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STATURE AND CRIME IN EARLY AMERICA

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ABSTRACT

This paper considers the extent to which crime in early America was conditioned on height. With data on inmates incarcerated in Pennsylvania state penitentiaries between 1826 and 1876, we estimate the parameters of Wiebull proportional hazard specifications of the individual crime hazard. Our results reveal that, consistent with a theory in which height can be a source of labor market disadvantage, criminals in early America were shorter than the average American, and individual crime hazards decreased in height.

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1. Introduction

Does an individual's genetic inheritance influence his or her propensity toward criminality? Nineteenth-century biological determinism, whose practitioners believed in genetic predisposition, is now scientifically discredited because there is no evidence that criminals are genetically disposed to criminal behavior. Yet, the absence of genetic predispositions does not mean that human biology and physiology have no bearing on criminal activity (Gottfredson and Hirschi 1990). Economists recognize that heritable physical attributes, such as attractiveness or obesity, translate into differential labor market opportunities (Hammermesh and Biddle 1984; Averett and Korenman 1996). Once differential legitimate job prospects are linked with criminal propensities, as in Becker (1968), economics offers a theoretical link between human physiology and participation in criminal activity. One test of the hypothesis, in fact, finds a statistical association between attractiveness and criminality (Mocan and Tekin 2006).

Instead of attractiveness per se, we investigate whether an individual's height is associated with criminality. Recent economic studies have uncovered positive associations between height and labor market outcomes, which suggest that taller individuals experience labor market advantages and, therefore, have fewer incentives to turn to crime (Persico et al 2005; Case and Paxson 2008). Bodenhorn and Price (2010) found an association between body mass index and criminality using records from 19th century prisons. This paper finds, consistent with several labor market studies, an inverse relationship between crime and adult stature, even after controlling for several features likely to influence an individual's decision to select into criminal activity.

Using data from nineteenth-century Pennsylvania penitentiaries and a continuous-time hazard approach, we find that tall nineteenth-century individuals had a lower hazard of entry into prison. So long as age at entry into prison and age at entry into criminal activity are highly correlated, our study provides evidence on the connection between an individual's physical characteristics and a particular, and perhaps unexpected, labor market realization. Specifically, we find that the hazard of prison entry for individuals in the fifth quintile of stature was 20 to 30 percent lower than for individuals in the first quintile. The lesser hazard for taller individuals holds regardless of the set of included individual controls, and even when we restrict the analysis to violent or property crimes, to rural or urban residence, and to whites and African Americans. Nineteenth-century criminals were short.

We remain agnostic about the mechanism at work. Shorter stature may have been an indicator of lesser cognitive ability then as now (Case and Paxson 2008a), but when we include additional controls designed to capture (albeit imperfectly) cognitive capabilities, namely literacy and pre-incarceration occupation, the independent effect of height is not diminished. If anything, the incarceration hazard is further reduced for the tallest quintile after the inclusion of measures of cognitive abilities. A second possible mechanism may be that nineteenth-century Americans discriminated against short people generally, and that discrimination manifested itself as poorer labor market outcomes that led to a greater hazard of criminal activity. Discrimination is difficult to measure and differentiate from other factors even with data well suited to the purpose. What evidence we have, however, points against generalized discrimination in the criminal justice system. Taller prisoners received longer sentences, all else constant, than shorter criminals. Such evidence, of course, does not and cannot address whether shorter people were more likely to be selected into the criminal justice system, contingent on having been arrested and tried for criminal activity. The evidence, when viewed in conjunction with a growing literature concerning the modern connection between stature and labor market outcomes, suggests that we are identifying an instance in which height has meaningful functional consequences.

2. Height and Labor Market Outcomes

Physical height has been used for many years by economic historians, development economists and anthropologists as a marker of economic prosperity (Steckel 2009; Thomas et al 1990; Thomas and Strauss 1992). Even though genes control about 80% of individual variation in height, environmental factors have a known role and account for the other 20% (Silventoinen 2003; Batty et al 2009). The extent to which a person reaches his or her genetic height potential is correlated to the socio-economic and epidemiological environment in which an individual is raised. Such factors as medical technology, access to health care, attitudes toward preventive medicine, the virulence of the disease environment, pollution, and the availability of nutrients all influence the 20% of individual height not accounted for by an individual’s genetic inheritance (Komlos and Baur 2004).

Broadly speaking, poor nutrition and greater disease incidence slow growth velocity during a person’s formative years and typically reduce attained adult height. Recent evidence suggests real long-term health consequences associated with lesser stature that follow from nutritional deficiencies and greater disease load in youth that carry over into lower adult productivity (Fogel 1994). Further, Waaler (1984) reported an inverted U-shaped pattern of life expectancy in height and Batty et al (2009) reviewed the evidence and reported that height is negatively associated with all-cause mortality, especially with mortality due to coronary heart disease. The relevant physiological explanations include increased pulmonary function, greater lung capacity and wider coronary vessel diameter in taller people.

Because adult height results from a complex interaction of genetic, hormonal, biochemical, disease and nutritional factors during youth and adolescence and because the family has some control over the last two factors, Schultz (2002) contended that height may be appropriately viewed as a consequence of investments in health human capital. His contention is certainly consistent with the recent literature that correlates body size, typically height and/or body mass index, and wages (Schultz 2002; Hübler 2009; Kortt and Leigh 2009; Averett and Korenman 1996). In general, in modern economies taller individuals and those within the normal BMI range receive a wage premium relative to shorter and fatter individuals. Moreover, the body size-market premium relationship has deep historical roots in that Pritchett (1997) found that taller pre-adult slaves were positively selected into the 19th century interstate slave trade.

The empirical association between body size and wages raises the question: what is the mechanism through which height affects wages? It might be that taller or more muscular individuals have a direct productivity advantage in certain occupations. Greater pulmonary function and lung capacity provide an advantage in physically demanding occupations, which was almost certainly the driving factor for the positive selection on height in slave markets. But there might also be indirect wage effects. There is some evidence that taller people are happier and more self-confident and that happiness or self-confidence may improve the productivity of coworkers or it may explain why customers respond favorably to taller employees (Deaton and Arora 2009; Rees et al 2009). Alternatively, customers, coworkers and employers may discriminate against short, fat people.

