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The Effect of Education and School Quality on Female Crime

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Abstract

This paper estimates the effects of educational attainment and school quality on crime among American women. Using changes in compulsory schooling laws as instruments, we estimate significant effects of schooling attainment on the probability of incarceration using Census data from 1960-1980. Using data from the 1960-90 Uniform Crime Reports, we also estimate that increases in average schooling levels reduce arrest rates for violent and property crime but not white collar crime. The estimated reductions in crime for women are smaller in magnitude than comparable estimates for men; however, the effects for women are larger in percentage terms (relative to baseline crime rates). Our results suggest small and mixed direct effects of school quality (as measured by pupil-teacher ratios, term length, and teacher salaries) on incarceration and arrests. Finally, we show that the effects of education on crime for women is unlikely to be due to changes in labor market opportunities and may be more related to changes in marital opportunities and family formation.

1 Introduction

Historically, men have committed crime at much higher rates than women. As a result, most research on the determinants of and trends in crime has focused on men. Yet, the share of female arrests has increased significantly in the U.S. over the past few decades. As shown in Figure 1, this increase is particularly dramatic for property and white collar crimes.

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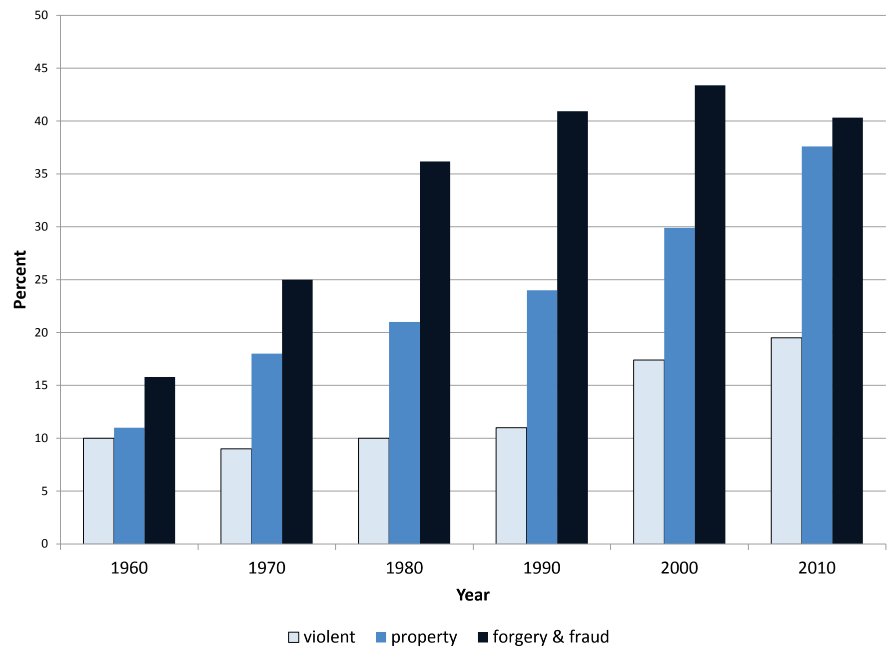
Figure 1: Percent of Arrests by Women, 1960-2010 UCR



According to the FBI's Uniform Crime Reports (UCR), women accounted for roughly 40% of all arrests for property and white collar crimes in 2010, up substantially from 1970. While women still account for only 20% of arrests for violent crime, this is twice the share of only a few decades ago.[1]

Given these trends, it is becoming increasingly important to understand the determinants of crime among women as well as men, especially factors that may be influenced by policy.[2] To that end, this paper studies the extent to which educational investments, as measured by years of completed schooling and the quality of elementary and secondary schooling, discourage criminal activity among women. Most sociological theories of crime (e.g. strain, conflict, labeling, control theories) as well as economic theories based on human capital and rational choice (Becker, 1968; Freeman, 1996; Lochner, 2004) suggest that human capital investments should reduce (most types of) crime, and there is growing evidence from the U.S. and other developed countries that this is the case. However, nearly all of this evidence

[1]Throughout this paper, violent crimes refer to murder and non-negligent manslaughter, robbery and aggravated assault; property offenses include burglary, larceny-theft, motor vehicle theft, and arson; white collar crimes include forgery and counterfeiting, fraud, and embezzlement. Figures for embezzlement were not available as far back as 1960, so Figure 1 only reports statistics for forgery (including counterfeiting) and fraud. Statistics from 1960-90 are taken from Schwartz and Steffensmeier (2007), while statistics from 2000 and 2010 are taken directly from the FBI's UCR.

[2]See Steffensmeier and Streifel (1992), Schwartz and Steffensmeier (2007), and Engelhardt et al. (2008) for discussions of the underlying causes for increased criminal activity by women.

is based on studies of men.[3] While Hjalmarsson et al. (2015) and Machin et al. (2011) attempt to estimate the causal effects of educational attainment on crime for women as well as men, the estimated effects for women in both studies are very imprecise.[4]

There are many reasons to think that the impacts of education on crime may differ between men and women. To begin, the nature of many criminal offenses differs by gender: crime tends to be of a more personal nature for women (e.g. female homicides are often perpetrated against their husbands or partners) and women are much less likely to participate in gang-related activities (Steffensmeier and Streifel, 1992; Schwartz and Steffensmeier, 2007). This suggests that the extent to which schooling influences social networks and family formation is likely to be particularly important for women. Additionally, women participate much less in the labor market and are more involved in household production than men, so their opportunity costs of crime likely differ. On the one hand, the lower employment rates by women suggest that the wage returns to education may be less relevant to their decisions to engage in crime. On the other hand, women typically have higher labor supply elasticities than men (Blundell and MaCurdy, 1999).[5] Women's traditional role as secondary earners in families suggests that education's impact on their marital prospects may be important if family resources are an important determinant of crime. Similarly, women's traditional role as primary child caregivers (especially in single-parent homes) means that any effects of schooling on fertility are also likely to be important for female crime if the presence of children factors into decisions to engage in criminal activity (e.g. stronger incentives to avoid incarceration). We consider some of these possible channels by which education may affect female crime in conjunction with work, marriage, and childbearing decisions.

Estimating the causal effect of education on crime is challenging, because factors not observed by the researcher may determine both schooling choices and criminal behavior. For example, individuals with self-control problems or who discount the future heavily may perform poorly in school or place little value on the long-run returns to education, and they may also be more likely to engage in crime. Additionally, youth living in neighborhoods rampant with gangs and criminal opportunities may find them attractive, prompting them to drop out of school at young ages. Lochner and Moretti (2004) address these endogeneity

[3]See Lochner (2010, 2011) and Hjalmarsson and Lochner (2012) for recent surveys.

[4]Both studies estimate statistically insignificant effects of education on female crime with large standard errors relative to the impacts one might expect given rates of female offending. In the case of Hjalmarsson et al. (2015), the Swedish schooling reforms they study had much weaker effects on female education levels, so their instrumental variable is not as powerful for studying female crime. This is not the case for the increase in the minimum schooling age in the U.K. studied by Machin et al. (2011). In this study, standard errors are quite large relative to baseline crime rates among women but not men.

[5]Lochner and Moretti (2004) argue that the increase in wages associated with education can explain most of the impacts of education on crime for men.

problems by using changes in state-level compulsory schooling laws over time as instrumental variables (IV) to estimate the causal effect of educational attainment on the probability of incarceration and arrest rates for American men. Their estimates reveal that an additional year of schooling reduces the probability of incarceration by slightly more than 0.1 percentage points for white men and 0.4 percentage points for black men. These reflect 10-15% reductions relative to baseline incarceration rates for high school dropouts. An additional year of average schooling levels in a state reduces arrest rates by 11% or more. Other recent studies taking a similar estimation approach (using changes in schooling age reforms as instruments for educational attainment) reach similar conclusions for men in Sweden (Hjalmarsson et al., 2015) and the United Kingdom (Machin et al., 2011).

