature generally shows that significant departures are driven by factors, such as race, sex or age, that under strict adherence to the guidelines should not influence sentence length.

[13] For an introduction to the guidelines departure literature see Griswold (1987), Albonetti (1997), Reitz (1998); Engen and Gainey (2000; 2001), Mustard (2001) and Bushway and Piehl (2001). My discussion follows Bushway and Piehl (2001).

This study does not isolate the behavior of judges; rather it focuses on the jury that, as we have seen, exercised greater control over sentencing than modern juries. Such a study can offer some insights into contemporary operation of the law and the extent to which race and other personal characteristics influenced the application of the law in an egalitarian era of jury-based criminal sentencing. The data also allow for comparisons of the extent of jury discretion across jurisdictions, something that is possible only if jury discretion is empirically isolated from systemic discrimination.

Like Bushway and Piehl (2001), I follow Griswold (1987) in estimating an empirical specification of the following general form:

$$\text{Sentence length}_i = \alpha + \beta \text{ Presumptive sentence}_i + \gamma X_i + \epsilon_i$$

One advantage of this formulation is that it explicitly incorporates the effects of recommended sentences. That is, the specification assumes that the observed sentence is driven by the statutory recommendation, in addition to a set of personal characteristics or other factors captured in the vector X. By removing the variation due to the presumptive sentence, it is possible to isolate the jury discretion induced by extralegal factors and determine whether any group or groups were the victims of unwarranted departures from contemporary sentencing norms.

As Bushway and Piehl (2001, p. 748) note, however, the coefficient on the presumptive sentence variable should be fixed so that the regression is only explaining the difference between the recommended sentence and the actual sentence. If we were to estimate β, or the coefficient on the presumptive sentence, the resulting estimate would include both jury response to defendant characteristics as well as its response to presumptive sentences developed by prior actors, namely the legislators who devised the statutory sentences. Estimating β defeats any attempt to isolate jury

discretion from the actions of other actors in the system. The results reported below are estimated holding the coefficient constant (i.e., $\beta=1$).[14]

A second issue that arises in devising an appropriate estimation strategy is that in levels the data are positively skewed. Linear regression techniques might then generate curvilinear errors terms, which will lead to inefficient standard errors and potentially inappropriate inference. The positive skew is easily dealt with by taking the natural log of the sentences. Estimating the equation with the left-hand side variable expressed in log terms has the added advantage of facilitating the interpretation of the coefficients. Estimated γ's are simply the percentage change in dependent variable caused by a changes in X. The advantages of estimating in logarithms rather than levels becomes clear if we think about comparing a six-month departure from an 18-month presumptive sentence and a six-month departure from a 48-month presumptive sentence. Clearly, the former represents a more significant proportionate departure than the latter, but estimating the equation in levels forces these two departures to have identical meanings, making interpretation of the estimates γ's less than obvious (Bushway and Piehl 2001, p. 747).

A third empirical issue involves developing an appropriate measure of the presumptive sentence. Table 1 reports summary statistics for 12 representative crimes found in the nineteenth century data (described below). The first column reports the statutory guidelines in 1794 and the second column reports the revised guidelines adopted in 1829.[15] Column 3 reports the number of

[14] Preliminary regressions that estimate β suggest that it was generally less than one, which suggests that contemporary juries tended to impose sentences less harsh than the presumptive sentence. In isolating jury discretion, it is more important to fix β than to impose any specific value on it.

[15] A comprehensive search of Pennsylvania session laws between 1790 and 1880 found that statutory recommendations changed for some infractions after 1829, but there was no overhaul of the criminal code as occurred in 1829.

commitments to Pennsylvania penitentiaries between 1826 and 1876 and the fourth column reports average sentences. *A priori* there is no correct choice of recommended sentence other than it should reflect an understanding of the guidelines and the institutional structure in which they are established and enforced (Bushway and Piehl 2001, p. 747). Pennsylvania statutes provided wide limits within which juries were given substantial discretion, yet mean sentences, with few exceptions, approximate the midpoint of the sentencing ranges. Regressions are estimated using the midpoint as the presumptive sentence.

A fourth empirical issue involves how best to model the sentencing decision, because how we conceptualize jury processes will determine the preferred estimation strategy. Like Albonetti (1997) and Bushway and Piehl (2001), I assume that the sentencing decision occurs at a single stage. That is, upon convicting the defendant and after having received any instruction about the law from the judge, the jury chose sentence length, a choice that naturally included a sentence of length zero. The modern literature remains divided about how best to model the sentencing decision. Some, like Albonetti (1991), argue that it is best modeled as a two-stage process: the decision to incarcerate is made, followed by the choice of sentence length if incarceration is warranted. Others, like Bushway and Piehl (2001), estimate a single-stage model, assuming that the incarceration decision is inseparable from the sentence length decision. The latter approach holds that sentence choice occurs as part of a single process where a zero-length sentence is a possible outcome, even probable in some instances. I follow the latter approach.

Regardless of how the sentencing decision is formulated (one stage or two), it is imperative to recognize that sentences are censored. Censoring occurs because observed sentences fall between lower and upper threshold values. Two censoring points emerge in the nineteenth century data. First,

only criminals convicted and sentenced to serve more than 12 months were to be sent to one of the state prisons. Criminals sentenced to less than 12 months were to serve their sentences in a city or county jail. For as yet unknown reasons, 332 criminals whose records appear in the prison records served less than 12 months in the prison and the shortest terms observed in the final sample are three months. These observations are retained because they provide information on contemporary sentencing practices. Second, the longest observed term is 29 years (348 months) and there are a handful of sentences between 20 and 22 years. These were not, however, the harshest sentences handed down by nineteenth century Pennsylvania courts. Statutory law considered first degree murder a capital crime. It is reasonable to assume that hanging was a more severe punishment than 29 years in prison, so there is an unobserved upper censoring point because the data contain no information on the number of hangings nor the characteristics of those executed. For unknown reasons juries sentenced 35 convicts to an average 100-month term for first degree murder despite the presumptive death penalty. These observations, too, are included in the statistical analysis.

Bushway and Piehl (2001, pp. 745-46) offer a useful way to think about censoring observed in criminal sentences. Assume there is a latent variable y* (sentence length) that is only observed when it falls between the lower and upper bounds. The Tobit model was designed to solve the censoring problem by including all available information into a likelihood function under the assumption that y* would be normally distributed if it was fully observed. Thus, we can write the Tobit likelihood function for presumptive nineteenth-century sentences as:

$$y_i = \begin{cases} 0 \text{ if } y_i^* \leq 12 & \text{--> } F_N(0 \mid \mu_i, \sigma) \\ y_i^* \text{ if } 12 \leq y_i^* < 360 & \text{--> } f_N(y_i \mid \mu_i, \sigma) \end{cases}$$

$$\iota\ 360 \text{ if } y_i^* \geq 360 \qquad \text{--->} \qquad 1\text{-}F_N\,(360 \mid \mu_i, \sigma)$$

which is, in effect, the product of two probits and a linear regression model, a feature exploited here. Because sentences less than one year are not fully observed and sentences in excess of 360 months are not observed at all, the effects of extralegal factors are estimated using linear regressions on the noncensored values. Least squares estimates are, of course, inefficient and potentially biased toward zero compared to maximum likelihood estimates, but Green (1990, pp. 730-31) reports that least squares estimates scaled by the inverse of the proportion of nonlimit to total observations will approximate the maximum likelihood Tobit estimates. Given that 98 percent of the observations are nonlimit, the resulting least squares estimates should not exhibit a significant bias, though the extent to which they are inconsistent is unknowable.

