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More Sports, Less Crime:

Title IX Reduced Female Arrests

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Abstract

Advocates of youth sports programs, including the United Nations Office on Drugs and Crime, argue that athletic competition reduces crime among participants, thereby generating external social benefits. However, next to nothing is known about the causal impact of sports participation on crime. Using data from the 1980-2000 Uniform Crime Reports, and exploiting plausibly exogenous variation in female sports participation generated by the introduction of the 1972 educational amendments to Title IX, we find that a 10 percentage-point increase in female high school sports participation reduced arrests among affected cohorts of 25-to-39-year-old females by 1 arrest per 1,000 population. Arrest reductions are observed for both property and violent offenses. These results are consistent with substantial Title IX-induced gains in educational attainment and labor market outcomes for affected female birth cohorts. The magnitudes of our estimates suggest that the full implementation of Title IX generated approximately $9 billion in social benefits from crime reduction.

Keywords: Title IX; sports participation; crime; arrests

JEL Codes: I2, H5

1. Introduction

"Sports offer an important opportunity for building life skills of at-risk youth that allow them to better cope with daily life challenges and move away from involvement in violence, crime or drug use."

- United Nations Office on Drugs and Crime (2018)

More than seven million American high school students participate in some form of school-sponsored athletic program (National Federation of State High School Association 2018) and an additional 500,000 students participate in college athletics (National College Athletic Association 2019). Youth sports participation has been linked to important benefits for participants, including increased educational attainment (Barron et al. 2000; Eide and Ronan 2001; Pfeifer and Cornelissen 2010; Stevenson 2010; Gorry 2016), higher aspirations for post-secondary schooling (Rees and Sabia 2010), and improved labor market outcomes (Barron et al. 2000; Persico et al. 2004; Stevenson 2010; Cabane and Clark 2015).[1]

However, many advocates of youth sports programs claim that the benefits of playing sports extend beyond participants. A May 2018 White House Council of Economic Advisers report stated that youth sports programs enhance the "development of generalizable skills" that yield benefits not only for participants, but "for society as a whole" (Council of Economic Advisers, 2018). The most commonly cited positive externality from youth sports participation is reduced crime (European Commission 2012). The United Nations (UN) argues that youth sports programs serve an important peacekeeping role by deterring crime and curbing drug use (UN 2030 Agenda for Sustainable Development 2015; UN Office on Drugs and Crime 2018). Furthermore, a number of high-profile U.S. Federal and state anti-crime initiatives — such as the Violent Crime Control and Law Enforcement Act of 1994 and the Empire State After School Program of 2018 — included expansions in sports-related funding.[2]

There are a number of channels through which youth sports participation could reduce crime. Cognitive and non-cognitive skill development among sports participants may

[1] See also Long and Caudill 1991; Ewing 1998; Eccles and Barber 1999; Ewing 2007.

[2] The U.S. Violent Crime Control and Law Enforcement Act of 1994 allocated $50 million in federal funding for "midnight basketball" programs. New York State allocated $45 million for after school programs, including school sports, with the objective of fostering social cohesion and creating safer communities (State of New York Governor's Press Office 2018).

facilitate human capital acquisition and improve labor market outcomes (Council of Economic Advisers 2018), resulting in higher opportunity costs of crime. In addition, sports-induced improvements in physical health (Lechner 2009) and psychological wellbeing (Downward and Rasciute 2011), as well as reductions in risky health behaviors such as substance use (Lisha and Sussman 2010; Kwan et al. 2014), may result in less crime (Evans-Cuellar et al. 2004; 2006; Anderson et al. 2015). Sports participation may also expose youths to positive mentoring (Rees and Sabia 2010), improve social networks (Eccles and Barber 1999; Perks 2007), and broaden opportunities for post-secondary educational scholarships (Rees and Sabia 2010; Stevenson 2010), each of which could reduce criminal activity. Finally, school-based sports programs may have short-run incapacitation effects due to increased time spent practicing and participating in regular season games (Sampson and Laub 1990).

On the other hand, school sports programs could have unintended consequences that increase crime. School sports may crowd out time for academic pursuits or reduce school resources available for other human capital-enhancing schooling programs, each of which could increase crime (Maloney and McCormick 1993). Moreover, sports participation may expose youths to lower quality peers, increasing the risk of criminal behavior (Gaviria and Raphael 2001). Finally, large-scale sporting events that attract a substantial fan base may increase crime on game days due to large crowds, problem drinking, or unexpected game outcomes (Lindo et al. 2018; Card and Dahl 2011; Rees and Schnepel 2009).[3]

This study provides the first causal evidence on the impact of sports participation on crime. We exploit the introduction of Title IX, which required educational institutions in the U.S. to achieve greater gender parity in sports participation rates, to isolate the impact of female high school sports participation on female arrests. Title IX is estimated to have increased female high school sports participation by more than 600 percent between 1971 and 1978 (Stevenson, 2010). We exploit the heterogeneous bite of this national policy shock across (i) states with different pre-Title IX male sports participation rates, and (ii) birth cohorts differentially exposed to Title IX, to identify the effect of female sports participation on female criminal arrests. Females are a vastly understudied demographic group in the economics of crime literature (Campaniello and Gavrilova 2018; Beatton et al. 2018) despite

[3] These latter studies have estimated the impact of sporting events on local crime rather than the effect of sports participation on criminal activity among athletes.

their comprising a rising share of total arrestees and evidence that female criminal behavior is more sensitive to human capital investments than male crime (Cano-Urbina and Lochner 2019).

Using data from the 1980 to 2000 Uniform Crime Reports (UCR), our results show that a 10 percentage-point higher pre-Title IX male sports participation rate was associated with a 2 to 3 percent decline in arrests among affected cohorts of 25-to-39 year-old females. Arrest reductions are observed for both violent and property offenses. The magnitudes of our estimated arrest effects are consistent with what one would expect given (i) the magnitudes of labor market and schooling effects of Title IX, and (ii) prior estimates of arrest elasticities with respect to employment and education. Event study analyses and a variety of placebo tests add confidence to a causal interpretation of our findings. Instrumental variables (2SLS) estimates show that a 10 percentage-point increase in female sports participation led to a 1 arrest per 1,000 population percent decline in the total arrest rate of 25-to-39 year-old females. Together, the magnitudes of our estimates imply that the 25 percentage-point increase in female sports participation caused by the full implementation of Title IX generated approximately $9 to $21 billion in cost savings from declines in property (larceny, burglary, and motor vehicle theft) and violent (aggravated assaults) offenses.

2. Background

2.1. Title IX Legislation

The second-wave feminist movement of the 1960s and 1970s promoted greater gender equality in state and Federal law. Among the aims of this movement included "equal pay for equal work" protections, access to legal abortion, an Equal Rights Amendment (ERA) to the U.S. Constitution, and greater gender parity in government spending on education. Spurred by this activism, Congresswoman Edith Green and Senator Birch Bayh drafted the educational amendments to Title IX (20 U.S. Code § 1681), signed into law by President Richard Nixon on June 23, 1972. These amendments state, in part:

> "No person in the United States shall, on the basis of sex, be excluded from participation in, be denied the benefits of, or be subjected to discrimination under any

> educational program or activity receiving financial assistance."[4] (20 U.S. Code § 1681 – Sex)

While the language of these educational amendments to Title IX did not limit "educational programs" to sports, athletics constituted a key funding area with large gender disparities, including, in some cases, official gender segregation (Schulkind 2017). Hence, enforcement of Title IX by the Executive Branch focused almost exclusively on metrics of opportunities available for female athletic participation.[5]

The introduction of Title IX in Congress generated very little opposition on Capitol Hill or from the White House due to strategic efforts by its proponents. Lobbyists supporting the legislation were explicitly told to stand down:

> "[Congresswoman Green] said, 'I don't want you to lobby. Because if you lobby, people will ask questions about this bill, and they will find out what it would really do...' And she was absolutely right. It was quite a big break that no one was watching." (Simpson 2012)

Thus, Title IX was signed into law without much fanfare or attention from advocates or proponents.

The Title IX amendments apply to any educational institution that receives federal financial assistance from the U.S. Department of Education, which includes approximately 16,500 local school districts, 7,000 postsecondary institutions, charter schools, for-profit schools, and other education agencies across all 50 states and the District of Columbia (Department of Education Office for Civil Rights, 2015).

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[4] Single-sex schools, religious schools, and military institutions were exempt from the Title IX amendments.
[5] Until the year 2001, Federal guidelines for compliance with Title IX focused on athletic participation. In recent years, new policy guidelines have been issued by the Office of Civil Rights to expand Title IX's scope to include sexual harassment (2001; 2006; 2014; 2015; 2016), other extracurricular activities (2006; 2014), and services to pregnant students (2006; 2013). In addition, the Obama administration expanded Title IX's reach to include science, technology, engineering, and mathematics fields (White House: Office of the Press Secretary, 2012). See Department of Education guidelines available at: https://www2.ed.gov/about/offices/list/ocr/frontpage/faq/rr/policyguidance/index

Educational institutions were given up to six years to comply with Title IX, which was enforced by the Department of Education's Office for Civil Rights. A three-part test was used to determine compliance with Title IX:

> "(1) The number of male and female athletes is substantially proportionate to their respective enrollments; or
> (2) The institution has a history and continuing practice of expanding participation opportunities responsive to the developing interests and abilities of the underrepresented sex; or
> (3) The institution is fully and effectively accommodating the interests and abilities of the underrepresented sex."
>
> - U.S. Department of Education 1979

The first and largest impact of Title IX was on female sports participation (Stevenson 2007). Between 1971, the final year prior to Title IX's enactment, and 1978, the last year of Title IX's six-year compliance window, the share of females participating in high school sports increased from 1 in 27 to 1 in 4, or by more than 600 percent (Stevenson 2010). While greater equity in sports participation for male and female students is a reasonable way to interpret the requirements of Title IX (Stevenson 2007), there is little evidence that compliance was achieved through reductions in overall male sports participation. Modest compositional changes in male sports following Title IX did appear to occur, however, with some sports, such as soccer, expanded and others, such as wrestling, becoming less prevalent. This finding suggests some movement toward sports that are more commonly popular across both sexes (Stevenson 2007).

2.2. Human Capital and Labor Market Effects of Title IX

Stevenson (2007; 2010) pioneered the literature on the economic impacts of Title IX. First, she concluded that the full implementation of Title IX was responsible for a 25 to 30 percentage-point increase in female sports participation (Stevenson 2007). Then using U.S. Census data from 1980 and 2000, Stevenson (2010) finds that a 10-percentage-point increase in female sports participation induced by Title IX led to a 1 to 3 percentage-point increase in

college attendance and labor force participation among affected female cohorts. These findings suggest important human capital and labor market benefits of Title IX.

The education and labor market benefits of Title IX also appear to extend to health. Kaestner and Xu (2010) find that a 20 percentage-point increase in female sports participation led to a 3 percentage-point decline in the probability of obesity. Moreover, Schulkind (2017) show evidence of an intergenerational health benefit of Title IX, with mothers in affected cohorts experiencing a reduced prevalence of low-birthweight newborns and improved Apgar scores.

Household composition may also be impacted by Title IX. Clarke and Ayres (2014) find that a 10 percentage-point increase in state-level female sports participation generated a 5 percentage-point increase in fertility rates, driven by a 6 percentage-point increase in the proportion of mothers who were unmarried.[6]

2.3. Theoretical Links between Sports Participation and Crime

High school sports participation may affect criminal activity through a number of channels, including human capital acquisition, labor market outcomes, time availability, and physical or mental health. First, if sports participation increases educational attainment through cognitive or non-cognitive skill development, increased post-secondary scholarship availability, or greater aspirations to attend college, such schooling effects may reduce crime. Indeed, economists have found that youth athletic participation is associated with improved educational outcomes, particularly for women, even after accounting for non-random selection into sports participation (Barron et al. 2000; Eide and Ronan 2001; Lipscomb 2007; Rees and Sabia 2010; Stevenson 2010; Felfe et al. 2016).[7] This crime reduction may come

[6] Clarke and Ayres (2014) also find evidence of increased secularism associated with Title IX.

[7] Barron et al. (2000) instrument for school sports participation using school size, library books per student, and teacher-student ratio and find that high school athletic participation is associated with increased educational attainment. They also find that high school sports participation generates greater schooling benefits than participation in other extracurricular activities. Eide and Ronan (2001) use height as an instrument for high school sports participation and find that playing sports has heterogeneous effects on educational attainment by race and gender, with positive effects for females and some negative impacts for white males. Lipscomb (2007) estimate an individual fixed effects model and find that youth athletic participation is associated with an increase in math and science test scores. Rees and Sabia (2010) use a similar identification strategy to Eide and Ronan (2001) and find limited evidence that teen sports participation increases educational attainment, but do find evidence of an increase in aspirations to attend college, particularly among females. Pfeifer and Cornelissen (2010) study German youth, and using a very rich set of individual, family, and birth cohort controls, find that sports participation in Germany is associated with an increase in educational attainment. Finally, Felfe et al. (2016) use a propensity score matching estimator and find that athletic participation is positively associated with school performance.

through several channels. First, school-based sports participation may have a short-run incapacitation effect. Many high school sporting activities take place after school and on weekends, and time spent in school-sponsored sporting activities may reduce time available for criminal activities (Sampson and Laub 1990). On the other hand, time or resources spent on school sports could crowd out academic pursuits, potentially increasing crime.