We follow a similar empirical strategy to that of Lochner and Moretti (2004) to study criminal behavior among American women; however, we go beyond the current literature by also examining the extent to which elementary and secondary school quality impacts adult crime rates. A few studies suggest that improvements in school quality may lead to reductions in criminal activity during early adulthood. For example, using randomized school admission lotteries, Cullen et al. (2006) and Deming (2011) find that students who 'win' the opportunity to attend better-performing public schools commit less crime during school and the first few years after leaving school. Weiner et al. (2009) show that desegregation initiatives in some U.S. states led to substantial improvements in school quality for blacks. Among blacks experiencing desegregation, high school graduation rates increased by a few percentage points and homicide arrest rates declined by 1/3 at ages 15-19. Little is known about the longer run impacts of school quality on crime, and there are no studies that examine the effects of more direct measures of quality like pupil-teacher ratios, school term length, or teacher wage rates.[6]

We simultaneously consider the effects of educational attainment and school quality on female incarceration and arrest rates, using state-specific changes in compulsory schooling laws as instruments for attainment as in Lochner and Moretti (2004) and cohort- and state-specific measures of school quality from Card and Krueger (1992a) and Stephens and Yang (2014). In examining the impacts of school quality, we consider both the direct effects holding schooling attainment constant as well as the indirect effects through increases in attainment. By simultaneously considering the impacts of school quantity (years of schooling) and qual-

[6]Evidence on the effects of these types of school quality measures on earnings is mixed (Card and Krueger, 1992a; Heckman et al., 1996; Hanushek, 2002). In their analysis of state-level school quality on earnings, Heckman et al. (1996) argue that interactions between region of birth and region of residence are important to account for selective migration and the possibility that skills acquired by attending school in one region may not be rewarded equally in other regions of the country. Although these forces are less likely to be important for our analysis of criminal behavior, we also consider specifications that account for these interaction effects.

ity, we address important concerns raised by Stephens and Yang (2014) that increases in compulsory schooling laws are correlated with improvements in school quality in the U.S.[7]

Based on U.S. Census data from 1960, 1970 and 1980, our IV estimates suggest that an additional year of schooling reduces incarceration rates by 0.04-0.08 percentage points for white and black women. These estimates are largely unaffected by controls for school quality. The direct effects of quality improvements on incarceration are relatively small and mixed, while the indirect effects of quality through increased schooling attainment are generally positive and modest in size.

A similar picture emerges when we estimate the effects of schooling attainment and quality on state-level arrest rates for women using data from the 1960, 1970, 1980 and 1990 UCR. Regardless of whether we control for school quality, our IV estimates suggest significant effects of educational attainment on arrest rates for violent and property crime but not white collar crime. By contrast, school quality improvements have mixed effects on state-level female arrest rates.

In Section 2, we estimate the effects of educational attainment and school quality on female incarceration, while we estimate their impacts on state-level arrest rates in Section 3. To better understand why schooling affects female crime, Section 4 examines the impacts of schooling attainment and quality on work, marriage, and childbearing. We offer some concluding thoughts in Section 5.

2 The Effect of Education on Female Incarceration

We use individual-level data from the 1960, 1970, and 1980 US Censuses to estimate the effect of education on the probability of female incarceration.

Table 1 presents descriptive statistics for our sample of 20-60 year-old women from the US Censuses. Over the 1960-80 period, 0.02-0.04 percent of women were in prison at the time of the Censuses. Average education increased almost two years, while high school graduation rates increased by 20 percentage points. The table also reports the fraction of women who experienced different compulsory schooling laws (based on their state of birth) when they were age 14.[8] As demonstrated in the table, compulsory schooling ages generally increased over time; however, Lochner and Moretti (2004) show that there is considerable cross-state variation in the time patterns for these laws, with some states even relaxing

[7]We also consider specifications that account for region-specific cohort trends as suggested by Stephens and Yang (2014).

[8]These laws were first compiled by Acemoglu and Angrist (2001) and subsequently used by Lochner and Moretti (2004) in their analysis of the effects of education on crime among men. See Appendix A for details.

Table 1: Descriptive Statistics by Census Year: Mean (Standard Deviation)

Variable	1960	1970	1980
In prison	0.0003	0.0002	0.0004
	(0.0175)	(0.0157)	(0.0193)
Education	10.6265	11.4287	12.3201
	(3.0467)	(2.8425)	(2.6817)
High School Grad	0.5177	0.6455	0.7752
	(0.4997)	(0.4784)	(0.4175)
Age	38.86	38.57	37.36
	(11.23)	(12.04)	(12.06)
Black	0.1061	0.1070	0.1225
	(0.3080)	(0.3091)	(0.3278)
Compulsory attendance ≤ 8	0.3277	0.2083	0.1487
	(0.4694)	(0.4061)	(0.3558)
Compulsory attendance $= 9$	0.4373	0.4551	0.4101
	(0.4961)	(0.4980)	(0.4919)
Compulsory attendance $= 10$	0.0667	0.0712	0.0978
	(0.2495)	(0.2571)	(0.2971)
Compulsory attendance ≥ 11	0.1683	0.2655	0.3434
	(0.3741)	(0.4416)	(0.4748)
Sample size	409,522	904,532	2,779,968

Notes: Census data obtained from the Integrated Public Use Microdata Series (IPUMS) using the US Census of: 1960 1% sample, 1970 Form 1 and Form 2 State 1% samples, and 1980 5% sample.

compulsory schooling laws during some periods. See Appendix A for further details on the sample construction and variables used in our analysis.

Table 2 presents the unconditional relationship between schooling and female incarceration in the Census data. The table shows that female incarceration rates are typically more than twice as high for high school dropouts as for those who finished high school. Incarceration rates are lowest for college graduates. Figure 2 indicates that the relationship between schooling attainment and incarceration conditional on individual characteristics (age, state of birth, state of residence, cohort of birth, and year) is negative over most grades with particularly strong drops in incarceration associated with high school completion.

To estimate the causal effect of education on female crime, we consider the following linear probability model:

$$P_{it} = E_{it}\beta + x_{it}'\delta + \epsilon_{it}, \tag{1}$$

where P_{it} is an indicator variable equal to one if individual i observed in year t is incarcerated and zero otherwise; E_{it} reflects years of completed schooling for this individual; and x_{it} is a vector of observed covariates that includes indicator variables for state of residence, state of

Table 2: Census Incarceration Rates for Women (in Percentage Terms)

	All Years	1960	1970	1980
White women				
HS drop	0.04	0.03	0.03	0.05
HS grad	0.02	0.01	0.01	0.02
Some Coll	0.02	0.01	0.01	0.02
Coll +	0.00	0.00	0.00	0.01
Black women				
HS drop	0.20	0.17	0.15	0.22
HS grad	0.09	0.04	0.05	0.10
Some Coll	0.11	0.04	0.04	0.12
Coll +	0.06	0.00	0.00	0.07

Notes: HS drop are individuals with lest than 12 years of schooling, HS grad are individuals with exactly 12 years of schooling, Some Coll are individuals with 13-15 years of schooling and Coll + are individuals with at least 16 years of schooling.

birth, decade of birth, census year and various interactions.[9] Importantly, some specifications control for state-of-residence-specific time effects, which account for differences across states over time in terms of their law enforcement and criminal justice policies, as well as labor market conditions. We also consider a specification that additionally controls for region-of-birth-specific cohort trends as suggested by Stephens and Yang (2014). An alternative set of specifications controls for state-of-residence-specific age patterns to account for any differences in policies toward younger vs. older offenders. In the next subsection, we also introduce school quality measures by cohort and state of birth as additional covariates.