Two dominant hypotheses currently exist to explain the positive association between height and wages. Persico et al (2004) contend that boys who are taller in adolescence engage in more human capital building activities, and it is the accumulation of human capital in adolescence that is responsible for higher earnings as adults. Specifically, they find that height at age 16 is more positively associated with adult wages than is adult height. Thus, taller teens have different social experiences and developmental trajectories than shorter teens, experiences that include greater participation in athletics and clubs which contributes to the taller teens' social and human capital.

Alternatively, Case and Paxson (2008a) contend that taller people earn more not because of height per se, but because height is a marker for cognitive ability. It is cognitive ability, not height that is rewarded in labor markets. Just as with physiological development, poor nutrition and environmental insults during the developmental years impair cognitive development. When they include measures of cognitive ability in childhood in a wage regression that also includes height, the coefficient on height is reduced by half compared to the coefficient in a regression without a measure of childhood cognitive ability. Further Case and Paxson (2008b) find that the cognitive advantage apparent in childhood persists into old age.

Case and Paxson (2008a; 2008b) do not argue that height is an unimportant determinant of labor market outcomes. Rather, they argue that because height and cognition develop together, the inclusion of height by itself in a Mincer-style wage regression captures a separate cognitive effect. Height remains associated with outcomes even after controlling for cognitive ability, but the size of the association declines. Thus, care must be taken in interpreting the effect of height on labor market outcomes when independent measures of cognitive abilities are unavailable.

In this paper, we consider the consequences of stature on a particular labor market outcome, namely the decision to eschew legitimate employment in favor of criminal activity. We first offer a highly stylized theory of how height can influence the transition into criminal activity. We then test it against data from mid-nineteenth century Pennsylvania state penitentiaries.

3. Theory and Method

We follow the approach of Kiefer (1988), Gyimah-Brempong and Price (2006), Price (2009), and Bodenhorn and Price (2009) and adopt a continuous-time approach to criminal activity. We assume that over their life-cycle, individuals are presented with opportunities for criminal activity, and opt into crime if doing so implies an expected utility greater than the expected utility from engaging only in legitimate activities. Individual transitions into crime are viewed as a hazard that consists of the product of two probabilities or $h(t) = \eta\pi$ where η is the probability that an individual faces an opportunity for criminal activity, and π is the probability that the opportunity for crime is acceptable.

Because height has been found to be positively correlated with earnings, and varies across individuals as a function of income and occupation, the attractiveness of criminal activity for an individual may be a function of his or her height (Case and Paxson 2008a; Deaton and Arora 2009; Hübler 2009; Maloney and Carson 2008). Suppose the probability that criminal activity is acceptable is given by $\pi = \int_{y^*(\phi)}^{\infty} f(y)dy$, where $f(y)$ is the probability of earning y from criminal activity, ϕ is a monotonic measure of an individual's height, and

$y^*(\phi)$ is an individual's reservation earnings from criminal activity defined as the minimum level of earnings from crime at which he would engage in criminal activity. If labor market success, measured by earnings, increases with respect to height then $\partial y^*(\phi)/\partial\phi > 0$. In general, the effect of height on the individual transition into crime is $\partial\pi/\partial\phi = \partial\pi/\partial y^*(\phi)\times\partial y^*(\phi)/\partial\phi$. If the probability that a crime is acceptable decreases with respect to the reservation earnings from crime, then $\partial\pi/\partial y^*(\phi) > 0$, and it follows that the probability that crime is acceptable decreases with respect to height, or $\partial\pi/\partial\phi < 0$.

In examining the effect of height on individual transitions into crime, we use a Weibull proportional hazard regression specification of the form $h(t) = h(o)exp(\sum_i\beta_k X_k)$, where $h(o) = pt^{p-1}exp(\beta_o)$, p is a shape parameter, t is time, and $exp(\beta_o)$ is the scale parameter.[1] For a given explanatory variable X_k, its effect on the individual transition into crime is given by β_k. Given that we observe an individual making a transition into criminal activity at time t, our econometric specification of individual crime hazards allows us to determine the impact that height has on the probability of making a transition into criminal activity, controlling for other factors.

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[1] Let T be a non-negative random variable measuring the time to an event. Then the survivor function—the reverse cumulative distribution function of T—is $S(t) = 1 - F(T) - Pr(T > t)$, where $F(T) = Pr(T \leq t)$. If survival probabilities have a Weibull distribution, then $S(t) = exp(-\lambda t^p)$, where λ is a scale parameter, and p is a shape parameter. If we let $\lambda = exp(\beta_o)$, The unconditional hazard function is:

$$h(t) = [\partial S(t)/\partial t)]/S(t) = [\,exp(\beta_o)\, pt^{p-1} exp(-exp(\beta_o t^p)]/exp(-\lambda t^p) = exp(\beta_o)pt^{p-1}$$

conditioning h(t) on a vector explanatory variables $\sum_i\beta_k X_k$ results in a Wiebull proportional hazards regression model (Cleves, Gould, and Gutierrez, 2004):

$$h(t|\mathbf{X}) = [\partial S(t)/\partial t)]/S(t) = [\,exp(\beta_o)\, pt^{p-1} exp(-exp(\beta_o t^p)]/exp(-\lambda t^p) = exp(\beta_o + \sum_i\beta_k X_k)pt^{p-1}$$

where **X** is a vector of explanatory variables. A Weibull specification of crime assumes that individual crime hazards are a monotone function of time (Gottfredson and Herschi, 1990), which is a plausible assumption if individuals are less or more likely to engage in crime over time.

Because our simple Wiebull specifications may omit covariates that explain individual transitions into criminal activity, we control for possible biases in the parameter estimates by estimating the Weibull hazard specification with unobserved shared frailty. Frailty is defined as an unobservable and random risk factor measuring unobservable individual predisposition toward crime shared by groups of individuals that modifies the individual's crime hazard function.[2] Viewed as a random effect, introducing individual frailty into a Weibull proportional hazard specification identifies causal effects because it is assumed that within and across shared groups the unobserved frailties account for the unobserved covariates that may condition the hazard, thus eliminating any omitted variable bias. If shared frailty also accounts for the unobserved and unmeasured covariates that are potentially important determinants of success or failure in criminal activity, parameter estimates of a Weibull individual crime hazard specification with individual frailty will also mitigate — perhaps even eliminate — any sample selection bias.