Second, additional schooling may reduce crime by increasing human capital acquisition, thereby increasing the gains from legitimate labor market work (Council of Economic Advisers 2018; Machin and Meghir 2004). Finally, and relatedly, cognitive and non-cognitive skills acquired by playing sports, as well as availability of post-secondary scholarships, may increase the returns to education, resulting in greater investments in schooling (Stevenson 2010).

There is substantial evidence that more time spent in school — either through additional days spent in school over an academic year (Jacob and Lefgren 2003; Luallen 2006; Fischer and Argyle 2018) or through additional years spent in school due to higher minimum dropout ages (Lochner and Moretti 2004; Machin et al. 2011; Anderson 2014; Bell et al. 2016; Cano-Urbina and Lochner 2019; Beatton et al. 2018) — is negatively related to crime. Evidence for short-run incapacitation effects is fairly strong (Jacob and Lefgren 2003; Anderson 2014; Beatton et al. 2018).[8] Moreover, because crime-reducing effects of schooling appear to extend into adulthood, this could suggest a longer-run human capital channel through which increased education reduced crime (Lochner and Moretti 2004; Machin et al. 2011).

While most studies in the education-crime literature focus on men, or simply study women as a sub-sample among others, a number of recent crime economists have raised the importance of understanding the determinants of female criminal behavior and how they might differ from men (Cano-Urbina and Lochner 2019; Campaniello and Gavrilova 2018; Beatton et al. 2018).[9] The gender gap in arrest rates, particularly for property crime, is

[8] Jacob and Lefgren (2003) exploit variation in the number of school days off induced by teacher strikes and find that property crime arrests increase during strike days, suggesting a possible incapacitation effect. Along the same lines, Fischer and Argyle (2018) exploit temporal variation in the adoption of a four-day school week across jurisdictions in Colorado and find that a four-day week school schedule is associated with a 20 percent increase in juvenile offenses. Anderson (2014) and Beatton et al. (2016) show that increases in the minimum legal school dropout age is negatively related to contemporaneous juvenile crime.

[9] Campaniello and Gavrilova (2018) comment that while criminologists, psychologists and sociologists have extensively examined criminal behavior of women, the subject is still vastly understudied by economists. They identify a small number of papers in economics that attempted to empirically examine crime among women (Bartel 1979; Corman et al. 2014; Cano-Urbina and Lochner 2019; Beatton et al. 2018; Campaniello and

narrowing and new evidence suggests that human capital investments may player a larger role for females than males in deterring crime (Beatton et al. 2018).

Cano-Urbina and Lochner (2019) study the impact of education on female criminal behavior and find that the crime-reducing effects of education are much larger (in percentage terms) for females than males in the U.S. Using changes in compulsory schooling laws as an instrument for educational attainment, they find that a one-year increase in educational attainment reduces arrest rates for both violent and property crime by more than 50 percent among 20-to-59-year-old females.

In addition to schooling-related pathways, there are other human capital channels through which sports participation may reduce crime. Participating in high school sports could improve social networks (Eccles and Barber 1999; Perks 2007) or lead to more socially cooperative behaviors (Celse et al. 2017), each of which could reduce crime. Sports may also improve physical health (Lechner 2009) or psychological health benefits (Downward and Rasciute 2011), each of which reduce criminal activity (Evans-Cuellar et al. 2004; 2006; Anderson et al. 2015).

Finally, sports participation may affect criminal activity through its effects on longer-run labor market outcomes (Barron et al. 2000; Stevenson 2010). If high school sports participation increases labor force participation (Stevenson 2010) and earnings (Barron et al. 2000; Lechner 2009; Stevenson 2010), or enhances job-related leadership skills (Kuhn and Weinberger 2005), these labor market effects could reduce criminal activity (Mustard 2010; Gould et al. 2002; Schnepel 2017; Yang 2017).[10]

2.4. Empirical Evidence on Sports and Crime

Empirical evidence on the relationship between sports participation and crime has largely been based on cross-sectional studies that treat sports participation as exogenous to unmeasured determinants of criminal behavior (Hartmann and Massoglia 2007; McGinn et al. 2008; Nichols 2010; Ekholm 2013; Spruit et al 2016).[11] Therefore, most studies are

Gavrilova 2018). Beatton et al. (2018) emphasize a similar point. They point that existing studies on education and crime rarely consider female crime, usually as a supplement to the focus on men (Lochner and Moretti 2004; Machin et al. 2011; Bell et al. 2016). Hjalmarsson et al. (2015) is an exception.

[10] For other studies that have explored the effect of labor market opportunities on crime, see also, Raphael and Winter-Ebmer (2001), Levitt (2004), Oster and Agell (2007), Lin (2008), and Fone et al. (2019).

[11] A meta-analysis by Spruit et al. (2016) analyze 51 published and unpublished studies, with 48 independent samples, containing 132,366 observations on sports participation and delinquent behavior (i.e. minor crime, or misconduct). Overall, they were unable to find any significant association between sports participation and

unable to disentangle a causal impact of sports from (i) a spurious association due to difficult-to-measure factors, or (ii) reverse causality.

If sport participants are positively selected on difficult-to-measure characteristics such as ability, discipline, social consciousness, or family background characteristics, then estimates of the crime-reducing effects of sports may be inflated (Rees and Sabia 2010). There is, in fact, evidence that sports participants are more likely to be white and drawn from higher income families (Council of Economic Advisers 2018). On the other hand, if sports attract those most at risk for delinquent behaviors, then crime-reducing effects of sports participation may be biased toward zero. Finally, if time spent in criminal endeavors crowds out time available for sports, then an estimated association between sports and crime could reflect reverse causality.

Only three studies of which we are aware have attempted to disentangle selection effects from a causal impact of sports participation on crime. Caruso (2011) uses a panel of 140 region-years from Italy and, employing a two-way fixed effects model, finds that a 10 percent increase in sports participation is associated with an 8 percent reduction in juvenile crime and a 3 percent reduction in adult property crime. Brosnan (2019) uses a panel of 969 jurisdiction-years from the United Kingdom and using a similar two-way fixed effects model, finds that a 10 percent increase in sports participation is associated with a 0.7 percent reduction in property crimes among 16-to-24 year-olds.[12] Finally, Hartmann and Massoglia (2007) exploit individual-level panel data on 763 youths across 13 waves (largely years) and, controlling for factors related to risky behavior prior to sports participation, find that playing sports is associated with a decline in subsequent shoplifting, but an increase in drunk driving.

A second set of studies has examined the impact of local sports program interventions on crime. However, these studies have either been qualitative in nature (Carmichael 2008; Kelly 2013; Parker et al. 2014; Parker et al. 2017), used a before-after estimator among treatment sites (Winther and Currie 1987), or compared crime in treatment and non-treatment sites at a single point in time (Hartmann and Depro 2006). Nearly all are case studies of

delinquency. Their findings suggest that adolescent athletes are neither more nor less delinquent than non-athletes. However, this meta-analysis did not differentiate among identification strategies nor did it explore long-run versus short-run impacts on crime.

[12] Local authorities are the primary unit of local government, responsible for provision of a range of services, including education, public housing, social services and leisure and recreation services in UK (Brosnan 2019).

particular interventions.[13]

3. Data and Methods

3.1. Data

We begin our analysis using nationally representative individual-level panel data to document the association between middle/high school sports participation among females and female criminal activity. This follows the spirit of Stevenson's descriptive analysis (2010; p. 285). We draw nationally representative data on 2,077 females from the public use files of the National Longitudinal Study of Adolescent and Adult Health (NLSAAH) for this purpose.[14] Our sports participation measure, *School Sports*, is collected at Wave I when female respondents were in middle or high school during the 1994-95 academic year. *School Sports* is set equal to 1 if the female respondent reports participating on a school sports team in softball, basketball, field hockey, ice hockey, soccer, swimming, track, volleyball, wrestling, tennis, or other sports; *School Sports* is set to 0 otherwise. We find that 46 percent of female respondents participated in a school sport.

At Wave IV (2007-8), when our sample of respondents were ages 25 to 31, we measure self-reported adult criminal behavior. We find that 9.4 percent of female respondents report some form of illicit behavior, with 7.7 percent reporting having committed a property crime (*Property Crime*),[15] and 3.2 percent reporting some violent activity (*Violent Activity*)[16] .

For our primary analysis, which focuses on the criminal arrest effects of Title IX, we use data from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR)

[13] Using a before-after estimator, Winther and Currie (1987) find a reduction youth crime rates in Aboriginal communities in Manitoba after a sports program was implemented. Hartmann and Depro (2006) use a cross-sectional estimator and find lower property crime rates in cities that adopted midnight basketball leagues.
[14] The NLSAAH is a nationally representative sample of United States youths, who were in grades 7 through 12 during the 1994–1995 academic year. In Wave I, data were collected from adolescents, their parents, and their schools. Subsequent in-home surveys were conducted in 1996 (Wave II), 2000-1 (Wave III), 2007-8 (Wave IV), and 2016-18 (Wave V, not yet released). Our analysis uses data drawn from Wave I and Wave IV.
[15] *Property Crime* is set equal to 1 if the respondent reports a property crime and is set to 0 otherwise. A property crime is coded as having been committed if the respondent "deliberately damage[d] property that didn't belong to [her]," stole something, or went "into a house or building to steal something."
[16] *Violent Activity* is set equal to 1 if the respondent reports a violent crime and is set to 0 otherwise. A violent crime is coded as having been committed if the respondent "got into physical fight," "hurt someone badly enough in a physical fight that he or she needed care from a doctor or nurse," or they "shot or stabbed someone." We recognize that this measure more broadly captures violent behavior that may not always be criminal in nature (including self-defense). Results from regressions when *Violent Crime* excludes "physical fights" are qualitatively similar to those reported below.

from 1980 to 2000. We compile state-by-year arrest rates per 1,000 females ages 25-to-39. Examining such an age cohort allows for longer-run human capital and labor market effects of Title IX. State-by-year population data are collected from the National Cancer Institute's Survey of Epidemiology and End Results (SEER).

Arrest data are collected via voluntary reports from more than 16,000 city, county, and state agencies. While these data may understate the true levels of crime because not every crime is reported to law enforcement (Gould et al. 2002), there is a high degree of correlation between arrest reports from UCR and actual crimes committed when the latter are measurable (Lochner and Moretti 2004). Moreover, measurement error is unlikely to be correlated with Title IX.[17]

To further guard against idiosyncratic measurement error in arrests, we follow an approach similar to Anderson (2014) by (i) excluding age-specific state-by-year arrest counts greater than or less than two standard deviations from the state-specific mean, and (ii) controlling for the number and share of agencies reporting in a county in each year.

Our primary analysis focuses on violent crime arrests and property crimes arrests. Violent crime arrests are then disaggregated into robbery, aggravated assault, and murder and property crime arrests disaggregated into larceny, burglary, motor vehicle theft, and arson. Over the period from 1980 to 2000, the mean property crime arrest rate for 25-to-39-year-old females was 4.63 per 1,000, while for violent crime arrests, the mean per-thousand female arrest rate was 1.03 (Table 1).[18] Figure 1 reveals that female arrest rates rose during the period from 1980 to 1994 and began to decline in the mid-1990s.

We obtain data on gender-specific state sports participation rates from the National Federation of State High School Associations (NFSH). Annual sports participation survey data has been compiled since the 1969-1970 academic year.[19] The sports participation rate is calculated as the ratio of the number of high school students participating in a high school sport to the gender-specific state population ages 14-to-17.[20]

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[17] Underreporting can vary by the type of crime or the law enforcement agency. However, empirical techniques described above (state fixed effects) should capture any time-invariant cross-state differences in reporting.
[18] Number of observations slightly vary across types of crimes. Note that, each cell represents state, crime, age group, and year. Not all crimes are reported for every state, year, and age group.
[19] Iowa is the only state that failed to report female sports participation until 1981 (National Federation of State High School Associations).
[20] We also experimented with an alternate measure of the female high school sports participation rate whereby we estimated state-level female high school enrollment using 1990 census data. Our findings were quantitatively similar.

Figure 2 shows a sharp increase in female sports participation rate between 1971 and 1978, on the order of 30 percentage-points, which corresponds to the Title IX compliance window.[21] Noteworthy is the fact the male sports participation rate does not decline over this period, suggesting that compliance with Title IX was not achieved by substantially reducing resources to men's sports (Stevenson 2007). Following 1978, the female sports participation rate slowly approached the male sports participation rate. By 2014, the gender gap in sports participation rates was about 15 percentage-points, or about one-third of the gender gap observed in 1972.