Table 3 reports both ordinary least squares (OLS) and IV estimates of β (shown in percentage terms, i.e. coefficients multiplied by 100), the effect of one year of school on the probability of incarceration. Panel A reports estimates for white women and panel B for black women. OLS estimates indicate that an additional year of school, on average, lowers incarceration rates by about .006 percentage points for white women and .024 percentage points for black women. We account for the endogeneity of schooling by using compulsory attendance laws as instruments for educational attainment as in Lochner and Moretti (2004). The second line in both panels of Table 3 presents these IV estimates, which indicate that an additional year of school, on average, reduces incarceration rates by .04 to .06 percentage points among white women and .07 to .08 percentage points among black women.[10] While the

[9]For black females, the covariates also include state-of-birth dummies interacted with a dummy for black women born in the South who turn age 14 in 1958 or later to account for the impact of *Brown v. Board of Education.*

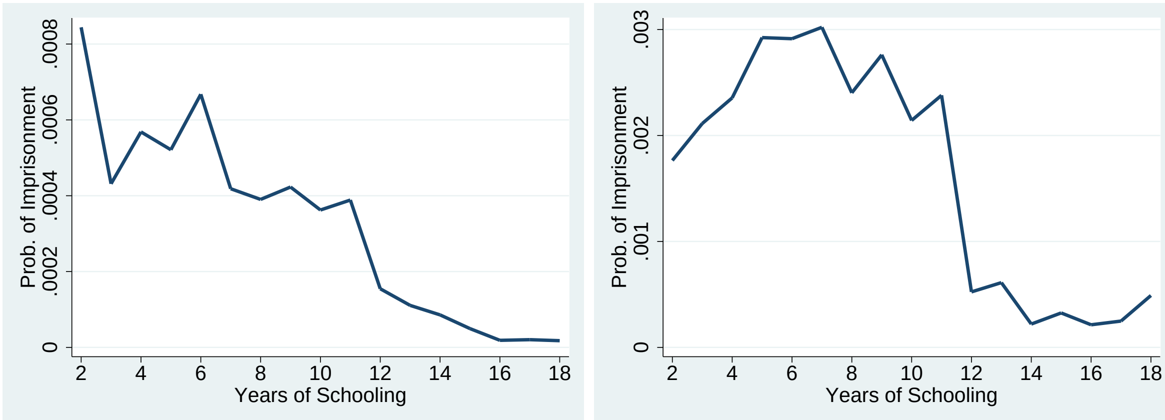
[10]First-stage estimates on the excluded instruments are statistically significant with F-statistics well above

Table 3: Effect of Years of Education on Imprisonment (in Percentage Terms)

	(1)	(2)	(3)	(4)	(5)
A. White Females					
OLS estimates	-0.006	-0.006	-0.006	-0.006	-0.006
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
IV estimates	-0.035	-0.035	-0.052	-0.047	-0.059
	(0.010)	(0.011)	(0.021)	(0.019)	(0.028)
First-Stage:					
Compulsory attendance = 9	0.146	0.137	0.064	0.071	0.049
	(0.019)	(0.019)	(0.017)	(0.018)	(0.014)
Compulsory attendance = 10	0.220	0.202	0.118	0.136	0.074
	(0.027)	(0.026)	(0.024)	(0.024)	(0.020)
Compulsory attendance $\geq$ 11	0.324	0.309	0.178	0.200	0.129
	(0.025)	(0.024)	(0.022)	(0.024)	(0.019)
F-statistic for excluded instruments	55.49	53.83	22.75	24.86	15.03
B. Black Females					
OLS estimates	-0.024	-0.024	-0.024	-0.024	-0.024
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
IV estimates	-0.078	-0.077	-0.066	-0.080	-0.083
	(0.044)	(0.047)	(0.080)	(0.071)	(0.106)
First-Stage:					
Compulsory attendance = 9	0.384	0.358	0.225	0.252	0.174
	(0.037)	(0.036)	(0.030)	(0.031)	(0.030)
Compulsory attendance = 10	0.431	0.393	0.241	0.282	0.190
	(0.063)	(0.062)	(0.054)	(0.056)	(0.048)
Compulsory attendance $\geq$ 11	0.452	0.428	0.264	0.314	0.203
	(0.056)	(0.055)	(0.044)	(0.046)	(0.044)
F-statistic for excluded instruments	39.22	35.88	19.68	24.33	11.82
Additional controls:					
State of residence $\times$ year effects		y	y	y	y
State of residence $\times$ age			y		
State of residence $\times$ broad age group				y	
Region of birth $\times$ cohort trend					y

Notes: Standard errors corrected for state of birth-year of birth clustering are in parentheses. All regressions include dummies for state of residence, dummies for state of birth (excluding Alaska and Hawaii), dummies for age groups (age 20-22, 23-25, ..., 56-58, and 59-60), dummies for decade of birth (1914-1923, 1924-1933, ..., 1964-1974), and dummies for census year. The regressions for black females also include state-of-birth dummies interacted with a dummy for black women born in the South who turn age 14 in 1958 or later to account for the impact of *Brown v. Board of Education*. "broad age group" reflects three dummies for the following age groups: 20-34, 35-49, and 50-64. The sample size for white females is 3,613,313 and for black females is 480,709. The F-test for excluded instruments for white females is distributed $F_{(3,2985)}$ and for black females is distributed $F_{(3,2568)}$.

Figure 2: Probability of incarceration by years of schooling conditional on age, state of birth, state of residence, cohort of birth, and year



(a) White Females

(b) Black Females

Notes: Regression-adjusted probability of incarceration is obtained by conditioning on age, state of birth, state of residence, cohort of birth and year effects.

estimated effects for whites are statistically significant (at 0.05 level), they are not for blacks due to the smaller sample sizes and resulting reduction in precision. The lack of precision for black women also means that we cannot reject equality of effects across races (based on the IV estimates). The estimates are quite robust across specifications and represent sizeable impacts relative to baseline incarceration rates for uneducated women.

The fact that IV estimates are significantly larger (in absolute value) than OLS estimates for white women is consistent with the findings of Lochner and Moretti (2004) for men. This may suggest that unmeasured factors which lead to higher levels of schooling also lead to higher rates of incarceration, contrary to most theories of crime.[11] However, it is also possible that IV estimates are larger due to heterogeneity in the effects of education on incarceration in the population, with larger than average effects of education among young women that are most responsive to compulsory schooling laws.[12] It may also be the case that additional schooling at the grade margins affected by the instrument (i.e. grades 9-12) has particularly large effects on incarceration as observed in Figure 2. As discussed in Lochner and Moretti

10, the level below which concerns about weak instruments arise (Staiger and Stock, 1997). The estimates indicate that increases in compulsory schooling ages lead to increases in educational attainment. See Lleras-Muney (2002) and Lochner and Moretti (2004) for in-depth analyses of the impacts of compulsory attendance laws on schooling.

[11]This would be consistent with the 'emancipation' hypothesis, which suggests that higher rates of female crime may be expected with an increased equalization in gender roles that may accompany schooling (Steffensmeier and Streifel, 1992). The fact that IV estimates are also larger than OLS estimates for men (Lochner and Moretti, 2004) casts doubt on this potential explanation.

[12]See Imbens and Angrist (1994) for a discussion of local average treatment effects and instrumental variables.

Table 4: School Quality Measures over Time for White Women

Variable	1960	1970	1980
Pupil/Teacher ratio (10's of students)	2.943	2.776	2.536
	(0.479)	(0.424)	(0.386)
Term length (100's of days)	1.729	1.754	1.774
	(0.127)	(0.097)	(0.059)
Relative teacher wage	1.061	1.050	1.030
	(0.253)	(0.222)	(0.181)

Notes: Table reports mean (standard deviation) for school quality measures based on our sample of white women ages 20-60 in 1960, 1970, and 1980 U.S. Censuses. Pupil-teacher ratios reported in 10s of students per teacher. Term length reported in 100s of days. Relative teacher wage reflects the state average salary for teachers divided by the national average of all state averages of teacher salary.