4. Data

The data come from ledgers maintained by clerks at Pennsylvania's Eastern State Penitentiary (1829-1869) in Philadelphia and the Western State Penitentiary (1826-1876) in Pittsburgh. At intake the clerks recorded basic information about the prisoners, including

[2] For an overview of proportional hazard models with unobserved frailty , see Cleves, Gould and Guitierrez (2004, Chapter 9), and Wienke (2003). For specific applications of proportional hazard models with unobserved frailty see Price (2009) and Price, Darity, and Headen (2008) In general, a Weibull proportional hazard specification with individual frailty is:

$$h(t|\,\boldsymbol{X}) = \alpha_i\, h(t|\,\boldsymbol{X}) \;=\; \alpha_i\, exp(\beta_o \;+\textstyle\sum_i \beta_k X_k\;\;)pt^{p-1}$$

where α_i is the frailty individual *i*—some unobserved individual specific effect—assumed to have a mean of unity, and a variance of $\theta > 0$.

their names, ages, nativities (country or state), pre-incarceration occupation, the crime for which they were incarcerated, sentence length, prior convictions if any, and the county (and sometimes the specific court) of conviction.

Two Descriptive Registers, one each from the Eastern and Western penitentiaries, also included identifying information such as complexion and race. African Americans were identified as either black or mulatto. Clerks also included sundry white complexions such as dark, ruddy, or pale. The registers also included hair and eye color, as well as brief descriptions of unique marks, scars, tattoos or deformities. Clerks also recorded each prisoner's height and at the Eastern State Penitentiary, they even recorded foot length, probably to facilitate the provision of clothing and shoes. The Eastern State records also included a notation indicating whether the prisoner abused alcohol (intemperance) and, beginning in the early 1840s, whether he was married. In short, the registers provide a wealth of information about the prisoners and open windows into their pre-incarceration lives.

// Table 1 about here //

The state prison data do not measure criminal activity per se, but rather criminal activity that has been reported, investigated, prosecuted, and resulted in a state prison sentence. All measures of crime, however, suffer from some type of filtering by the decisions of agents other than the individual committing the crime, whether it be the victim, the police or the courts. The state prison data have some important advantages over other commonly used crime measures, however. Arrests are generally noisy measures of criminal activity. The decision to make an arrest is often left to the discretion of individual police officers, and many arrests do not even result in prosecutions let alone convictions. Arrest records, moreover, contain only limited information on the characteristics of the suspected criminals. Commitments to jails are dominated by minor offenses, like disorderly conduct, the prosecution of which varies greatly over time and across jurisdictions. In contrast, state prison commitments capture the commission of more serious crimes, those that are more consistently prosecuted across time and space and that perhaps are more compatible with the model of criminal behavior described above.

Table 1 provides names, definitions and summary statistics for the variables used in the empirical analysis. Age at first entry into the penitentiary is the dependent variable. We exclude subsequent convictions and incarcerations for recidivists because we are interested in determining how an individual's characteristics, principally his height, influence his initial transition into criminal activity. The personal characteristics and post-incarceration labor market opportunities of recidivists differ sufficiently from those of first-time offenders that it is reasonable to exclude them (Witte 1980). The average age at first admission was 28.5 years, and ages ranged from 16 to 89 years. The prison accepted a small number of youth, one as young as 11 years, but they are excluded because the factors that influence youth entry into crime may differ systematically from the factors that influence more mature criminal behavior (Thornberry 2005). Modern moderate to chronic offenders demonstrate a tendency to enter into criminal activity between ages 15 and 19. Given the lag between criminal onset and incarceration, we chose age 16 as the relevant cutoff. The results are not materially affected by including the prisoners less than 16 years, or by excluding all prisoners under 18 years.

// Table 2 about here //

Our hypothesis is that an individual's height influences his transition into crime, so height is the independent variable of principal interest. The average prisoner was 66.3 inches tall at initial incarceration, which is considerably below the modern US average of about 70.1 inches and below the 68.5 inch average of white males in 1860 (Komlos and Baur 2004). Line 3 of Table 1 reports an alternative measure of height -- the z-score of height-at-age using standards reported in Steckel (1996). This statistic, too, suggests that nineteenth century prisoners were relatively short.[3] Nineteenth-century Americans were understandably shorter than modern Americans, but were nineteenth century prisoners shorter than the general contemporary population? Table 2 reports age specific height comparisons of men incarcerated in the Pennsylvania prisons and two comparison groups. The first compares white prisoners with enlistees in the Union Army. At every age, prisoners were notably (and statistically significantly) shorter. The second compares black prisoners with a sample of free blacks whose heights were registered with authorities in antebellum Virginia (Bodenhorn 1999). Like whites, black prisoners were shorter at every age than their noninstitutionalized counterparts, and the differences are statistically significant for age groups for which there are a sizeable number of observations. These simple comparisons of sample means are suggestive of an increased hazard of entry into criminal behavior that is correlated with lower stature. Our Weibull hazard specifications control for other individual characteristics to further explore the extent to which stature influenced the transition into crime.

Control variables include an individual's race, ethnicity, and place and time of conviction. If the entering prisoner was African American, the clerks recorded whether the individual was black or mulatto. Whether the clerk inquired into the prisoner's ancestry is unknown, so the mulatto variable may be capturing individuals of mixed race or simply those of lighter than average complexions. We sometimes aggregate both categories into an

[3] We estimated all the regressions reported in Tables 3 through 5 using the z-score instead of the individual's height. The results were comparable in the magnitude of the estimated effect and in statistical significance.

African American variable, but we keep them separate when possible to control for possible complexion effects found elsewhere (Bodenhorn 2006; Bodenhorn and Ruebeck 2007; Goldsmith et al 2006, 2007; Gyimah-Brempong and Price 2006).

We control for ethnicity with indicator variables for Irish, German or other non-US descent. The last category includes individuals from several countries, but most are Canadian, British and Scottish, which is consistent with early to mid-nineteenth century immigration patterns. Because the remaining groups represent a small proportion of incarcerations and were predominantly English-speaking, we include them in a single category.