3.2. Empirical Approach

To benchmark our results with the existing associative literature on sports and crime (Spruit et al. 2016), we first draw data from the NLSAAH and estimate an ordinary least squares (OLS) model of the following form:

$$Crime_{i4} = \lambda_0 + \lambda_1 School\ Sports_{i1} + \propto' \boldsymbol{X}_{i1} + \varepsilon_{i4} \quad (1)$$

where *i* indexes the female respondent, the numeric subscript indexes the wave at which the variable is measured (Wave I and Wave IV, respectively), and $Crime_{i4}$ and $School\ Sports_{i1}$ are defined as above. The vector $\boldsymbol{X}_{i1}$ includes individual characteristics (age, race/ethnicity, grade-in-school, and Peabody Picture Vocabulary Test Score) and family characteristics (parental educational attainment, parental marital status, and parental household income). However, given that our estimate of λ_1 may be biased due to difficult-to-measure unobservables or reverse causality, we next use a plausibly exogenous policy shock generated by Title IX to estimate the causal impact of youth female sports participation on criminal arrests.

Turning to the UCR, we estimate the following reduced form model of the effect of Title IX on female arrest rates for those ages 25-to-39:

[21] According to the National Federation of State High School Associations, the modest dip in sports participation (observed for both females and males) in 1978-79 was due to a change in data collection techniques relative to prior years and likely does not reflect a true decline in net sports participation (See: http://www.nfhs.org/ParticipationStatics/PDF/Participation Survey History Book.pdf). Given that the change in data collection practice did not vary across states, the year effects included in our IV estimates (Section 3.3 below) will capture this.

$$FArrest_{iast} = \beta_0 + \beta_1(Post\ TitleIX_i * MSport_s^{1971}) + \delta' \boldsymbol{X}_{st} + \psi_t + \gamma_a + \kappa_s + \tau_i + \varepsilon_{iast} \quad (2)$$

where *i*, *a, s*, and *t* index birth cohort, age, state, and year of crime, respectively. The dependent variable, $FArrest_{iast}$, is the number of female arrests per thousand population ages 25-to-39 in birth cohort *i* at age *a*, residing in state *s* at year *t.* The interaction term $(Post\ TitleIX_i * MSport_s^{1971})$ is the product of whether a birth cohort was born in 1954 or later ($Post\ TitleIX_i$) interacted with the state male high school sports participation rate in 1971 ($MSport_s^{1971}$).[22] Following Stevenson (2010), we use the male high school sports participation rate in 1971 rather than the male-female high school sports participation gap in 1971 for two reasons. First, prior to the 1971-1972 school year (pre-Title IX), gender-specific sports participation rates were not reported to the NFSH, often because female sports participation was very low.[23] Second, pre-Title IX female sports participation is more likely to be correlated with state characteristics related to attitudes toward females than is pre-Title IX male sports participation rate, and is therefore more likely to be exogenous (Stevenson 2010). We used a birth year cutoff of 1953 for treatment exposure because an individual would be exposed to Title IX for at least one year of high school (which is assumed to have begun at age 14) if they were born in 1954 or later. This is a very conservative definition of treatment. In sensitivity checks discussed below, we experiment with (i) assigning partial treatment to cohorts that were treated by Title IX for less than four years of high school, and (ii) dropping all cohorts that were treated by Title IX for less than four years of high school. Finally, the vector $\boldsymbol{X}_{st}$ includes the following state-level time-varying controls: economic and political controls (unemployment rate, per capita income, years of schooling attained for males ages 25-54, and whether the Governor was a Democrat), social welfare policy controls

[22] Note that a birth year of 1954 or later implies that a female student is covered by Title IX for at least one year of high school if the policy were implemented in 1972. In addition, crime counts are reported for five-year age intervals in the UCR for those ages 25 and older. Some age cohorts include partially treated birth cohorts. In alternative specifications, we omit partially treated birth cohorts, set $Post\ TitleIX_i$ to 1 for partially treated cohorts, and assign partial treatment to partially treated cohorts. The results are quantitatively similar to those reported in our main coding and are shown below.

[23] In 1969-1970, 50 states and the District of Columbia began to report high school sports participation in each sport. The first annual report did not disaggregate data based on male and female participants. After the passage of Title IX, all states, with the exception of Iowa, began reporting sports participation rates for females (Stevenson, 2010). In sensitivity analysis available upon request, we find that the 1971 male-female gap in sports participation was negatively related to crime, with estimated crime elasticities that were qualitatively similar to estimates reported in this paper.

(Temporary Assistance for Needy Families or TANF asset limits, maximum TANF benefit level, TANF lifetime limit, TANF work requirement, Earned Income Tax Credit refundable rate, and whether vehicles are exempt from Supplemental Nutrition Assistance Program asset tests), and crime control policies (police employment to population ratio, stand your ground gun laws, and shall issue laws).[24] We also include a set of fixed effects for the state where the arrest occurred (κ_s), year of arrest (ψ_t), age of arrestee at time of arrest (γ_a), and birth cohort of arrestee at arrest (τ_i).

An important measurement limitation in our left-hand side variable is that we cannot measure the arrestee's state of residence during high school. We can only measure the state in which the arrest occurred. If mobility among criminals (or non-criminals) is unrelated to pre-Title IX male sports participation, then estimates of β_1 (in terms of percentage changes in arrests) should be unbiased. However, if Title IX induced migration of women to states with lower (or higher) pre-Title IX male sports participation rates, such population shifts could affect crime rates.[25] To explore this concern, we undertake a few strategies. First, we replace the left hand-side variable in equation (2) with the share of the state population comprised of females ages 25-to-39 using data from the Surveillance, Epidemiology, and End Results Program (SEER). Second, we use data from the 1980, 1990, and 2000 Censuses and replace the left hand-side variable in equation (2) with an indicator for whether a female ages 25-to-39 currently resides in a state different from her state of birth. The results of both exercises, shown in Appendix Table 1, show no evidence that mobility among affected cohorts is related to pre-Title IX male sports participation.

Our key coefficient of interest, β_1, is identified from cross-state variation in the pre-Title IX male sports participation rate interacted with whether an individual was born in a cohort exposed to Title IX. In Figure 3, we show the distribution of male sports participation rates across states in 1971. States with higher pre-Title IX male sports participation rates such as North Dakota, Nebraska, and Minnesota are expected to experience larger increases in female sports participation in response to Title IX than states with lower pre-Title IX male

[24] Data for state and year specific economic controls are obtained from the Census Bureau's Current Population Survey Annual Social and Economic Supplement (ASEC). Economic controls are collected for males between ages 24-54.

[25] If Title IX induces mobility from states with lower male sports participation rates (in high school) to states with higher male sports participation rates (in adulthood), then population shifts could increase crime rates in states with higher pre-Title IX male sports participation rates, which would tend to understate crime reducing effects of Title IX. If the reverse were true, crime-reducing effects may be biased upward in absolute magnitude.

sports participation rates such as Rhode Island, North Carolina, and the District of Columbia.[26]

We take a number of tacks to assess the credibility of our identification strategy. First, we conduct event-study analyses to ensure that our policy estimates are not confounded by differential crime trends among affected and unaffected birth cohorts across states that were more or less bound by Title IX:

$$FArrest_{iast} = [\sum_{i=-5}^{10} \beta_i \, (D_i * \, \mathrm{M}Sport_s^{1971})] + \delta' \boldsymbol{X}_{st} + \kappa_s + \, \psi_t + \gamma_a + \tau_i + \varepsilon_{iast} \qquad (3)$$

where D_i denotes a dichotomous variable that is set equal to 1 for birth cohort i up to five or more years prior and ten or more years after Title IX. The reference category is comprised of those born in the one-to-two (year) cohorts prior to Title IX. This approach allows us to test for common pre-treatment trends between "treated" and "control" cohorts.

A second threat to identification would be if our estimates of β_1 are contaminated by geographic-specific time shocks or birth cohort-specific state crime trends that are coincidental to the Title IX reform. For example, if states facing a larger bite from Title IX were investing more in education or crime prevention than states that were less affected by Title IX, and these effects were concentrated on birth cohorts affected by Title IX, then the effect we attribute to Title IX may be capturing other unmeasured policies. To address these possibilities, we augment equation (2) with controls for census division-specific year effects, state-specific linear time trends, birth cohort-specific linear time trends, and age-specific linear time trends:

$$\begin{aligned} FArrest_{iast} = \; & \beta_0 + \beta_1 (Post\ TitleIX_i * MSport_s^{1971}) \\ & + \delta' \boldsymbol{X}_{st} + \psi_t + \gamma_a + \kappa_s + \tau_i + \\ & + \theta_a * t + \upsilon_s * \mathrm{t} + \xi_i * t + \eta_{ts} + \varepsilon_{iast} \end{aligned} \qquad (4)$$

where θ_a*t denotes an age-specific linear time trend, υ_s*t denotes a state-specific linear time trend, ξ_i is the birth cohort-specific linear time trend, and η_{ts} is a census division-specific year effect. In addition, we also experiment with controlling for the male arrest rate for 25-

[26] Note that, since participation counts include number of participants in each sport, and some athletes play in more than one sport, the participation rate can be greater than one.

to-39 year-olds to capture unmeasured state-specific crime policies (or trends) that could be associated with both Title IX's state-specific bite and cohort-specific arrest trends that commonly affect males and females.

Next, we conduct a set of placebo tests where we randomly assign a male sports participation rate between 0 and 1 to each of the 50 states and the District of Columbia a total of 2,000 times. Then, we estimate equation (4) and report estimates of β_p, the placebo analog to β_1. Following Chetty et al. (2009), we define $G(\widehat{\beta_p})$ to be the cumulative distribution function of placebo effects and conduct a nonparametric test for how frequently we would expect the Title IX effect we observe under the placebo policy. The statistic $G(\beta_p)$ then yields a p-value for the hypothesis that $\beta_p = \beta_1$.[27]

We also conduct additional placebo tests where we estimate equation (2) using a sample of arrests of (i) females ages 20-to-24 from 1990 to 2000, all of whom were treated by Title IX; and (ii) females ages 54-to-59 from 1980 to 1990, none of whom were affected by Title IX. We then randomly assign the year of the placebo Title IX intervention.

3.3 Instrumental Variables (2SLS)

In addition to the reduced form approach described above, we estimate an instrumental variables model to uncover the impact of Title IX-induced female sports participation on female arrests using a two-stage least squares (2SLS) approach. The first stage equation is given by:

$$Female\ Sports_{st} = \propto_0 + \propto_1 (Post\ TitleIX_i * MSport_s^{1971}) + \delta' \boldsymbol{X}_{st} + \kappa_s + \psi_t + \gamma_a + \tau_i + \varepsilon_{ist} \quad (5)$$

where *Female Sports*$_{st}$ is the female sports participation rate varying at the state-by-year level. The instrument ($Post\ TitleIX_i * MSport_s^{1971}$), is hypothesized to impact female sports participation but be uncorrelated with unmeasured determinants of female arrests. The second stage equation is estimated using predicted values of female sports participation. The instrumental variables (2SLS) estimate can then be interpreted as the impact of Title IX-

[27]This approach will also address concerns that serial correlation can bias standard errors, leading to over-rejection of the null hypothesis of no effect (Bertrand et al. 2004). Because this test does not make parametric assumptions about error structure, it does not suffer from over-rejection bias of the t-test. See Rosenbaum (1996).

induced increases in female sports participation on female arrest rates.

The identifying assumption of our instrumental variables model is that the instrument only affects female arrests through its effects on female sports participation. Our instrument would not satisfy the exclusion restriction if, for example, Title IX changed other dimensions of education for affected cohorts that differed across states coincidentally with 1971 male sports participation rates. This could occur if, for example, (i) Title IX directly impacted school spending on other dimensions of schooling that experienced important gender gaps, or (ii) if Title IX differentially affected other education investments for females and males that were correlated with 1971 male sports participation rates.

With regard to point (i), we have uncovered no evidence that the U.S. Office on Civil Rights used metrics other than female school sports participation to determine compliance with the original 1972 amendments to Title IX (e.g. through the "three-part test") until early 2000s. With regard to point (ii), there is no evidence that pre-Title IX male high school sports participation rates were related to differences in female economic wellbeing prior to Title IX. Specifically, Stevenson (2010) finds that pre-Title IX male sports participation was unrelated to (1) the percent of females taking home economics, math or science courses prior to Title IX, (2) female employment in 1970, or (3) college enrollment in 1970. Moreover, we find no evidence that the pre-Title IX male sports participation rate was related to female arrests in 1970 (see Appendix Table 3). Together, these results provide descriptive support for the exclusion restriction.

4. Results

4.1. NLSAAH Descriptive Results

We begin by documenting the association between female school sports participation and self-reported adult female crime using data from the NLSAAH. [28] Panel I of Table 2 shows estimates of λ_1 from equation (1). In column (1) we control for individual demographic characteristics, in column (2) we add family-level characteristics, and in column (3) we add a control for the Peabody Picture Vocabulary Test (PPVT) score. Across specifications, we find that school-based athletic participation among females is associated with a 3.4 percentage-point decline in the probability of adult female criminal activity.