Taken together, the choices of how best to handle the data imply the following estimating equation:

$$\ln(\text{Sentence length}_i) - \ln(\text{Presumptive sentence}_i) = \alpha + \gamma\, X_i + \epsilon_i$$

Taking logarithms of the actual and presumptive sentences reduces the positive skew exhibited by the sentences in levels and facilitates interpretation of the estimated γ coefficients as the percentage deviation from the presumptive sentence attributable to the characteristics included in X. Second, normalizing by the presumptive sentence is the equivalent of constraining β=1. Imposing this constraint ensures that the estimated γ coefficients reflect jury decision making in the face of statutory guidelines. If β were estimated, the model would implicitly be estimating not only the effect of extralegal factors in jury decision making, but jury responses to the presumptive sentences themselves, which would confound interpretation of the γ's. Finally, because the data do not include

the full set of sentencing outcomes (0 months to the death penalty), the γ coefficients are estimated using ordinary least squares. And because the scaling factor is not substantially different from one (1/0.98), no adjustment is made to the estimated coefficients.

4. Data

The data come from ledgers kept by the wardens at the Pennsylvania's Eastern State Penitentiary in Philadelphia and Western State Penitentiary in Pittsburgh. Extant records from Eastern State include the "Descriptive Registers" maintained between 1829 and 1857 and those from Western State include the "Convict Docket and Descriptive Registers," from 1826 through 1876. These records include basic information about convicts, including their names, ages, nativities, occupations, the crimes for which they were incarcerated, sentence lengths, prior convictions, and counties of conviction. The two "Descriptive Registers" also include identifying information, such as complexion and race. Whites are recorded with complexion descriptions such as dark, ruddy or pale. African Americans were identified as black or mulatto. The registers also included eye and hair color, and brief descriptions of any unique marks, scars or deformities. Prison clerks at Eastern State also recorded each prisoner's height and foot length, probably to facilitate the provision of clothing and shoes.

"Convict Reception Registers," kept between 1842 and 1869 represent the second set of prisoner records from the Eastern State Penitentiary. In addition to the convict information contained in the "Descriptive Registers," the "Reception Records" include information on whether the prisoner was married, literate, temperate, had been bound as an apprentice in his or her youth, and whether

his or her parents were still alive.[16] In all records, the conditions under which the prisoner left the prison was recorded in the remarks section. Most were freed after having served their time. About 10 percent received a pardon and were released prior to serving their entire sentence. Another three percent died in prison and 0.3 percent escaped, a remarkable feat given the Eastern State Prison's architecture.

Given the nature of the data, most series are coded as dichotomous (0,1) variables. The exceptions, of course, are such characteristics as age, sentence length, crime, and prior convictions. Age is recorded in years. Sentence length, though recorded in the prison records in years, is transformed into its equivalent in months, which follows standard practice in the literature on criminal sentencing. In the 50 years of prison records used here, convicted felons were incarcerated for 67 different crimes, ranging from abortion to vagrancy. Table 1, discussed previously, provides information on the crimes with the largest number of incarcerations. More than half of all prisoners (54.4%) were larcenists. Convicted burglars made up another 9.3% of all convicts, counterfeiters 4.3% and arsonists 2.3%. Violent criminals, including those convicted of rape (2.3%), manslaughter (2.3%), attempted murder (2.3%) and second-degree murder (2.8%), represented a sizable minority of prisoners.

5. The determinants of criminal sentences

Table 2 provides summary statistics on variables included in the regression analysis. The average sentence length was 33.2 months and ranged, in the final sample, from three to 348 months. The average prisoner had 0.25 prior convictions, but nearly 84 percent had not been convicted before

[16] The results reported below do not control for marital status, literacy or alcohol abuse because doing so excludes about two-thirds of the observations.

serving the observed sentence. Because there was a small number of convicts with multiple convictions – the maximum was nine prior convictions – the regressions include a dummy variable that equals one if the prisoner had a prior conviction.[17] Table 1 previously reported summary statistics on specific crimes. The basic regression specification includes the two basic criminal indicators – crime type (a series of dummy variables for 18 crime categories) and/or whether the criminal had previously been convicted of a serious felony. If juries respected the rule of law, crime type or severity and criminal history should emerge as the principal determinants of sentence length.

Although the character of the crime will determine sentence lengths if juries follow the law under either the rationalist or utilitarian approach, a large literature investigating modern sentencing behavior finds that personal characteristics also influence the dispensation of criminal justice. To account for any systematic disparities that may have arisen in sentencing, the regressions also include personal characteristics of the offenders. Age and its square is included to capture any age-related disparities. A dummy variable for females is included, as are dummy variables for black and mulatto prisoners. Bodenhorn (2002; 2006) and Bodenhorn and Ruebeck (2007) show that nineteenth-century Americans treated mulattoes preferentially relative to blacks so African American complexion is controlled for in all regressions reported below. Ethnicity is captured with three dummy variables – Irish, German and All Other Immigrants, the last of which is comprised mostly of people from Great Britain, France and Scandinavia. Clark (1973) and Ignatiev (1995) document the discrimination and difficult labor market conditions suffered by Irish immigrants through the 1850s and beyond and Walker (1980, p. 57) considers urban American society at mid-century more "boiling cauldron" than melting pot in which Americans of non-Irish descent "hated

[17] Future research will investigate the determinants of repeat offending. Given the high rates of recidivism among a small proportion of criminals, it is a topic worthy of a study of its own.

the Irish for their poverty, their Catholicism, and their alleged love of drink." Germans were similarly distrusted for their strange customs, foreign language, their fondness for beer instead of whiskey, and their liking to drink it on Sundays. If such resentments and prejudices were salient, they may have manifested themselves as excessive criminal sentences or, in criminological terminology, as unwarranted sentencing disparities based on personal characteristics.

Finally, because some modern critics of the criminal justice system interpret the disproportionate incarceration rate of lower class, low-income groups as evidence of either systemic bias against the poor or of the ability of the wealthy to purchase preferential justice, the regressions control for social class (Walker 1980). Occupations are grouped into one of nine separate categories based on Otis Dudley Duncan's classification scheme (Reiss 1965). Duncan created index scores for hundreds of occupations. Educated professionals, such as doctors, lawyers and college professors populate the highest ranks, followed by managers and small-business proprietors. Common laborers, menial personal service providers (house servants or washerwomen) and unskilled farm workers fall at the bottom of the index. Skilled craftsmen, clerical and sales workers form the intermediate groups. The summary statistics reveal that convicted criminals were drawn from every occupational and social class, but common laborers represented 35 percent of all convicts, followed by craftsmen, proprietors and semi-skilled operatives. Professionals and clerical or sales workers were substantially underrepresented among the prison population.