We include two indicator variables for Pennsylvania's two mid-nineteenth century urban places – Philadelphia and Pittsburgh. In the late twentieth century, cities experienced disproportionate crime rates (Glaeser and Sacerdote 1999). The evidence for the nineteenth century is mixed. Between 1850 and 1870, Philadelphia's population represented between 17.7 and 19.5 percent of the state's population, yet only 15 percent of prisoners were sentenced by Philadelphia courts. Pittsburgh, however, did send a disproportionate share of prisoners to the Western State Penitentiary. Between 1850 and 1870, Pittsburgh accounted for 6.0 to 7.4 percent of the state's population, yet it accounted for 16 percent of incarcerations at Western State. We include these indicator variables to capture any systematic urban influence on crime hazards, such as greater urban anonymity, more opportunities for criminal activity, or greater expected profit following from it.

Finally, we include indicator variables for relevant time periods. If nineteenth-century recessions were associated with higher than average unemployment rates, they may be associated with earlier transitions into crime. Three recessions are included: the back-to-back recessions of 1837 and 1839 are combined because the 1838 recovery between the 1837 and 1839 financial panics was short-lived and the recession is dated by the NBER to have persisted through 1843. The second is the panic and brief recession of 1857/58; and the third is the prolonged and relatively deep recession of 1873.

We also control for three other events or eras that were likely to have influenced employment or crime rates. The crush of Irish immigrants between 1845 and 1849 is known to have taxed municipal administrative capacities, private charities and immigrant aid groups and surely represented a substantial increase in local labor supplies, especially in and around Philadelphia (McCaffrey 1976, Clark 1973). Whether the wave of Irish immigration increased or decreased criminal hazards depends on the net wage effect of the immigration. As Goldin (1994) showed, substantial immigration can have offsetting wage effects. The labor supply curve shifts right, which tends to drive wages down and, potentially, make crime more attractive. It is also possible that the influx of immigrants increases the demand for locally-produced goods, which will simultaneously shift the labor demand curve to the right. Thus, the wage effect is ambiguous a priori. The Civil War, on the other hand, is expected to unambiguously increase wages because the war pulled able-bodied men out of labor markets, which would have driven wages up and made crime relatively less attractive. Finally, we include an indicator variable for the post-Civil War period, which we label Emancipation, to capture the effect of returning soldiers (likely to be a short-term phenomenon) and the effect of former slaves migrating northward and toward Pennsylvania's major urban places, which was part of a longer-term movement of blacks out of the Upper South and into Mid-Atlantic cities (DuBois 1899).

5. Results

5.1 Linear height specifications

Table 3 reports hazard ratios estimated for the full sample of 8,558 usable observations and for several subgroups, including only those prisoners incarcerated for committing theft in its various manifestations (larceny, burglary, horse theft and receiving stolen goods) and violent crimes (assault, manslaughter, murder, rape and robbery), for criminals sentenced in urban (Philadelphia and Pittsburgh) and rural counties, and for whites and African Americans. The full specifications include controls for race, ethnicity, urban conviction (and, presumably, residence) and the recessions and eras discussed above. We estimate the regressions with shared frailty *conditioned on the type of crime* committed by individuals.[4] Conditioning the shared frailty on type of crime controls for any potential unobserved heterogeneity that can condition the individual crime hazard arising from (1) differential treatment of criminals across crime types; (2) differential costs and benefits across crime types; and (3) differential selection of individuals into particular criminal activities[5]

From a diagnostic standpoint, the regressions appear well specified. The chi-square statistics for the log-likelihood ratio tests are all large and statistically significant ($p < 0.001$), as are the z-tests for the appropriateness of the Weibull specification [ln(p)] and the shared frailty assumptions [$\ln(\theta)$]. Moreover, most of the estimated hazards on the control variables are significant and consistent with expectations. African Americans, for example, were at

[4] Our data allow us to identify and measure the type of crime according to the following categories: larceny, burglary, horse theft, receiving stolen goods, assault, manslaughter, murder, rape, and robbery.

[5] In our crime type shared frailty specification, we also report in Table 3, 4 and 6 on specification for Theft only and Violent (crime) only.. Because there are five types of violent crimes (assault, manslaughter, murder, rape and robbery) and four types of theft (larceny, burglary, horse theft and receiving stolen goods), there is sufficient variation by type of crime to identify the parameters conditional on the shared frailty.

about a 10 to 30 percent higher risk for criminal incarceration than whites. Studies of free African American households in antebellum Philadelphia and other Mid-Atlantic cities reveal that they were less wealthy on average than whites and African American heads of households were employed in less lucrative occupations (Hershberg 1973; Bodenhorn and Rubeck 2007). Both features of African American life point toward the attractiveness of crime relative to legitimate labor market opportunities, and explain recurring complaints by contemporaries about disproportionate African American criminality (Curry and Klump 2009; Quillian and Pager 2002).[6] Lower hazards for immigrants appear counterintuitive, but likely follow the fact that many arrived in the U.S. in their mid-twenties and so were not at risk to commit crimes and be sentenced to prison until later ages than were natives.

Estimated hazard ratios on Philadelphia and Pittsburgh are consistent with studies of twentieth century offending. According to Glaeser and Sacerdote (1999), there is more crime in cities because cities offer levels of anonymity not found in rural areas. Relative anonymity reduced the probability of detection, arrest and conviction, which increased the expected returns to crime. Moreover, urban locales often offer more profitable crime opportunities than rural places. Combined, these factors made urban crime potentially more lucrative, which increased the risk of entry into crime.

The effects of recessions on crime hazards are not easily summarized. The recession between 1837 and 1843 witnessed reduced crime hazards. Whereas, the major and persistent 1873 recession increased crime hazards, especially in urban areas and more so for whites than blacks, the short-lived 1857/58 recession had a small and statistically insignificant effect. Crime hazards declined during the massive wave of Irish and German immigration in

[6] Even DuBois (1899) who was sympathetic to the plight of poor African Americans in late-nineteenth century Philadelphia decried their greater criminal propensities.

the 1840s, which suggests that the immigrants increased the demand for locally produced goods sufficiently to have increased the demand for labor and, with it, wages. Crime actually became less lucrative during the post-recession recovery and with the influx of immigrants. During the Civil War, on the other hand, the supply of labor declined as war mobilization siphoned young white males from Pennsylvania's labor markets. Wages increased and crime became less attractive. In the post-war period, however, rapid demobilization and, quite possibly, the migration of former slaves from neighboring Maryland and Virginia drove wages down, made crime more attractive and the era witnessed increased crime hazards.