[28] We use cross-sectional weights provided by NLSAAH. Unweighted estimates are quantitatively similar and available upon request. Standard errors are clustered at the school level.

When we disaggregate between property and violent activity effects (Panel II), we find that the magnitude of the estimated association is larger for property crime than violent activity, though each estimated correlation remains negative. While these descriptive results are suggestive of crime-reducing effects of sports participation, they may also reflect difficult-to-measure unobservables or reverse causality. Hence, we next turn to Title IX as a source of plausibly exogenous variation in female sports participation.

4.2 Reduced Form Estimates of the Effect of Title IX

Table 3 presents reduced form estimates of the relationship between Title IX and the female arrest rate among affected cohorts using data from the UCR.[29] In our most parsimonious specification that includes state, year, age, and birth cohort fixed effects (column 1), we find that a 10 percentage-point higher male sports participation rate prior to Title IX (in 1971) is associated with 0.095 fewer violent crime arrests per thousand 25-to-39 year-old females, or a 9.3 percent decline relative to the mean.[30] The decline in the violent crime arrest rate we observe is driven by a 0.078 per 1,000 population decline in the arrest rate for assaults and a 0.017 decline in the arrest rate for robberies. The addition of controls for demographic characteristics (column 2) and economic controls, political conditions, social welfare policies, and crime policies (column 3) has relatively little impact on our estimates of β_1. In the model saturated with observable controls (column 3), we find that a 10 percentage-point higher male sports participation rate prior to Title IX (in 1971) is associated with a 0.089 decline in the violent crime arrest rate for affected cohorts of females.

In addition, we also find evidence that female property crime arrests fell for affected cohorts in states where Title IX had more bite. A 10 percentage-point higher male sports participation rate prior to Title IX (in 1971) is associated with a reduction of 0.131 female property crime arrests per 1,000 ages 25-to-39 (column 3), representing a 2.8 percent decline relative to the mean. This decrease in the property crime arrest rate is driven by decreases in larcenies (0.091 per 1,000 population), burglaries (0.029 arrests per 1,000 population), and motor vehicle thefts (0.009 per 1,000 population). For total violent and property crime, we

[29] For ease of presentation, our tables show estimates of β_1 and φ_1. Standard errors are clustered at the state level. All estimates are weighted using state-by-year age-specific population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program.

[30] Note that the male sports participation rate ranges from 0 to 1; thus the marginal effect represents the arrest effects of a *100* percentage-point increase in male sports participation.

find that a 10 percentage-point increase in pre-Title IX male sports participation is associated with a 4.0 percent decline in arrests.

In Table 4, we explore the robustness of the results in Table 3 to controls for spatial heterogeneity — census division-specific year effects (column 2) and state-specific linear time trends (column 3) — as well as controls birth cohort-specific linear time trends (column 4) and age-specific linear time trends (column 5). Across offenses, estimates of β_1 change only modestly relative to the baseline model, but generally remain statistically distinguishable from zero at conventional levels. The results from this most fully saturated model (column 6) show that a 10 percentage-point increase in pre-Title IX male sports participation is associated with a 5.7 percent decline in violent crime arrests (driven by a 4.9 to 10.6 percent decline in assaults and robberies), and a 3.0 percent decline in property crime arrests (driven by a 2.4 to 5.6 percent decline in larcenies, burglaries and motor vehicle thefts).

How plausible are these effect sizes? Results from Stevenson (2010) showed that a 10 percentage-point higher pre-Title IX male sports participation rate was associated with 0.039 additional years of schooling and a 1.3 to 1.9 percentage-point increase in labor force participation among affected female cohorts. Lin (2008) finds that a 1 percentage-point decline in unemployment leads to a 2 to 4 percent reduction in the overall arrest rates and estimates from Cano-Urbina and Lochner (2019) suggest that 0.039 additional years of schooling reduces female arrests by approximately 2 percent. Thus, the total arrest declines of 3 to 4 percent we detect are plausible to the extent that human capital and labor market channels are important mechanisms for Title IX-induced arrest reductions.[31]

4.3 Event-Study Analysis and Placebo Tests

We next explore the plausibility of the common trends assumption through event-study analyses and a series of placebo tests. First, we present results from our event-study analyses. Figure 4 (panels a through d) present event study analyses for violent offenses and Figure 5 (panels a through e) shows results for property offense arrests. We find little evidence of "lead" effects whereby female arrests in unaffected (older) cohorts differ in states

[31] In Appendix Table 4, we also explore the relationship between Title IX and drug-related arrests, a Type II classified crime by the FBI. Our results show a 10 percentage-point higher male sports participation rate prior to Title IX (in 1971) is associated with a reduction of 0.113 to 0.269 female drug crime arrests per 1,000 population of females ages 25-to-39.

where Title IX had a larger bite relative to where it had less of a bite. The negative crime effects we observe are in the "lags," that is, for affected (younger) birth cohorts in states where Title IX had a larger bite. These results are consistent with a causal impact of Title IX on female arrests. For violent crime arrests (Figure 4, panel a) and assaults (panel b), lagged effects are somewhat larger about three birth cohorts following our treated birth cohort (birth year 1957 and later). This may be so for at least two reasons. First, those born in 1957 and later had a full four years of high school with which to have been potentially exposed to Title IX. Such females could, therefore, have been more affected. Second, schools had until 1978 to fully with Title IX mandates. This may mean that its full effects were delayed as schools took steps to ensure female students' access to school sports. On the other hand, for property crime effects (Figure 5, panel a), arrest declines happen more immediately, driven by larceny (panel b) and burglary (panel c) reductions.

How often would our estimates of β_1 arise by chance? In Figure 6 (panels a through d) and Figure 7 (panels a through e), we plot the empirical distribution of estimated placebo effects $G(\widehat{\beta_p})$, obtained by randomly assigning pre-Title IX male sports participation rates across states. The vertical line in each figure denotes the estimate of β_1 from column (3) of Table 3. Results from our permutation tests suggest that the probability we observe a value as extreme as our estimate of β_1 in our distribution of placebo coefficients, $G(\widehat{\beta_p})$, ranges from 0.001 to 0.018. This evidence provides support for the hypothesis that Title IX caused the crime reduction among affected females.

In Table 5, we report findings from an alternative placebo exercise where we draw cohort-years of females that were all treated by Title IX (those ages 20-to-24 between 1990 through 2000) or never affected by Title IX (those ages 45-to-54 between 1990 and 2000). When we randomly assign a placebo treatment year to each cohort, we find no evidence that this placebo policy affected female arrests. These findings add to our confidence that the Title IX effects we detect in our main specifications represent causal impacts of the policy change.

4.4 Sensitivity Tests

In Table 6, we explore the sensitivity of our estimates of β_1 to a variety of specification and data quality checks. In column (1), we reproduce estimates from column

(3) of Table 3 for comparison.[32] In column (2), we add a control for the offense-specific arrest rate for 25-to-39-year-old males to the right-hand side of the estimating equation. This control is designed to account for unmeasured arrest trends common to males and females that could be coincidentally correlated with Title IX. Our estimated treatment effect is similar in magnitude (and has similar precision) to our baseline model.

In column (3), we explore the robustness of our estimates to restricting the sample to those states that provided non-missing crime data for at least 75 percent of all years in our analysis sample, generating a more balanced panel. Again, our estimates of the effects of Title IX are similar in magnitude and precision to our baseline model.

Next, in column (4), we present unweighted estimates. This approach estimates the treatment effect for the average state (by giving each state equal weight) rather than for the average individual. Our estimates are similar to those obtained from weighted regressions.

In the final two columns of Table 6, we explore the sensitivity of our estimates to the definition of our treatment variable. As noted above, the UCR does not provide arrest counts by individual age for those ages 25 and older, but rather for five-year age bins. In our baseline estimates, for each year, a five-year age bin is coded as treated if at least one birth cohort in the age bin were born in 1954 or later. This is very conservative approach in the sense that many in an age bin will be untreated because they were born too early to have been impacted by Title IX, potentially biasing estimated treatment effects toward zero. In column (5), we assign one of five treatment values (0.2, 0.4, 0.6. 0.8, 1.0) based on the share of birth cohorts in the year-specific five-year age bin that were born in 1954 or later. This also has the advantage of giving greater treatment "weight" to those who were exposed to Title IX for multiple years of high school. The results in this specification are consistent with our baseline model.

Finally, in column (6), we exclude birth cohorts that were assigned partial treatment in column (5). This specification ensures cohorts were either exposed to four years of Title IX (born in 1957 or later), or not exposed at all. The results remain similar to our baseline model.

The results presented above provide strong evidence that Title IX reduced arrests among affected cohorts females. In Table 7, we explore heterogeneity in the effects of Title

[32] Appendix Table 7 presents parallel analysis beginning with a baseline specification from column (6) of Table 4.

IX by pre-Title IX state demographic and socioeconomic characteristics. Specifically, we examine the pre-Title IX (1970) (i) share of the state population that are non-Hispanic white (columns 1 and 2) and average personal income (columns 3 and 4) and explore heterogeneous treatment effects by whether the value of the 1970 measure is above or below the state median.[33]

We find that the impact of Title IX on female violent offense arrests differs by ethnicity/race and personal income. We find arrest reductions are substantially larger in states with higher proportions of non-Hispanic white residents (columns 1 vs 2). This result is consistent with evidence that non-Hispanic white women are more likely to use sports as a path to upward mobility (Eide and Ronan 2001; Eitzen 2005). The differential for property crime arrests, however, is not as substantial by race/ethnicity. We also find that the benefits of Title IX may also be larger in states with higher levels of pre-Title IX personal income (columns 3 and 4).

4.5 Instrumental Variables (2SLS) Estimates

Finally, Table 8 shows instrumental variables (2SLS) estimates of the impact of Title IX-induced female sports participation. First-stage estimates show that the implementation of Title IX is associated with a statistically significant 8 to 23 percentage-point increase in the state-level female sports participation rate (depending on set of controls included in Appendix Table 5), a result consistent with Stevenson (2007). F-statistics easily satisfy the instrument relevance standards proposed by Staiger and Stock (1997), with values exceeding 20 across models.

Instrumental variables (2SLS) estimates in column 1 of Table 8 show that a Title IX-induced 10 percentage-point increase in female sports participation reduced total female arrests by approximately 1 arrest per 1,000 population. The magnitude of this effect corresponds to an approximately 17.5 percent decline in arrests. This decline is driven by reductions in violent and property arrests, by 0.41 and 0.59 arrests per 1,000 population, respectively. The inclusion of controls for state-specific time varying controls (column 2) have relatively little effect on the estimated arrest effects, though the standard errors on property offenses are somewhat larger. The addition of controls for state-specific linear time trends, age-specific linear time trends, and birth cohort-specific linear time trends generate

[33] We draw data from the 1970 census.

somewhat larger estimates, particularly for property crime and larceny arrests, though we also note that the instrument is somewhat weaker in column (3).[34] In summary, the pattern of evidence from our 2SLS estimates are consistent with our reduced form models and suggest important crime-related social benefits from the introduction of Title IX/.

5. Conclusions

Advocates of youth sports programs, including the United Nations Office of Drugs and Crime, argue that sports participation may generate positive externalities by reducing crime. While there is emerging evidence that high school sports participation benefits participants in the form of enhanced human capital acquisition and improved labor market outcomes, very little is known about the causal impact of youth sports participation on adult criminal activity.

This study exploits the introduction of Title IX as a quasi-experiment to estimate the causal impact of female sports participation on female arrests. Using state-level arrest data between 1980 and 2000 from the Uniform Crime Reports (UCR), reduced form estimates show that a 10 percentage-point higher pre-Title IX state male sports participation rate was associated with a 3.3 to 4.0 percent reduction in total arrests, with declines observed for both violent and property offenses. Event-study analyses and a variety of placebo tests confirm a causal interpretation of our results. Finally, 2SLS estimates show that a Title IX-induced 10 percentage-point increase in female sports participation was associated with a 17.5 percent in total female arrests.

The magnitudes of our lower-bound 2SLS estimates suggest that the approximately 25 percentage-point increase in female sports participation induced by Title IX generated $9 to billion in social benefits from property and violent crime reductions.[35] This suggests a potentially important efficiency rationale for public spending on youth sports programs.

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[34] We also attempted a 2SLS model that included census division-by-year fixed effects in addition to the above-mentioned time trends. However, the F-stat in the first-stage from this specification was only 7.3, failing to reach the Staiger and Stock (1997) standard for instrument strength. Thus, we decline to present these findings.