5.1 Determinants of sentences – full sample

Table 3 reports the basic regressions results for the final sample of 9,925 prisoners committed to the Eastern or Western State penitentiaries between 1819 and 1876. The first column

reports results when controlling only for criminal history and the personal characteristics of the criminals. The second column includes dummy variables for 18 different crimes for which both presumptive and reliable midpoint sentences could be determined.[18] Offense-level controls mitigate one source of potential bias. It may be that individuals convicted of one crime consistently receive a longer sentence than individuals convicted of a second crime even if the presumptive statutory midpoints are the same. If members of a particular group are disproportionately convicted of the crime receiving the longer sentences, and the crime category is not controlled for, it will appear that members of that group receive longer sentences (Mustard 2001). The third column adds controls for decades because public concern with certain crimes may have changed over time. If members of a group were more likely to commit or be convicted of certain crimes in decades in when those crimes were more severely punished, we may incorrectly infer that members of that group received disparate punishments.[19] County fixed effects included in the final column control for any persistent regional factors that may bias coefficients if the convicts' observed or unobserved personal characteristics are correlated with place of conviction. The regressions estimate the extent to which an individual with a certain criminal history, convicted in a given court of a given crime received a sentence that differed from that received by an otherwise comparable individual based on the race, ethnicity, sex or occupation of the first individual. This is the definition of an unwarranted disparity, which will be identified by economically meaningful and statistically significant coefficients on

[18] Including only 18 of the 67 crimes reduces the sample from 10,952 to 9,925. Some observations were dropped because statutory guidelines for the crimes could not be located, because there were only a handful of incarcerations for the crime in question (abortion, incest, obscenity), or both. The included crime categories are arson, adultery, bigamy, breaking and entering, burglary, counterfeiting, forgery/fraud/embezzlement, kidnaping, larceny, manslaughter, murder (first-degree), perjury, rape, receiving stolen goods, rioting, robbery, and sodomy. Second-degree murder is the excluded category in all cases.

[19] The excluded category is the eleven-year period between 1819 and 1829.

observable personal characteristics.

Column 1 of table 3 indicates that, controlling only for criminal history and personal characteristics, convicts with a prior conviction received a sentence 27 percent (= $e^{0.242}$) longer than a first-time offender. Older offenders received longer sentences. Fifty year-olds received the maximum sentence while the average age of all convicts at intake was just 28 years. Irish offenders served about 5 percent, or about 1.7 months, shorter prison terms, holding all else constant.

Columns 2 and 3 sequentially include crime-category and time controls, but Column 4 reports the preferred specifications with the full set of crime, time and place controls. Once the full set of controls are included, a number of notable disparities emerge. The age at which sentences reach their maximum is just 32.5 years, fairly close to the mean age at incarceration. Evaluated at the mean sentence of 33 months, women served 3.4 fewer months than men. African Americans did not receive different sentences than native-born whites. Immigrants, however, received somewhat shorter sentences than native-born whites. Irish immigrants served 2.6 fewer months in prison, Germans 1.7, and other immigrants about 1.2 fewer months in prison than native-born whites.

Shorter sentences for immigrants fails to accord with historical accounts of the immigrants' treatment by natives in other dimensions. Clark (1973) and Ignatiev (1995) offer accounts of uprooted and poverty-stricken Irish who made their way to America only to be confronted with powerful nativist prejudices. Discrimination, according to Clark, encouraged the Irish to look to each other for support and this inward-looking attitude resulted in a strong Irish nationalism reinforced by the Catholic Church, parochial schools, and a dense network of mutual aid societies. Unlike African Americans who established a comparably self-reliant community (Nash 1988) in the face of virulent racism and social ostracism, the Irish not only became "white," but developed a powerful

political presence in urban America (Ignatiev 1995). The German experience was similar. Steinberg's (1989) account of nineteenth-century Philadelphia criminal justice offers two reasonable explanations for the apparent preferential treatment afforded immigrants. First, he notes how immigrants often crowded into courtrooms when one of their own was on trial. Some came to observe; others came to influence the outcome. Second, two of Philadelphia's long-time quarter session judges were of Irish descent and were well known for their lenient treatment of Irish defendants who appeared in their courtrooms.

Coefficients on the occupation categories in Column 4 suggest that the connection between a defendant's economic resources and the quality of his or her legal defense are not as simple as the popular media might lead us to believe. Common wisdom holds that because wealthier defendants can mount more vigorous defenses they avoid prison or are sentenced to shorter terms to be served in lower security prisons.[20] The available data contain no information on defendant wealth or access to economic resources beyond his or her pre-incarceration occupation. Studies making use of nineteenth century censuses find that occupation and wealth are highly correlated, so broad occupational categories are used here to proxy for access to economic and legal resources (Bodenhorn and Ruebeck 2007; Ferrie 1994). Individuals in the four high-status and, generally, high-wealth groups (Professionals, Proprietors, Sales and Clerical) received longer sentences than farmers, craftsmen, factory operatives, and laborers. Professionals, for instance, received 18 percent (6.2 months) longer prison terms than laborers. Salesmen received 15 percent (5 months) longer sentences and proprietors and clerical workers each received sentences about 10 percent (3.4 months) longer. Although professionals, proprietors and clerical workers received longer sentences

[20] See popular debates surrounding O.J. Simpson and Martha Stewart trials.

than blue-collar workers when they were convicted of crimes involving the violation of a trust, such as counterfeiting, fraud and forgery, they also received longer sentences for arson, burglary, murder and rape. Common men dominated juries during the "age of the common man" and these men seemingly had little patience for criminal acts committed by wealthy, successful and powerful men.

How are we to interpret the regression results found in Table 3? First and foremost, based on the proportion of the variance explained, the decade in which a criminal was convicted had the largest effect. The unreported decadal coefficients are individually and jointly significant and range between 0.10 in the 1850s to 0.30 in the 1870s and their inclusion raises the adjusted R^2 from 0.03 to 0.23.[21] Among individual characteristics, a convict's criminal history had the largest effect on sentence length; having one or more prior convictions increased the sentence length by about 25 percent. A defendant's age influenced the jury's decision, but race did not. Women received significantly shorter sentences. The effects of ethnicity and occupational status also influenced jury decision making, but not in ways that traditional historical accounts would have predicted. Immigrants received shorter sentences, all else equal, while higher status men in more lucrative occupations received longer sentences across the entire gamut of crimes.

