

# The Long-Run Effects of Corporal Punishment in Schools<sup>1</sup>

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**Abstract:** Corporal punishment is used in schools in about 70 countries, including in 19 states in the United States. Despite its prevalence as a tool to discipline students, it remains remarkably understudied. We leverage the staggered state-level bans of school corporal punishment in the United States over the past several decades in conjunction with data on social and economic outcomes from the American Community Survey (ACS) and the General Social Survey (GSS), using a difference-in-differences design to measure the causal effects of school corporal punishment. We find that the presence of corporal punishment in schools increases educational attainment, increases later-life social trust and trust in institutions, and leads to less authoritarian attitudes toward child-rearing, and greater tolerance of free speech. Additionally, exposure to corporal punishment in school decreases later-life crime. We find no effects on mental or physical health. These results hold up to dynamic difference-in-differences specifications – which reveal non-existence of pre-trends – and a wide variety of other robustness checks. Observing that only a small share of students are exposed to corporal punishment, we argue that the effects primarily represent spillovers resulting from restraining the behavior of disruptive students.

## 1 Introduction

Corporal punishment was historically a standard method to enforce discipline in schools around the world. Teachers in the 19<sup>th</sup> century were encouraged to employ corporal punishment

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over other methods of discipline, and there was little to no organized opposition to the practice until the 20<sup>th</sup> century<sup>3</sup>. To this day, it remains legal and widely-practiced in a majority of countries in the developing world and in numerous countries in the developed world – including the United States, where it is still permitted and practiced in 19 states. Advocates claim that it serves as a strong deterrent for particularly negative or disruptive behavior by student, teaches that bad behavior has consequences, and fosters a better social and educational environment for all children. Detractors argue that it is an example of cruel and unusual punishment, has no identifiable benefits or even backfires, and is a relic of a bygone era.

In the United States, in the wake of *Ingraham v. Wright* – a failed Supreme Court challenge to the practice of school corporal punishment – a wave of state-level corporal punishment bans began in the 1970s and intensified over the course of the subsequent two decades. Leveraging these staggered state-level bans of school corporal punishment along with survey data from the American Community Survey (ACS) and the General Social Survey (GSS), we use the information on each respondent’s childhood state-of-residence and birth cohort to determine whether or not they were exposed to school corporal punishment. We then estimate difference-in-differences regression specifications to measure the effects of exposure to school corporal punishment on various outcomes. We find that exposure to corporal punishment in school increases later-life social trust and trust in institutions, leads to a less authoritarian attitude toward child-rearing, and increases tolerance of free speech. Furthermore, its effects are not limited merely to the domain of beliefs and values: exposure to school corporal punishment results in increased educational attainment and reduced later-life crime – in particular, reduced property crime and reduced crime against society.

We show that these results are robust to a very broad variety of additional specifications and strategies. In particular, we allow for the corporal punishment exposure regressor to vary more continuously than a simple indicator variable. We add a variety of interacted fixed-effects to allow for highly flexible cohort effects that vary across time, race, and sex. We add state-specific linear cohort trends to allow for abolishing and non-abolishing states to be on different

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<sup>3</sup> Poland, which banned school corporal punishment in 1783, and Luxembourg, which banned it in 1845, were the two outliers.

trajectories in terms of the outcome variables. We compute standard errors using two-way clustering. We run several different permutation tests as an alternative, more robust method of conducting inference. And we run dynamic difference-in-differences specifications with pre-treatment period indicators in order to show the non-existence of pre-trends and deal with any potential bias in the static specification. Finally, we show in a series of placebo checks that other variables plausibly correlated with corporal punishment bans – such as school spending and state legislative/gubernatorial partisan control – are, in fact, not driving the effect.

Next, we investigate heterogeneity in effects, first finding some evidence that it is significantly stronger for female students than male students. Given that female students are physically punished with a much lower frequency than male students, this finding is consistent with the intuition that the “positive” effects may largely reflect spillovers. While we find no statistically-significant evidence of heterogeneities by race of the individual (potentially due to insufficient statistical power), we do find racial heterogeneity of another sort. In particular, effects (amongst whites) are stronger in mixed-race neighborhoods than in all-white neighborhoods. Furthermore, leveraging recent cross-sectional administrative data on the extent of corporal punishment by race, we find that nearly all of the effects are stronger where corporal punishment is used disproportionately on black students. *However*, these strengthened effects proceed through the white students (e.g., a sharper reduction in crimes committed by whites but no additional effect on crimes committed by blacks). We offer speculative interpretations of these findings, including heightened racial ingroup/outgroup distinction in response to black students being singled out as “the other”; these results remind us that increased trust in institutions and society are neither inherently good outcomes, nor are they necessarily generated by just institutions and fair experiences.

We note that only a minority of students are themselves physically punished even when corporal punishment is in effect, although all students are effectively exposed to it and face the threat of corporal punishment. Since we do not observe whether individuals in our sample were themselves personally subject to corporal punishment, our estimates measure the systemic effect of school corporal punishment, rather than the individual effects of being punished. Nor do we

argue that these findings justify corporal punishment. Indeed, when corporal punishment has been banned, this has largely been done on moral and ethical grounds, rather than on the basis of purely instrumental arguments about (in)effectiveness (Gershoff et al., 2015).

## **2 Political Economic Context**

### **2.1 School Corporal Punishment in the United States**

Since the dawn of modern educational systems – and, indeed, long before – corporal punishment has been used by teachers across the world to enforce behavioral standards in the classroom. Middleton (2008) documents extensively the history of school corporal punishment in the 19<sup>th</sup> and early-20<sup>th</sup> century, noting that it was widely-utilized by educators and considered a fair way of disciplining school children. Educators of the era were, however, cautioned to use it judiciously and sparingly, lest its deterrent effect be lost. In this era, there existed minimal organized opposition against corporal punishment in the United States, and with the exception of New Jersey in 1867, in no state was school corporal punishment banned or otherwise challenged until the 1970s.

Then, in 1970, the first large-scale opposition to corporal punishment in the United States began to coalesce. In October of that year, after refusing Principal Willie J. Wright’s order to submit voluntarily to corporal punishment, 14-year-old student James Ingraham was placed face-down on a table by the Assistant Principal at his school, who restrained his arms and paddled him harshly over 20 times – while a staff member simultaneously restrained his legs. Ingraham’s parents argued that this constituted a violation of the Eighth Amendment to the U.S. Constitution, which bans “cruel and unusual punishment”. After losing the case in several lower-level courts, *Ingraham v. Wright* was argued before the Supreme Court, where school corporal punishment was upheld as constitutional in a close 5-4 decision.

In the meantime, though, individual states began to take matters into their own hands, legislating state-level bans on corporal punishment. In 1971, Massachusetts became the second state to ban corporal punishment in public schools – after New Jersey, 104 years earlier. Other states followed – with momentum picking up over the 1980s and 1990s – such that, today, 31

states and the District of Columbia ban corporal punishment in public schools, whereas in the 19 remaining states it is still legal<sup>4</sup>. Table BANLIST lists the dates of these state corporal punishment bans. Figure BANFIG maps the states where corporal punishment is banned and those where it remains legal.

[Table BANLIST about here]

[Figure BANFIG about here]

All these state bans have translated into a substantial decrease in the prevalence of school corporal punishment in the United States, declining from 4% of the total number of schoolchildren in 1978 to 1% by 2014 (Gershoff, Purtell, and Holas 2015). Still, the 19 states where school corporal punishment remains legal together constitute more than one-third of U.S. student population, and as of 2014, a student is hit in a U.S. public school an average of once every 30 seconds. In other words, by no means has the practice evaporated, even today.

More broadly, while school corporal punishment is banned in more than 100 countries around the world, it remains prevalent in a great many developing countries – including most countries in Africa and many in Asia and Latin America. Thus, for both these countries and much of the United States, questions about the efficacy of corporal punishment remain highly salient and relevant.

## **2.2 Literature Review**

Despite its remaining prevalence and the ongoing debate, the effects of school corporal punishment have so far been heavily understudied. While there is a sizeable literature within social psychology studying the effects of corporal punishment of children, it tends to be entirely correlational: observing whether children who received more punishment had worse subsequent outcomes. As such, it suffers from the issue that more troubled children are more likely to misbehave and hence receive punishment – and more troubled or less effective parents and educators are potentially more likely to employ it. Papers in this literature include Bryan and Freed (1982), Belsky, Lerner, and Spanier (1984), and Straus and Yodanis (1996).

In a book on corporal punishment in this psychology literature, Gershoff, Purtell, and Holas

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<sup>4</sup> Only two states – New Jersey and Iowa – ban corporal punishment in private schools.

(2015) review the stylized facts of school corporal punishment in the United States. They additionally run static difference-in-differences regressions studying the effects of school corporal punishment bans on juvenile crime, finding no significant effects. However, their regression specifications do not account for the extent of exposure to corporal punishment by birth cohort – instead merely examining juvenile crime before and after a subset of state-level bans. Because the first cohort of children unexposed to school corporal punishment do not reach their teenage years until a decade after the corporal punishment ban in their corresponding state, contemporaneous juvenile crime is a very slow-moving variable in terms of its potential responsiveness to corporal punishment bans. As such, because the crime data used by Gershoff, Purtell and Holas only spans 20 years (1980-1999) and because only one state passed a corporal punishment ban in the first half of the 1980s, their specifications may lack the statistical power necessary to detect effects. This issue is made worse by the statistical bias inherent in static difference-in-differences specifications in certain settings, which biases the measured coefficient toward the immediate short-term effect, away from true average effect over the short- and long-term.

A broad and growing literature within economics considers the effects of schooling – and particular facets of schooling – on outcomes beyond test scores, including social capital and other socio-political attitudes. This literature includes such papers as Algan, Cahuc, and Shleifer (2013), who study the effects of specific teaching practices (such as rote memorization or an emphasis on group projects) on social capital; Cantoni et al. (2017), who study the effects of a recent curricular reform in China on political ideology; Lochner and Moretti (2004), who study the effects of education on crime; and Gentile and Imberman (2012), who study the effects of school uniforms on student achievement and behavior.

The very large literature on the effects of childhood experiences on later-life outcomes additionally relates to this paper. Amongst the most closely-related papers in that literature are Currie and Tekin (2012), who study childhood maltreatment and future crime; Bald et al. (2019), who study the causal impact of removing children from abusive/neglectful families on various later-life socio-economic outcomes; and Giuliano and Spilimbergo (2013), who study the effects

of growing up during a recession using the same main dataset – the General Social Survey – as we do. Our work also relates to the literature studying the effects of social capital on crime, which includes Buonanno, Montolio, and Vanin (2009), Akcomak and ter Weel (2012), and others. Finally, our research relates to the literature examining the effects of the erosion of traditional cultural norms and practices on broader society. Papers in this literature include Gruber and Hungerman (2008), who study the decline in religiosity; Wolfers (2006) and Stevenson and Wolfers (2006), who study the rise of divorce; and Doepke and Zilibotti (2017), who study parenting styles and the decline in authoritarian parenting. That is, the decline of corporal punishment in the United States can be thought of as a microcosm of the decline in strictness in educational practice – and social norms more broadly. All of these various literatures within economics, however, have so far overlooked school corporal punishment. We aim to fill this gap.

### **3 Empirical Framework**

#### **3.1 Data**

Since 2000, the U.S. Census Bureau has conducted the American Community Survey (ACS), asking a random sample of U.S. respondents various demographic questions. From 2000 to 2004, its annual sample size was approximately 600,000. Since 2005, its annual sample size was approximately 2 million. ACS micro data is publicly-available, including geocodes on their state of residence, county of residence, and state of birth. We focus on questions pertaining to education in the ACS.

Since 1972, the University of Chicago has conducted the General Social Survey (GSS), asking a random sample of U.S. respondents a variety of socio-political and economic questions. From 1972 until 1993, the GSS was administered annually, with a sample size of approximately 1500 in each wave. Since 1994, it has been administered bi-annually, with a sample size of approximately 3000 in each wave. This yields a cumulative sample size of approximately 60,000. The restricted-access GSS Sensitive Data files contain extensive geocode information for each individual – notably, their state of residence, their county of residence, and their state of

residence as a child (at age 16).

Within the GSS, we utilize the questions on social trust, trust in various institutions, parenting priorities (i.e., traits which respondents perceive as important/unimportant to instill in children), attitudes toward free speech made by various groups, educational attainment, mental health, attitudes toward violence, racial attitudes, and gender attitudes. In all cases where we analyze multiple closely-related outcomes, for full transparency we create a z-score index composed of the full set of related outcomes and use the index as an outcome variable as well. (For example, there are multiple different questions on trust toward various institutions – the federal government, the education system, businesses and corporations, etc. – so we create an index for overall trust in institutions.)