[35] If we use our larger point estimate from column (3) of Table 8, the estimated cost savings rises to $21 billion. To generate our cost savings estimates, we first gather the part I property (larceny, burglary, and motor vehicle theft) and violent (aggravated assault) offenses committed between 1981 and 2000 (available from: https://ucr.fbi.gov/crime-in-the-u.s/2000/table1_crime81-00.xls). We generate the share of each type of offense committed by 25-to-39 year-old females using the 1981-2000 UCR files. We then take the product of the aforementioned offenses committed and the share of crime committed by 25-to-39 year-old females to generate a national level estimate of the number of offenses committed by 25-to-39 year-old females. Using our instrumental variables (2SLS) estimates, we calculate the change in crime for each offense category due to a 25

How do these external benefits compare to the costs of implementing Title IX? To our knowledge, little attempt has been made to ascertain the costs of implementation, which include hiring coaches for female teams, expanding facilities, purchasing equipment, and opportunity costs of time spent promoting and directing women's sports. However, estimates from Stevenson (2007) argues that the costs are largely mitigated by economies of scale, substitutability of labor and capital across "gender-neutral" sports, and compliance being achieved through expansions of gender-neutral sports. Back-of-the envelope calculations of the average per-female athlete cost of $295 to $886.[36] At the upper bound, this could suggest that the 25 percentage-point rise in female sports participation generated approximately $1.6 billion in implementation costs (2018$). Our findings suggest that the social benefits of crime reduction exceed these costs.

There are important limitations of this study worthy of note. First, like other studies in the Title IX literature, we lack panel data that allow us to identify females who participated in sports because of Title IX and to follow their human capital acquisition, labor market outcomes, and criminal arrests. Our estimates should be interpreted as intent-to-treat estimates. Second, the vast majority of crime (roughly 85 percent of violent crimes and 70 percent of property crimes between 1980 and 2000) is committed by men and our policy experiment is identifying a specific local average treatment effect for women. Thus, we do not claim that Title IX has had a large effect on overall crime rates or that increasing female sports participation is the most effective strategy to fight crime. However, the reduction in the gender gap in arrests (Cano-Urbina and Lochner 2019; Campaniello and Gavrilova 2018; Beatton et al. 2018) as well as heterogeneous crime responses to human capital investments by gender (Cano-Urbina and Lochner 2019) has increased the importance of studying female criminal behavior and the policy interventions that are most effective at deterring it. Finally,

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percent increase in the female sports participation rate. We multiply the percent decrease in crime by the crimes committed estimate for 25-to-39 year-olds to generate the number of crimes not committed due to a 25 percent increase in the female sports participation due to Title IX. These estimated decreases in crime are then multiplied by a per crime cost of a larceny related offense of $4,132, a per crime cost of a burglary related offense of $7,651, a per crime cost of a motor vehicle theft related offense of $12,545, and a per crime cost of a aggravated assault related offense of $125,213, using crime cost estimates from McCollister et al. (2010).

[36] Schulkind (2013) estimates average costs per athlete of $426 from two schools in the northeast (pp.21, footnote 27). The US Census Bureau (2017) estimates that public expenditures on the average public primary-secondary school student is approximately $12,210 (in $2018), which includes the costs of publicly provided school sports. In addition, the National Federation of State High School Associations estimates that 1 to 3 percent of all school budgets are spent on extracurricular activities, including sports (NFHS 2015). Given that national sports participation rates for females are approximately 43 percent, this would imply an average cost per athlete of $295 to $886.

our work has not considered how particular types of sports (e.g. team versus more individualized sports) may have heterogeneous effects on females' criminal behavior. This dimension may be an important path for future Title IX research.

6. References

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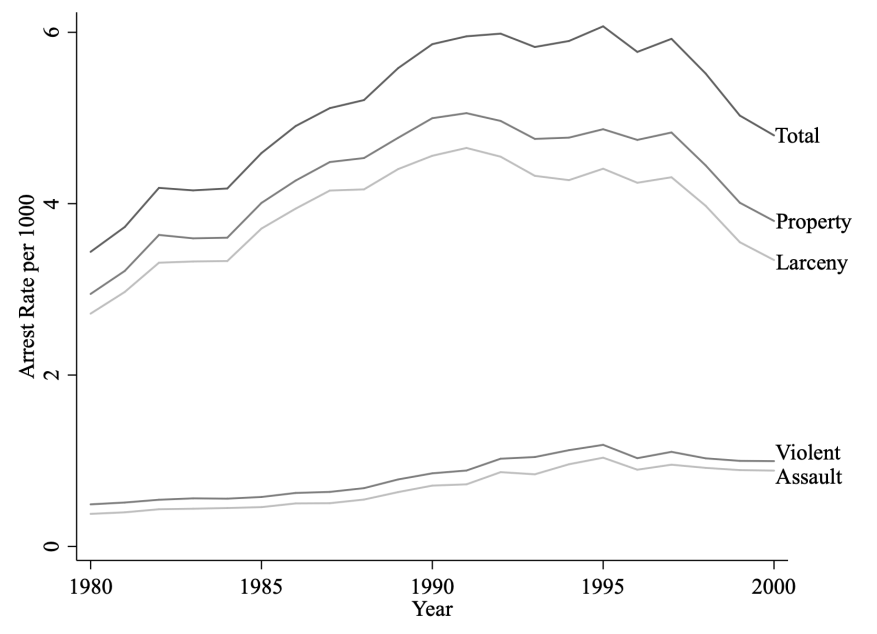
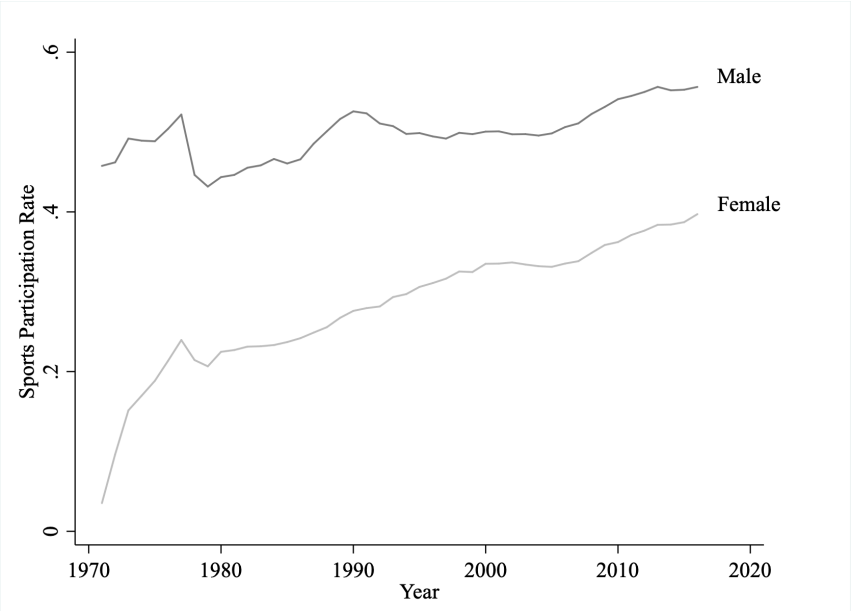
Figure 1. Female Arrest Rates among 25-to-39-Year-Olds, 1980-2000



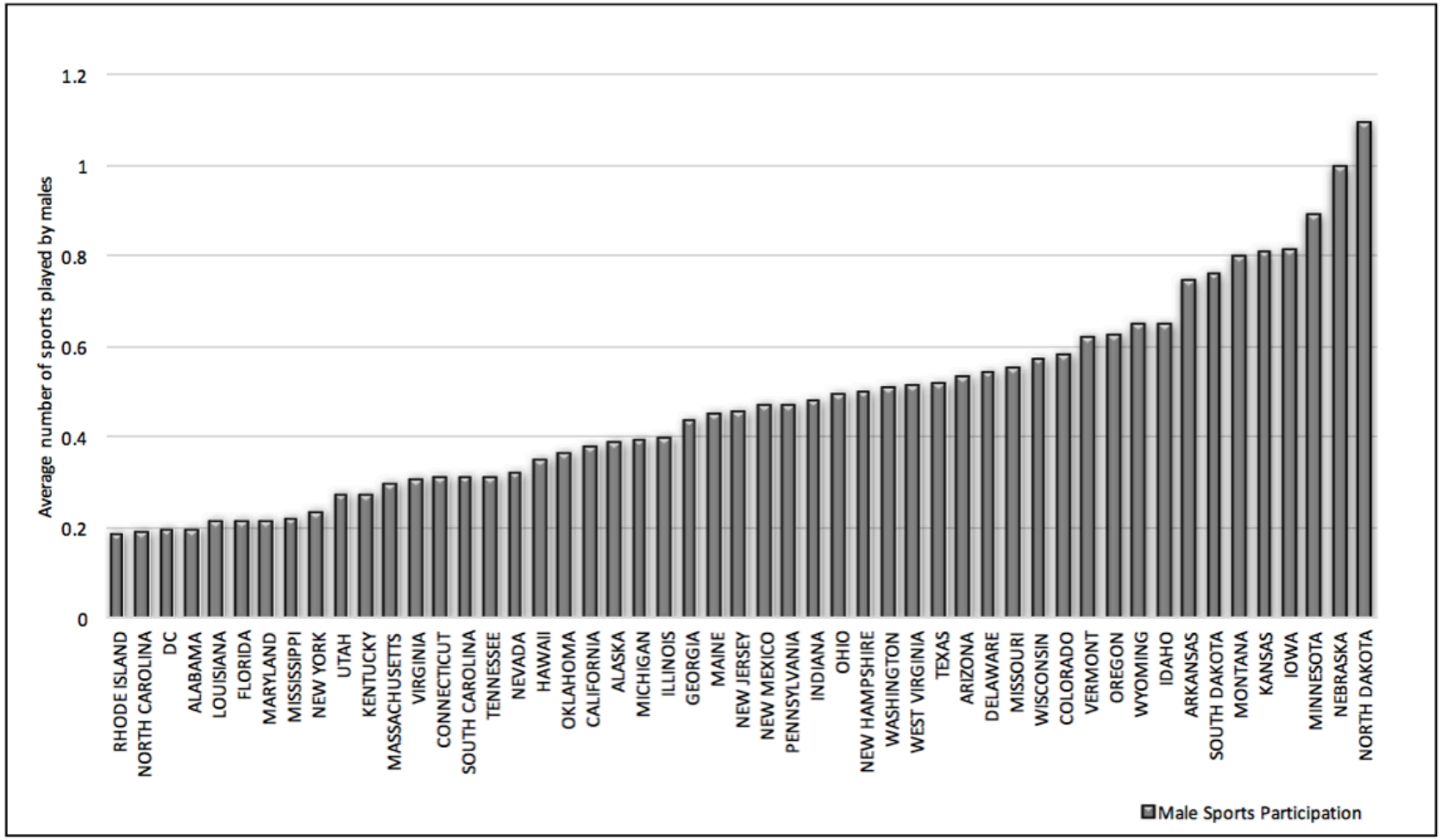
Notes: Arrest rates are arrests per 1,000 of the relevant age group population. Arrest counts are drawn from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR), and age and gender specific population estimates are from the National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER).

Figure 2. Sports Participation Rate by Gender, 1970-2016



Notes: Sports participation numbers are collected by National Federation of State High School Association (NFSH) (Athletic Participation Survey). A participant is a high school varsity team member. The participation rate is calculated by dividing total memberships in a year by the annual age and gender specific population estimates from the National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER).

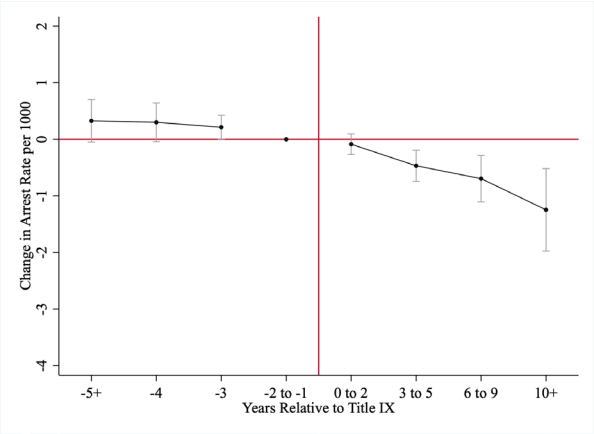
Figure 3. Male Sports Participation Rate by State, 1971-1972



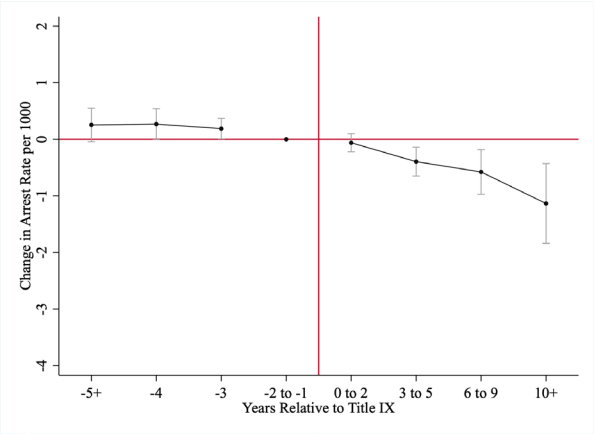
Notes: State level sports participation numbers are collected by National Federation of State High School Association (NFSH) (Athletic Participation Survey). A participant is a high school varsity team member. The participation rate is calculated by dividing total memberships in a year by the annual state-level age and gender specific population estimates from the National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER). We have a slightly different state ranking compare to Stevenson (2010). We digitalize male sports participation numbers from original National Federation of State High School Association's documents, which may have caused small differences. However, these differences are miniscule.