The standard approach holds that if personal characteristics systematically influence the dispensation of criminal justice, the system is subject to abuse and prone to discriminatory practices. An alternative approach holds that if society can be divided into identifiable groups based on differences in crime supply functions, differential punishments may be called for if people respond to incentives in predictable ways (Farmer and Terrell 2001). The difficulty, as Mustard (2001) notes,

[21] It is common in the disparities literature for the regressions to explain only a small fraction of the overall variance. That the preferred specification (Column 4) explains nearly one-fourth of the variance increases my confidence that the regressions are identifying consequential patterns.

lies in the often observed higher criminal propensities of racial and ethnic minorities and immigrant groups, which confounds a sorting out the competing explanations because they generate similar empirical implications. On their face, the results shown in Table 3 are economically counterintuitive. The opportunity cost of prison sentences was higher for people with greater earning power outside prison, so that efficient criminal pricing – holding the probability of detection and conviction constant – would imply shorter rather than longer sentences for wealthier professionals and vice versa for low-income immigrants. It may have been the detection and conviction rates were lower for white-collar workers. Or, it may have been that blue-collar jurors exacted a kind of class-based retributive justice against those with greater economic resources. Future research will seek to sort out the relative contribution of these two effects.

5.2 Violent and non-violent crimes

Tables 4 and 5 parse the data by broad crime category to reveal any subtleties in sentencing not apparent when all crimes are considered together. The tables report the same sets of regressions as Table 3, except Table 4 includes only those convicts imprisoned for violent crimes -- murder, manslaughter, rape and robbery – while Table 5 includes only those convicted of nonviolent offenses, such as adultery, bigamy, burglary, counterfeiting, forgery, fraud, perjury, receiving stolen goods and sodomy.

Because the results are similar regardless of whether the regressions control for crime, time and place, the discussion focuses on the preferred specifications reported in Column 4. In cases involving violent crime, criminal history and age mattered. No other personal characteristic did. A prior conviction increased the defendant's sentence by nearly 15 percent or 4.9 months. Maximum

sentences were handed out to 45 year-old offenders. Neither sex, race, ethnicity nor occupation exerted any meaningful influence on sentence length. Some of the coefficients on occupations are large, but they are imprecisely estimated. The lack of significant coefficients in regressions of sentences on personal characteristics suggests that juries took their charge to protect the public from dangerous felons seriously and did not systematically treat defendants of any particular race, ethnicity or social class differently.

It is in sentencing of nonviolent offenders that juries exhibited discriminatory tendencies. Criminal history was a principal determinant of sentence length for nonviolent offenders. Having a prior conviction increased sentence length by about 23 percent so that the average recidivist served about 7.6 months more than the typical first-time offender. Maximum sentence length occurred for 55 year-old offenders, or those about twice the average age of prisoners at intake. Women received sentences about 13 percent shorter than men, all else constant. Race had no notable effect on sentence length, but Irish and German immigrants received sentences about 6 to 9 months shorter than native-born whites. Other immigrants were also treated leniently but not to the same extent.

Whereas occupational status had no effect on the sentences given violent offenders, it had a large influence on the sentences imposed on nonviolent offenders. Professionals and salesmen received 17 percent (5.8 months) longer sentences; proprietors and clerical workers 10 percent (3.3 months) longer sentences. When nonviolent offenders are considered alone, farmers also received significantly longer sentences. Thus, whereas juries relied principally on the facts of the crime in handing down sentences for violent offenders, personal characteristics systematically influenced the dispensation of justice for nonviolent offenders. But the effects run counter to the standard interpretation of the era that blacks, the Irish and other immigrants were ill-treated and that the

wealthy and powerful could bend the system to their will.

5.3 Regional and urban effects

Aggregation bias plagues many modern studies of criminal sentencing. Pope and Feyerharm (1990) and Kramer and Ulmer (1996) note that it is common to uncover differences in sentencing based on personal characteristics in certain local jurisdictions only to have those effects disappear when data from several jurisdictions are combined for analysis. Some contemporaries, such as black activist Martin Robison Delany, noted general differences in the treatment of African Americans in eastern and western regions of nineteenth century Pennsylvania (Sterling 1971). He found the citizens of Pittsburgh to be more welcoming than those in Philadelphia, where Nash (1988) also found evidence of virulent racism. It is also well known that cities are more conducive to crime because they afford greater rewards for criminal activity and reduced probabilities of detection. Glaeser and Sacerdote (1999), for example, attribute more than half of higher urban criminal propensities to social, family and personal characteristics, only some of which are observable in nineteenth-century data. If nineteenth-century juries also believed there was some correlation between observable characteristics and a defendant's propensity toward criminal activity, statistical discrimination may have led to systematic sentence differentials.

Table 6 provides regional coefficient estimates for the pooled sample, as well as violent and nonviolent crimes separated by the prison in which the convicted served his or her sentence. Criminals convicted in roughly the eastern half of the state were sent to the Eastern State Penitentiary, while those convicted in the western half served their terms at Western State. Thus, place of incarceration controls for region of conviction, with most convicts coming from counties

including or contiguous to the state's two large metropolitan areas. Each regression includes the full set of crime, time and place controls.

The results in Table 6 reveal some regional differences in sentencing behavior. Tests of differences in coefficients were performed and a dagger (†) following a coefficient implies that it is significantly different ($p<0.05$) from the coefficient in the preceding column. Although juries throughout Pennsylvania imposed longer sentences on repeat offenders, western juries sentenced repeat offenders to significantly longer terms than eastern juries. At the same time, however, western juries handed down significantly shorter sentences for female offenders, regardless of the nature of the offense.

African American offenders generally received sentences no longer than native-born whites, but sentences imposed by eastern juries (modestly longer than for whites) were significantly different than sentences imposed by western juries (modestly shorter than whites). With the exception of the *Other Immigrant* group, no notable regional differences in the treatment of ethnic offenders emerge from the data. Irish and German convicts typically received shorter sentences than native-born whites, especially for nonviolent offenses. No consistent pattern of differential treatment based on occupational status is evident.

Table 7 considers only those convicted in Philadelphia and Allegheny counties (roughly Philadelphia and Pittsburgh cities). Pittsburgh juries sentenced repeat offenders to longer prison terms than Philadelphia juries. Women received shorter sentences from Pittsburgh juries, as did African Americans, proprietors and other immigrants. Germans and service workers received longer sentences. In brief, no consistent pattern of prejudicial treatment emerges when the data are separated by region or major city and analyzed separately.

5.4 Determinants of excessive sentences

Table 8 provides marginal effects from four probit regressions in which the dichotomous dependent variable equals one if the convict received a sentence exceeding the 90th percentile of sentences for the crime for which he or she was convicted. To guard against having the results driven by the inclusion of one or two particularly long sentences, only those crimes for which more than 50 convicts were sent to prison are included. For most crimes, 100 or more sentences are observed (see Table 1).

As in every previous regression, criminal history remains a powerful predictor of sentence length. Having been previously convicted of a serious felony increased the likelihood of receiving an excessive sentence by about 12 percent, regardless of whether the sentence was handed down in eastern or western Pennsylvania, or one of the state's principal metropolitan areas.

Regional differences in jury behavior also emerge in the distribution of excessive sentences. Women in the western half of the state were about 9 percent less likely to receive a harsh sentence; and women convicted in Allegheny County (Pittsburgh) were about 12 percent less likely to receive an especially punitive sentence. Notable regional differences are also evident in the likelihood of African Americans receiving an excessive sentence. Whereas they were more likely to receive a longer term in eastern Pennsylvania, they were 5 to 9 percent less likely to receive a harsh sentence in the western half of the commonwealth. The only notable ethnic difference is for German immigrants in Philadelphia.