(2015), this can lead to differences between OLS and IV estimators even in the absence of endogeneity.[13]

2.1 Impacts of School Quality

School quality may also affect criminal behavior as suggested by the findings in Cullen et al. (2006) and Deming (2011). Hence, we next incorporate three measures of school quality from Card and Krueger (1992a) and extended by Stephens and Yang (2014): (*i*) pupil/teacher ratios, (*ii*) school term lengths, and (*iii*) average teacher salaries.[14] Since state-level quality measures are not very reflective of the quality of schools attended by blacks from most of the cohorts we study (Card and Krueger, 1992b), we limit our attention to white women in this analysis. For expositional purposes, we have scaled these measures so pupil/teacher ratios reflect 10s of pupils per teacher, term lengths are in 100s of days, and relative teacher salary reflects state average teacher salary divided by a measure of national average teacher salary. The evolution of these measures over time for our sample of white women is reported in Table 4. See Appendix A for further details.

Table 5 presents both OLS and IV estimates analogous to those of Table 3 (for white women) only including all three measures of school quality. The OLS estimates of the impact of educational attainment are unaffected by the inclusion of school quality measures, while the IV estimates are slightly greater in magnitude than those in Table 3. Even though the first-stage estimated effects of schooling laws are weaker than when we omit quality

[13] Applying the exogeneity test of Lochner and Moretti (2015), which is robust to heterogeneous grade-specific effects, we reject exogeneity of schooling for whites but not blacks (e.g. p-values of 0.041 and 0.564, respectively, for specification (3) in Table 3). This suggests that the difference between OLS and IV estimates for white women cannot be fully explained by greater impacts of education at some margins than others.

[14] The authors are grateful to Melvin Stephens Jr. and Jeff Lingwall for sharing these data.

measures, they are still significant (with F-statistics exceeding 10) and suggest that tougher compulsory schooling laws are associated with more years of education.[15]

The estimates in Table 5 suggest little direct effect of school quality on the likelihood of incarceration. Only the coefficients on relative teacher wages are statistically significant across most specifications; however, they suggest that higher teacher wages lead to increases in the probability of incarceration (holding schooling constant).[16] Table 6 shows, however, that improvements in all three quality measures (i.e. lower pupil-teacher ratios, longer school terms, and higher teacher wages) lead to significantly higher levels of educational attainment among white women.[17] Thus, school quality improvements indirectly reduce incarceration rates by increasing schooling attainment. In most specifications, these indirect effects are stronger than the direct effects for pupil-teacher ratios and term length, while they are very similar in magnitude (and of opposite sign) for teacher wages. Based on the estimates reported in columns 3 or 4 of Tables 5 and 6, the total effect of a 1 student reduction per teacher would be to lower the probability of incarceration by .001 percentage points, while an extra 10 days added to the school year would result in a reduction of roughly twice that size. Changes in teacher wages have negligible total effects on female incarceration, since the indirect and direct effects roughly cancel each other.[18]

3 The Effect of Education on Female Arrest Rates

The Census data do not allow us to distinguish the differential effects of education on different types of offenses. We, therefore, merge data on female arrests from the 1960, 1970, 1980, and 1990 FBI's Uniform Crime Reports (UCR) with measures of educational attainment from corresponding U.S. Censuses to separately study the impacts of education on female arrest rates for property, violent, and white collar offenses. Appendix B describes these data in detail.

The basic relationship we estimate is:

$$\ln\left(A_{cast}\right) = \beta E_{ast} + \gamma B_{ast} + d_{ca} + d_{ct} + d_{at} + d_{as} + d_{cs} + d_{st} + d_{cst} + \varepsilon_{cast} \qquad (2)$$

[15] These results alleviate concerns raised by Stephens and Yang (2014) regarding the ability to instrument for schooling using compulsory schooling laws due to contemporaneous changes in school quality.

[16] Adding interactions for region of residence × region of birth to specification (2) as suggested in Heckman et al. (1996) produces very similar results. Results available upon request.

[17] The estimated effect of term length in column (5) is the sole exception.

[18] Total effects are calculated by summing the direct and indirect effects, where the latter are obtained by taking the estimated effects of schooling attainment on incarceration from Table 5 and multiplying them by the estimated effects of quality on years of schooling reported in Table 6.

Table 5: Effect of Education and School Quality on Imprisonment for White Women (in Percentage Terms)

	(1)	(2)	(3)	(4)	(5)
A. OLS estimates					
Years of education	-0.006	-0.006	-0.006	-0.006	-0.006
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pupil/Teacher ratio (10's of students)	0.008	0.010	0.009	0.009	0.006
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)
Term length (100's of days)	-0.009	-0.005	-0.018	-0.021	-0.027
	(0.019)	(0.020)	(0.024)	(0.024)	(0.024)
Relative teacher wage	0.008	0.006	0.007	0.008	0.002
	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)
B. IV estimates					
Years of education	-0.061	-0.058	-0.094	-0.078	-0.066
	(0.018)	(0.018)	(0.037)	(0.031)	(0.038)
Pupil/Teacher ratio (10's of students)	-0.027	-0.020	-0.021	-0.021	0.001
	(0.013)	(0.013)	(0.015)	(0.015)	(0.008)
Term length (100's of days)	-0.008	0.005	-0.004	-0.006	-0.050
	(0.020)	(0.021)	(0.027)	(0.026)	(0.028)
Relative teacher wage	0.029	0.026	0.043	0.038	0.017
	(0.010)	(0.010)	(0.018)	(0.016)	(0.013)
First Stage					
Compulsory attendance = 9	0.051	0.046	0.001	0.004	0.036
	(0.017)	(0.017)	(0.017)	(0.018)	(0.013)
Compulsory attendance = 10	0.143	0.133	0.070	0.085	0.062
	(0.024)	(0.023)	(0.022)	(0.022)	(0.018)
Compulsory attendance $\geq$ 11	0.191	0.186	0.092	0.108	0.100
	(0.023)	(0.022)	(0.021)	(0.023)	(0.018)
F-statistic for excluded instruments	27.19	28.64	10.1	12.34	11.29
Additional controls:					
State of residence $\times$ year effects		y	y	y	y
State of residence $\times$ age			y		
State of residence $\times$ broad age group				y	
Region of birth $\times$ cohort trend					y

Notes: Standard errors corrected for state of birth-year of birth clustering are in parentheses. All regressions include dummies for state of residence, dummies for state of birth (excluding Alaska and Hawaii), dummies for age groups (age 20-22, 23-25, …, 56-58, and 59-60), dummies for decade of birth (1914-1923, 1924-1933, …, 1964-1974), and dummies for census year. "broad age group" reflects three dummies for the following age groups: 20-34, 35-49, and 50-64. The sample size is 3,495,789. The F-test for excluded instruments is distributed $F_{(3,2692)}$.

Table 6: Effect of School Quality on Years of Completed Schooling for White Women

	(1)	(2)	(3)	(4)	(5)
Pupil/Teacher ratio (10's of students)	-0.627	-0.570	-0.340	-0.414	-0.078
	(0.033)	(0.032)	(0.034)	(0.035)	(0.033)
Term length (100's of days)	0.020	0.192	0.161	0.209	-0.375
	(0.113)	(0.109)	(0.116)	(0.120)	(0.103)
Relative teacher wage	0.384	0.377	0.412	0.415	0.250
	(0.049)	(0.049)	(0.054)	(0.055)	(0.041)
Additional controls:					
State of residence × year effects		y	y	y	y
State of residence × age			y		
State of residence × broad age group				y	
Region of birth × cohort trend					y
R^2	0.12	0.12	0.12	0.12	0.12

Notes: Standard errors corrected for state of birth-year of birth clustering are in parentheses. All regressions include dummies for state of residence, dummies for state of birth (excluding Alaska and Hawaii), dummies for age groups (age 20-22, 23-25, ..., 56-58, and 59-60), dummies for decade of birth (1914-1923, 1924-1933, ..., 1964-1974), and dummies for census year. "broad age group" reflects three dummies for the following age groups: 20-34, 35-49, and 50-64. The sample size is 3,495,789.

where $\ln(A_{cast})$ is the natural logarithm of the female arrest rate for offense c, five-year age group a, in state s, in year t; E_{ast} is average years of schooling (or the high school graduation rate) for women in age group a living in state s in year t. B_{ast} is the proportion of black females in age group a in state s in year t. Both E_{ast} and B_{ast} are obtained from the Census. The d's are indicator variables to control for unobserved heterogeneity across states, age groups, criminal offenses, and years. Most notably, d_{st} (and d_{cst}) account for variation in enforcement policies across states and over time (by offense type). Fixed effects d_{ca} account for well-documented differences in age profiles by offense type, while d_{at} and d_{as} allow for systematic variation in age-crime profiles over time and across states.