As for the effects of height on individual crime hazard— our primary interest — greater stature was associated with significantly reduced crime hazards. Across the entire sample and the sub-groups, an inch of additional height reduced the hazard of entering the penitentiary by between 2.2 and 4.7 percent. The result is remarkably consistent for property and violent crimes, for urban and rural places, and for whites and African Americans. The results from the linear height specification imply that greater stature, after controlling for a number of individual characteristics as well as environmental factors, was associated with a lower criminal hazard. This result is fully consistent with existing studies that report a correlation between greater stature and more favorable labor market outcomes (Persico et al 2004; Case and Paxson 2008a). It is also consistent with our hypothesis that greater stature is correlated with lesser criminality. Crime is less lucrative for taller people because they had more attractive legitimate labor market opportunities.

5.2 Quintile height specifications with shared frailty

Hosmer and Lemeshow (1999) contend that it is important with any hazard model to test the linearity assumption for any continuous variable of interest. We estimated the effect by including height, its square and its cube to check for nonlinearities (regressions not reported). Only the linear term was statistically significant. Instead of higher orders of the linear variable, Hosmer and Lemeshow's (1999) preferred method is to categorize continuous variables into sets of mutually exclusive dummy variables. They argue that if the researcher has a prior about how best to categorize the data, he or she should make use of that prior, otherwise start with quartiles or quintiles. Our analysis suggests that quintiles are adequate and we report those results here with shared frailty on the type of crime. Table 4 reports the estimated hazard rates on the height quintiles, excluding the first quintile as the reference group. To save space we do not report the estimated hazard rates on the other control variables because they are not substantively different from those reported in Table 3.

The estimated hazard rates in Table 4 suggest a modestly reduced hazard around the median height, compared to individuals in the first height quintile, which might suggest a labor market return to normality. However, the hazard for criminals in the fifth height quintile is about 20 percent below the hazard for criminals in the first height quintile. Not unexpectedly, crime hazards among the tallest were lower for theft than for violent crimes, probably because violence may have been motivated more by passion rather than rational maximizing calculation. Still, the reduced fifth quintile hazard for violent crime suggests that height was sufficiently well rewarded in legitimate labor markets to make violence appear less rewarding. The height effect is consistent across urban and rural places. It is notable that the height effect was more pronounced among African Americans than whites. The crime hazard for the tallest African Americans was about 35 percent lower than for the shortest, while the crime hazard for the tallest whites was reduced by about 20 percent.

There might be reason to believe that instead of heterogeneity on crime, that there was some heterogeneity across counties, either because of differential treatment of prisoners by courts, or because of differential cost and benefits of crime across counties. In Table 5, we report the results of the quintile height specification with shared frailty conditioned on the county of conviction rather than the type of crime. Conditioning shared frailty on county of conviction rather than type of crime does not appear to substantially change the parameter estimates. There is a modest reduction in crime hazards for individuals in the third quintile and a large and statistically significant reduction in crime hazards for the tallest fifth of individuals. The tall hazard remains lower for theft than for violence and for blacks than whites.

5.3 Decile height specification with shared frailty

As a check for sensitivity to alternative nonlinear specification of height, and because we have no well formed prior concerning the appropriate number of height categories, Table 6 reports parameter estimates when heights are divided into deciles. Because the parameter estimates seemingly are not sensitive to conditioning shared frailty on type of crime or county, we report only type of crime shared frailty estimates for sake of brevity. The full set of controls reported in Table 3 are included here, but not reported. The results are consistent with those reported earlier. There is some evidence of reduced hazards around the middle of the height distribution, but they are small and sometimes not statistically significant. Reduced hazards among the tallest individuals persist, however, and are somewhat more pronounced for those in the tenth rather than the ninth decile. In the full sample estimates, for example, the hazard for individuals in the tenth height decile was about 24 percent lower than that for the shortest individuals. The hazard for individuals in the ninth decile was about 22 percent lower.

The difference between the ninth and tenth deciles for prisoners convicted of theft was about 7 percentage points, whereas the reduced hazards for violent offenders found when using quintiles disappears for the tallest individuals when the heights enter as deciles. On the other hand, the height hazard for the tallest blacks becomes even more pronounced.

5.3 Controlling for cognitive abilities

Last but not least, we attempt to control for the possibility that the effects of stature on the crime hazard are biased due to height being positively correlated with cognitive ability (Case and Paxson, 2008a).The first two columns of Table 7 reports parameters estimates for a subsample of the Eastern State Penitentiary for the period during which clerks recorded whether an entering prisoner was literate. We report results for models including height quintiles and conditioning on shared frailty by county of conviction. Literacy, which was not a well defined characteristic, is an imperfect measure of cognitive ability, but it does capture whether an individual had a very basic contemporary education. In addition to literacy, the third and fourth columns of Table 7 report the height hazards when dummy variables for broad occupational categories are included in the regressions. Again, occupation is an imperfect measure of cognition, but it is not unreasonable to assume that attorneys, doctors and other professionals or shopkeepers and merchants had greater cognitive abilities than common laborers (the excluded group).

The hazards estimated from the Weibull regression reported in the first column include all crimes. The literacy hazard itself is not significantly different from 1, meaning that literacy per se had no influence on entry into criminal behavior. The consequence of including literacy in the regression is to decrease the hazard for the second through fifth height quintiles, all of which are now statistically significant. The size and significance of the fifth height quintile is larger than the other quintiles, however. A test of coefficient equality rejects the null hypothesis of equality of quintiles 2 through 4 with quintile 5 at $p<0.005$ or better. Thus, once we control for literacy alone as a measure of cognition, the height effect does not become smaller and less significant (as in Case and Paxson 2008a) for the tallest individuals. If anything, controlling for cognitive abilities modestly reduces the criminal hazard slightly, which may be interpreted that, after controlling for cognitive ability, the attractiveness of legitimate labor market activities increased for the tallest individuals. Interestingly, the effect also occurs at lesser heights (above the shortest) when we control for literacy.