Large and significant increases in the likelihood of individuals from particular occupational classes receiving an excessive sentence are evident for professionals in western Pennsylvania and Pittsburgh. Proprietors were also 8 to11 percent more likely to receive an excessive sentence,

relative to laborers, in both eastern and western Pennsylvania, though the effect disappears in the urban samples. Salesmen, clerical workers and craftsmen were sometimes more likely to receive an excessive sentence, but no consistent regional pattern emerges and the coefficient on the craftsman variable is modest in size.

A focus on excessive sentences reveals some subtleties to nineteenth-century criminal sentencing that is not evident from least squares analysis of all sentences. While the earlier OLS estimates fail to reveal any persistent discrimination against African Americans, an analysis of excessive sentences reveals that blacks and mulattoes convicted in eastern Pennsylvania were more likely to receive an especially punitive sentence, even after controlling for several features of the crime and the criminal's personal characteristics. Other than more lenient treatment of women in the west, no consistent pattern emerges in correlating other personal characteristics with the incidence of excessive sentences.

6. Concluding remarks

A fundamental problem in comparing modern and historical episodes in criminal sentencing follows from the centrality of the jury in the nineteenth century and that of the judge in the modern era. Albonetti (1991; 1997) believes that modern judges are subject to prejudices that may lead to systematically longer sentences for some groups as a result of statistical discrimination. The purpose of this study is to understand how nineteenth-century juries navigated the complexities of imposing punishments when they were just as racked by uncertainty and bounded rationality as modern judges and just as subject to well-known social prejudices against certain groups. Unlike judges constrained by formal or informal sentencing guidelines, nineteenth century juries acted with considerable

discretion. Uncertainty and bounded rationality does not excuse the discriminatory exercise of discretion, but it sheds light on the extent to which racial and ethic stereotypes had salience for juries. When a jury's members attributed certain criminal propensities to different groups, sentencing disparities naturally emerged.

The data reveal several unexpected results. Traditional accounts of the era portray rampant and virulent racism and nativism, yet African Americans and immigrants were not subject to unwarranted sentencing disparities. On average, blacks were treated no differently than native-born whites at the sentencing stage of the criminal process, though they were somewhat more likely to receive an excessive sentence. A second intriguing result is the favorable treatment of immigrants, especially the Irish. Not only were Irish criminals not subject to longer prison terms following conviction, they typically received substantially shorter sentences. Only additional research will provide a deeper understanding of this result, but it may provide some evidence for the power of weak ties (Granovetter 1973; 1983). Social interactions are believed to positively influence the level of criminal activity (Glaeser, Sacerdote and Scheinkman 1996; Patacchini and Zenou 2008), and may have done so in the mid-nineteenth century. Weak ties and social interactions created through social clubs, churches, and beneficial societies may have worked to the benefit of immigrant offenders because they provided resources or other forms of support for criminal offenders. Alternatively, if the traditional interpretation of immigrant poverty and misery are accurate, contemporary juries may have taken pity on immigrant offenders or believed that they posed a lesser threat than native-born criminals. Future research may be able to sort out the relative importance of these alternative explanations.

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Table 1: Representative crimes, presumptive sentences, commitments and mean actual sentences

Crime	Guidelines (1794) (months)	Guidelines (1829) (months)	Commitments	Mean sentences (months)
Arson	60 - 144	12 - 120	272	52.1
Assault	none	none	216	31.2
Bigamy	0 - 24	0 - 24	97	16.1
Burglary	0 - 120	24 - 120	1020	50.3
Counterfeiting	48 - 180	12 - 84	472	35.7
Horse theft	0 - 84	12 - 48	550	
Larceny	0 - 36	0 - 36	5961	24.3
Manslaughter	24 - 120	24 - 72	247	41.1
Murder (2d)	60 - 216	48 - 144	315	93.5
Rape	120 - 252	12 - 144	258	66.7
Receiving stolen goods	0 - 24	0 - 24	207	25.4
Robbery	0 - 120	12 - 84	322	46.6

Notes: 1794 presumptive sentences for bigamy, burglary, larceny, receiving stolen goods and robbery were enacted in 1790. There was no presumptive sentence for assault. Tyson (1827, p. 61) reports that the "punishment of [sic] assault and battery, being at common law, may be for any period."
Sources: Pennsylvania General Assembly, Vol. II (1810, chapter MDV); Pennsylvania General Assembly, Vol III (1810, chapter MDCCLXVI); Pennsylvania General Assembly (1829, chapter 204); Tyson (1827); Wedgwood (1844); Eastern State Penitentiary (1829-1857); Eastern State Penitentiary (1842-1869); Western State Penitentiary (1826-1876).

Table 2: Summary statistics for convicts sentenced to Pennsylvania penitentiaries, 1819-1876

Variable	Description	Mean	Std Dev
Sentence length	Number of months convict sentenced to serve	33.19	28.53
Prior convictions	Number of prior convictions	0.25	0.68
Age	Defendants age in years at intake	28.82	10.34
Female	'=1 if defendant had female given name	0.04	
Black	'=1 if recorded as "black"	0.09	
Mulatto	'=1 if recorded as "mulatto"	0.07	
Irish	'=1 if born in Ireland	0.10	
German	'=1 if born in a German state	0.07	
Other immigrant	'=1 if born outside US, Ireland and Germany	0.07	
Professional	'=1 if employed in professional occupation	0.01	
Clerical	'=1 if employed in clerical position	0.02	
Sales	'=1 if employed as salesman	0.01	
Labor	'=1 if employed as common laborer	0.35	
Operative	'=1 if employed in semi-skilled manufacturing	0.18	
Service	'=1 if employed in services	0.07	
Craftsman	'=1 if employed as skilled craftsman	0.26	
Proprietor	'=1 if self-employed proprietor	0.20	

Notes: Professionals are mostly doctors and lawyers; Clerical are clerks and bookkeepers; Services are housekeepers, stewards, washerwomen, etc.; Craftsmen include blacksmiths, coopers, saddlers, etc.; Proprietors are boarding house keepers, shopkeepers and merchants.
Sources: Author's calculations from information found in Eastern State Penitentiary (1829-1857), Eastern State Penitentiary (1842-1869), Western State Penitentiary (1826-1876). Occupations classified consistent with categories reported in Reiss (1965).