An important difference between this analysis and our Census-based incarceration analysis above is that we now use state-level averages (rather than individual-level measures) for arrests and educational attainment. Since our individual-based analysis of incarceration enabled us to distinguish between state of birth and state of current residence, we could freely control for age- and time-specific effects by state of residence while still exploiting variation in compulsory schooling ages across cohorts and states of birth. This is not possible with our aggregated analysis using the UCR data. Instead, our analysis here computes measures of compulsory schooling levels and school quality levels that applied to residents in each state s from age group a in year t based on those residents' state of birth and year of birth. Thus, our instruments now represent the fractions of age a individuals living in state s in year t that were born in states that had compulsory schooling ages of 9, 10, and 11 or more when they were age 14.[19] School quality measures are calculated in an analogous way. (See Appendix B for details.) By construction, these instruments and quality measures only vary at the state-cohort level, so it is not possible to control for unrestricted state-age and state-year effects due to multicollinearity. To flexibly account for different enforcement policies across states over time, we control for state-year effects; however, we are then only able to control for broad age group (i.e. ages 20-34, 35-49, 50-60) effects by state (as in column (4) of Tables 3 and 5). Even this proves too demanding in some specifications.

Table 7 presents OLS estimates of the effects of education on log arrest rates for all crimes (panel A) and separately for violent, property, and white collar offenses (panel B). The estimates in panel A indicate that a one-year increase in average years of schooling among women is associated with a 12-15 percent decline in female arrest rates. The OLS estimates in columns 4-6 for high school suggest that a 10 percentage point increase in the high-school graduation rate is associated with a 7-12 percent reduction in arrest rates. Because the estimated effects of high school completion rates are qualitatively similar to those for average years of schooling, the rest of our discussion focuses on the latter (columns 1-3).

Panel B of Table 7 estimates separate effects of schooling by broad type of offense. A one-year increase in average female education reduces arrest rates by about 30 percent for violent crimes (murder, robbery, assault) and roughly 10 percent for property crimes (burglary, larceny, motor vehicle theft, arson). Estimated effects of education on arrests for white collar offenses (forgery, fraud, embezzlement) are negligible and statistically insignificant. Table 8 examines arrests by more detailed offense types, estimating separate models for violent offenses, property offenses, and white collar offenses. These estimates reveal strong effects of education on murder, assault, motor vehicle theft, and embezzlement, all decreasing more than 30 percent in response to a one year increase in average schooling levels. It is also noteworthy that education appears to *increase* forgery, with estimates statistically significant in specifications that do not control for state-specific age effects.

Table 9 reports estimates of models analogous to those of Table 7 using the changes in compulsory schooling laws as instruments for educational attainment. The weaker first-stage effects of compulsory attendance laws on average education levels (compared to the effects reported in Table 3 for our individual-level analysis of incarceration) is not surprising, since we can no longer exploit variation in the laws across states of birth within current state of

[19]This approach improves on that of Lochner and Moretti (2004), who use compulsory schooling laws that applied in state s when the mid-point of age group a in year t was age 14. The approach taken in this paper accounts for cross-state migration patterns and yields more powerful instruments.

Table 7: OLS Estimates of the Effect of Schooling on Log Arrest Rates

	Average years of education			High school graduation rate		
	(1)	(2)	(3)	(4)	(5)	(6)
A. All Offenses	-0.146	-0.128	-0.117	-1.189	-1.118	-0.668
	(0.056)	(0.056)	(0.056)	(0.313)	(0.313)	(0.342)
B. Effects by Broad Offense Type						
Violent crime	-0.362	-0.306	-0.291	-2.578	-2.179	-1.727
	(0.060)	(0.063)	(0.062)	(0.345)	(0.364)	(0.389)
Property crime	-0.139	-0.091	-0.082	-1.430	-1.243	-0.802
	(0.062)	(0.065)	(0.063)	(0.366)	(0.384)	(0.389)
White collar crime	0.059	0.002	0.019	0.431	0.038	0.533
	(0.062)	(0.058)	(0.063)	(0.365)	(0.334)	(0.387)
Controls:						
age × offense effects	y	y	y	y	y	y
offense × year effects	y	y	y	y	y	y
age × year effects	y	y	y	y	y	y
state × year	y	y	y	y	y	y
state × offense effects	y	y	y	y	y	y
state × offense × year effects		y	y		y	y
state × broad age group			y			y

Notes: Standard errors for state-year-age clustering are in parentheses. The dependent variable is the logarithm of the arrest rate by age, type of offense, state, and year. Average schooling and high school graduation rate is by age group, state, and year. All models control for the percentage black. There are eight age groups: 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55-59. There are 50 states plus the District of Columbia and four years 1960, 1970, 1980, and 1990. All models are weighted by cell size calculated as the number of women in each cell from the Census.

Table 8: OLS Estimates of the Effect of Schooling on Log Arrest Rates for Detailed Offense Types

	Average years of education			High school graduation rate		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Violent offenses only						
Murder	-0.420	-0.378	-0.356	-2.223	-2.133	-1.260
	(0.093)	(0.099)	(0.112)	(0.566)	(0.605)	(0.676)
Robbery	0.004	-0.090	-0.101	0.061	-0.677	-0.028
	(0.108)	(0.123)	(0.119)	(0.729)	(0.822)	(0.772)
Assault	-0.419	-0.402	-0.404	-2.600	-2.388	-1.710
	(0.076)	(0.074)	(0.100)	(0.426)	(0.414)	(0.582)
B. Property offenses only						
Burglary	-0.014	0.056	0.168	-0.972	-0.501	0.230
	(0.089)	(0.092)	(0.099)	(0.530)	(0.564)	(0.595)
Larceny	0.003	0.009	0.117	-0.832	-0.697	-0.043
	(0.068)	(0.067)	(0.084)	(0.402)	(0.403)	(0.504)
Vehicle theft	-0.381	-0.373	-0.300	-3.866	-4.115	-3.782
	(0.126)	(0.141)	(0.127)	(0.786)	(0.881)	(0.802)
Arson	-0.083	-0.087	0.006	-1.713	-2.226	-1.645
	(0.130)	(0.133)	(0.126)	(0.894)	(0.947)	(0.878)
C. White collar offenses only						
Forgery	0.178	0.215	0.104	1.133	1.282	0.994
	(0.083)	(0.076)	(0.096)	(0.536)	(0.485)	(0.591)
Fraud	0.046	0.016	-0.101	0.604	0.562	0.267
	(0.072)	(0.062)	(0.085)	(0.452)	(0.389)	(0.486)
Embezzlement	-0.489	-0.412	-0.525	-2.945	-2.500	-2.861
	(0.122)	(0.121)	(0.139)	(0.840)	(0.792)	(0.875)
Controls:						
age × offense effects	y	y	y	y	y	y
offense × year effects	y	y	y	y	y	y
age × year effects	y	y	y	y	y	y
state × year	y	y	y	y	y	y
state × offense effects	y	y	y	y	y	y
state × offense × year effects		y	y		y	y
state × broad age group			y			y

Notes: Standard errors for state-year-age clustering are in parentheses. The dependent variable is the logarithm of the arrest rate by age, type of offense, state, and year. Average schooling and high school graduation rate is by age group, state, and year. All models control for the percentage black. There are eight age groups: 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55-59. There are 50 states plus the District of Columbia and four years 1960, 1970, 1980, and 1990. All models are weighted by cell size calculated as the number of women in each cell from the Census.

residence with our aggregated data. Still, in specifications that do not include state-specific age-group fixed effects (i.e. columns 1, 2, 4 and 5), the first-stage F-statistics for the excluded instruments satisfy conventional criteria for "strong" instruments (Staiger and Stock, 1997) and yield IV estimates that are precise enough to rule out small effects for all but white collar crime. For example, column 2 suggests that a one-year increase in average years of schooling reduces arrests for violent crime by about 50 percent and property crime by 67 percent, both statistically significant. Specifications that also control for state-specific age group effects (columns 3 and 6) are much less precise. As discussed earlier, controlling for state-specific year effects and state-specific age effects leaves little available within-state variation across cohorts even when the state-age effects are based on broad age groups of 10-15 years.