The second column of Table 7 reports estimated hazard ratios for entry into crime for a sample of prisoners incarcerated for theft. The results are comparable to those for the all-crime sample and the hazard for the tallest quintile is again significantly lower than for all other quintiles ($p<0.0007$ or less).

Columns 3 and 4 report estimated hazards when we include dummy variables for occupational categories, in addition to literacy, as an alternative signifier of cognitive abilities. Again, the results are comparable to earlier estimates. Individuals in the fifth height quintile were at a reduced hazard for entry into crime than those in all other quintiles ($p<0.02$ or less).

6. Conclusions

In this paper we consider the extent to which criminal activity in the nineteenth century was conditioned on an individual's height. With data on convicts incarcerated in Pennyslvania penitentiaries between 1826 and 1876, we estimate the parameters of a Wiebull proportional hazard specifications of individual crime, in which the transition into criminal activity is a function of labor market disadvantage that is conditioned on height.. Our results suggest early America criminals were short as crime appears to have been conditioned on height — taller individuals had a lower probability of making a transition into criminal activity.

Our parameter estimates of the effects of height are identified if the assumption that all individual heterogeneity is an unobserved frailty. Because our parameter estimates of the individual crime hazard support this assumption, our results suggest that being short in early America had a causal effect on an individual's decision to engage in criminal behavior. While we remain agnostic about the mechanism and refrain from speculating about why being short translated into higher crime hazards, specific mechanisms posited elsewhere are consistent with labor market disadvantage being conditioned on height. If in early America, short individuals faced labor market disadvantage because of either lower productivity due to lesser physical capacity for heavy work, lesser productivity due to lower cognitive ability, or because they faced discrimination, the relative opportunity costs of crime would be lower which would increase the likelihood of transitioning into criminal activity

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Table 1: Variable definitions and summary statistics

Variable	Definition	Mean
Age	Age at first entry into penitentiary	28.45
		(10.13)
Height	Height (in inches)	66.30
		(2.61)
Height	Z score of height compared to modern US standard	-1.29
		(1.01)
Theft	=1 if convicted of larceny, burglary, horse theft, receiving stolen goods	0.61
Violent	=1 if convicted of assault, manslaughter, murder, rape, robbery	0.18
Black	=1 if prisoner was black	0.08
Mulatto	=1 if prisoner was mulatto	0.06
Irish	=1 if prisoner was Irish immigrant	0.11
German	=1 if prisoner was German immigrant	0.08
Other immigrant	=1 if prisoner immigrated from country other than Ireland or Germany	0.07
Professional	=1 if previously employed in profession	0.01
Proprietor	=1 if previously employed as shopkeeper	0.02
Sales	=1 if previously employed in sales	0.01
Service	=1 if previously employed in services	0.04
Operative	=1 if previously employed as semi-skilled factory operative	0.18
Craftsman	=1 if previously employed in skilled trade	0.26
Clerical	=1 if previously employed as clerk / accountant	0.02
Farmer	=1 if previously employed as farmer	0.06
Laborer	=1 if previously employed as common laborer	0.37
No occupation	=1 if no previous occupation reported	0.02
Philadelphia	=1 if convicted in Philadelphia court	0.15
Pittsburgh	=1 if convicted in Pittsburgh court	0.16
1830s	=1 if convicted between 1830 and 1839	0.14
1840s	=1 if convicted between 1840 and 1849	0.18
1850s	=1 if convicted between 1850 and 1859	0.23
1860s	=1 if convicted between 1860 and 1869	0.30
1870s	=1 if convicted between 1870 and 1879	0.13
Recession 1837	=1 if convicted between 1837 and 1843	0.13
Recession 1857	=1 if convicted in 1857 or 1858	0.06
Recession 1873	=1 if convicted between 1873 and 1880	0.07
Irish immigration	=1 if convicted during peak Irish immigration years (1845-1849)	0.08
Civil War	=1 between 1861 and 1865	0.08
Emancipation	=1 if convicted after 1865	0.32

Notes: N= 8641. Standard deviations of continuous variables in parentheses.
Sources: Eastern State Penitentiary (1829-1857; 1842-1869), Western State Penitentiary (1826-1876).

Table 2: Average heights of males Pennsylvania prisoners and comparison groups

Age group	White Pennsylvania prisoners	Union Army enlistees	Height difference	Black Pennsylvania prisoners	Virginia free blacks	Height difference
16 years	63.77	65.34	-1.57***	63.49	63.82	-0.33
	(2.26)	(2.96)	{4.06}	(3.36)	(3.38)	{0.47}
	[n=75]	[n=156]		[n=33]	[n=80]	
17 years	65.32	66.15	-0.83***	64.58	64.48	0.10
	(2.11)	(2.62)	{3.37}	(2.77)	(3.18)	{0.19}
	[150]	[300]		[48]	[107]	
18 years	65.66	66.65	-0.99***	65.37	66.32	-0.95***
	(2.56)	(2.51)	{6.78}	(2.48)	(2.74)	{2.58}
	[322]	[3679]		[83]	[137]	
19 years	66.05	67.32	-1.27***	65.64	67.49	-1.85***
	(2.45)	(2.66)	{8.71}	(2.66)	(2.59)	{5.38}
	[388]	[1975]		[92]	[156]	
20 years	65.98	67.81	-1.83***	65.62	67.16	-1.54***
	(2.33)	(2.64)	{12.59}	(2.41)	(2.73)	{4.42}
	[392]	[1623]		[77]	[237]	
21 years	66.45	67.84	-1.39***	65.94	67.20	-1.26***
	(2.49)	(2.70)	{10.85}	(2.65)	(2.80)	{4.42}
	[555]	[1892]		[106]	[966]	
22 years	66.50	67.90	-1.40***	66.15	67.37	-1.22***
	(2.39)	(2.63)	{10.58}	(2.59)	(2.98)	{3.39}
	[508]	[1470]		[76]	[532]	
23 years	66.46	67.93	-1.47***	66.09	67.79	-1.70***
	(2.57)	(1.69)	{10.05}	(2.43)	(2.81)	{5.03}
	[441]	[1323]		[80]	[384]	
24 years	66.38	68.10	-1.72***	66.60	67.58	-0.98***
	(2.54)	(2.66)	{11.55}	(2.62)	(2.90)	{2.65}
	[425]	[1166]		[75]	[281]	
25 years and up	66.54	67.95	-1.41***	66.17	67.52	-1.35***
	(2.63)	(2.65)	{29.38}	(2.74)	(2.79)	{9.88}
	[4198]	[10981]		[490]	[2705]	

Sources: Pennsylvania prisoners: see Table 1; Union Army enlistees: Center for Population Economics (2010); Virginia free blacks (Bodenhorn 2002).