Figure 4. Event-Study Analysis of Female Violent Crime Arrest Rate

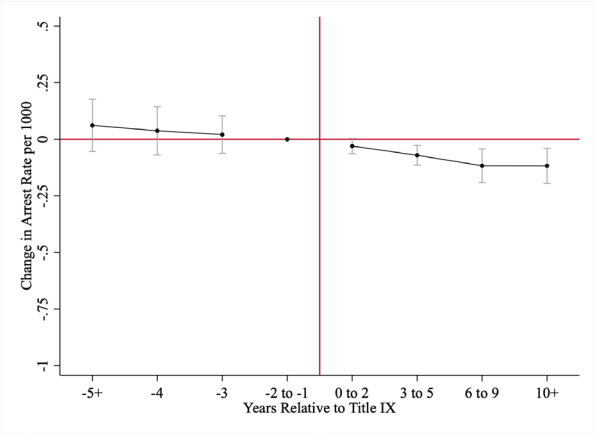
Panel (a): All Violent Offense Arrests



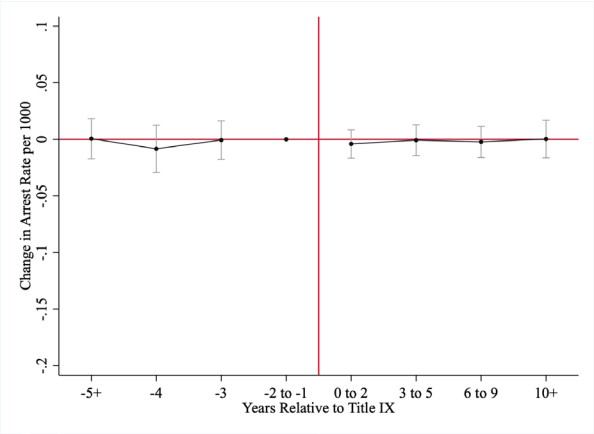
Panel (b): Assault Arrests



Panel (c): Robbery Arrests



Panel (d): Murder Arrests

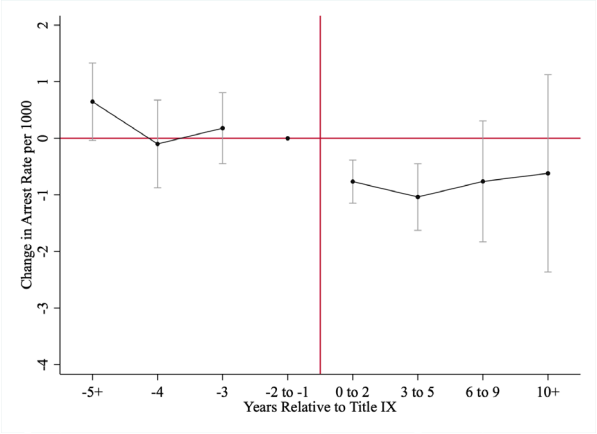


Notes: OLS estimates are generated using data from the FBI's Uniform Crime Reports (UCR). Dotted lines represent 95 percent confidence intervals.

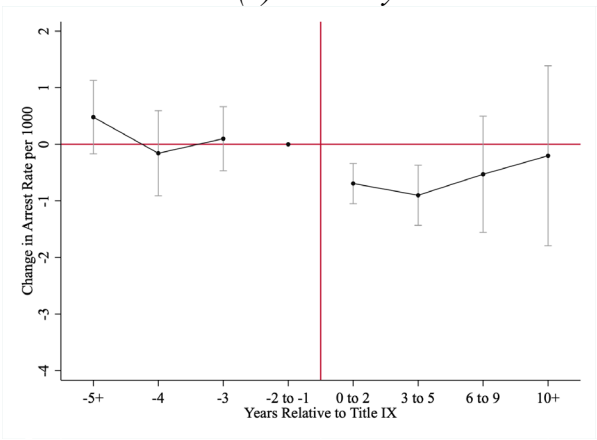
All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Figure 5. Event Study Analysis of Female Property Crime Arrests

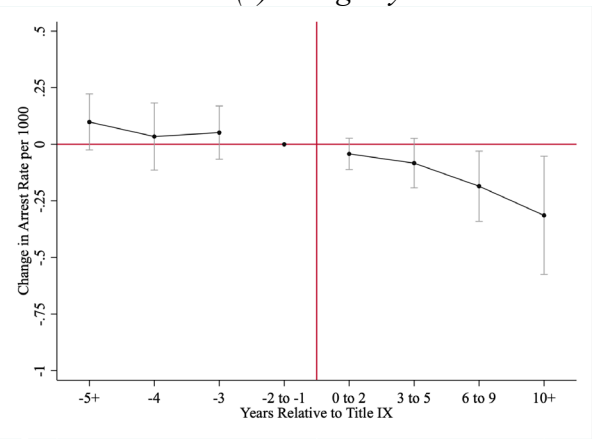
Panel (a): All Property Offense Arrests



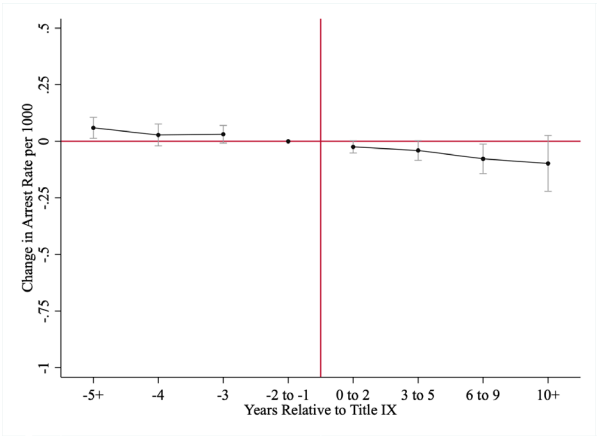
Panel (b): Larceny Arrests



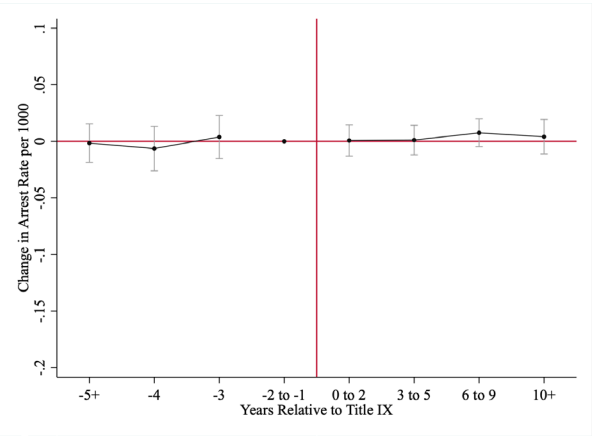
Panel (c): Burglary Arrests



Panel (d): Motor Vehicle Theft Arrests



Panel (e): Arson Arrests

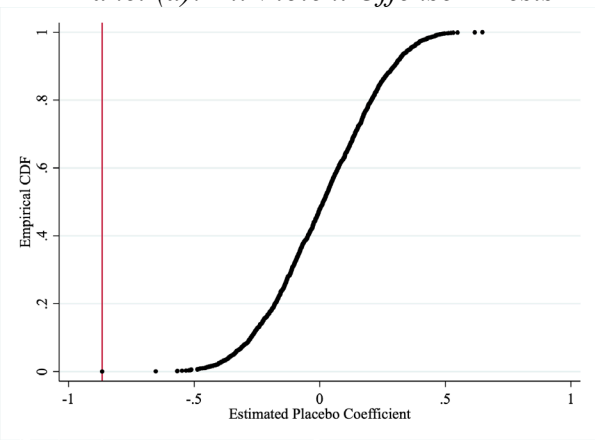


Notes: OLS estimates are generated using data from the FBI's Uniform Crime Reports (UCR). Dotted lines represent 95 percent confidence intervals.

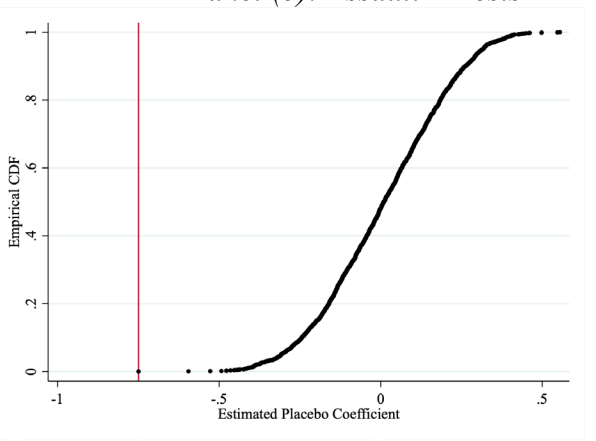
All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws

Figure 6. Distribution of Placebo Estimates of Violent Crime Arrests

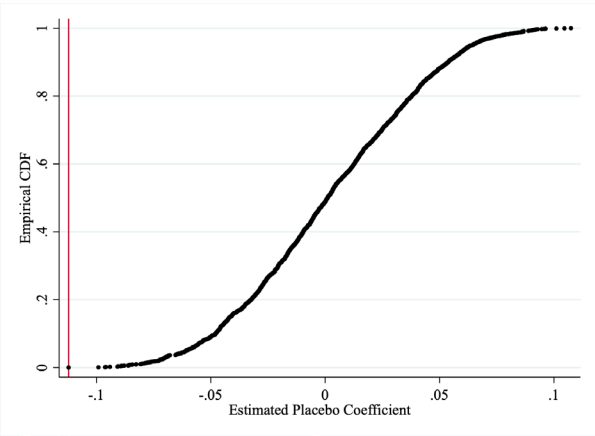
Panel (a): All Violent Offense Arrests



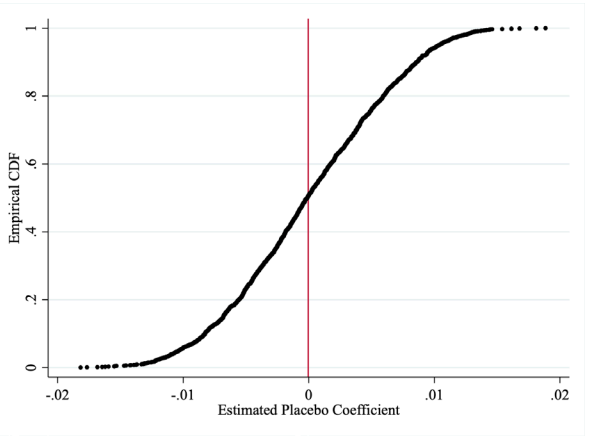
Panel (b): Assault Arrests



Panel (c): Robbery Arrests



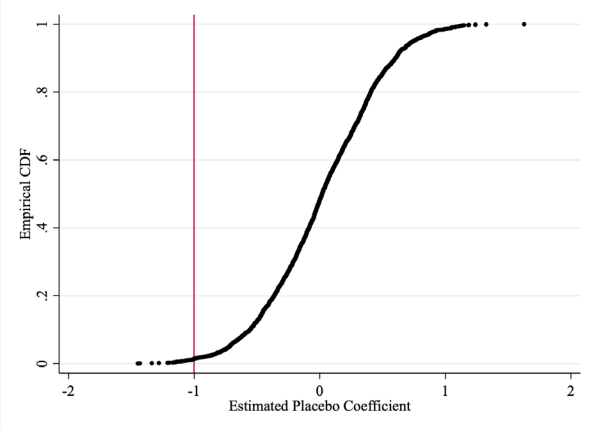
Panel (d): Murder Arrests



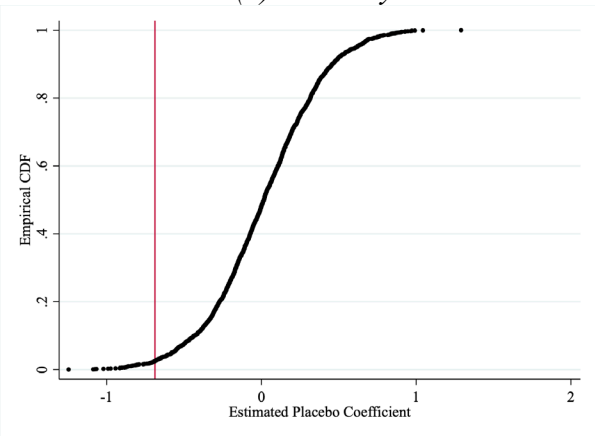
Notes: Figures present empirical distributions of placebo effects for Title IX. The CDFs are constructed from 2,000 placebo estimations. The effect of Title IX is randomly assigned and repeated 2,000 times.
All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, high school degree rate, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Figure 7. Distribution of Placebo Estimates of Property Crime Arrests