3.1 Impacts of School Quality

We now include our three measures of state- and cohort-specific school quality in estimating equation (2).[20] Table 10 reports OLS (panel A) and IV (panel B) estimates, where we estimate the effects of average education on all types of offenses. Except for the specifications that include state-specific age effects (i.e. columns 3 and 6), both OLS and IV estimates of the impact of average educational attainment are statistically significant and very similar to their counterparts when we do not control for school quality (Tables 7 and 9).[21]

The effects of school quality on arrest rates are also statistically significant for all three measures of quality; however, the estimated effects of pupil/teacher ratios is the opposite of what one would expect. Increases in term length and teacher wages reflect improvements in school quality, while increases in pupil/teacher ratio are associated with reductions in quality. Holding average years of schooling constant, increases in the pupil/teacher ratio, term length, and teacher salary all lead to subsequent reductions in female arrest rates. Since the pupil/teacher ratio is measured in tens of students per teacher, the estimates from columns 1 and 2 indicate that a one student increase per teacher reduces female arrest rates by 4-9 percent. Since term length is measured in hundreds of days, the estimates from columns 1 and 2 indicate that a 10 day increase in term length would reduce female arrest rates by about 13 percent. Similarly, the estimates from columns 1 and 2 indicate that a 10 percent increase in teacher wages above the national average reduces female arrest rates by about 6 percent.[22]

[20]See Appendix B for details on our treatment of these data.

[21]Once we control for state-specific age effects (columns 3 and 6), our compulsory schooling laws become fairly weak instruments as evidenced by the low F-statistics.

[22]The indirect effects of improvements in quality through increased schooling attainment are all positive but smaller (in absolute value) than the direct effects. Therefore, the total effect of the pupil-teacher ratio is still of unexpected sign.

Table 9: IV Estimates of the Effect of Schooling on Log Arrest Rates

	Average years of education			High school graduation rate		
	(1)	(2)	(3)	(4)	(5)	(6)
A. All Offenses	-0.401	-0.367	-0.491	-2.114	-1.925	-1.852
	(0.115)	(0.111)	(0.247)	(0.604)	(0.582)	(1.125)
First Stage						
Compulsory attendance = 9	0.389	0.391	0.179	0.077	0.077	0.038
	(0.056)	(0.060)	(0.044)	(0.011)	(0.011)	(0.008)
Compulsory attendance = 10	0.490	0.492	0.245	0.085	0.086	0.044
	(0.066)	(0.070)	(0.052)	(0.011)	(0.011)	(0.008)
Compulsory attendance $\geq$ 11	0.582	0.584	0.224	0.116	0.117	0.059
	(0.076)	(0.081)	(0.061)	(0.011)	(0.012)	(0.009)
F-statistic for excluded instruments	21.88	19.34	7.53	36.46	32.76	15.66
B. Effects by Broad Offense Type						
Violent crime	-0.700	-0.502	-0.648	-3.692	-2.578	-2.553
	(0.128)	(0.145)	(0.252)	(0.699)	(0.758)	(1.205)
Property crime	-0.647	-0.669	-0.801	-3.328	-3.230	-3.122
	(0.173)	(0.186)	(0.287)	(0.869)	(0.869)	(1.264)
White collar crime	0.178	0.128	0.009	0.843	0.307	0.455
	(0.137)	(0.134)	(0.261)	(0.694)	(0.590)	(1.140)
Controls:						
age × offense effects	y	y	y	y	y	y
offense × year effects	y	y	y	y	y	y
age × year effects	y	y	y	y	y	y
state × year	y	y	y	y	y	y
state × offense effects	y	y	y	y	y	y
state × offense × year effects		y	y		y	y
state × broad age group			y			y

Notes: Standard errors for state-year-age clustering are in parentheses. The dependent variable is the logarithm of the arrest rate by age, type of offense, state, and year. Average schooling and high school graduation rate is by age group, state, and year. All models control for the percentage black. There are eight age groups: 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55-59. There are 50 states plus the District of Columbia and four years 1960, 1970, 1980, and 1990. All models are weighted by cell size calculated as the number of women in each cell from the Census. The F-test for excluded instruments is distributed $F_{(3,1403)}$.

Finally, Table 11 reports separate estimates for each broad type of offense. The results show that increases in average education significantly reduce female arrest rates for violent and property offenses but have no significant effect on arrests for white-collar offenses. The results also show that increases in term length significantly reduce violent crime arrests, increases in teacher wages significantly reduce property crime arrests, and increases in the pupil/teacher ratio significantly reduce arrests for all types of crime.

4 Why does Education Reduce Female Crime?

Education impacts many aspects of life. By raising skill levels, education improves labor market opportunities. It may also affect marriage opportunities, childbearing decisions, and family formation. More generally, education may affect social networks and bonds. We next discuss these potential channels through which education may affect crime and provide new estimates of the causal impacts of education on labor supply and childbearing for women in our 1960-1980 Census samples.[23]

Lochner and Moretti (2004) argue that the estimated effects of education on male crime can be largely explained by the well-documented effects of education on wages and the tradeoff men may face between work and crime (Becker, 1968; Freeman, 1996; Lochner, 2004).[24] There are reasons to question whether this same explanation applies to female crime given the substantial differences in labor market participation between men and women: in our sample, roughly 65% of black and white women had worked during the year, much less than the 83% and 94% of black and white men, respectively. Thus, it is possible that wage returns to education may be less relevant for female crime; however, the fact that female labor supply is more elastic to changes in wages (Blundell and MaCurdy, 1999) suggests that the opposite might be true.

Using Census data on weeks worked last year, we estimate the effects of education and school quality on female labor supply decisions using OLS and IV specifications analogous to those reported in column 3 of Tables 3 and 5. While the OLS results reveal a positive correlation between educational attainment and female labor supply, the IV results suggest weak and potentially negative causal effects of schooling on employment (defined by positive weeks worked) and the number of weeks worked. See columns 1 and 2 of Table 12. The IV results in this table, which do not control for school quality measures, show that an additional

[23]Schooling may also alter preferences for risk, self-control, or time discounting. See Oreopoulos and Salvanes (2011) for a recent survey of evidence on the broad ranging impacts of education on individuals.

[24]Lochner and Moretti (2004) show that the estimated effect of education on male arrest rates is very similar to what one obtains by multiplying the estimated effects of education on wages by the effect of local wages on arrest rates as estimated by, e.g., Gould et al. (2002).