We obtain information on the timing of state laws banning school corporal punishment from the Center for Effective Discipline, an advocacy group which has compiled this data. Because over one-third of individuals relocate from one state to another between their childhood and the age at which they respond to the ACS or GSS, the aforementioned childhood state-of-residence variable is crucial for our study. Since exposure to school corporal punishment occurs during childhood, it is ideal to assign treatment based on each individual's childhood home-state rather than their current state of residence, as the latter would tend to add substantial noise.

We additionally use data on state gubernatorial/legislative control from the State Partisan Balance dataset by Klarner (2013) and data on school spending from the Annual Survey of State and Local Government Finances conducted by the Census Bureau. We use these in certain specifications to account for the potentiality of confounds from these key variables relevant to state education policies and outcomes which may plausibly be correlated with school corporal punishment bans.

Bi-annually from 1968 until 2000 – and with reduced frequency thereafter – the Office for Civil Rights of the U.S. Department of Education conducted surveys on enrollment by race at the school level throughout the United States. The primary purpose of these surveys was to track progress/reversion with regard to desegregation. Incidentally, these surveys also collected data (from administrators) on the number of formal incidences of corporal punishment by race prior



to each state’s corporal punishment ban. We use this data in specifications decomposing the effect by the proportionality/disproportionality of corporal punishment across races.

Finally, we obtain crime data from the National Incident-Based Reporting System (NIBRS) of the Federal Bureau of Investigation (FBI), which publicly reports incident-level crime data. In particular, the NIBRS contains an offender file reporting the characteristics (race, gender, birth cohort, etc.) of each criminal. While NIBRS data is not available for every state (police stations in some states continue to report their crime statistics through the much less granular Uniform Crime Reports), it is available for the majority. We collapse the NIBRS crime count data to the police station level. The one drawback of the NIBRS is that it does not report the childhood home-state of each offender, adding noise to the regression specifications with crime as an outcome. In order to account for population differences by birth cohort, we merge the NIBRS data with the National Institute of Health data on population by state by year by birth cohort (which is itself computed from Census microdata).

### 3.2 Econometric Approach

As noted, the GSS Sensitive Data discloses the childhood home-state of each respondent. This allows analysis of various outcomes in banning versus non-banning states, for individuals born into cohorts before versus after the end of corporal punishment exposure. In other words, we run difference-in-differences specifications on childhood state and birth cohort:

$$Y_{ichst} = \alpha + \beta \cdot \mathbf{1}[CP\ Exposure_{ch}] + \gamma_c + \varphi_h + \tau_t + \omega_{t-c} + \eta_s + \varepsilon_{ichst}$$

where  $Y_{ichst}$  denotes the value of some outcome variable  $Y$  for individual  $i$  born in cohort  $c$  in home-state  $h$ , currently living in state  $s$  in year  $t$ .  $\gamma_c$  denotes cohort fixed-effects.  $\varphi_h$  denotes home-state fixed-effects.  $\tau_t$  denotes year fixed-effects.  $\omega_{t-c}$  denotes age fixed-effects.  $\eta_s$  denotes current-state fixed effects.  $\mathbf{1}[CP\ Exposure_{ch}]$  is an indicator variable for whether school corporal punishment was legal while an individual born in cohort  $c$  was attending school in home-state  $h$  – i.e., between the ages of 5 and 17. We cluster standard errors by childhood home-state since this is the level at which treatment was assigned.

We run a number of closely-related alternative specifications that build on this baseline specification. For one, we replace the indicator variable with  $YrsExposure_{ch}$ , a more continuous

measure of the number of years of exposure to corporal punishment. In some specifications, we add year-by-cohort, race-by-cohort, and sex-by-cohort fixed-effects to allow for highly flexible cohort effects that differ across time, race, or sex. In others, we add state-specific linear cohort trends, thereby allowing for states which abolish corporal punishment and those which do not to be on different trends in terms of the outcome variables. To allow for correlation amongst observations within not only home-states but also current-states, we report two-way clustered standard errors. As an alternative, robust method of calculating p-values in-sample, we run several permutation tests with 2000 repetitions: (i) randomizing both the treatment states and each state’s year of treatment, (ii) fixing the treatment states but randomizing each state’s year of treatment, and (iii) fixing the years of treatment but randomizing which states receive treatment.

Furthermore, we run a dynamic specification with pre-treatment periods. We do this both to ensure that pre-trends do not exist in the data and in response to the concerns raised by Borusyak and Jaravel (2017) that coefficients estimated by running a static difference-in-differences specification in a dynamic setting may be plagued with a particular form of bias whereby the static coefficient is outside the convex hull of the true coefficients for each post-treatment period. The dynamic specification is as follows:

$$Y_{ichst} = \alpha + \sum_{m=A}^B \beta_m \cdot I_{hc}^m + \beta_{(B,\infty)} \cdot I_{hc}^{(B,\infty)} + \gamma_c + \varphi_h + \tau_t + \omega_{t-c} + \eta_s + \varepsilon_{ichst}$$

where  $I_{hc}^m$  is an indicator variable denoting whether cohort  $c$  was either the  $m^{\text{th}}$  or  $(m + 1)^{\text{th}}$  cohort in state  $h$  to never be exposed to corporal punishment and all other variables are as before. We also run closely-related event-study specifications, which focus in particular on the subset of states where corporal punishment was actually banned.

## 4 Results

### 4.1 Educational Attainment

We begin by investigating the effects of school corporal punishment on educational attainment. Educational attainment is an outcome of first-order significance due to both its positive impacts on the individuals attaining it and broader positive externalities on the rest of

society which have been uncovered by the economics of education literature. Table EDUCATTAIN reports the effects of childhood exposure to school corporal punishment on later-life trust outcomes. As can be observed, corporal punishment exposure induces a statistically-significant increase in educational attainment by approximately one-tenth of a year, on average. Alternative specifications can be run with indicator variables for high-school diploma attainment or Bachelor's degree attainment on the left-hand-side instead of years of education; the effects on these outcomes are significant as well. In particular, the fact that the effect on B.A. attainment is significant (if anything, more so than that on high school diploma attainment) is a first hint that school corporal punishment may have enduring effects beyond grade school itself, since corporal punishment is not practiced in colleges or universities.

[Table EDUCATTAIN about here]

#### **4.2 Social Trust and Confidence in Institutions**

We next turn to investigating effects on measures of trust. Social trust – i.e., the question of whether people can generally be trusted – is a central outcome in the literature on social capital which has been found to have broad-reaching implications. Membership in social clubs or community organizations is another important measure of social capital. Confidence in institutions is an important indicator of system support and nation-building. Together, these measures yield broad information about individuals' trust in the various components of their society. Table SOCTRUST reports the effects of childhood exposure to school corporal punishment on later-life trust outcomes.

[Table SOCTRUST about here]

As can be seen, corporal punishment induces sharply and significantly higher social trust – and higher confidence in most types of institutions. When an index variable is formed from all of the confidence in institutions questions in the GSS, it too is strongly and significantly positive. Evidence of increased membership in social clubs is also found.

#### **4.3 Authoritarian Parenting Tendencies and Free Speech Support**

We next turn to attitudes toward children and parenting tendencies. Does exposure to corporal punishment alter these outcomes? Table CHILDATT shows evidence of reduced

tendencies of authoritarian parenting. The GSS asks a series of questions on which attributes it is most important to instill in children. Running each of these as an outcome, we see that “obedience” is seen as significantly less important and “free thought” as significantly more important. Other attributes (popularity, work ethic, and altruism) are unaffected. Furthermore, the GSS asks a series of questions on government spending preferences – on such matters as national defense, social security, foreign aid, the environment, etc. Amongst these, exposure to corporal punishment has an effect on only one – increased preferences for government spending on childcare – which is consistent with the generally warmer attitude toward children observed in these results. Such an attitude is also manifested in a significantly reduced likelihood to say it is “not fair” to children to bring them into this world and a marginally-significant increase in the number of children individuals actually have. We find no effects, however, on the belief that spanking children is an acceptable method of punishment.

[Table CHILDATT about here]

Turning away from authoritarianism in parenting and toward authoritarianism more generally, we investigate the effects on exposure to school corporal punishment on support for free speech. In particular, the GSS asks a series of three questions about a variety of groups (communists, fascists, atheists, gay activists, racists, and Muslim extremists). For each group, the first question asks whether they should be permitted or banned from giving a public speech; the second asks whether they should be permitted or banned from teaching in a college or university; the third asks whether they should be permitted or banned from having a book of theirs in a public library. We create indices across the groups for each of these questions – and then one index merging the three questions together as well. As seen in Table FREESPEECH, we find that support for free speech is significantly boosted by childhood exposure to corporal punishment.

[Table FREESPEECH about here]

We additionally investigate attitudes toward the use of violence – by individuals and by the state. The GSS asks a series of questions about when it would be acceptable for “an adult man to hit a male stranger”. We create an index from these questions. The GSS also asks whether

individuals can envision circumstances in which they would approve of police hitting an adult male citizen. There are no effects of exposure to corporal punishment on these outcomes.

#### **4.4 Crime**

Given the above results and the literature on the effects of social capital on crime, it seems reasonable to next investigate the effects of school corporal punishment on crime, an important material outcome. As noted, the National Incident-Based Reporting System (NIBRS) from which the offender-level crime data is available lacks childhood home state as variable, so we are forced to proxy for this with current state of residence, adding some noise to the specifications. The massive sample size in the millions, however, may more than make up for this imprecision. Following the standard FBI categorization, we group crimes into violent crimes, property crimes, and crimes against society (a category including drug offenses, illegal gambling, prostitution, bribery, fraud, and other crimes without a direct, individual victim). We collapse the NIBRS data to the police station level in order to obtain a panel of crime counts, and we cluster our standard errors by state. In order to account for the fact that different cohorts may differ in population size and ensure that this isn't driving the result, we control for the (remaining) population of each birth cohort in each state in the given year. Table CRIMEREGS reports the results of these regressions.

[Table CRIMEREGS about here]

As can be seen, there is a significant reduction in later-life property crime and crimes against society induced by exposure to school corporal punishment. The effect on violent crime has the same sign but is non-significant. These are particularly important findings, as they demonstrate that school corporal punishment continues to deliver tangible, material benefits to society years after it was applied. It is not something which delivers long-run effects that merely dwell within the realm of people's minds, values, and beliefs.

#### **4.5 Robustness**

To ensure that these results are genuine and not the consequence of specific standard errors or functional form decisions, we undertake a large number of robustness checks. First, we use the number years of exposure to school corporal punishment as the main right-hand-side variable

instead of a 0/1 indicator variable to allow for the treatment to vary more continuously. Effects remain significant, as seen in Table ROBCONTIN.

[Table ROBCONTIN about here]

We additionally allow for more flexible cohort effects that differ across time, race, and sex – and more flexible state effects that differ across time as well. That is, we add cohort-by-year, cohort-by-race, cohort-by-sex, and state-by-year fixed-effects to our regression specifications. The results of these regressions are reported in Table ROBFE. The effects on social trust and confidence in institutions endure strongly. The effects on attitudes toward child-rearing and free speech endure as well, albeit with reduced significance. [Note: Effects on educational attainment not included for computational reasons; to be included in future versions.]

[Table ROBFE about here]

Next, we allow for state-specific linear cohort trends. In other words, we might be concerned that corporal punishment abolishing and non-abolishing states are on fundamentally different trajectories in terms of the various outcome variables for reasons non-related to the abolitions themselves – or, alternatively, that the abolitions are outcomes of these differential trajectories. The addition of state-specific cohort trends helps address this. Table ROBTREND shows that the effects on social trust, confidence in institutions, and crime hold up strongly to the addition of these trends. The effects on parenting style and free speech attitudes are less robust, losing significance at conventional levels but retaining their sign.

[Table ROBTREND about here]

To deal with the concern that the error term may be correlated not only within childhood home-state (the level where treatment was assigned) but *also* within current state of residence, we re-run our main specifications with two-way clustering on home state and current state. As seen in Table ROBTWOWAY, this does not substantively affect the results either. [Note: Effects on educational attainment not included for computational reasons; to be included in future versions.]

[Table ROBTWOWAY about here]

As an alternative within-sample method of calculating p-values that is robust to the realities

of finite-cluster inference, we run 2000-repetition permutation tests. We (i) randomize both the set of 32 states which are treated and the timing of treatment, (ii) fix the treated states but randomize the year in which treatment is assigned, and (iii) fix the years in which treatment is assigned but randomize the 32 states receiving treatment. Figures ROBPERM1FIG, ROBPERM2FIG, and ROBPERM3FIG plot the results of these three different permutation tests for the social trust outcome. As can be seen, in all cases the result stands up strongly to the permutation test. Table ROBPERM runs permutation test (i) for the other outcome variables as well, demonstrating that they, too, hold up. [Note: Effects on educational attainment not included for computational reasons; to be included in future versions.]