Notes: standard deviations in parentheses; sample sizes in brackets; t-statistics for test of difference in sample means in braces.

Table 3: Weibull hazard estimates: Linear height specification with shared frailty on crime

	Full sample	Theft only	Violent only	Urban only	Rural only	African American only	White Only
Height	0.975[c]	0.973[c]	0.976[b]	0.972[c]	0.977[c]	0.953[c]	0.978[c]
	(6.20)	(5.38)	(2.35)	(3.72)	(4.75)	(4.54)	(4.93)
Black	1.109[b]	1.128[b]	1.147	0.999	1.169[c]	---	---
	(2.42)	(2.36)	(1.41)	(0.01)	(2.97)		
Mulatto	1.207[c]	1.192[c]	1.331[c]	1.045	1.285[c]	1.087	---
	(3.92)	(3.02)	(2.76)	(0.53)	(4.22)	(1.36)	
Irish	0.695[c]	0.598[c]	0.846[b]	0.567[c]	0.766[c]	---	0.693[c]
	(10.13)	(10.32)	(2.37)	(9.64)	(5.85)		(10.14)
German	0.577[c]	0.592[c]	0.468[c]	0.539[c]	0.589[c]	---	0.583[c]
	(13.03)	(10.36)	(6.49)	(8.46)	(10.11)		(12.74)
Other	0.714[c]	0.683[c]	0.842[a]	0.605[c]	0.772[c]	---	0.707[c]
immigrant	(7.83)	(7.18)	(1.67)	(7.17)	(4.71)		(7.88)
Philadelphia	1.231[c]	1.215[c]	1.238[c]	1.179[c]	---	1.199[b]	1.235[c]
	(5.97)	(4.33)	(2.62)	(3.52)		(2.25)	(5.45)
Pittsburgh	1.165[c]	1.139[c]	1.313[c]	---	---	1.017	1.182[c]
	(4.51)	(3.05)	(3.55)			(0.15)	(4.67)
Recess 37	0.911[c]	0.925[a]	0.877	0.861[b]	0.948	1.007	0.892[c]
	(2.61)	(1.85)	(1.3)	(2.24)	(1.27)	(0.08)	(2.87)
Recess 57	0.990	1.070	0.933	0.955	1.001	1.130	0.969
	(0.20)	(1.13)	(0.58)	(0.54)	(0.02)	(0.82)	(0.62)
Recess 73	1.087[a]	1.072	1.055	1.231[b]	1.045	0.914	1.100[a]
	(1.71)	(1.05)	(0.47)	(2.17)	(0.77)	(0.48)	(1.89)
Irish	0.888[c]	0.931	0.843	0.829[b]	0.922	1.146	0.861[c]
Immigration	(2.80)	(1.36)	(1.60)	(2.48)	(1.59)	(1.20)	(3.26)
Civil War	0.884[c]	0.870[b]	0.987	0.810[c]	0.926	1.178	0.851[c]
	(2.87)	(2.51)	(0.13)	(2.56)	(1.51)	(1.30)	(3.52)
Emancipation	1.142[c]	1.133[c]	1.279[c]	1.024	1.199[c]	1.230[b]	1.121[c]
	(4.40)	(3.23)	(3.51)	(0.42)	(5.04)	(2.24)	(3.57)
Observations	8558	5248	1526	2621	5937	1157	7401
χ^2: $\Sigma(\beta X)=0$	491.29	365.88	111.60	230.11	266.34	47.86	390.47
Z: $\ln(p)=0$	149.23	118.89	62.15	86.70	122.05	56.66	137.98
Z: $\ln(\theta)=0$	-2.045	-5.06	-4.57	-4.49	-7.25	-4.80	-6.34

Notes: ^a implies p≤0.10; ^b implies p≤0.05; ^c implies p≤0.01. Values in parentheses are absolute values of z-statistics

Sources: See Table 1.

Table 4: Weibull hazard estimates: Quintile height specification with shared frailty on crime

	Full sample	Theft only	Violent only	Urban only	Rural only	African American only	White Only
1st quintile	Reference	Reference	Reference	Reference	Reference	Reference	Reference
2nd quintile	0.960	0.959	0.996	0.947	0.955	0.916	0.968
	(1.34)	(1.08)	(0.05)	(1.05)	(1.21)	(1.11)	(0.98)
3rd quintile	0.899[c]	0.886[c]	1.043	0.884[a]	0.912[b]	0.904	0.899[c]
	(2.94)	(2.60)	(0.50)	(1.87)	(2.08)	(0.99)	(2.71)
4th quintile	0.942	0.961	0.879	0.902	0.954	0.795[b]	0.961
	(1.56)	(0.82)	(1.43)	(1.49)	(1.02)	(2.27)	(0.96)
5th quintile	0.795[c]	0.769[c]	0.851[a]	0.791[c]	0.802[c]	0.673[c]	0.812[c]
	(6.68)	(6.03)	(1.95)	(3.59)	(5.37)	(4.13)	(5.62)
Race	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Urban	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Recessions	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Periods	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8558	5248	1526	2621	5937	1157	7401
χ^2: $\Sigma(\beta X)=0$	506.35	382.33	114.18	230.78	279.01	47.50	406.66
Z: $\ln(p)=0$	149.45	119.21	62.22	86.74	122.27	56.69	138.22
Z: $\ln(\theta)=0$	-6.80	-5.06	-4.52	-4.49	-7.24	-4.77	-6.33

Notes: ^a implies p≤0.10; ^b implies p≤0.05; ^c implies p≤0.01. Values in parentheses are absolute values of z-statistics

Sources: see Table 1.