Table 10: Effects of Education and School Quality on Log Arrest Rates

	Average years of education			High school graduation rate		
	(1)	(2)	(3)	(4)	(5)	(6)
A. OLS estimates						
Education / HS Grad	-0.180	-0.147	-0.065	-1.081	-0.965	-0.415
	(0.070)	(0.071)	(0.065)	(0.402)	(0.405)	(0.356)
Pupil/Teacher ratio (10's of students)	-0.510	-0.440	-0.302	-0.513	-0.460	-0.304
	(0.118)	(0.122)	(0.143)	(0.117)	(0.119)	(0.141)
Term length (100's of days)	-1.330	-1.286	-0.885	-1.425	-1.364	-0.935
	(0.327)	(0.333)	(0.355)	(0.318)	(0.325)	(0.351)
Relative teacher wage	-0.656	-0.639	-0.324	-0.599	-0.582	-0.313
	(0.171)	(0.179)	(0.163)	(0.171)	(0.180)	(0.163)
B. IV estimates						
Education / HS Grad	-0.510	-0.442	-0.330	-2.636	-2.270	-0.787
	(0.226)	(0.218)	(0.405)	(0.998)	(0.974)	(1.475)
Pupil/Teacher ratio (10's of students)	-0.892	-0.784	-0.468	-0.816	-0.716	-0.342
	(0.257)	(0.249)	(0.288)	(0.195)	(0.191)	(0.181)
Term length (100's of days)	-1.203	-1.176	-0.768	-1.462	-1.395	-0.954
	(0.342)	(0.325)	(0.362)	(0.315)	(0.299)	(0.315)
Relative teacher wage	-0.520	-0.517	-0.176	-0.410	-0.423	-0.271
	(0.193)	(0.189)	(0.262)	(0.201)	(0.197)	(0.216)
First Stage						
Compulsory attendance = 9	0.180	0.180	0.056	0.045	0.045	0.025
	(0.046)	(0.049)	(0.039)	(0.008)	(0.008)	(0.007)
Compulsory attendance = 10	0.299	0.300	0.162	0.056	0.056	0.036
	(0.056)	(0.060)	(0.047)	(0.009)	(0.009)	(0.008)
Compulsory attendance $\geq$ 11	0.328	0.328	0.084	0.079	0.079	0.044
	(0.067)	(0.072)	(0.053)	(0.009)	(0.010)	(0.008)
F-statistic for excluded instruments	10.89	9.50	5.44	23.66	20.98	10.85
Controls:						
age × offense effects	y	y	y	y	y	y
offense × year effects	y	y	y	y	y	y
age × year effects	y	y	y	y	y	y
state × year effects	y	y	y	y	y	y
state × offense effects	y	y	y	y	y	y
state × offense × year effects		y	y		y	y
state × broad age group			y			y

Notes: Standard errors for state-year-age clustering are in parentheses. The dependent variable is the logarithm of the arrest rate by age, type of offense, state, and year. Average schooling and high school graduation rate is by age group, state, and year. All models control for the percentage black. There are eight age groups: 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55-59. There are 50 states plus the District of Columbia and four years 1960, 1970, 1980, and 1990. All models are weighted by cell size calculated as the number of women in each cell from the Census. The F-test for excluded instruments is distributed $F_{(3,1354)}$.

Table 11: IV Estimates of the Effects of Years of Schooling and School Quality on Log Arrest Rates by Broad Offense Type

	Violent crime	Property crime	White collar crime
Education	-0.521	-0.789	0.085
	(0.273)	(0.310)	(0.244)
Pupil/Teacher ratio (10's of students)	-0.855	-0.821	-0.652
	(0.263)	(0.257)	(0.273)
Term length (100's of days)	-1.848	-0.778	-1.275
	(0.635)	(0.683)	(0.730)
Relative teacher wage	-0.369	-0.874	-0.169
	(0.225)	(0.205)	(0.264)

Notes: This table reports estimated effects (from a single IV regression) of interactions between indicators for broad offense type (violent, property, white collar) and the following: years of schooling, pupil/teacher ratio, term length, and relative teacher wage. Compulsory schooling laws interacted with indicators for broad offense type are used as instruments for years of schooling interacted with broad offense type. Other controls include age × offense effects, offense × year effects, age × year effects, state × year, state × offense effects, state × offense, and state × offense × year effects.

year of schooling reduces the probability of employment by about 0.03 for white women and 0.02 for black women, where only the former is statistically significant. Estimated effects on weeks worked are negative but statistically insignificant for both races. Accounting for school quality (see Table 13), the estimated effects of schooling on female labor supply are even more negative (though still modest) for white women. These results further suggest that changes in school quality have no direct effects on employment decisions, while a 10 day increase in school term length would lead to a modest (but statistically significant) increase in weeks worked. It is worth noting that these estimates are in sharp contrast to significant positive effects of educational attainment on weeks worked among men.[25]

Tables 12 and 13 suggest that is unlikely that education reduced crime among women (at least over our sample period, 1960-1980) by encouraging work instead. However, as discussed in the introduction, education may affect women's choices to engage in crime for a number of other reasons.

In particular, the family may play a more important role in criminal decisions for women than for men (Steffensmeier and Streifel, 1992; Schwartz and Steffensmeier, 2007). To what extent, then, do educational attainment and school quality affect marriage and childbearing outcomes among women? Due to assortative mating in marriage markets (Becker, 1991), we would expect that education improves women's marital prospects. Evidence from twin studies suggest that an additional year of schooling raises that of a woman's spouse by 0.2-0.4

[25]Estimated effects of schooling on employment (based on IV specifications in Table 12) are 0.035 (0.008) for white men and 0.008 (0.010) for black men. Estimated effects on weeks worked are 3.132 (0.656) and 1.481 (0.606) for white and black men, respectively.

Table 12: Effects of Years of Schooling on Female Labor Supply and Childbearing

	Employment	Weeks Worked	Number of own Children in Household
A. Whites			
OLS estimates	0.031	1.339	-0.144
	(0.000)	(0.013)	(0.002)
IV estimates	-0.031	-0.804	0.575
	(0.014)	(0.679)	(0.194)
Sample Size	3,613,313	3,613,313	1,708,103
B. Blacks			
OLS estimates	0.037	1.754	-0.167
	(0.000)	(0.019)	(0.002)
IV estimates	-0.021	-0.213	0.595
	(0.017)	(0.829)	(0.190)
Sample Size	480,709	480,709	215,488

Notes: Standard errors corrected for state of birth-year of birth clustering are in parentheses. All regressions include dummies for state of residence, dummies for state of birth (excluding Alaska and Hawaii), dummies for age groups (age 20-22, 23-25, ..., 56-58, and 59-60), dummies for decade of birth (1914-1923, 1924-1933, ..., 1964-1974), dummies for census year, State-of-residence $\times$ year effects, and State-of-residence $\times$ age. The regressions for black females also include state-of-birth dummies interacted with a dummy for black women born in the South who turn age 14 in 1958 or later to account for the impact of *Brown v. Board of Education.* For "Employment" specifications, the dependent variable is a dummy equal to one if the respondent worked positive weeks last year, and zero if not. For the "Weeks Worked" specifications, the dependent variable is the number of weeks worked over the year, including respondents with zero weeks worked. Regressions for the "Number of own Children in Household" only include females ages 20-40.

Table 13: IV Estimated Effects of Years of Schooling and School Quality on Labor Supply and Childbearing: Whites Only

	Employment	Weeks Worked	Number of own Children in Household
Years of education	-0.056	-2.406	0.444
	(0.026)	(1.275)	(0.210)
Pupil/Teacher ratio (10's of students)	-0.011	-0.476	-0.311
	(0.010)	(0.515)	(0.057)
Term length (100's of days)	0.023	2.516	0.242
	(0.016)	(0.780)	(0.246)
Relative teacher wage	0.006	0.232	0.106
	(0.014)	(0.657)	(0.109)
Sample Size	3,495,789	3,495,789	1,708,103

Notes: Standard errors corrected for state of birth-year of birth clustering are in parentheses. All regressions include dummies for state of residence, dummies for state of birth (excluding Alaska and Hawaii), dummies for age groups (age 20-22, 23-25, ..., 56-58, and 59-60), dummies for decade of birth (1914-1923, 1924-1933, ..., 1964-1974), dummies for census year, State-of-residence $\times$ year effects, and State-of-residence $\times$ age. For "Employment" specifications, the dependent variable is a dummy equal to one if the respondent worked positive weeks last year, and zero if not. For the "Weeks Worked" specifications, the dependent variable is the number of weeks worked over the year, including respondents with zero weeks worked. Regressions for the "Number of own Children in Household" only include females ages 20-40.

years (Behrman and Rosenzweig, 2002; Oreopoulos and Salvanes, 2011). Using quarter of birth as an instrument for own schooling attainment, Lefgren and McIntyre (2006) estimate negligible effects of women's schooling on the likelihood of marriage but significantly positive effects on husband's earnings. An extra year of education results in an additional \$4,000 in spousal earnings.[26] These additional resources and the family stability that likely comes with them may help explain the significant reductions in crime associated with educational attainment among women. The effects of education on spousal quality may also be important due to changes in social networks, creation of social bonds, and/or exercise of informal social control (Sampson and Laub, 1990; Laub et al., 1998; Warr, 1998; Sampson et al., 2006).