[Figure ROBPERM1FIG about here]

[Figure ROBPERM2FIG about here]

[Figure ROBPERM3FIG about here]

[Table ROBPERM about here]

Next, we move on to dynamic difference-in-differences specifications with pre-treatment period indicators in order to both show the non-existence of pre-trends and deal with any potential bias in the static specifications. As previously noted, recent work in applied econometrics – notably Borusyak and Jaravel (2017) – has argued that running static difference-in-differences specifications in a setting that is fundamentally dynamic may potentially lead to a particular form of bias whereby the static difference-in-differences coefficient is outside the convex hull of the true coefficients for each post-treatment period. Figures TRUSTDDD and TRUSTEVSTUD, respectively, plot the dynamic difference-in-differences specifications using all the variation and the event-study specification focusing solely on treated states. Figures EDUCDDD and EDUCEVSTUD do the same for the years of education outcome. In all cases, there is little to no evidence of pre-trends. For educational attainment, the effects manifest themselves over time, increasing in the number of years to which individuals were exposed to corporal punishment. For social trust, effects manifest themselves more sharply upon the total abolition of corporal punishment. The situation is similar for the other outcomes in the GSS.

[Figure TRUSTDDD about here]

[Figure TRUSTEVSTUD about here]

[Figure EDUCDDD about here]

[Figure EDUCEVSTUD about here]

We repeat this procedure for crime outcomes in Figures CRIMEDDD and CRIMEEVSTUD. Once again, pre-trends are found to be non-existent. The effects on crime are phased in more gradually over the course of several cohorts in the aftermath of total abolition.

[Figure CRIMEDDD about here]

[Figure CRIMEEVSTUD about here]

Finally, we consider whether the effects might be driven by plausibly-correlated variables which are potentially important for outcomes of students. School spending is one such variable; lower spending could engender lower-quality schools and a worse educational and social environment. However, Figure SCHSPEND reveals that the correlation between this variable and corporal punishment abolition is weak at best and, to the extent it exists, it predates the abolition of corporal punishment – evolving smoothly before and after the ban rather than spiking downward or only beginning to decline after the ban is instituted. As such, it seems unlikely to be driving the effects.

[Figure SCHSPEND about here]

State partisan control is another potential confound. States with more exposure to Democratic governors/legislatures might be more likely to abolish corporal punishment *and* also more likely to implement other policies that could have an effect on social capital and crime. Figures PARTISANGOV and PARTISANHOUSE reveal that, indeed, states with more exposure to Democratic governors (and legislatures) are more likely to abolish corporal punishment. Again, though, the cohort patterns of exposure in years to Democratic governors or Democrat-controlled legislatures predate the abolition of corporal punishment – evolving smoothly before and after the ban. Still, because the correlation is significant, we perform the following exercise: we re-run our main specifications on the subset of cases where Democratic governors oversaw the abolition of corporal punishment and, separately, the subset of cases where Republican governors oversaw its abolition. We also do the parallel exercise for legislatures, examining



separately the cases with a Democratic-controlled legislature (House and Senate) and the cases with a Republican-controlled legislature. The effects hold up in all cases, as seen in Table ROBPART, indicating that partisanship correlated with the corporal punishment abolitions is not what was responsible for the effects.

[Figure PARTISANGOV about here]

[Figure PARTISANHOUSE about here]

[Table ROBPART about here]

#### 4.5 Heterogeneities

We next examine whether there are any significant heterogeneities in the effects of school corporal punishment. We first turn to potential heterogeneities by sex. Table HETSEX re-runs the main specifications with the effects decomposed by sex. There is some evidence that the effects are stronger amongst women than amongst men. It is worth noting that female students are administered corporal punishment much less frequently than male students. As such, this finding is consistent with the intuition that the positive effects of corporal punishment are primarily operationalized through spillovers to the students who are *not* corporally-punished. That is, by keeping misbehaving students in check, corporal punishment may foster a better social environment for the remaining majority of students (who are disproportionately female). The table also investigates heterogeneities by parental education (a potential proxy for how comfortable a student might be in educational environments); the differences between these coefficients are, however, non-significant.

[Table HETSEX about here]

Because the number of black respondents in the GSS is much lower than the number of white respondents – partially due to the fact that, unlike many other surveys, the GSS does not conduct a regular oversample of black respondents – we lack the statistical power to investigate heterogeneities in the effect between black and white respondents. However, we are able to investigate the effects on white respondents in racially-homogeneous versus racially-heterogeneous settings. The GSS features a question on the racial composition of one's neighborhood. For individuals who report living in the same city as when they were growing up,

we thus implicitly have information on the racial composition of their neighborhood/school as a child<sup>5</sup>. Table HETNEIGH shows that the effects of school corporal punishment (on whites) are non-existent in all-white neighborhoods; for the most part, it is in mixed-race neighborhoods where they manifest themselves.

[Table HETNEIGH about here]

To further investigate the extent to which the effects of school corporal punishment are racialized, we utilize administrative data collected by the Office for Civil Rights of the Department of Education. The school-level data they collected for desegregation purposes – which has been used in a few economics papers studying the effects of desegregation (e.g., Cascio et al. 2010) – also contained information on the frequency of formal incidences of corporal punishment by race, as reported by administrators. We use this data to compute the proportionality of corporal punishment across races; that is, we take the pre-abolition ratio of the share of the black student body receiving corporal punishment to the share of the white student body receiving corporal punishment and interact this ratio with the school corporal punishment indicator variable. We then reproduce our main specifications now featuring this interaction effect. Table HETPROP reports these results.

[Table HETPROP about here]

Nearly all the effects are further intensified where black students are corporally-punished disproportionately. The exception is confidence in institutions, which has a somewhat smaller increase where black students are punished disproportionately. The crime-reducing effect, in particular, is substantially strengthened. Notably, however, we can use the NIBRS data to ask whether the decrease in crime is being operationalized through a reduction in crime committed by whites or a reduction in crime committed by blacks – as the NIBRS reports the race of each offender. Table HETPROPCRIME reveals that the further decrease in crime proceeds entirely through reduced crime committed by whites. This suggests that the effect of school corporal

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<sup>5</sup> This is, of course, imperfect, as it assumes that either individuals live in the same neighborhood of the city where they grew up or that racial composition is roughly the same in other neighborhoods in the city. Since the question merely asks whether one lives in an all-white neighborhood or in a neighborhood with *any* blacks, the latter of these two assumptions quite plausibly holds (even the most white neighborhood in a mixed-race city is likely to have some black residents).

punishment is not that of a simple deterrent. Rather, the effect is more circuitous and more likely to proceed through the social capital channel.

[Table HETPROPCRIME about here]

These results may reflect heightened racial ingroup/outgroup distinction in response to black students being singled out as “the other”. Indeed, heightened ingroup/outgroup distinction is often spoken of as the “dark side” of social capital (e.g., in Satyanath, Voigtlander, and Voth 2017). As a way of testing this hypothesis, we further leverage the NIBRS data to examine whether the reduction in crime committed by whites is a reduction in white-on-white crime, white-on-black crime, or both. The results of these specifications in Table HETPROPCRIMEWHT reveals that, indeed, the reduction comes in the form of white-on-white crime, consistent with the explanation of heightened ingroup/outgroup distinction.

[Table HETPROPCRIMEWHT about here]

#### **4.6 Effects on Parents**

Another relevant question to act is whether the parents of children exposed to corporal punishment are themselves affected by their child’s exposure. The GSS includes as a variable the birth cohort of one’s first child, allowing the procedure of computing corporal punishment exposure for each respondent to be repeated for respondents’ children. It is somewhat less direct to determine which state each respondent’s child grew up in than which state the respondent themselves grew up in. For the majority of individuals – those who live in the same state now as during their childhood – their child’s home state is more-or-less clear. For those who moved between childhood and the point at which they responded to the GSS, we assign their child’s home state to be the respondent’s childhood home state if their child was born closer to when they were 16 than their present age; we assign their child’s home state to be the respondent’s current state if their child was born closer to their present age than when when they were 16. We exclude individuals whose children are under age 5 from the sample (as they would not yet have attended school). In other specifications, we exclude individuals whose children are under age 18 from the sample. The latter alternative excludes the contemporaneous effect of children’s exposure to corporal punishment on their parents in order to determine whether there are

enduring effects even after their children have left school.

The results of these specifications are shown in Table PAREFFECTS. Interestingly, when children are exposed to school corporal punishment, their parents respond with reduced social trust and reduced trust in institutions. This suggests that parents may be strongly averse to the idea of their children receiving – or being under threat of receiving – physical punishment from some third-party. Their negative reaction frays away some of the boosted social capital resulting from their own childhood exposure to corporal punishment. The effects on support for free speech and parenting style also move in the opposite direction of the main results. That said, it is important to note that the main results – the later-life effects of corporal punishment on those who were themselves exposed – pooled individuals who were and were not parents of their own children. In other words, the strongly positive effects identified earlier are net of these countervailing negative effects.

## **5 Conclusion**

Spanning nations and time, school corporal punishment has been a common feature of education systems around the world to this very day. Despite its continued use and the vigorous debate around its morality and efficacy, its effects have been heavily understudied in the academic economics literature. In the wake of an ultimately-failed Supreme Court challenge to school corporal punishment in the United States, individual states began banning its use within their borders – a process that began in the 1970s and intensified over the 1980s and 1990s. Leveraging these staggered state-level bans of school corporal punishment for difference-in-differences identification, we use survey data from the American Community Survey (ACS) and General Social Survey (GSS) to study its long-run effects. We find that school corporal punishment induces significant and robust increases in educational attainment, social capital, confidence in institutions, tolerance of free speech, and anti-authoritarian parenting attitudes. No effects are found on mental health or attitudes toward violence (by individuals or the state). Furthermore, turning to crime data from the National Incident-Based Reporting System (NIBRS), we find that school corporal punishment induces reductions in later-life property crime

and crime against society (but no effect on violent crime).

We show that these effects are robust to a wide variety of alternative identification strategies and methods of computing standard errors. Adding additional fixed-effects, adding state-specific cohort trends, computing standard errors with two-way clustering, running a variety of 2000-repetition permutation tests, estimating dynamic difference-in-differences specifications, and accounting for important potential confounds do not eliminate the results or their statistical significance.

We argue that, because only a small fraction of students are actually subject to corporal punishment, it is implausible that the effects we find proceed exclusively through those who are corporally-punished. Our coefficients measure the societal effects of corporal punishment rather than the direct effects on the individual. That is, the positive spillovers of corporal punishment are likely to be at least as important as any direct effects. If corporal punishment brings disruptive students and bullies into line, it may serve to improve the educational and social environment for the remaining majority of students. Indeed, it may also deter students who would have otherwise misbehaved from doing so, thereby improving their outcomes as well.

Turning to heterogeneities, we find evidence that effects are strongest where black students receive disproportionate corporal punishment relative to white students. However, the intensified effects proceed through white students. For example, the further reduction in crime where black students receive disproportionate punishment is a reduction in crime committed *by whites*, suggesting, again, that the effects of corporal punishment are not merely that of an individual-level deterrent but rather broader effects that play out through the channel of social capital. Relating this to the “dark side of social capital”, we present evidence that the channel of this effect is heightened racial ingroup/outgroup distinction in response to black students being singled out as “the other”. Finally, in a curious twist, we find that when individuals’ children are exposed to corporal punishment, the effect is instead to reduce social trust and trust in institutions, potentially reflecting distaste on the part of parents for the practice or threat of their children being hit.