Table 5: Weibull hazard estimates: Quintile height specification with shared frailty on county

	Full sample	Theft Only	Violent only	Urban only	Rural only	African American only	White Only
1st quintile	Reference	Reference	Reference		Reference	Reference	Reference
2nd quintile	0.954	0.964	0.960		0.952	0.874[a]	0.966
	(1.53)	(0.96)	(0.56)		(1.28)	(1.71)	(1.05)
3rd quintile	0.880[c]	0.888[b]	1.012		0.897[b]	0.877	0.878[c]
	(3.54)	(2.54)	(0.13)		(2.47)	(1.29)	(3.33)
4th quintile	0.926[b]	0.977	0.871		0.950	0.765[c]	0.945
	(2.02)	(0.47)	(1.50)		(1.12)	(2.63)	(1.37)
5th quintile	0.775[c]	0.772[c]	0.826[b]		0.800[c]	0.654[c]	0.792[c]
	(7.42)	(5.90)	(2.26)		(5.43)	(4.43)	(6.32)
Race	Yes	Yes	Yes		Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes		Yes	Yes	Yes
Urban	Yes	Yes	Yes		Yes	Yes	Yes
Recessions	Yes	Yes	Yes		Yes	Yes	Yes
Periods	Yes	Yes	Yes		Yes	Yes	Yes
Observations	8611	5246	1526		5987	1160	7451
χ^2: $\Sigma(\beta X)=0$	487.14	354.38	115.39		277.12	38.92	374.47
Z: ln(p) =0	146.61	117.93	59.87		120.24	54.12	135.20
Z: ln(Θ) =0	-12.59	-11.29	-6.98		-12.44	-3.34	-12.03

Notes: ^a implies p≤0.10; ^b implies p≤0.05; ^c implies p≤0.01. Values in parentheses are absolute values of z-statistics

Sources: See Table 1.

Table 6: Weibull hazard estimates: Decile height specification with shared frailty on crime

	Full sample	Theft Only	Violent only	Urban only	Rural only	African American only	White Only
1st decile	Reference	Reference	Reference	Reference	Reference	Reference	Reference
2nd decile	0.923[a]	0.906[a]	0.951	0.826[c]	1.002	0.894	0.932
	(1.73)	(1.72)	(0.46)	(2.47)	(0.02)	(0.98)	(1.39)
3rd decile	0.932	0.918	1.028	0.884[a]	0.963	0.969	0.930
	(1.64)	(1.60)	(0.27)	(1.73)	(0.69)	(0.29)	(1.54)
4th decile	0.920[b]	0.917[a]	0.936	0.859[b]	0.949	0.797[b]	0.942
	(2.02)	(1.67)	(0.68)	(2.19)	(1.01)	(2.12)	(1.31)
5th decile	---	---	---	---	---	---	---
6th decile	0.866[c]	0.847[c]	1.022	0.811[c]	0.913[a]	0.863	0.870[c]
	(3.43)	(3.13)	(0.22)	(2.84)	(1.76)	(1.31)	(3.05)
7th decile	0.901[b]	0.914	0.852	0.822[b]	0.947	0.749[b]	0.923[a]
	(2.38)	(1.62)	(1.56)	(2.54)	(1.00)	(2.53)	(1.67)
8th decile	1.108	1.039	1.196	1.211	1.114	0.955	1.129
	(0.70)	(0.21)	(0.46)	(0.53)	(0.67)	(0.14)	(0.74)
9th decile	0.773[c]	0.772[c]	0.813[a]	0.733[c]	0.810[c]	0.716[b]	0.781[c]
	(5.46)	(4.33)	(1.82)	(3.57)	(3.67)	(2.46)	(4.88)
10th decile	0.759[c]	0.703[c]	0.853	0.716[c]	0.795[c]	0.585[c]	0.791[c]
	(5.85)	(5.93)	(1.41)	(3.59)	(4.07)	(4.15)	(4.60)
Race	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Urban	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Recessions	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Periods	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8558	5248	1526	2621	5937	1157	7401
χ^2: $\Sigma(\beta X)=0$	511.47	387.90	116.14	238.17	280.19	53.73	410.18
Z: $\ln(p) = 0$	149.53	119.32	62.29	86.91	122.30	59.61	138.29
Z: $\ln(\theta) = 0$	-6.83	-5.08	-4.55	-4.51	-7.26	-4.76	-6.34

Notes: ^a implies p≤0.10; ^b implies p≤0.05; ^c implies p≤0.01. Values in parentheses are absolute values of z-statistics

Sources: See Table 1.

Table 7: Weibull hazard estimates: Quintile height specification with shared frailty on county controlling for cognitive abilities

	Literacy sample	Theft Only	Literacy sample	Theft Only
1st quintile	Reference	Reference	Reference	Reference
2nd quintile	0.888[c]	0.826[c]	0.894[c]	0.817[c]
	(2.73)	(3.51)	(2.58)	(3.69)
3rd quintile	0.880[b]	0.884[a]	0.887[b]	0.870[b]
	(2.48)	(1.86)	(2.33)	(2.10)
4th quintile	0.896[b]	0.879[a]	0.907[a]	0.873[b]
	(2.08)	(1.89)	(1.80)	(1.99)
5th quintile	0.756[c]	0.674[c]	0.781[c]	0.678[c]
	(5.62)	(6.21)	(4.95)	(6.08)
Literacy	0.973	1.004	1.006	1.030
	(0.64)	(0.07)	(0.15)	(0.57)
Occupations	No	No	Yes	Yes
Race	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes
Urban	Yes	Yes	Yes	Yes
Recessions	Yes	Yes	Yes	Yes
Periods	Yes	Yes	Yes	Yes
Observations	4219	2587	4219	2587
χ^2: $\Sigma(\beta X)=0$	270.93	237.90	376.89	299.21
Z: $\ln(p)=0$	105.52	85.49	107.70	87.26
Z: $\ln(\theta)=0$	6.16	4.56	-5.88	-4.69