Table 3:
Determinants of Sentencing Outcomes -- Full Sample

	(1)	(2)	(3)	(4)
Multiple priors	0.242 (0.173)***	0.242 (0.016)***	0.242 (0.015)***	0.212 (0.016)***
Age	0.010 (0.003)***	0.013 (0.003)***	0.011 (0.003)***	0.013 (0.003)***
Age squared	-0.0001 (0.00004)***	-0.0002 (0.00004)***	-0.0001 (0.00004)***	-0.0002 (0.00003)***
Female	0.002 (0.033)	-0.123 (0.031)***	-0.111 (0.030)***	-0.141 (0.030)***
Black	0.032 (0.022)	0.026 (0.020)	0.016 (0.021)	-0.006 (0.021)
Mulatto	0.034 (0.025)	0.027 (0.023)	0.027 (0.023)	0.010 (0.023)
Irish	-0.054 (0.022)**	-0.073 (0.020)***	-0.059 (0.020)***	-0.081 (0.020)***
German	-0.020 (0.025)	-0.070 (0.022)***	-0.050 (0.022)**	-0.054 (0.022)**
Other Immigrant	0.030 (0.026)	-0.018 (0.023)	-0.016 (0.023)	-0.036 (0.023)
Professional	0.108 (0.065)*	0.217 (0.063)***	0.218 (0.062)***	0.171 (0.056)***
Proprietor	0.050 (0.049)	0.146 (0.043)***	0.148 (0.042)***	0.097 (0.041)***
Sales	0.088 (0.071)	0.169 (0.064)***	0.185 (0.064)***	0.140 (0.067)**
Service	0.011 (0.027)	0.041 (0.025)*	0.035 (0.025)	0.019 (0.024)
Operative	-0.030 (0.019)	-0.002 (0.017)	-0.002 (0.017)	-0.013 (0.017)
Craftsman	0.009 (0.017)	0.026 (0.015)*	0.028 (0.015)*	0.012 (0.015)
Clerical	0.043 (0.055)	0.140 (0.049)***	0.145 (0.048)***	0.090 (0.048)*
Farmer	-0.016 (0.032)	0.039 (0.029)	0.037 (0.029)	0.033 (0.029)

Table 3:
Determinants of Sentencing Outcomes -- Full Sample

	(1)	(2)	(3)	(4)
Unknown occupation	-0.005 (0.040)	0.085 (0.034)**	0.063 (0.034)*	0.036 (0.034)
Constant	-0.289 (0.053)***	-0.405 (0.051)***	-0.583 (0.075)***	-0.866 (0.109)***
Observations	9925	9925	9925	9922
Adj R^2	0.03	0.03	0.23	0.27
Crime Controls	No	Yes	Yes	Yes
Decade Controls	No	No	Yes	Yes
County Fixed Effects	No	No	No	Yes

Notes: Dependent variable = ln(sentence) - ln(recommended range midpoint). Robust standard errors reported in parentheses. *** = $p < 0.01$; ** = $p < 0.05$; * = $p < 0.10$. Crime categories include: First Degree Murder, Arson, Rape, Counterfeiting, Manslaughter, Adultery, Robbery, Sodomy, Larceny, Bigamy, Receiving Stolen Goods, Forgery/Fraud/Embezzlement, Rioting, Kidnaping, Perjury, and Breaking and Entering. Second Degree Murder is the excluded category. Decades include 1830s through 1870s. The 1820s are the excluded category. The data include criminal incarcerations from 86 counties.
Sources: Author's calculations from data reported in Eastern State Penitentiary (1829-1857; 1842-1869) and Western State Penitentiary (1826-1876).

Table 4:
Determinants of Sentencing Outcomes -- Violent Crime Only

	(1)	(2)	(3)	(4)
Multiple priors	0.165 (0.070)**	0.113 (0.072)	0.106 (0.072)	0.137 (0.69)**
Age	0.029 (0.009)***	0.030 (0.009)***	0.031 (0.009)***	0.036 (0.009)***
Age squared	-0.0004 (0.0001)***	-0.0004 (0.0001)***	-0.0004 (0.0001)***	-0.0004 (0.0001)***
Female	-0.110 (0.125)	-0.123 (0.126)	-0.109 (0.125)	-0.150 (0.128)
Black	-0.0003 (0.070)	0.025 (0.070)	0.037 (0.072)	0.089 (0.074)
Mulatto	0.026 (0.069)	0.048 (0.070)	0.061 (0.072)	0.066 (0.073)
Irish	-0.038 (0.051)	-0.015 (0.051)	-0.014 (0.051)	-0.016 (0.051)
German	-0.108 (0.098)	-0.057 (0.097)	-0.033 (0.097)	0.032 (0.100)
Other Immigrant	-0.022 (0.078)	-0.016 (0.076)	-0.014 (0.077)	-0.015 (0.075)
Professional	0.100 (0.271)	0.147 (0.252)	0.0143 (0.257)	0.258 (0.264)
Proprietor	0.107 (0.174)	0.082 (0.172)	0.101 (0.168)	0.145 (0.158)
Sales	0.170 (0.271)	0.111 (0.270)	0.0132 (0.272)	0.088 (0.281)
Service	0.116 (0.080)	0.080 (0.081)	0.072 (0.081)	0.115 (0.084)
Operative	0.005 (0.054)	-0.008 (0.053)	0.002 (0.053)	0.041 (0.054)
Craftsman	0.032 (0.050)	0.013 (0.050)	0.015 (0.050)	-0.007 (0.052)
Clerical	0.107 (0.160)	0.061 (0.162)	0.066 (0.161)	0.127 (0.148)

Table 4:
Determinants of Sentencing Outcomes -- Violent Crime Only

	(1)	(2)	(3)	(4)
Farmer	-0.117	-0.110	-0.129	-0.128
	(0.091)	(0.089)	(0.090)	(0.086)
Unknown occupation	-0.168	-0.221	-0.241	-0.256
	(0.122)	(0.130)	(0.132)*	(0.137)*
Constant	-0.854	-0.903	-0.518	-1.612
	(0.154)***	(0.156)***	(0.231)**	(0.309)***
Observations	1259	1259	1259	1259
Adj R^2	0.02	0.06	0.06	0.18
Crime Controls	No	Yes	Yes	Yes
Decade Controls	No	No	Yes	Yes
County Fixed Effects	No	No	No	Yes

Notes: Dependent variable = ln(sentence) - ln(recommended range midpoint). Robust standard errors reported in parentheses. *** = $p < 0.01$; ** = $p < 0.05$; * = $p < 0.10$. Violent crimes include: Murder, Manslaughter, Rape and Robbery. Decades include 1830s through 1870s. The 1820s are the excluded category. The data include criminal incarcerations from 86 counties.
Sources: Author's calculations from data reported in Eastern State Penitentiary (1829-1857; 1842-1869) and Western State Penitentiary (1826-1876).