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## Tables and Figures

**Table BANLIST: Dates of Corporal Punishment Bans**

<b>State</b>	<b>Year of Introduction</b>
New Jersey	1867
Massachusetts	1971
Hawaii	1973
Maine	1975
D.C.	1977
New Hampshire	1983
New York	1985
Vermont	1985
California	1986
Nebraska	1988
Wisconsin	1988
Alaska	1989
Connecticut	1989
Michigan	1989
Minnesota	1989
North Dakota	1989
Oregon	1989
Virginia	1989
Iowa	1989
South Dakota	1990
Montana	1991
Utah	1992
Nevada	1993
Washington	1993
Maryland	1993
Illinois	1994
West Virginia	1994
Rhode Island	2002
Delaware	2003
Pennsylvania	2005
Ohio	2009
New Mexico	2011



**Table EDUCATTAIN: Effects on Educational Attainment**

	(1)	(2)	(3)
	Years of Education	H.S. Diploma Attainment	B.A. Degree Attainment
Outcome Type:	Linear	Indicator	Indicator
CP Indicator	0.102*** (0.020)	0.007† (0.004)	0.011** (0.004)
Year FEs	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes
Years of Data	All	All	All
Clustering	State	State	State
Observations	19,323,547	19,323,547	19,323,547

† Denotes significance at 10% level; \* Denotes significance at 5% level;  
 \*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1%  
 level

**Table SOCTRUST: Effects on Social Capital and Confidence in Institutions**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Social Trust	Community Org. Member	Confidence in Fed. Govt	Confidence in Courts	Confidence in Banks & Finance	Confidence in Business & Corps	Confidence in Press/ Media	Confidence in Science/ Research	Institutional Confidence Index
Outcome Type:	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator	Sum of Indics
CP Indicator	0.096*** (0.020)	0.151* (0.075)	0.044*** (0.013)	0.049† (0.026)	0.047** (0.018)	0.018 (0.015)	0.033*** (0.011)	0.058*** (0.021)	0.403*** (0.101)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State	State
Observations	17,526	6400	17,595	17,598	17,607	17,605	17,606	17,595	17,446

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table CHILDATT: Effects on Attitudes toward Children/Parenting**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Children	"Not Fair" to Have Children	Obedience in Children Importance	FreeThght in Children Importance	Popularity in Children Importance	WorkHard in Children Importance	HelpOthrs in Children Importance	Govt Childcare Spending Preferences
Outcome Type:	Linear	Indicator	Z-Score	Z-Score	Z-Score	Z-Score	Z-Score	Z-Score
CP Indicator	0.077† (0.045)	-0.223*** (0.044)	-0.110*** (0.040)	0.160*** (0.058)	-0.066 (0.050)	0.026 (0.051)	-0.036 (0.060)	0.093** (0.036)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	27,938	5388	14,474	14,474	14,474	14,474	14,474	14,087

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table FREESPEECH: Effects on Support for Freedom of Speech**

	(1)	(2)	(3)	(4)
	Public Speech Index	Library Book Index	Teach in College Index	Joint Free Speech Index
Outcome Type:	Z-Score Index	Z-Score Index	Z-Score Index	Z-Score Index
CP Indicator	0.397*** (0.144)	0.399** (0.151)	0.386*** (0.129)	1.329*** (0.386)
Year FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All
Clustering	State	State	State	State
Observations	15,793	15,568	15,013	14,391

† Denotes significance at 10% level; \* Denotes significance at 5% level;  
 \*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1%  
 level

**Table CRIMEREGRS: Effects on Crime**

	(1)	(2)	(3)
	Violent Crime	Property Crime	Crime Against Society
Outcome Type:	ln(Crime)	ln(Crime)	ln(Crime)
CP Indicator	-0.024 (0.020)	-0.056*** (0.020)	-0.060* (0.029)
Year FEs	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes
Home-State FEs	No	No	No
Years of Data	All	All	All
Clustering	State	State	State
Observations	2,936,917	2,936,917	2,936,917

† Denotes significance at 10% level; \* Denotes significance at 5% level;  
 \*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1%  
 level

**Table ROBCONTIN: Robustness – Continuous Corporal Punishment Exposure Measure**

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of Education	Social Trust	Institutional Confidence Index	Free Speech Index	Obedience in Children Importance	Free Thought in Children Imporance
Outcome Type:	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator
Years of CP Exposure	0.0113*** (0.0028)	0.0049*** (0.0016)	0.0191* (0.0094)	0.1875*** (0.0353)	-0.0095** (0.0037)	0.0105* (0.018)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All	All
Clustering	State	State	State	State	State	State
Observations	19,323,547	17,526	17,446	14,391	14,474	14,474

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table ROBFE: Robustness – Additional Fixed Effects**

	(1)	(2)	(3)	(4)	(5)
	Social Trust	Institutional Confidence Index	Joint Free Speech Index	Obedience in Children Importance	FreeThought in Children Importance
Outcome Type:	Indicator	Sum of Indics	Z-Score Index	Z-Score	Z-Score
CP Indicator	0.102*** (0.022)	0.405*** (0.125)	0.842† (0.470)	-0.044 (0.047)	0.105* (0.050)
Home-State FEs	Yes	Yes	Yes	Yes	Yes
Year-by-Cohort FEs	Yes	Yes	Yes	Yes	Yes
Sex-by-Cohort FEs	Yes	Yes	Yes	Yes	Yes
Race-by-Cohort FEs	Yes	Yes	Yes	Yes	Yes
CurState-by-Year FEs	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All
Observations	17,499	17,416	14,354	14,449	14,449

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table ROBTREND: Robustness – State-Specific Linear Cohort Trends**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of Education	Social Trust	Institutional Confidence Index	Joint Free Speech Index	Obedience in Children Importance	FreeThght in Children Importance	Property Crime	Crime Against Society
Outcome Type:	Indicator	Indicator	Sum of Indics	Z-Score Index	Z-Score	Z-Score	ln(Crime)	ln(Crime)
CP Indicator	0.080*** (0.030)	0.101*** (0.026)	0.294*** (0.113)	0.388 (0.492)	-0.013 (0.069)	0.112 (0.067)	-0.056*** (0.021)	-0.043 (0.032)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	19,323,547	17,526	17,446	14,391	14,474	14,474	2,936,917	2,936,917

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level



**Table ROBTWOWAY: Robustness – Two-Way Clustering**

	(1)	(2)	(3)	(4)	(5)
	Social Trust	Institutional Confidence Index	Joint Free Speech Index	Obedience in Children Importance	FreeThght in Children Importance
Outcome Type:	Indicator	Sum of Indics	Z-Score Index	Z-Score	Z-Score
CP Indicator	0.092*** (0.024)	0.398*** (0.109)	1.292*** (0.462)	-0.074 (0.056)	0.158*** (0.058)
Year FEs	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All
Clustering	State	State	State	State	State
Observations	17,526	17,446	14,391	14,474	14,474

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table ROBPERM: Robustness – Permutation-Based p-Values**

	(1)	(2)	(3)	(4)	(5)
	Social Trust	Institutional Confidence Index	Joint Free Speech Index	Obedience in Children Importance	Free Thought in Children Importance
Outcome Type:	Indicator	Sum of Indics	Z-Score Index	Z-Score	Z-Score
CP Indicator	0.096***	0.403***	1.329***	-0.110***	0.160***
(p-value:)	(<0.0005)	(0.0005)	(0.0025)	(0.0065)	(0.0005)
Year FEs	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All
Clustering	State	State	State	State	State
Observations	17,526	17,446	14,391	14,474	14,474

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table ROBPART: Robustness – Effects by Political Party in Power**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Trust (Dem Gov)	Social Trust (Rep Gov)	Social Trust (Dem Legis)	Social Trust (Rep Legis)	Institutional Confidence (Dem Gov)	Institutional Confidence (Rep Gov)	Institutional Confidence (Dem Legis)	Institutional Confidence (Rep Legis)
Outcome Type:	Indicator	Indicator	Indicator	Indicator	Sum of Indics	Sum of Indics	Sum of Indics	Sum of Indics
CP Indicator	0.085*** (0.021)	0.099*** (0.023)	0.078*** (0.019)	0.125** (0.049)	0.553*** (0.112)	0.315*** (0.101)	0.376*** (0.111)	0.508** (0.194)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	14,823	12,399	13,900	9,270	14,693	12,317	13,823	9,149

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table HETSEX: Heterogeneities by Sex**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Trust (Males)	Social Trust (Females)	Social Trust (Males), Robust	Social Trust (Females), Robust	Social Trust (HighEd)	Social Trust (LowEd)	Social Trust (HighEd), Robust	Social Trust (LowEd), Robust
Outcome Type:	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator
CP Indicator	0.046† (0.024)	0.134*** (0.029)	0.070† (0.035)	0.114*** (0.031)	0.125*** (0.034)	0.089 (0.049)	0.155*** (0.042)	0.086† (0.050)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Cohort FEs	No	No	Yes	Yes	No	No	Yes	Yes
Sex-by-Cohort FEs	No	No	Yes	Yes	No	No	Yes	Yes
Race-by-Cohort FEs	No	No	Yes	Yes	No	No	Yes	Yes
CurState-by-Year FEs	No	No	Yes	Yes	No	No	Yes	Yes
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	7,872	9,654	7,786	9,594	4,896	6,360	4,720	6,266

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table HETNEIGH: Heterogeneities by Neighborhood Racial Composition**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Trust, All-White	Social Trust, Mixed	Institutional Confidence Index, All-White	Institutional Confidence Index, Mixed	Joint Free Speech Index, All-White	Joint Free Speech Index, Mixed	Obedience in Children Importance, All-White	Obedience in Children Importance, Mixed
Outcome Type:	Indicator	Indicator	Sum of Indics	Sum of Indics	Z-Score Index	Z-Score Index	Z-Score	Z-Score
CP Indicator	-0.005 (0.050)	0.162*** (0.045)	-0.258 (0.316)	0.723** (0.276)	1.898 (1.503)	0.468 (0.820)	0.007 (0.192)	-0.057 (0.081)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	2,293	3,375	2,353	3,351	1,731	2,813	1,790	2,771

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table HETPROP: Heterogeneities by Racial Proportionality of Corporal Punishment**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Trust	Institutional Confidence Index	Joint Free Speech Index	Obedience in Children Importance	Free Thought in Children Importance	Violent Crime	Property Crime	Crime Against Society
Outcome Type:	Indicator	Sum of Indics	Z-Score Index	Z-Score	Z-Score	ln(Crime)	ln(Crime)	ln(Crime)
CP Indicator	0.109*** (0.026)	0.518*** (0.112)	1.361*** (0.434)	-0.115** (0.049)	0.157* (0.072)	-0.033† (0.019)	-0.052* (0.023)	-0.064* (0.031)
CP Ratio B-to-W	0.044 (0.036)	0.059 (0.200)	0.560 (0.664)	-0.092 (0.096)	0.172† (0.095)	-0.125† (0.066)	-0.083 (0.055)	0.047* (0.021)
CP Indic*CP Ratio	0.0010*** (0.0003)	-0.0120*** (0.0021)	0.0298** (0.0124)	-0.0026** (0.0011)	0.0007 (0.0012)	-0.0009*** (0.0002)	-0.0004† (0.0002)	-0.0021*** (0.0002)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	No	No	No
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	16,325	16,268	13,434	13,474	13,474	2,730,374	2,730,374	2,730,374

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table HETPROPCRIME: Heterogeneities by Racial Proportionality of Corporal Punishment (Crime Offender Race)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Violent Crime	Property Crime	Crime Against Society	Violent Crime	Property Crime	Crime Against Society
Outcome Type:	ln(Crime)	ln(Crime)	ln(Crime)	ln(Crime)	ln(Crime)	ln(Crime)
CP Indicator	-0.034† (0.018)	-0.039 (0.025)	-0.066* (0.032)	-0.026 (0.035)	-0.031 (0.029)	0.022 (0.034)
CP Ratio B-to-W	-0.098† (0.051)	-0.030 (0.051)	0.062*** (0.021)	-0.035 (0.044)	-0.204*** (0.027)	0.019 (0.016)
CP Indic*CP Ratio	-0.0007*** (0.0002)	-0.0004† (0.0002)	-0.0020*** (0.0003)	-0.0002 (0.0004)	0.0004 (0.0003)	0.0004 (0.0003)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	No	No	No	No	No	No
Years of Data	All	All	All	All	All	All
Offender Population	Whites	Whites	Whites	Blacks	Blacks	Blacks
Clustering	State	State	State	State	State	State
Observations	2,737,621	2,737,621	2,737,621	1,178,078	1,178,078	1,178,078

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

**Table HETPROPCRIMEWHT: Heterogeneities by Racial Proportionality of Corporal Punishment (Crime Victim Race)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Violent Crime	Property Crime	Crime Against Society	Violent Crime	Property Crime	Crime Against Society
Outcome Type:	ln(Crime)	ln(Crime)	ln(Crime)	ln(Crime)	ln(Crime)	ln(Crime)
CP Indicator	-0.043* (0.021)	-0.015 (0.018)	-0.004 (0.009)	-0.013 (0.024)	-0.005 (0.025)	0.022 (0.034)
CP Ratio B-to-W	-0.143*** (0.030)	0.055 (0.039)	0.047*** (0.003)	0.090*** (0.011)	0.000 (0.019)	0.019 (0.016)
CP Indic*CP Ratio	-0.0005*** (0.0002)	-0.0006*** (0.0002)	-0.0002† (0.0001)	-0.0002 (0.0002)	0.0002 (0.0003)	0.0000 (0.0001)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	No	No	No	No	No	No
Years of Data	All	All	All	All	All	All
Offender Population	Whites	Whites	Whites	Whites	Whites	Whites
Victim Population	Whites	Whites	Whites	Blacks	Blacks	Blacks
Clustering	State	State	State	State	State	State
Observations	2,260,995	2,260,995	2,260,995	352,450	352,450	352,450

† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level



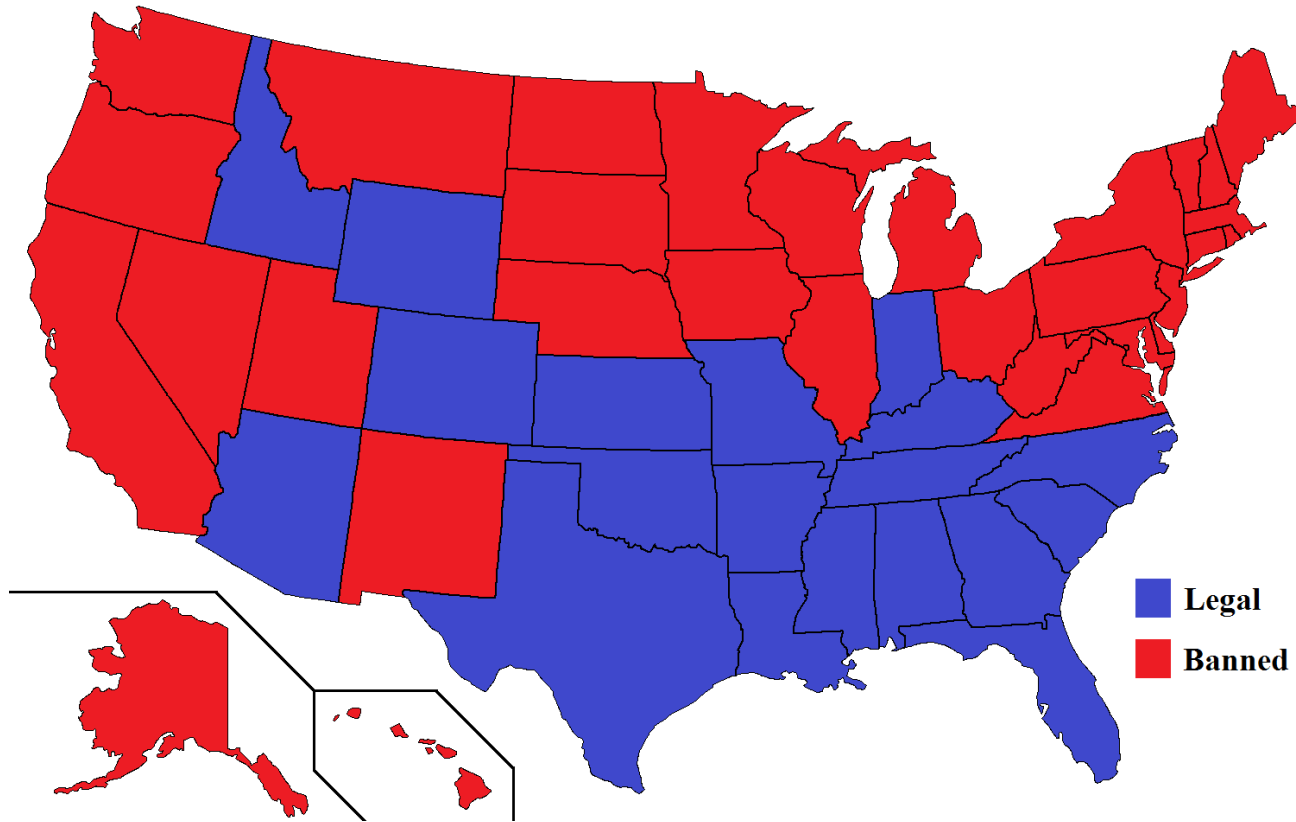
**Table PAREFFECT: Effects on Parents**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Trust, Indicator, +5	Institutional Confidence, Indicator, +5	Social Trust, Years, +5	Institutional Confidence, Years, +5	Social Trust, Indicator, +18	Institutional Confidence, Indicator, +18	Social Trust, Years, +18	Institutional Confidence, Years, +18
Outcome Type:	Indicator	Sum of Indics	Indicator	Sum of Indics	Indicator	Sum of Indics	Indicator	Sum of Indics
Own Child CP	-0.068*** (0.019)	-0.117 (0.099)	-0.0071*** (0.0019)	-0.0243*** (0.0091)	-0.063* (0.030)	-0.174 (0.155)	-0.0134*** (0.0032)	-0.0476** (0.0182)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	All	All	All	All	All	All	All	All
Clustering	State	State	State	State	State	State	State	State
Observations	6,980	6400	6,980	6,400	3,406	3,242	3,406	3,242

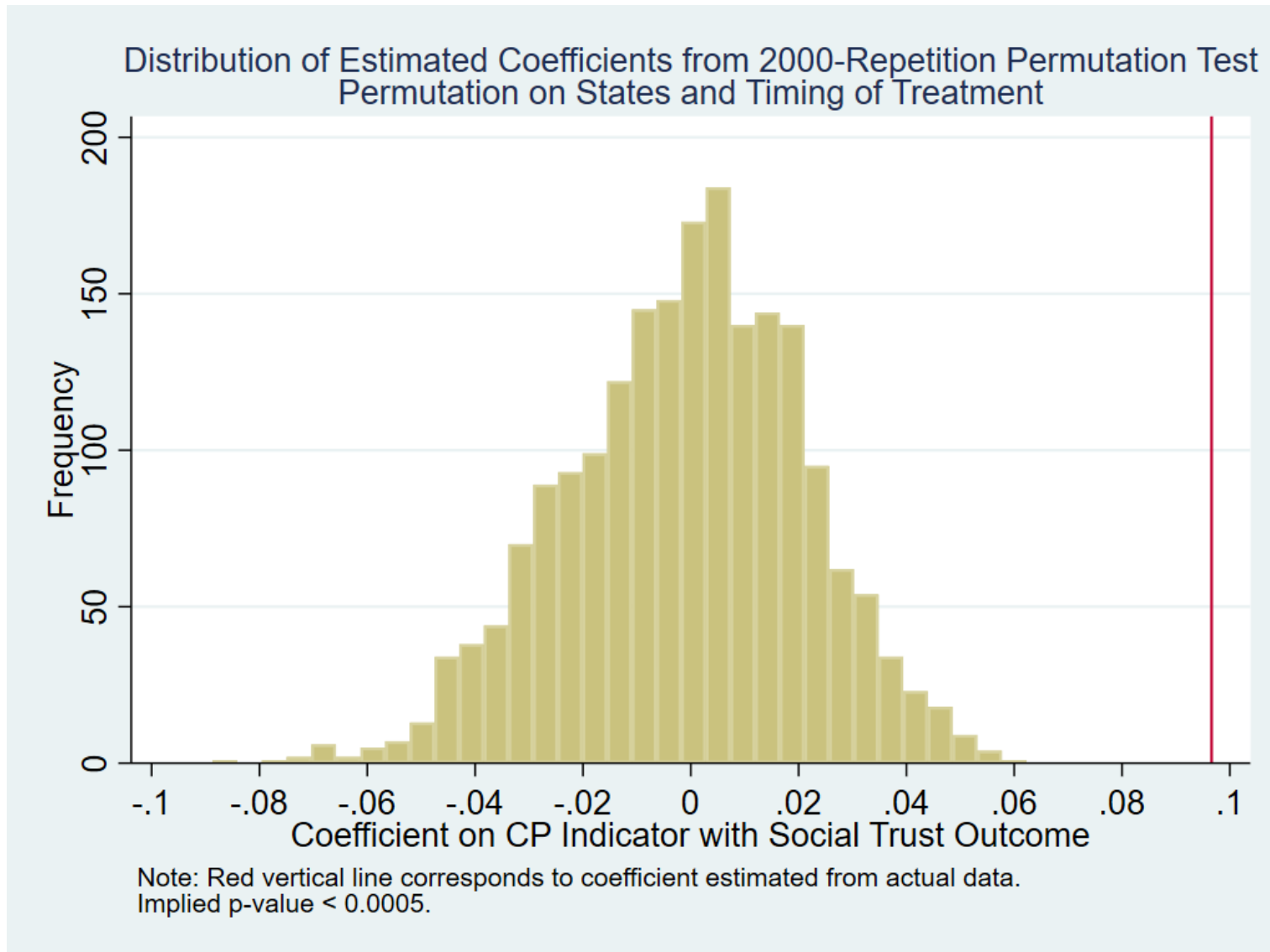
† Denotes significance at 10% level; \* Denotes significance at 5% level;

\*\* Denotes significance at 2.5% level; \*\*\* Denotes significance at 1% level

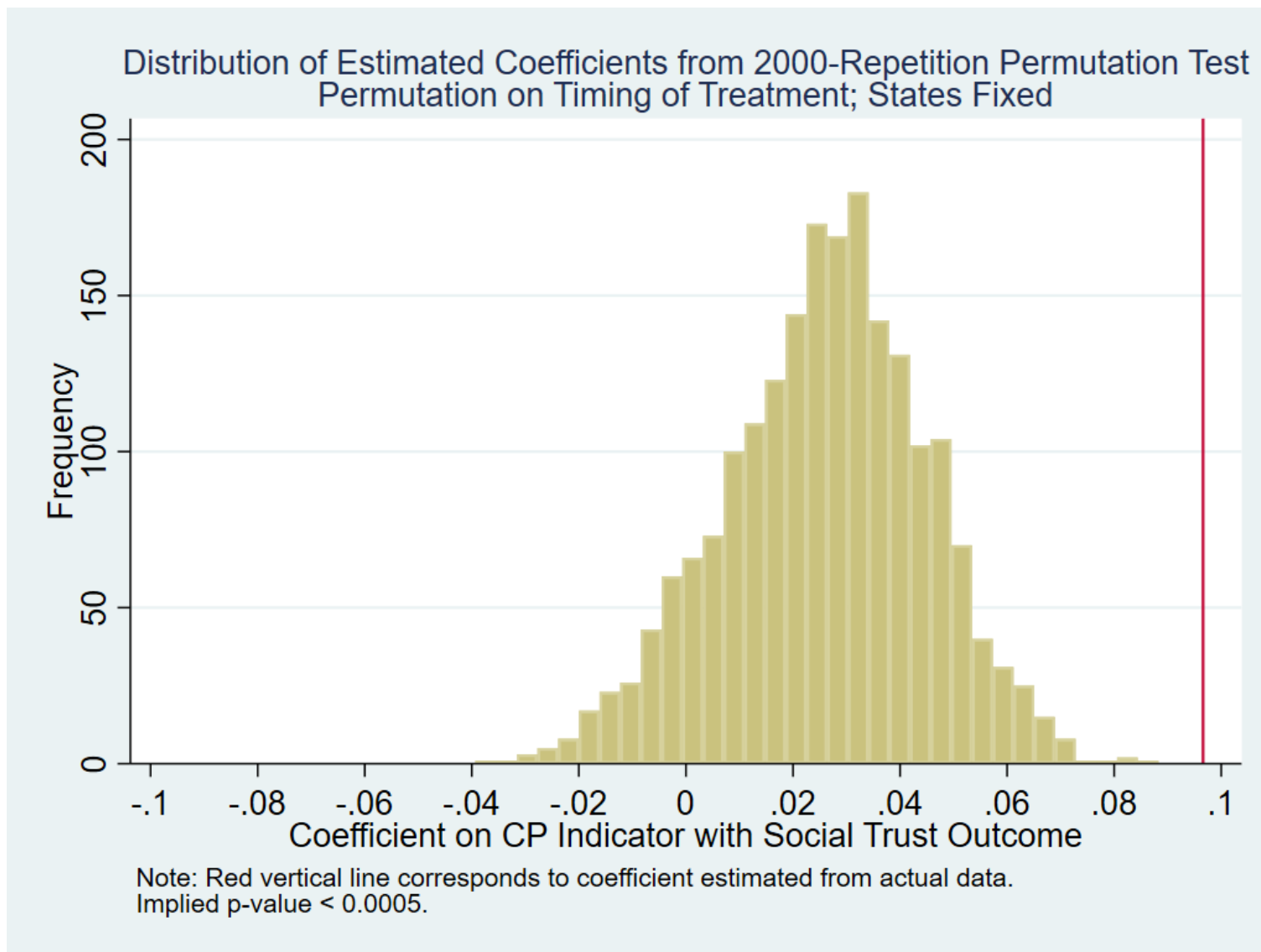
**Figure BANFIG: School Corporal Punishment Legality**