Another possible channel through which education may reduce criminal activity among women is the presence of children in the household, which both requires attention from mothers at home and likely raises the personal costs associated with incarceration. The existence of children may also alter women's social networks and build stronger family bonds. The IV results in Table 12 indicate that an additional year of schooling significantly increases the number of own children in the household by 0.6 for both black and white women.[27] Table 13 shows that reductions in class size as well as educational attainment increase the number of children in the household among white women.

5 Conclusions

This paper provides some of the first evidence that increases in educational attainment lead to significant reductions in female crime as measured by both incarceration and arrest rates. Our estimates suggest that a one-year increase in average schooling levels reduces female arrest rates for both violent and property crime by more than 50%, while there is little impact on white collar crime. Although, arrest and incarceration rates are much lower for women than men, the impacts of education are larger in percentage terms for women.

We show that the estimated effects of education on incarceration and arrests are very similar whether or not we control for state- and cohort-specific school quality levels as measured by pupil-teacher ratios, term length, and teacher wage rates. The estimated direct effects of these quality measures are more mixed depending on the measure of quality and whether we look at arrests or incarceration. The indirect effects of quality improvements through increased schooling are positive for all quality measures but are generally modest in

[26] Unfortunately, using compulsory schooling laws as instruments for educational attainment is unlikely to yield consistent estimates of the effect of own education on marriage outcomes, since these laws (and our school quality measures) affect aggregate schooling levels (for men and women) and entire marriage markets.

[27] These estimates are based on women ages 20-40 when few would have children old enough to have already left the home.

size.

Finally, we consider the extent to which education may reduce crime by influencing labor market opportunities, marriage opportunities, and childbearing decisions. Our results suggest that the estimated effects of education on female crime (at least from 1960-1980) are unlikely to be driven by improvements in labor market opportunities (as seems likely for men), since we find little effect of schooling on female labor supply behavior. Instead, education appears to improve the marital prospects of women and causes them to have more children, which may reduce incentives to engage in crime for a variety of reasons. Of course, the way in which education impacts female crime may have changed in more recent decades as women have increasingly entered the labor market, reduced their time at home, and raised fewer children.

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Appendix

A Data for Analysis of Education and Incarceration

For our analysis of incarceration, we use Census data from the 1960, 1970, and 1980 US Censuses. The Census data is obtained from the Integrated Public Use Microdata Series (IPUMS) and the particular samples we use are the: (i) 1 percent sample of the 1960 US Census, (ii) 1 percent state samples of the 1970 US Census, Form 1 and Form 2, and (iii) 5 percent state sample of the 1980 US Census.

The sample only includes black or white females ages 20-60 who were born in the 48 contiguous states (i.e. excludes Alaska and Hawaii). The indicator for incarceration is based on the variable for the group quarters type, set to one if the respondent is in a correctional institution and zero otherwise. Years of schooling are based on the highest grade of schooling completed (nursery and kindergarten are considered as zero years of schooling).

The analysis includes dummies for 14 age groups: 20-22, 23-25,..., 56-58, and 59-60. When we control for state-specific broad age categories, these are based on ages 20-34, 35-49, and 50-60. We also include six birth cohort dummies for women born in 1914-1923, 1924-1933, 1934-1943, 1944-1953, 1954-1963, and 1964-1974.

These data are merged with data on compulsory attendance laws based on two variables: (i) the state of birth of the respondent, and (ii) the year in which the respondent was age 14. As in Acemoglu and Angrist (2001) and Lochner and Moretti (2004), we define compulsory attendance as the maximum between (i) the minimum number of years that a child is required to stay in school and (ii) the difference between the earliest age that she is required to be in school and the latest age she is required to enroll. We create three indicator variables for states with compulsory schooling laws that require: (i) 9 years of schooling, (ii) 10 years of schooling, and (iii) 11 or more years of schooling. The omitted category in the analysis is those states requiring 8 or less years of schooling. For further details about these data, see Acemoglu and Angrist (2001) and Lochner and Moretti (2004).

Finally, these data are merged with measures of school quality based on two variables: (i) the state of birth of the respondent, and (ii) the year of birth of the respondent. The measures of quality are: (i) pupil/teacher ratios, (ii) school term length, and (iii) relative teacher salaries. Pupil/teacher ratios are re-scaled to reflect the number of pupils per teacher divided by 10. School term length is scaled to reflect hundreds of days. Teacher salaries are relative to the national average teacher salary, which is obtained for each year by taking a simple average over all state average salaries. For each year of birth, these measures correspond to average quality for public schools in their state of birth over the years in which the respondent was ages 6-17 (elementary and secondary school). For further details on these data, see Card and Krueger (1992a) and Stephens and Yang (2014).

B Data for Analysis of Education and Arrests

The data on female arrests is obtained from the FBI Uniform Crime Reports (UCR) for years 1960, 1970, 1980, and 1990. We compute the arrest counts by state, year, offense, and age group for females. The offenses considered in the analysis are those for violent, property, and white collar crimes. The violent crime offenses considered in the analysis include: murder and non-negligent manslaughter, robbery, and aggravated assault. The property crime offenses considered include: burglary - breaking or entering, larceny - theft (except motor vehicle), motor vehicle theft, and arson. The white collar crime offenses considered include: forgery and counterfeiting, fraud, and embezzlement. We use arrest counts for women ages 20-59 grouped as follows: ages 20-24, ages 25-29,..., age 55-59. Since the UCR data only contain population counts by state and year (not separately by age group), we must merge these data with Census data to determine age-specific arrest rates.

The data on arrest counts are merged with Census data for years 1960, 1970, 1980, and 1990. The Census data for 1960-1980 correspond to the same samples explained in Appendix A, while we use the 5 percent sample (with sample weights) for 1990. From the Censuses, we can compute the age distribution among the relevant female population, which can then be multiplied by the population covered by state-year in the UCR to calculate population counts by age, state, and year. We then divide the UCR arrest counts (by offense, age, state, and year) by the population counts (by age, state, and year) to create the arrest rate measures used in our analysis.

From the Census data, we also obtain measures of average years of completed education, high school graduation rates, and the fraction black by year, state, and age group, where the age groups match those from the UCR data. These measures are un-weighted for years 1960, 1970, and 1980, and are weighted using the Census sampling weights for 1990. Females from all races are included when computing these measures. Since schooling is only reported in intervals for grades 1-4 and 5-8 in the 1990 Census, we use average years of schooling within these categories from the 1980 Census to assign years of schooling for 1990 respondents in these two categories.

To incorporate the compulsory attendance laws and school quality into the analysis of arrest rates, we merge the Census data at the individual level with the compulsory attendance laws and with the school quality data following the exact same procedure as described in Appendix A. That is, we assign compulsory attendance laws for each woman based on the year in which she was age 14 and her state of birth. Similarly, we assign school quality measures for each woman based on her year of birth and her state of birth. Once these measures are assigned to the female respondents in the Census, we obtain averages of these measures by year, state of residence, and age group. Notice that in this case, the compulsory attendance laws are no longer indicator variables. Instead, they reflect the probability that a women from age group a living in state s in year t was born in a state that had a specific schooling law when she was age 14. In this way, we account for inter-state migration patterns and exploit the actual experiences of women in terms of their schooling laws and school quality.

Finally, the UCR arrest data is merged with the averaged Census data (which contains the averaged compulsory attendance laws and school quality measures) based on year, state, and age group. The Census data also contains the number of females in each cell, which is used as a weight in all regressions.