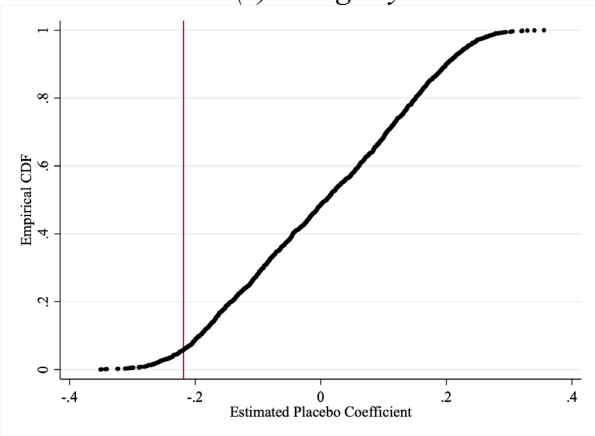
Panel (a): All Property Offense Arrests



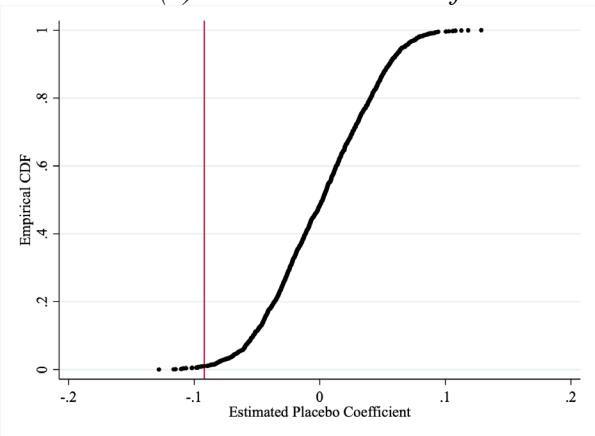
Panel (b): Larceny Arrests



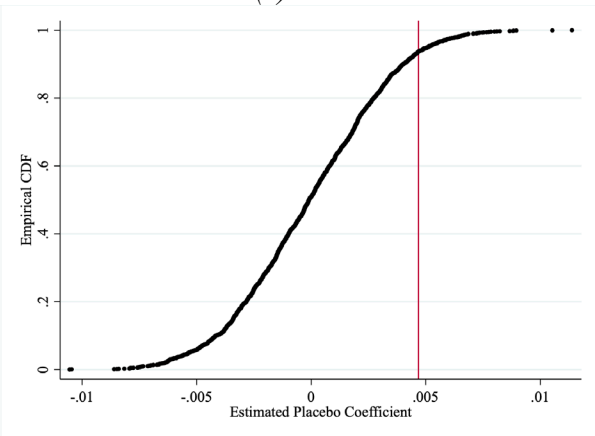
Panel (c): Burglary Arrests



Panel (d): Motor Vehicle Theft Arrests



Panel (e): Arson Arrests



Notes: Figures present empirical distributions of placebo effects for Title IX. The CDFs are constructed from 2,000 placebo estimations. The effect of Title IX is randomly assigned and repeated 2,000 times.
All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, high school degree rate, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Table 1. Descriptive Statistics, 1980-2000

	Mean	*St. Dev*	*N*	*Source*[a]
Dependent Variables				
Female Arrest Rates Per 1,000 Population for 25-to-39				
Total Crime Arrest Rate	5.66	2.30	2,976	UCR
Violent Crime Arrest Rate	1.02	0.67	2,917	UCR
Aggravated Assault Rate	0.851	0.61	2,916	UCR
Robbery Rate	0.144	0.11	2,861	UCR
Murder Rate	0.031	0.02	2,836	UCR
Property Crime Arrest Rate	4.63	1.88	2,969	UCR
Larceny Arrest Rate	4.12	1.63	2,965	UCR
Burglary Arrest Rate	0.337	0.34	2,891	UCR
Motor Vehicle Theft Arrest Rate	0.139	0.12	2,880	UCR
Arson Arrest Rate	0.024	0.01	2,865	UCR
Independent Variables				
State Specific Time Varying Controls				
Number of Agencies	325.44	299.52	2,976	UCR
Percent Black Population	0.110	0.12	2,976	SEER
Percent Hispanic Population	0.046	0.07	2,976	MORG
Unemployment Rate	0.046	0.02	2,976	MORG
Log(Weekly Earnings)	6.13	0.22	2,976	MORG
Years of Education	13.47	0.38	2,976	MORG
Democrat Governor	0.540	0.50	2,976	CQP
TANF Maximum Benefit	360.94	145.90	2,976	AC
Police per Capita	2.11	0.79	2,976	UCR
Shall Issue Law	0.306	0.46	2,976	AC
Stand Ground Law	0.007	0.08	2,976	AC

[a] Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR), the National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER), the Census Bureau's Current Population Survey Annual and Economic Supplements (ASEC), the Census Bureau's Current Population Survey Monthly Outgoing Rotation Group (MORG), the CQ Press (CQP) voting and elections collection, and author collected (AC).

Table 2. Estimates of Relationship Between Female High School Athletic Participation and Female Self-Reported Crime, National Longitudinal Study of Adolescent to Adult Health

	(1)	(2)	(3)
	Panel I: Total Crime		
School Sports	-0.034**	-0.034**	-0.034*
	(0.015)	(0.015)	(0.015)
Mean of DV	0.094	0.094	0.095
N	2,046	2,046	2,046
Individual Characteristics?	Yes	Yes	Yes
Family Characteristics?	No	Yes	Yes
Peabody Picture Vocabulary Test?	No	No	Yes

	Panel II: Offenses	
	Property Crime	*Violent Activity*
School Sports	-0.031**	-0.008
	(0.015)	(0.008)
Mean of DV	0.077	0.030
N	2,046	2,046
Individual Characteristics?	Yes	Yes
Family Characteristics?	Yes	Yes
Peabody Picture Vocabulary Test?	Yes	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the school level. OLS estimates are generated using public use data drawn from the National Longitudinal Study of Adolescent Health (Add Health). We use Wave I to construct the sports participation variable and wave IV to construct self-reported crime variables. Controls are obtained from Wave I and include individual characteristics (age, race, and ethnicity), family characteristics (parents' education, marital status, and income), and Peabody Picture Vocabulary Test Score.

Table 3. Estimates of Relationship Between Title IX and Female Arrests per 1,000 Population Ages 25-to-39, Uniform Crime Reports, 1980-2000

	(1)	(2)	(3)
Violent Crime	-0.945***	-0.942***	-0.886***
[N = 2,932]	(0.221)	(0.233)	(0.225)
Assault	-0.783***	-0.803***	-0.766***
[N = 2,930]	(0.218)	(0.222)	(0.220)
Robbery	-0.166**	-0.140**	-0.118**
[N = 2,878]	(0.074)	(0.057)	(0.048)
Murder	0.003	0.003	-0.000
[N = 2,859]	(0.005)	(0.005)	(0.006)
Property Crime	-1.310**	-1.109*	-1.012*
[N = 2,973]	(0.530)	(0.604)	(0.551)
Larceny	-0.908*	-0.765	-0.696
[N = 2,974]	(0.514)	(0.572)	(0.525)
Burglary	-0.294***	-0.253***	-0.219***
[N = 2,908]	(0.087)	(0.086)	(0.075)
Motor Vehicle Theft	-0.090**	-0.074*	-0.094***
[N = 2,896]	(0.044)	(0.038)	(0.033)
Arson	0.005	0.004	0.004
[N = 2,883]	(0.005)	(0.005)	(0.004)
Total Crime	-2.255***	-2.054***	-1.897***
N = 2,973]	(0.595)	(0.653)	(0.585)
State fixed effects?	Yes	Yes	Yes
Birth cohort fixed effects?	Yes	Yes	Yes
Age cohort fixed effects?	Yes	Yes	Yes
Year of crime fixed effects?	Yes	Yes	Yes
Demographic Controls?	No	Yes	Yes
State-specific time-varying controls?	No	No	Yes

*, **, and, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the state level are in parentheses. Demographic controls include share of the state population that are Hispanic and Black. Other state-specific, time-varying controls include economic and political controls (unemployment rate, per capita income, and educational attainment for males 25-54, and whether the Governor was a Democrat), social welfare policy controls (Temporary Assistance for Needy Families or TANF asset limits, maximum TANF benefit level, TANF lifetime limit, TANF work requirement, Earned Income Tax Credit refundable rate, and whether vehicles are exempt from Supplemental Nutrition Assistance Program asset tests), and crime control policies (police employment to population ratio, stand your ground gun laws, and shall issue laws).

Table 4. Sensitivity of Estimated Relationship Between Title IX and Female Arrest Rate to Controls for Spatial Heterogeneity and Cohort-Specific Time Trends, Uniform Crime Reports, 1980-2000

	(1)	(2)	(3)	(4)	(5)	(6)
Violent Crime	-0.886***	-0.888***	-0.582***	-0.888***	-0.888***	-0.582***
[N = 2,908]	(0.225)	(0.171)	(0.108)	(0.171)	(0.171)	(0.108)
Assault	-0.766***	-0.746***	-0.414***	-0.746***	-0.746***	-0.414***
[N = 2,906]	(0.220)	(0.167)	(0.087)	(0.167)	(0.167)	(0.087)
Robbery	-0.118**	-0.135**	-0.153**	-0.135**	-0.135**	-0.153**
[N = 2,855]	(0.048)	(0.060)	(0.068)	(0.060)	(0.060)	(0.068)
Murder	-0.000	-0.010	-0.010	-0.010	-0.010	-0.010
[N = 2,836]	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Property Crime	-1.012*	-1.263**	-1.387***	-1.265**	-1.263**	-1.391***
[N = 2,949]	(0.551)	(0.488)	(0.474)	(0.491)	(0.488)	(0.476)
Larceny	-0.696	-0.915**	-1.099**	-0.917**	-0.916**	-1.102**
[N = 2,950]	(0.525)	(0.447)	(0.434)	(0.449)	(0.447)	(0.436)
Burglary	-0.219***	-0.233***	-0.188**	-0.233***	-0.233***	-0.188**
[N = 2,855]	(0.075)	(0.079)	(0.080)	(0.079)	(0.079)	(0.080)
Motor Vehicle Theft	-0.094***	-0.095***	-0.070**	-0.096***	-0.095***	-0.070**
[N = 2,872]	(0.033)	(0.034)	(0.026)	(0.034)	(0.034)	(0.026)
Arson	0.004	-0.002	-0.000	-0.002	-0.002	-0.000
[N = 2,861]	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.005)
Total Crime	-1.897***	-2.133***	-1.948***	-2.135***	-2.133***	-1.951***
[N = 2,949]	(0.585)	(0.543)	(0.535)	(0.545)	(0.543)	(0.537)
State and year fixed effects?	Y	Y	Y	Y	Y	Y
Birth and age cohort fixed effects?	Y	Y	Y	Y	Y	Y
Census division-by-year fixed effects?		Y	Y	Y	Y	Y
State linear time trend?			Y			Y
Birth cohort linear time trend?				Y		Y
Age specific linear time trend?					Y	Y

*, **, and, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.
Notes: Standard errors are corrected for clustering at the state level are in parentheses. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Table 5. Falsification Tests on Samples of Fully Affected or Non-Affected Cohorts with Placebo Title IX Implementation Date

	(1)	(2)
	Placebo Age Group: 20-24, Years: 1990-2000	*Placebo Age Group: 45-54, Years: 1980-1990*
Violent Crime	0.008	-0.051
[N = 954]	(0.271)	(0.035)
Assault	-0.025	-0.052
[N = 954]	(0.239)	(0.035)
Robbery	0.123	-0.005
[N = 848]	(0.076)	(0.005)
Murder	-0.002	-0.007
[N = 836]	(0.011)	(0.007)
Property Crime	0.143	0.110
[N = 949]	(1.371)	(0.162)
Larceny	-0.093	0.163
[N = 947]	(1.261)	(0.151)
Burglary	0.078	-0.010
[N = 933]	(0.093)	(0.011)
Motor Vehicle Theft	0.036	-0.002
[N = 913]	(0.068)	(0.006)
Arson	0.002	-0.002
[N = 866]	(0.013)	(0.005)
Total Crime	0.389	0.063
[N = 949]	(1.582)	(0.175)

*, **, and' *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.
Notes: Standard errors are corrected for clustering at the state level are in parentheses All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Table 6. Sensitivity of Estimates to Sample, Weighting, and Definition of Treated Cohort