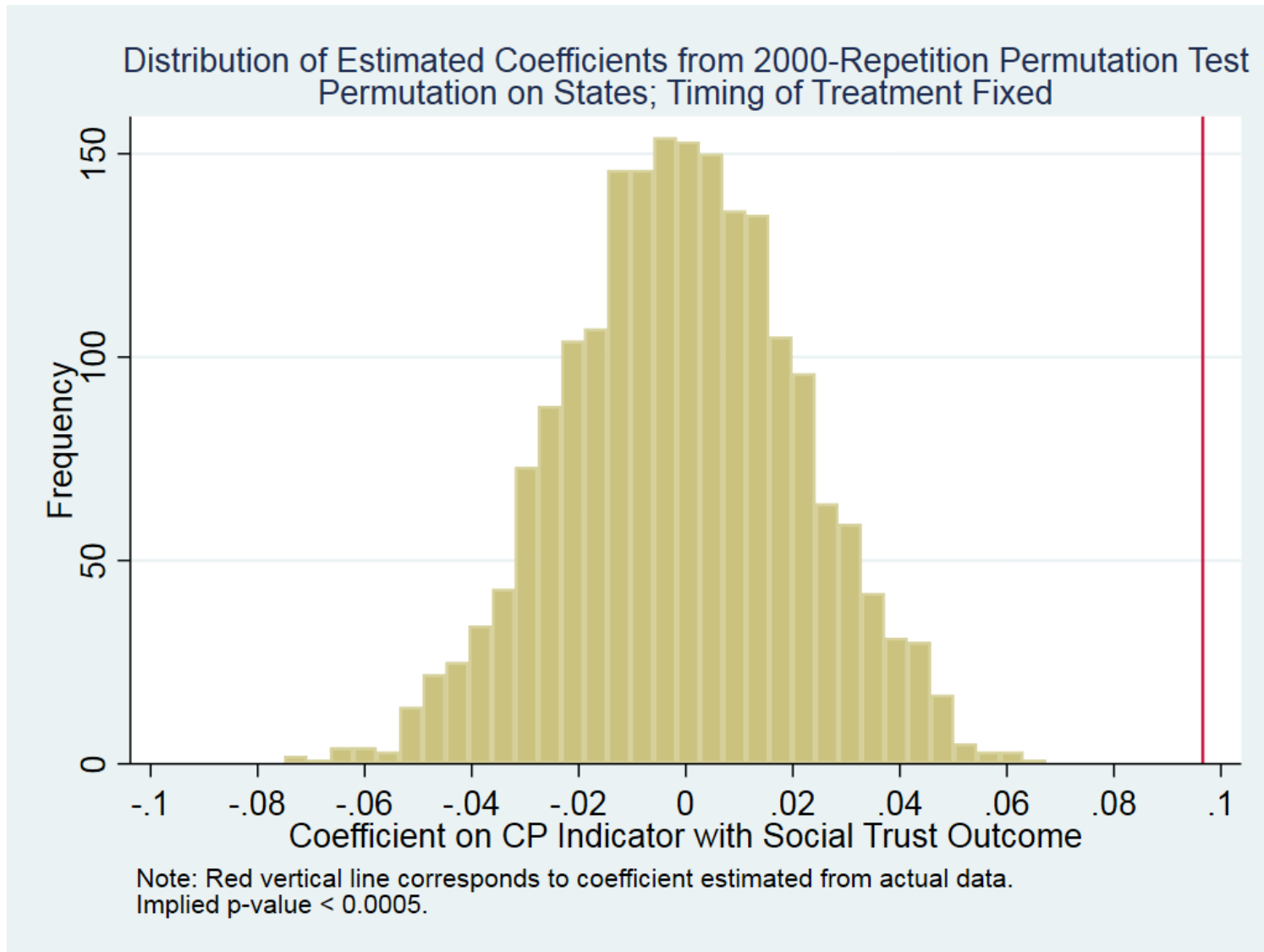
**Figure ROBPERM1FIG: Permutation Test (i) – Social Trust**



**Figure ROBPERM2FIG: Permutation Test (ii) – Social Trust**



**Figure ROBPERM3FIG: Permutation Test (iii) – Social Trust**



**Figure TRUSTDDD: Effects on Social Trust – Dynamic Difference-in-Differences**

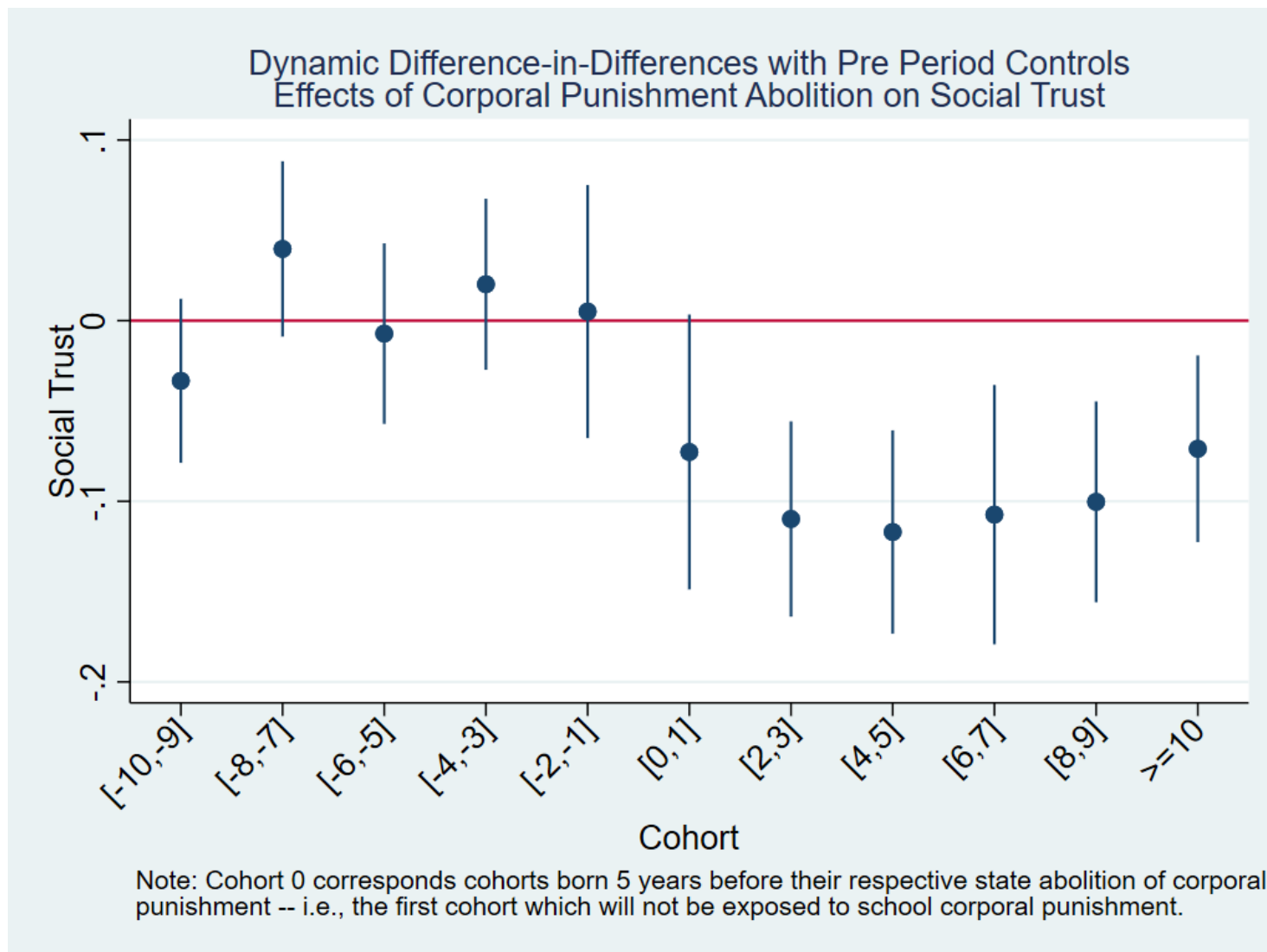
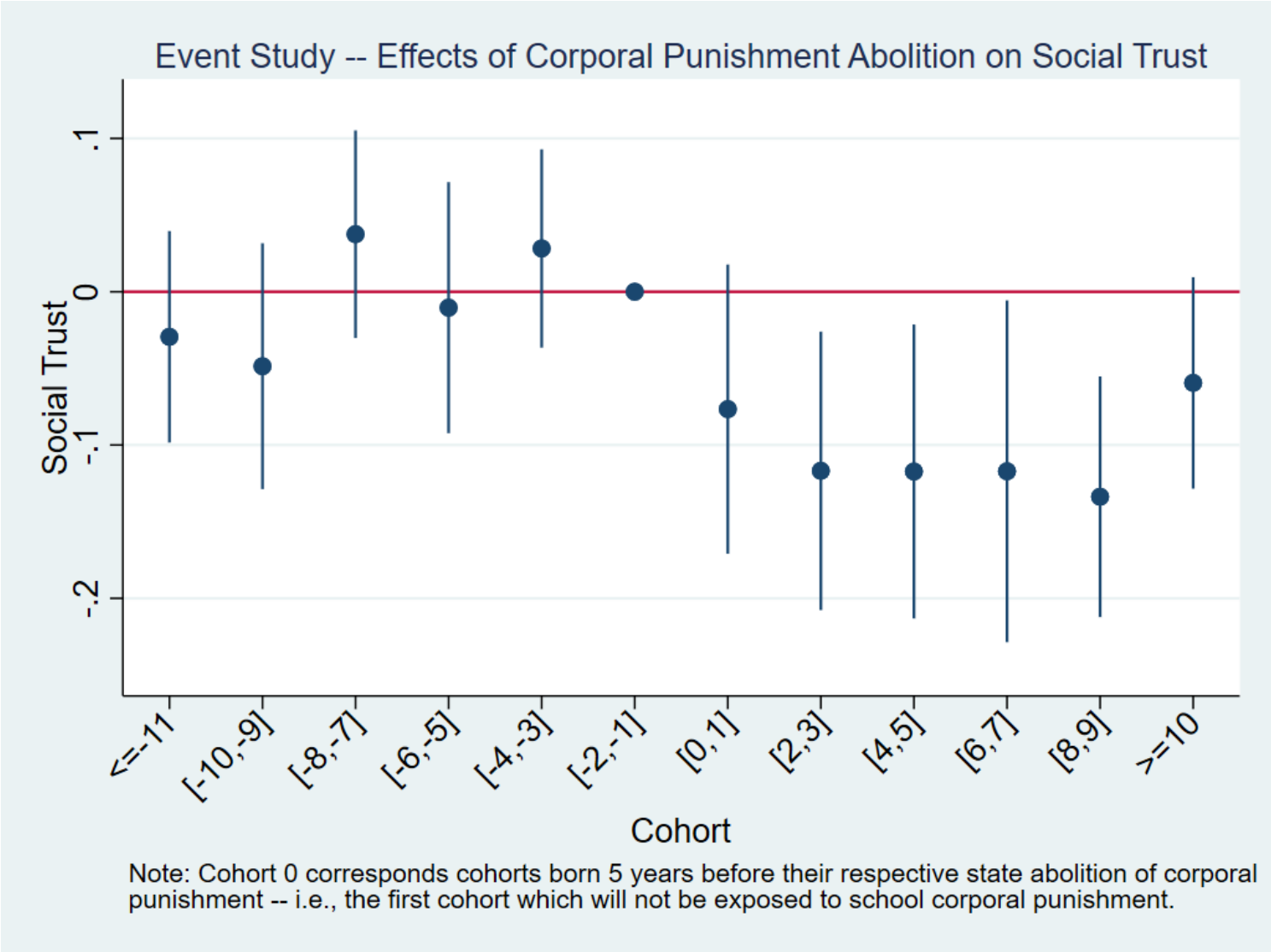
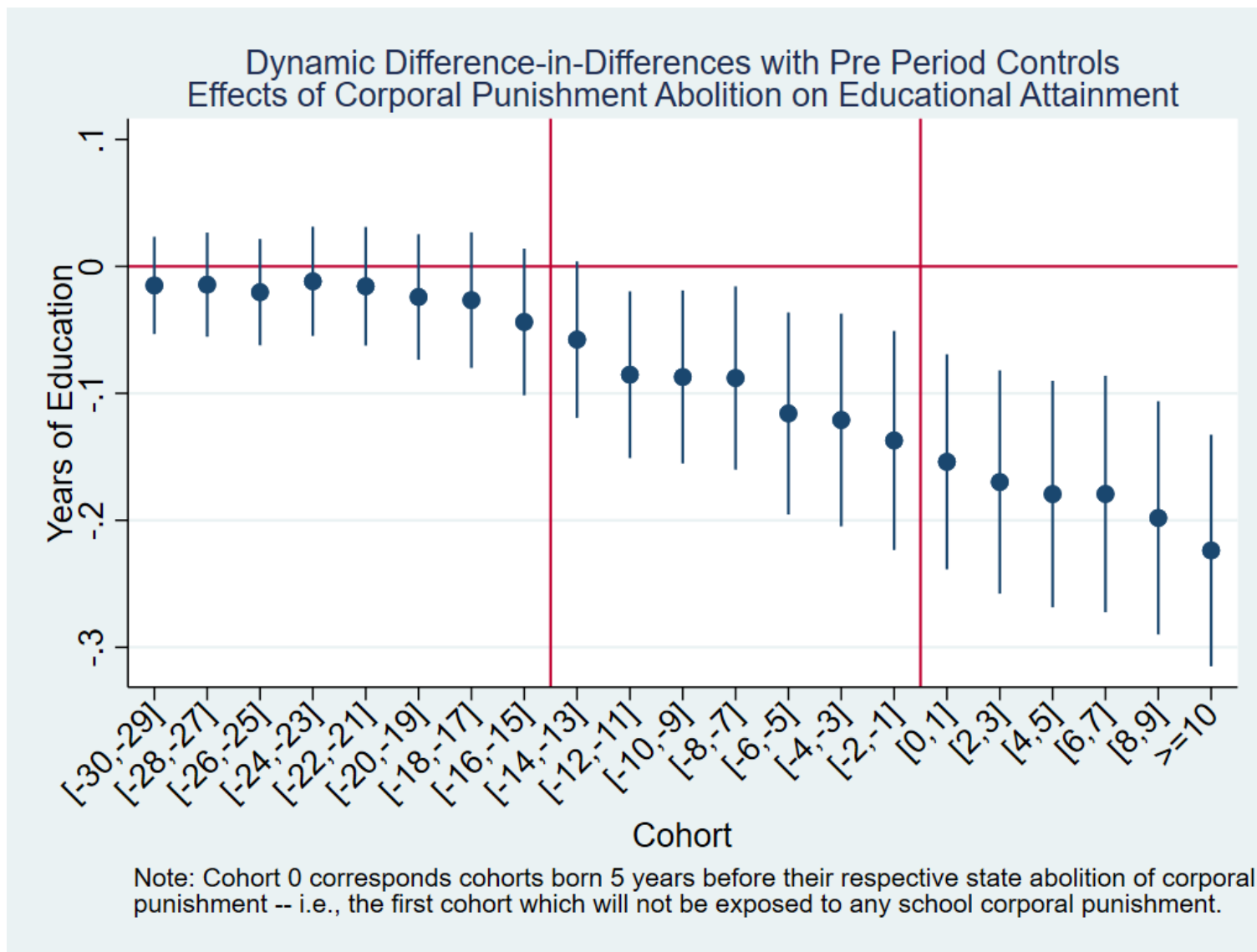