Table 5:
Determinants of Sentencing Outcomes -- Nonviolent Crime Only

	(1)	(2)	(3)	(4)
Multiple priors	0.228 (0.018)***	0.246 (0.016)***	0.246 (0.016)***	0.207 (0.016)***
Age	0.007 (0.003)**	0.010 (0.003)***	0.009 (0.003)***	0.011 (0.003)***
Age squared	-0.0001 (0.000)**	-0.0001 (0.000)***	-0.0001 (0.000)***	-0.0001 (0.00004)***
Female	0.008 (0.034)	-0.111 (0.031)***	-0.095 (0.031)***	-0.122 (0.030)***
Black	0.047 (0.023)**	0.037 (0.021)*	0.027 (0.021)	-0.001 (0.021)
Mulatto	0.049 (0.027)**	0.033 (0.024)	0.033 (0.024)	0.012 (0.024)
Irish	-0.023 (0.024)	-0.069 (0.022)***	-0.058 (0.022)***	-0.079 (0.022)***
German	-0.019 (0.025)	-0.070 (0.022)***	-0.051 (0.022)**	-0.064 (0.022)***
Other Immigrant	0.040 (0.028)	-0.017 (0.025)	-0.014 (0.024)	-0.038 (0.023)*
Professional	0.091 (0.066)	0.211 (0.065)***	0.215 (0.06)***	0.163 (0.060)***
Proprietor	0.028 (0.051)	0.151 (0.044)***	0.016 (0.043)***	0.102 (0.042)**
Sales	0.068 (0.072)	0.178 (0.064)***	0.194 (0.065)***	0.161 (0.069)**
Service	-0.012 (0.029)	0.036 (0.026)	0.027 (0.026)	0.006 (0.025)
Operative	-0.004 (0.020)	0.0003 (0.018)	-0.0001 (0.018)	-0.013 (0.018)
Craftsman	0.003 (0.018)	0.030 (0.016)*	0.032 (0.016)**	0.016 (0.016)
Clerical	0.026 (0.059)	0.154 (0.051)***	0.155 (0.050)***	0.098 (0.050)**

Table 5:
Determinants of Sentencing Outcomes -- Nonviolent Crime Only

	(1)	(2)	(3)	(4)
Farmer	0.006	0.064	0.067	0.068
	(0.033)	(0.031)**	(0.031)**	(0.031)**
Unknown occupation	0.005	0.104	0.086	0.048
	(0.042)	(0.035)***	(0.035)**	(0.035)
Constant	-0.211	-0.238	-0.486	-0.645
	(0.056)***	(0.055)***	(0.077)***	(0.105)***
Observations	8666	8666	8666	8663
Adj R^2	0.02	0.23	0.25	0.29
Crime Controls	No	Yes	Yes	Yes
Decade Controls	No	No	Yes	Yes
County Fixed Effects	No	No	No	Yes

Notes: Dependent variable = ln(sentence) - ln(recommended range midpoint). Robust standard errors reported in parentheses. *** = $p < 0.01$; ** = $p < 0.05$; * = $p < 0.10$. Nonviolent crimes include Adultery, Bigamy, Breaking and Entering, Burglary, Counterfeiting, Forgery/Fraud/Embezzlement, Larceny, Perjury, Receiving Stolen Goods and Sodomy. Decades include 1830s through 1870s. The 1820s are the excluded category. The data include criminal incarcerations from 86 counties.
Sources: Author's calculations from data reported in Eastern State Penitentiary (1829-1857; 1842-1869) and Western State Penitentiary (1826-1876).

Table 6:
Determinants of Sentencing Outcomes -- Regional Differences

	(1) East Full	(2) West Full	(3) East Violent	(4) West Violent	(5) East Nonviolent	(6) West Nonviolent
Multiple priors	0.182	0.256†	0.137	0.137	0.175	0.261†
	(0.018)***	(0.028)***	(0.076)*	(0.120)	(0.019)***	(0.029)***
Age	0.015	0.009	0.044	0.027	0.014	0.006
	(0.004)***	(0.004)***	(0.010)***	(0.012)**	(0.004)***	(0.004)
Age squared	-0.0002	-0.0001	-0.0005	-0.0004	-0.0002	-0.0001
	(0.000)***	(0.000)**	(0.000)***	(0.000)**	(0.000)***	(0.000)
Female	-0.092	-0.238†	0.032	-0.786†	-0.078	-0.204†
	(0.036)**	(0.048)***	(0.139)	(0.274)***	(0.036)**	(0.049)***
Black	0.006	-0.068†	0.150	-0.056†	0.012	-0.068†
	(0.023)	(0.040)*	(0.078)*	(0.163)	(0.024)	(0.042)*
Mulatto	0.020	-0.036†	0.082	0.087	0.031	-0.062†
	(0.027)	(0.042)	(0.081)	(0.147)	(0.028)	(0.045)
Irish	-0.097	-0.060	0.004	-0.029	-0.102	-0.060
	(0.027)***	(0.029)**	(0.069)	(0.078)	(0.029)***	(0.032)*
German	-0.065	-0.049	-0.066	0.184†	-0.072	-0.063
	(0.028)**	(0.034)	(0.121)	(0.165)	(0.029)**	(0.034)*
Other Immigrant	0.007	-0.081†	0.072	-0.130†	-0.013	-0.068
	(0.030)	(0.032)**	(0.107)	(0.107)	(0.031)	(0.034)**
Professional	0.167	0.228	0.234	0.346	0.174	0.210
	(0.078)**	(0.084)***	(0.336)	(0.342)	(0.078)**	(0.089)**
Proprietor	0.099	0.106	0.145	0.104	0.100	0.123
	(0.052)*	(0.067)	(0.212)	(0.223)	(0.053)*	(0.068)*
Sales	0.165	0.170	-0.086	0.678†	0.219	0.161
	(0.084)**	(0.103)*	(0.332)	(0.229)***	(0.079)***	(0.112)
Service	0.036	0.042	0.045	0.233†	0.028	0.024
	(0.031)	(0.038)	(0.107)	(0.112)**	(0.032)	(0.041)
Operative	-0.004	0.002	0.032	0.073	0.002	-0.001
	(0.022)	(0.026)	(0.073)	(0.082)	(0.022)	(0.028)
Craftsman	0.021	0.020	0.047	-0.036	0.021	0.026
	(0.020)	(0.022)	(0.071)	(0.077)	(0.021)	(0.023)
Clerical	0.126	0.061	0.039	0.0295	0.146	0.066
	(0.064)**	(0.069)	(0.180)	(0.221)	(0.068)**	(0.071)

Table 6:
Determinants of Sentencing Outcomes -- Regional Differences

	(1) East Full	(2) West Full	(3) East Violent	(4) West Violent	(5) East Nonviolent	(6) West Nonviolent
Farmer	0.066	0.025	-0.016	-0.145	0.095	0.061
	(0.041)	(0.041)	(0.128)	(0.115)	(0.043)**	(0.043)
Unknown occupation	0.040	-0.153	-0.346	-0350	0.05-	-0.085
	(0.037)	(0.095)*	(0.172)**	(0.136)***	(0.037)	(0.102)
Constant	-1.039	-1.201	-1.240	-0.324	-1.131	-0.695
	(0.219)***	(0.100)***	(0.308)***	(0.348)	(0.177)***	(0.107)***
Observations	5314	4608	690	569	4624	4039
Adj R^2	0.35	0.21	0.25	0.19	0.38	0.21
Crime Controls	Yes	Yes	Yes	Yes	Yes	Yes
Decade Controls	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable = ln(sentence) - ln(recommended range midpoint). Robust standard errors reported in parentheses. *** = $p < 0.01$; ** = $p < 0.05$; * = $p < 0.10$. Violent crimes include: Murder, Manslaughter, Rape and Robbery. Nonviolent crimes include Adultery, Bigamy, Breaking and Entering, Burglary, Counterfeiting, Forgery/Fraud/Embezzlement, Larceny, Perjury, Receiving Stolen Goods and Sodomy. Decades include 1830s through 1870s. The 1820s are the excluded category. The data include criminal incarcerations from 86 counties. Sources: Author's calculations from data reported in Eastern State Penitentiary (1829-1857; 1842-1869) and Western State Penitentiary (1826-1876).