	Baseline Estimates	*Control for Male Crime*	*State had >75% Non-Missing Years*	*Unweighted Estimates*	*Partially Treated Cohorts Coded as Such*	*Exclude Partially Treated Cohorts*
	(1)	(2)	(3)	(4)	(5)	(6)
Violent Crime	-0.886***	-0.912***	-0.776***	-0.735***	-0.928***	-0.873***
[N = 2,908]	(0.225)	(0.228)	(0.224)	(0.220)	(0.286)	(0.266)
Assault	-0.766***	-0.787***	-0.656***	-0.590***	-0.751***	-0.707***
[N = 2,906]	(0.220)	(0.230)	(0.214)	(0.184)	(0.244)	(0.228)
Robbery	-0.118**	-0.123***	-0.119**	-0.128***	-0.143***	-0.137***
[N = 2,855]	(0.048)	(0.039)	(0.056)	(0.038)	(0.039)	(0.037)
Murder	-0.000	-0.001	0.000	-0.003	-0.003	-0.003
[N = 2,836]	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Property Crime	-1.012*	-1.135**	-1.076*	-1.602***	-1.599**	-1.617**
[N = 2,949]	(0.551)	(0.447)	(0.615)	(0.523)	(0.629)	(0.604)
Larceny	-0.696	-0.813**	-0.802	-1.186**	-1.130*	-1.171**
[N = 2,950]	(0.525)	(0.397)	(0.572)	(0.494)	(0.598)	(0.569)
Burglary	-0.219***	-0.224***	-0.211**	-0.232***	-0.269***	-0.258***
[N = 2,855]	(0.075)	(0.079)	(0.091)	(0.061)	(0.075)	(0.074)
Motor Vehicle Theft	-0.094***	-0.096**	-0.074**	-0.154***	-0.182***	-0.171***
[N = 2,872]	(0.033)	(0.036)	(0.033)	(0.047)	(0.053)	(0.051)
Arson	0.004	0.004	0.004	0.002	-0.005	-0.004
[N = 2,861]	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.005)
Total Crime	-1.897***	-2.068***	-1.852***	-2.322***	-2.503***	-2.466***
[N = 2,949]	(0.585)	(0.479)	(0.664)	(0.620)	(0.740)	(0.714)

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the state level are in parentheses All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Table 7. Heterogeneity in Effects of Title IX, by 1970 State Characteristics

	Share of Non-Hispanic Whites (1970)		Personal Income (1970)	
	Below Median	***Above Median***	***Below Median***	***Above Median***
	(1)	(2)	(3)	(4)
Violent Crime	-0.369**	-1.047***	-0.660***	-0.938***
N = 1,488]	(0.140)	(0.336)	(0.205)	(0.269)
Assault	-0.305**	-0.955***	-0.580***	-0.743***
[N = 1,483]	(0.129)	(0.307)	(0.196)	(0.239)
Robbery	-0.061***	-0.095	-0.072**	-0.185*
[N = 1,462]	(0.015)	(0.097)	(0.032)	(0.092)
Murder	0.001	-0.000	-0.009	-0.001
[N = 1,451]	(0.006)	(0.010)	(0.007)	(0.010)
Property Crime	-0.980	-0.659	-0.750	-1.012
[N = 1,512]	(0.851)	(1.036)	(0.788)	(0.819)
Larceny	-0.935	-0.439	-0.523	-0.691
[N = 1,512]	(0.809)	(0.963)	(0.710)	(0.767)
Burglary	-0.066	-0.123	-0.158*	-0.219
[N = 1,400]	(0.045)	(0.160)	(0.079)	(0.156)
Motor Vehicle Theft	-0.012	-0.079	-0.056*	-0.111**
[N = 1,474]	(0.023)	(0.067)	(0.030)	(0.050)
Arson	0.009	0.014*	-0.000	0.007
[N = 1,468]	(0.005)	(0.008)	(0.005)	(0.008)
Total Crime	-1.336	-1.706	-1.441	-1.932**
[N = 1,511]	(0.959)	(1.170)	(0.915)	(0.731)

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the state level are in parentheses All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Table 8. 2SLS Estimates of The Effect of Female Sports Participation on Female Arrest Rates

	(1)	(2)	(3)
Violent Crime	-4.101***	-4.558***	-6.470**
[N = 2,932]	(1.036)	(1.166)	(2.506)
Assault	-3.365***	-3.943***	-4.220***
[N = 2,930]	(0.944)	(1.060)	(1.506)
Robbery	-0.739*	-0.611**	-1.966
[N = 2,878]	(0.386)	(0.298)	(1.196)
Murder	0.004	-0.002	-0.134
[N = 2,859]	(0.027)	(0.028)	(0.084)
Property Crime	-5.852**	-5.169	-16.607*
N = 2,973]	(2.461)	(3.130)	(9.515)
Larceny	-4.178*	-3.574	-13.020
[N = 2,974]	(2.370)	(2.935)	(8.078)
Burglary	-1.270***	-1.127***	-2.170
[N = 2,908]	(0.361)	(0.376)	(1.323)
Motor Vehicle Theft	-0.384*	-0.506**	-0.953*
[N = 2,896]	(0.201)	(0.204)	(0.502)
Arson	0.022	0.023	0.003
[N = 2,883]	(0.020)	(0.022)	(0.067)
Total Crime	-9.903***	-9.676***	-22.689*
[N = 2,973]	(2.882)	(3.535)	(11.332)
First Stage F-Stat	39.40	30.77	20.11
State fixed effects?	Yes	Yes	Yes
Birth cohort fixed effects?	Yes	Yes	Yes
Age cohort fixed effects?	Yes	Yes	Yes
Year of crime fixed effects?	Yes	Yes	Yes
Demographic and Crime Controls?	No	Yes	Yes
State-specific time-varying controls?	No	Yes	Yes
State linear time trend	No	No	Yes
Age cohort linear time trend	No	No	Yes
Birth cohort linear time trend	No	No	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the state level are in parentheses All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Appendix Table 1. Exploring Mobility Effects of Title IX

	(1)	*(2)*
	Share of Female Population	***Moved from Birth State***
*Post Title IX$_i$ * MSPORT$_s^{1971}$*	0.0006	-0.009
	(0.0012)	(0.018)
Mean of DV	0.041	0.644
N	2,952	3,844,805
Data Source	SEER	Census

Notes for column (1): Standard errors are corrected for clustering at the state level are in parentheses. Annual age and gender specific population shares between 1980 and 2000 are generated from age and gender specific population estimates from National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER).
Notes for column (2): Standard errors are corrected for clustering at the state are in parentheses. Data is obtained from public use 1980, 1990, and 2000 censuses of population.

Appendix Table 2. Estimated Correlation Between Male Sports Participation in 1971 and Female Crime Rates in 1970

	Female Crime Rate in 1970
Male Sports Participation in 1971	0.348
	(1.676)
Mean of DV	1.99
N	47

Notes: Arrest rates are arrests per 1000 of the relevant age group population. Arrest counts from UCR (Uniform Crime Reports) divided by age and gender specific population estimates from National Cancer Institute's Surveillance, Epidemiology and End Results Program.

Appendix Table 3. Estimated Effect of Title IX

	Years of Education	***Labor Force Participation***	***Log of Weekly Earnings***
	(1)	(2)	(3)
*Post Title IX$_i$ * MSPORT$_s^{1971}$*	0.112*	0.022*	0.059***
	(0.062)	(0.013)	(0.020)
N	3,213	3,213	3,213

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.
Notes: Standard errors are corrected for clustering at the state level are in parentheses. All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws)

Appendix Table 4. Estimates of Relationship Between Title IX and Female Drug Arrests per 1,000 Population Ages 25-to-39, Uniform Crime Reports, 1980-2000

	(1)	(2)	(3)	(4)
Drug Crime	-2.689***	-1.943***	-1.807***	-1.334***
[N = 2,954]	(0.894)	(0.496)	(0.419)	(0.309)
State fixed effects?	Yes	Yes	Yes	Yes
Birth cohort fixed effects?	Yes	Yes	Yes	Yes
Age cohort fixed effects?	Yes	Yes	Yes	Yes
Year of crime fixed effects?	Yes	Yes	Yes	Yes
Demographic Controls?	No	Yes	Yes	Yes
State-specific time-varying controls?	No	No	Yes	Yes
Census division-by-year fixed effects?	No	No	No	Yes
State linear time trend?	No	No	No	Yes
Birth cohort linear time trend?	No	No	No	Yes
Age specific linear time trend?	No	No	No	Yes

*, **, and, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.
Notes: Standard errors are corrected for clustering at the state level are in parentheses. Demographic controls include share of the state population that are Hispanic and Black. Other state-specific, time-varying controls include economic and political controls (unemployment rate, per capita income, and educational attainment for males 25-54, and whether the Governor was a Democrat), social welfare policy controls (Temporary Assistance for Needy Families or TANF asset limits, maximum TANF benefit level, TANF lifetime limit, TANF work requirement, Earned Income Tax Credit refundable rate, and whether vehicles are exempt from Supplemental Nutrition Assistance Program asset tests), and crime control policies (police employment to population ratio, stand your ground gun laws, and shall issue laws).

Appendix Table 5. First-Stage Estimate of the Effect of Title IX on Female Sports Participation Rate

*Post Title IX$_i$ * MSPORT$_s^{1971}$*	0.229***	0.182***	0.082***
	(0.037)	(0.038)	(0.028)
N	2,973	2,949	2,949
F-stat	39.40	30.77	20.11
State fixed effects?	Yes	Yes	Yes
Birth cohort fixed effects?	Yes	Yes	Yes
Age cohort fixed effects?	Yes	Yes	Yes
Year of crime fixed effects?	Yes	Yes	Yes
Demographic and Crime Controls?	No	Yes	Yes
State-specific time-varying controls?	No	Yes	Yes
State linear time trend	No	No	Yes
Age cohort linear time trend	No	No	Yes
Birth cohort linear time trend	No	No	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the state level are in parentheses. Education, earnings and labor force participation rates are obtained from monthly Current Population Survey (CPS) Basic Monthly Data between 1980 and 2000. Controls include state, age, birth cohort, and year fixed effects, unemployment rate and log earnings for males 24-54. Following Stevenson (2010), treated cohorts have all age groups attending high school for 4 years after the passage of title IX.

Appendix Table 7. Sensitivity of Estimates to Sample Selected, Weighting and Definition of Treated Cohort, Fully Saturated Specification with Time Trends

	Baseline Estimates	*Control for Male Crime*	*State had >75% Non-Missing Years*	*Unweighted Estimates*	*Partially Treated Cohorts*	*Exclude Partially Treated Cohorts*
	(1)	(2)	(3)	(4)	(5)	(6)
Violent Crime	-0.517***	-0.283***	-0.471***	-0.448***	-0.682***	-0.518***
[N = 2,908]	(0.108)	(0.085)	(0.119)	(0.084)	(0.122)	(0.103)
Assault	-0.343***	-0.151	-0.302***	-0.281***	-0.436***	-0.318***
[N = 2,906]	(0.087)	(0.093)	(0.091)	(0.064)	(0.093)	(0.093)
Robbery	-0.158**	-0.124**	-0.154*	-0.147***	-0.196***	-0.159***
[N = 2,855]	(0.067)	(0.054)	(0.077)	(0.047)	(0.057)	(0.045)
Murder	-0.011*	-0.006	-0.008	-0.016***	-0.023***	-0.020***
[N = 2,836]	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Property Crime	-1.347***	-0.340	-1.279**	-1.633***	-1.885***	-1.362**
[N = 2,949]	(0.485)	(0.415)	(0.511)	(0.441)	(0.615)	(0.548)
Larceny	-1.052**	-0.146	-1.007**	-1.315***	-1.478**	-1.074**
[N = 2,950]	(0.437)	(0.360)	(0.462)	(0.411)	(0.585)	(0.528)
Burglary	-0.173**	-0.124	-0.167*	-0.173***	-0.228***	-0.186***
[N = 2,855]	(0.081)	(0.083)	(0.096)	(0.047)	(0.066)	(0.053)
Motor Vehicle Theft	-0.078***	-0.044	-0.071**	-0.089*	-0.119**	-0.098**
[N = 2,872]	(0.026)	(0.026)	(0.030)	(0.046)	(0.052)	(0.040)
Arson	0.000	0.004	-0.000	0.009	0.003	0.004
[N = 2,861]	(0.005)	(0.005)	(0.006)	(0.012)	(0.010)	(0.007)
Total Crime	-1.854***	-0.584	-1.742***	-2.057***	-2.523***	-1.830***
[N = 2,949]	(0.525)	(0.431)	(0.551)	(0.478)	(0.656)	(0.581)

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Notes: Standard errors are corrected for clustering at the state level are in parentheses All models include state fixed effects, year fixed effects, birth cohort fixed effects, age fixed effects, and state-specific time-varying demographic controls. Controls include state-level number and share of reporting agencies in the state, demographic characteristics (share of Hispanic population and share of Black population), economic characteristics (unemployment rate, income, educational attainment, and high school degree rate for males 25-54, and an indicator for a democratic governor in a given state-year), welfare characteristics (TANF asset limits, max TANF benefit, TANF lifetime limit, TANF work requirement, refundable EITC rate, and indicators for SNAP exclusion of vehicles from assets), and gun policies (the proportion of police to population, indicators for stand your ground laws, and shall issue laws).