Figure TRUSTEVSTUD: Effects on Social Trust – Event Study



**Figure EDUCDDD: Effects on Educational Attainment – Dynamic Difference-in-Differences**





**Figure EDUCEVSTUD: Effects on Educational Attainment – Event Study**

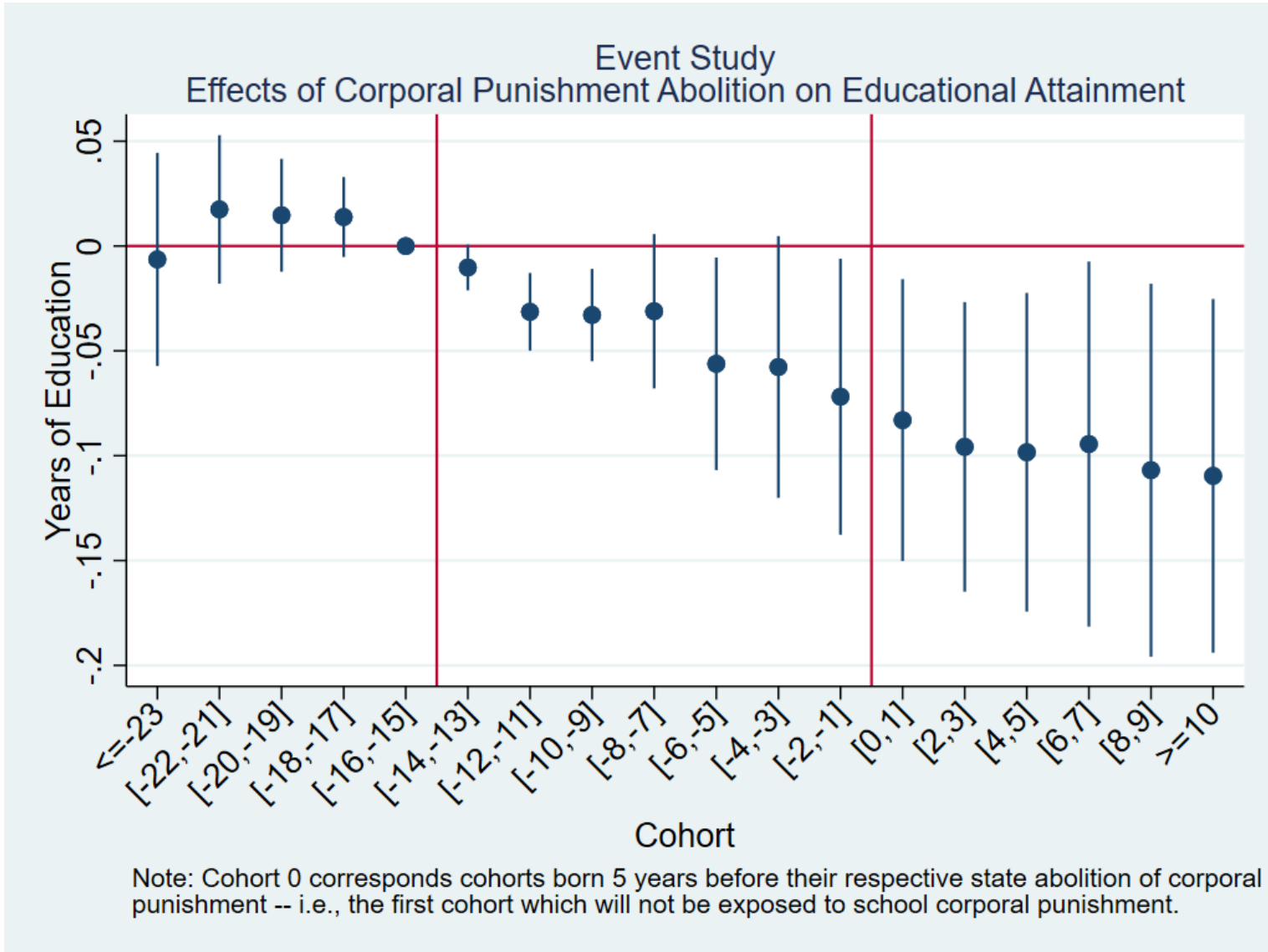


Figure CRIMEDDD: Effects on Property Crime – Dynamic Difference-in-Differences

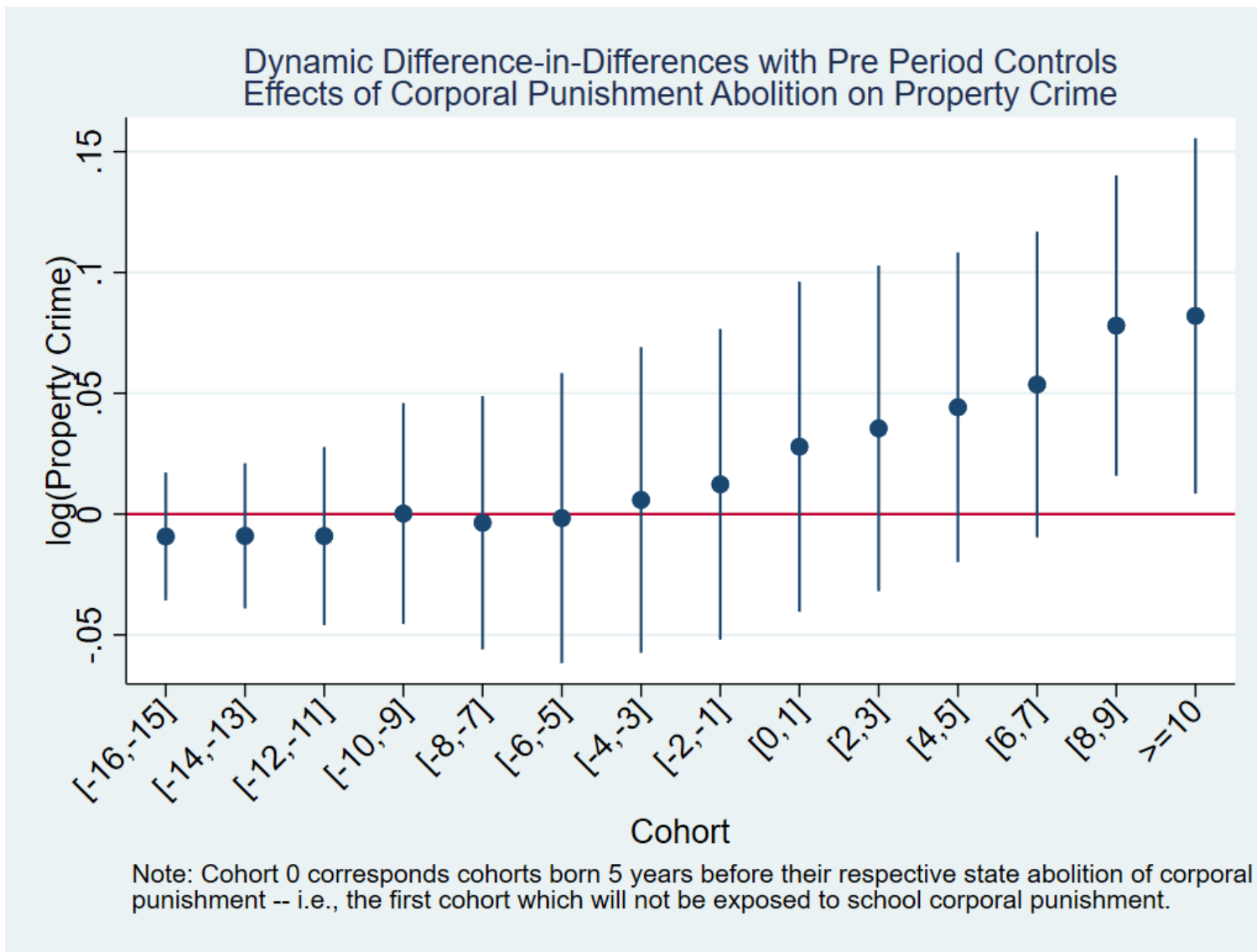
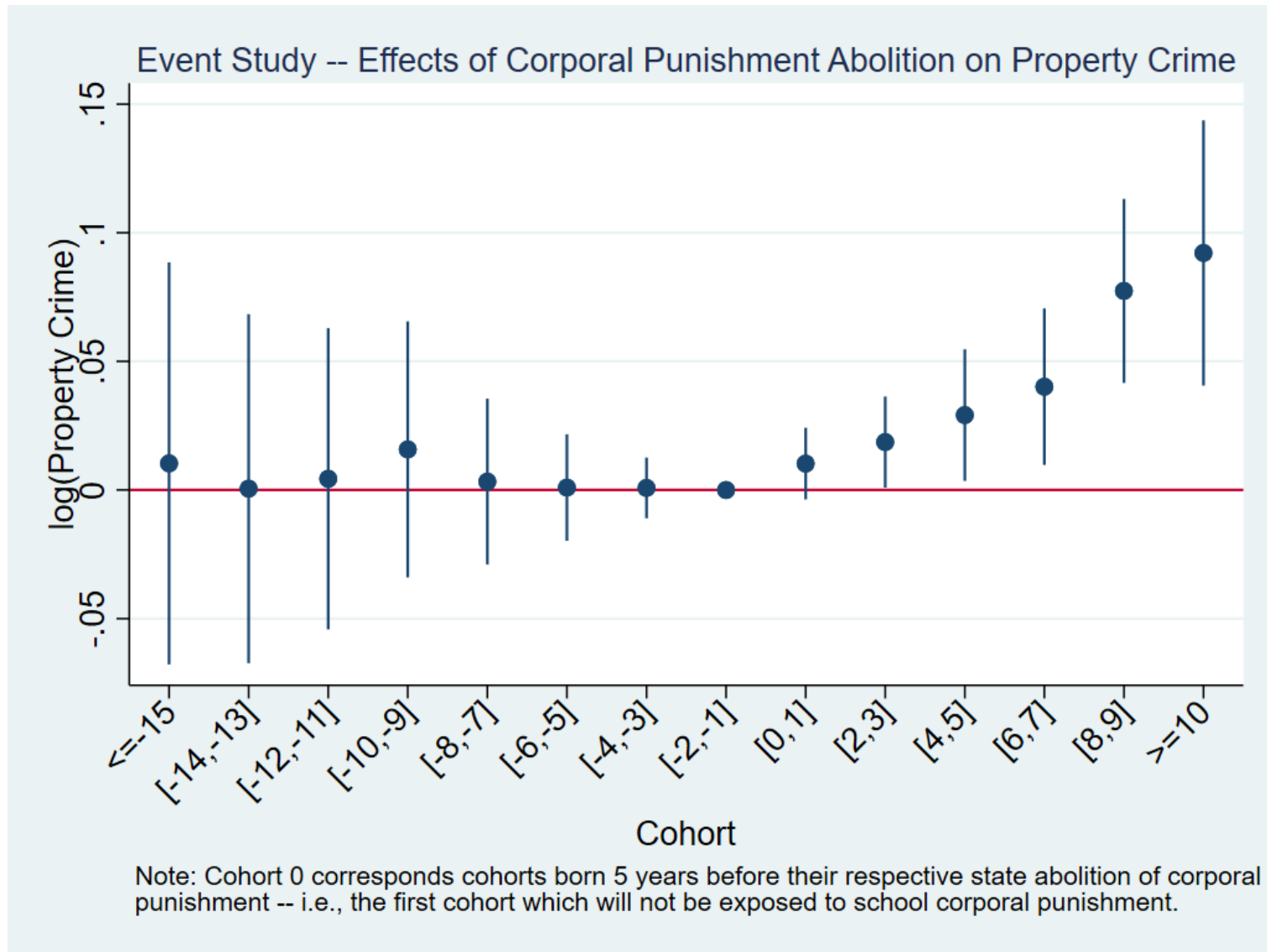