Table 7:
Determinants of Sentencing Outcomes -- Urban centers

	(1) Philadelphia-- violent	(2) Pittsburgh-- violent	(3) Philadelphia-- nonviolent	(4) Pittsburgh-- nonviolent
Multiple priors	0.156 (0.076)**	0.035 (0.201)	0.142 (0.023)***	0.260† (0.047)***
Age	0.029 (0.017)*	-0.029 (0.031)	0.003 (0.005)	0.008 (0.007)
Age squared	-0.0004 (0.0002)*	0.0005 (0.0004)	0.000 (0.000)	-0.0001 (0.0001)
Female	0.041 (0.183)	-0.749† (0.202)***	-0.108 (0.045)**	-0.259† (0.069)***
Black	0.304 (0.114)***	-0.222† (0.247)	0.009 (0.032)	-0.082 (0.065)
Mulatto	0.127 (0.114)	-0.182† (0.160)	0.027 (0.040)	-0.020 (0.073)
Irish	-0.057 (0.086)	-0.160 (0.164)	-0.020 (0.043)	-0.030 (0.047)
German	-0.229 (0.155)	0.130† (0.227)	-0.078 (0.045)*	-0.090 (0.050)*
Other Immigrant	0.255 (0.128)**	-0.129† (0.152)	0.040 (0.043)	-0.075† (0.052)
Professional	0.429 (0.251)*	NA	0.178 (0.092)*	0.309 (0.172)*
Proprietor	0.323 (0.158)**	-0.227† (0.201)	0.057 (0.058)	0.171 (0.084)**
Sales	-0.72 (0.371)	0.368 (0.301)	0.346 (0.085)***	0.013† (0.140)
Service	0.007 (0.173)	0.257† (0.142)*	0.054 (0.042)	-0.043 (0.061)
Operative	0.101 (0.111)	0.033 (0.136)	0.019 (0.036)	0.027 (0.044)
Craftsman	0.047 (0.098)	0.062 (0.132)	0.042 (0.032)	-0.010 (0.039)
Clerical	0.212 (0.196)	0.426 (0.134)***	0.107 (0.082)	-0.012 (0.105)

Table 7:
Determinants of Sentencing Outcomes -- Urban centers

	(1) Philadelphia--violent	(2) Pittsburgh--violent	(3) Philadelphia--nonviolent	(4) Pittsburgh--nonviolent
Farmer	0.325 (0.144)**	-0.400† (0.242)*	-0.018 (0.087)	-0.122 (0.133)
`No occupation	-0.369 (0.217)*	-0.104 (0.187)	0.017 (0.045)	-0.102 (0.156)
Constant	-0.014 (0.356)	1.229 (0.504)**	-1.495 (0.163)***	0.143 (0.171)
Observations	267	189	1726	1300
Adj R^2	0.26	0.15	0.53	.25
Crime Fixed Effects	Yes	Yes	Yes	Yes
Decade Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	No	No

Notes: Dependent variable = ln(sentence) - ln(recommended range midpoint). Robust standard errors reported in parentheses. *** = $p < 0.01$; ** = $p < 0.05$; * = $p < 0.10$. Violent crimes include: Murder, Manslaughter, Rape and Robbery. Nonviolent crimes include Adultery, Bigamy, Breaking and Entering, Burglary, Counterfeiting, Forgery/Fraud/Embezzlement, Larceny, Perjury, Receiving Stolen Goods and Sodomy. Decades include 1830s through 1870s. The 1820s are the excluded category.
Sources: Author's calculations from data reported in Eastern State Penitentiary (1829-1857; 1842-1869) and Western State Penitentiary (1826-1876).

Table 8:
Determinants of Excessive Sentences – Regions and urban centers
(marginal effects from probit regressions)

	(1) East	(2) West	(3) Philadelphia	(4) Pittsburgh
Multiple priors	0.128 (0.014)***	0.112 (0.020)***	0.111 (0.027)***	0.122 (0.032)***
Age	0.004 (0.002)*	-0.001 (0.003)	0.004 (0.005)	-0.002 (0.005)
Age squared	-0.00005 (0.00003)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Female	0.013 (0.026)	-0.089 (0.020)***	-0.014 (0.037)	-0.125 (0.029)***
Black	0.084 (0.019)***	-0.048 (0.021)**	0.069 (0.034)**	-0.087 (0.031)**
Mulatto	0.059 (0.021)***	-0.070 (0.019)***	0.054 (0.038)	-0.070 (0.033)*
Irish	-0.022 (0.017)	-0.006 (0.017)	-0.020 (0.033)	0.016 (0.030)
German	-0.026 (0.017)	-0.002 (0.021)	-0.106 (0.033)***	0.019 (0.035)
Other Immigrant	0.039 (0.022)*	-0.002 (0.019)	0.041 (0.038)	-0.010 (0.033)
Professional	0.080 (0.061)	0.109 (0.063)**	0.019 (0.088)	0.233 (0.157)*
Proprietor	0.106 (0.044)***	0.076 (0.043)**	-0.020 (0.053)	0.107 (0.077)
Sales	0.182 (0.082)***	0.116 (0.080)*	0.246 (0.120)**	-0.069 (0.092)
Service	-0.008 (0.021)	0.061 (0.031)**	-0.039 (0.034)	0.033 (0.045)
Operative	0.027 (0.016)*	0.013 (0.016)	-0.015 (0.031)	0.026 (0.028)
Craftsman	0.038 (0.015)***	0.025 (0.014)*	-0.008 (0.029)	0.027 (0.027)
Clerical	0.137 (0.054)***	0.067 (0.048)	0.060 (0.074)	0.092 (0.083)

Table 8:
Determinants of Excessive Sentences – Regions and urban centers
(marginal effects from probit regressions)

	(1) East	(2) West	(3) Philadelphia	(4) Pittsburgh
Farmer	0.029	0.022	-0.069	-0.020
	(0.027)	(0.025)	(0.052)	(0.064)
`No occupation	0.010	-0.041	-0.069	-0.025
	(0.025)	(0.054)	(0.034)*	(0.099)
Observations	5545	4989	2072	1629
Adj R^2	0.11	0.05	0.20	0.07
Crime Controls	Yes	Yes	Yes	Yes
Decade Controls	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	No	No

Notes: Dependent variable = 1 if sentence equals or exceeds the 90th centile of all sentences for that crime. Only those crimes for which 50 sentences are observed are included. Robust standard errors reported in parentheses. *** = $p < 0.01$; ** = $p < 0.05$; * = $p < 0.10$. Decades include 1830s through 1870s. The 1820s are the excluded category.
Sources: Author's calculations from data reported in Eastern State Penitentiary (1829-1857; 1842-1869) and Western State Penitentiary (1826-1876).