Figure CRIMEEVSTUD: Effects on Property Crime – Event Study



**Figure SCHSPEND: School Spending (Placebo test)**

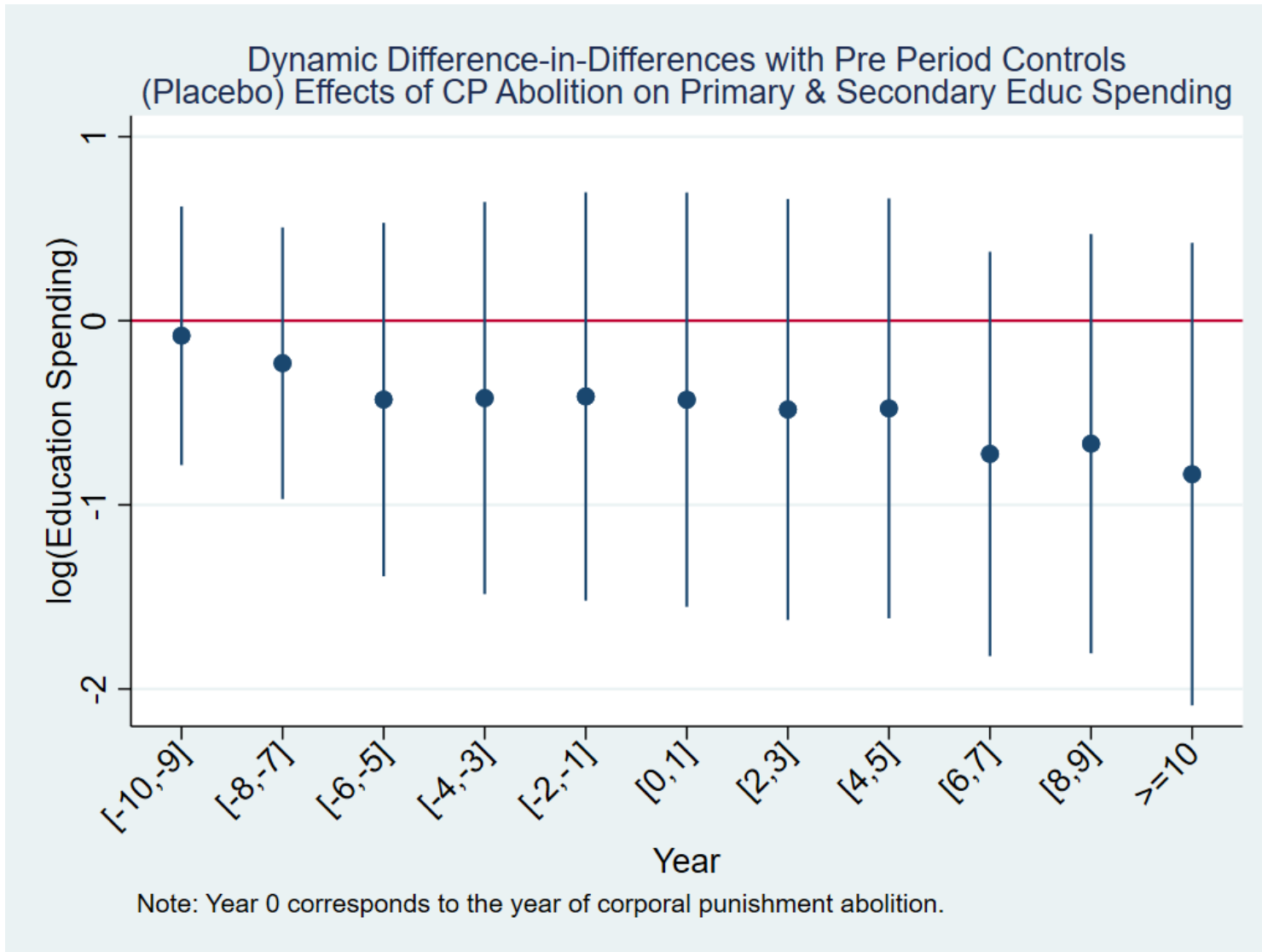


Figure PARTISANGOV: Cohort Partisan Exposure – Governors (Placebo Test)

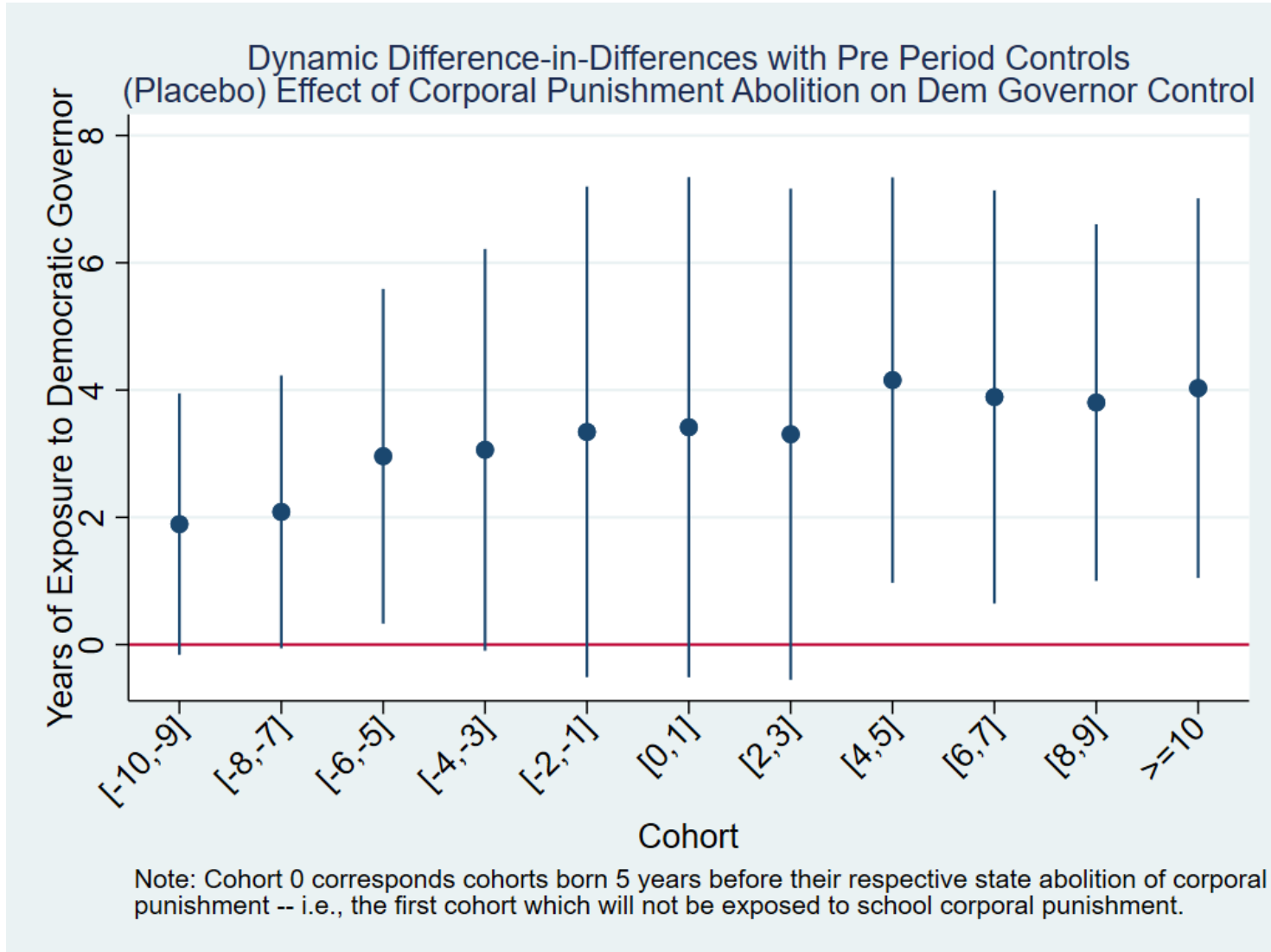


Figure PARTISANHOUSE: Cohort Partisan Exposure – State House (Placebo Test)

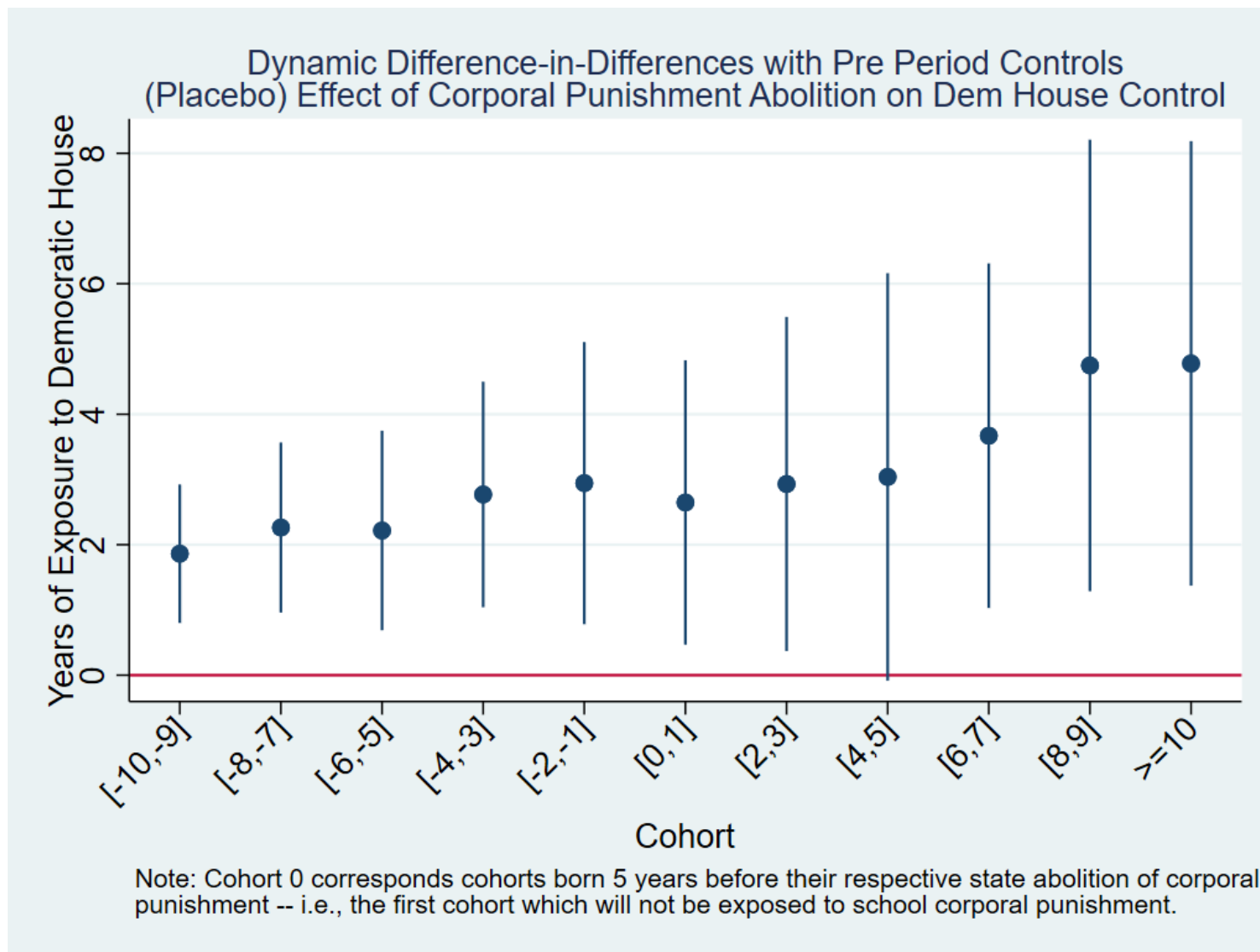


Figure PARTISANSEN: Cohort Partisan Exposure – State Senate (Placebo Test)

