

The Long-Run Effects of Corporal Punishment in Schools¹

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Abstract: Corporal punishment (such as spanking) 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 cohort difference-in-differences design to measure the causal effects of school corporal punishment on later-life outcomes. We find that the abolition of corporal punishment in schools led to, on average, lower educational attainment, higher crime rates, higher social trust and trust in institutions, and lower tolerance of free speech. We find no effects on mental or physical health. These results hold up to event-study difference-in-differences specifications – which reveal non-existence of pre-trends – and a wide variety of other robustness checks. We find some suggestive evidence that these effects are the result of spillovers, rather than direct effects on the (relatively small) fraction of students actually receiving corporal punishment.

1 Introduction

Corporal punishment was historically a standard method to enforce discipline in schools around the world. Teachers in the 19th century were encouraged to employ corporal punishment over other methods of discipline, and there was little to no organized opposition to the practice

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until the 20th century³. 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 abolition of corporal punishment in schools led to lower later-life social trust and trust in institutions, more authoritarian attitudes toward child-rearing, and decreased tolerance of free speech. Furthermore, its effects are not limited merely to the domain of beliefs and values: abolition of school corporal punishment resulted in decreased educational attainment and increased later-life crime – in particular, property crime and crime against society. A likely mechanism for these findings is that no less harmful alternative was offered to the teachers to sustain the discipline in the class; suspensions and detentions might be even more harmful for the recipients than the practiced form of corporal punishment.

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-

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

specific linear cohort trends to allow for abolishing and non-abolishing states to be on different 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 event-study difference-in-differences specifications including pre-treatment periods in order to show the non-existence of pre-trends and deal with 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 “negative” effects of the abolition reforms 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 directly observe whether individuals in the samples of the GSS and ACS 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. To provide some additional suggestive evidence on the matter, we ran a survey of our own design through survey panel company PureProfile asking respondents about their experiences with corporal punishment and other forms of school discipline. We find suggestive evidence that the effects of increased years of education, social trust, trust in institutions, and anti-authoritarian attitudes are associated with exposure of *one's schoolmates* to corporal punishment. The coefficients on own exposure to corporal punishment have the opposite sign in most cases. This is consistent with our main findings operating through spillover effects on the students not receiving corporal punishment. We also find some evidence that corporal punishment bans are associated with increases in the use of suspensions and expulsions and no decrease in the use of corporal punishment itself, suggesting that the bans may have led to a shift in schools' disciplinary strategies and that our main results may partially reflect the harms of these other disciplinary approaches.

We do not 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 its (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 19th and early-20th 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 legal⁴. 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

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

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 (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.

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).

Finally, in order to conduct an investigation into the mechanisms behind the effects of

corporal punishment, we ran a large survey of more than 10,000 respondents through the survey company PureProfile. PureProfile maintains a panel of survey respondents intended to be representative of the U.S. population; for a fee, they distribute surveys to the members of their panel on behalf of researchers or marketers. Our survey asked respondents a variety of questions about the frequency with which they became aware of corporal punishment being used on other students in the school(s) they attended and the frequency with which it was used on them. The survey included a detailed description and comprehension check of the definition of corporal punishment to ensure that individuals understood the meaning of the term. Additionally, we asked questions about other forms of discipline in their schools (such as detention, suspension, and expulsion), a variety of school disruptions (such as classroom interruptions, bullying, and fights), our outcome variables of interest (years of education, social trust, authoritarian attitudes, etc.), and some basic demographic and geographic questions necessary for our research design (such as birth cohort and school(s) attended by the individual). While the survey is much smaller than the ACS, GSS, or NIBRS and only focuses on one point in time, it is useful for a more suggestive, correlational examination of the channels and mechanisms underlying corporal punishment.

3.2 Econometric Approach

As noted, the ACS and the GSS Sensitive Data disclose 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:

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

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 . γ_c denotes cohort fixed-effects. φ_h denotes home-state fixed-effects. τ_t denotes year fixed-effects. ω_{t-c} denotes age fixed-effects. η_s denotes current-state fixed effects. $\mathbf{1}[CP \text{ Exposure}_{ch}]$ 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.

The key identification assumption for a difference-in-differences specification such as this one is that of parallel trends: the outcome variable of interest would have evolved analogously in treatment and control if, counterfactually, the treatment group had not received treatment. The primary issue with this assumption pertains to policy endogeneity. Passage of state laws is not randomly-assigned; hence the states that adopted the law may plausibly have been on a different trajectory than the states which did not do so. To deal with this conjecture, we identify principal dimensions in which states may have been on different trajectories that could plausibly have influenced schoolchildren – education funding and state partisan lean. We show that these variables do not explain our effects. Furthermore, in some specifications, 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.

Furthermore, we run a number of other closely-related alternative specifications that build on the 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 other 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. 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 an event-study 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), amongst others, that coefficients estimated by running a static difference-in-differences specification in a 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 event-study 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^{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 CRIMEREGRS 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 abolition reforms might have tangible, material costs to society years after it was applied. These reforms should likely be accompanied with transitions to less harmful ways to ensure the discipline, which apparently was not done

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. Figure ROBPERMFIG plots 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.⁵

[Figure ROBPERMFIG about here]

[Table ROBPERM about here]

Next, we move on to event-study 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 multi-period setting where treatment effects are heterogeneous

⁵ Permutation tests on educational attainment are not included for computational reasons. Since each iteration would entail a regression with 20 million observations, the tests would be very time-consuming to run.

over time may potentially lead to a particular form of bias wherein the static difference-in-differences coefficient is outside the convex hull of the true coefficients for each post-treatment period. Figure MAINOUTCOMES plots the event-study difference-in-differences specification, including the education, social trust, free speech, and property crime outcomes. In no case is any evidence of pre-trends found. Prior to abolition of corporal punishment, social trust, support for free speech, educational attainment, and crime are flat across cohorts. In the cohorts after the full abolition of corporal punishment, social trust, support for free speech, and educational attainment decrease; property crime increases.

[Figure MAINOUTCOMES 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 both very weak (non-significant) and small in magnitude. 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. Thus, 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 likely to be responsible for the effects.

[Table ROBPART about here]

4.6 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 results are statistically significant for children of higher-education parents and not statistically significant at conventional levels for children of lower. So, here too, the results are consistent with the aforementioned mechanism.

[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 child⁶. 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

⁶ 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).

where they manifest themselves. This result suggests some interactions of corporal punishment with matters of identity salience.

[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. The data is spotty at best and is missing for many school districts in many years; however, for those that did report some data, it can be used to compute the 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 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]

4.7 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.

[Table PAREFFECTS about here]

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.

4.8 PureProfile Survey Results – Direct Effects or Spillovers?

We next turn to the survey we ran through PureProfile. Individuals were able to provide the following answers in response to the question of how frequently they experienced (or became aware of other students experiencing) corporal punishment in the schools they attended: Never, Once every several years, Once a year, Once a month, Once a week, or Daily. Consequently, it is possible to report the outcome variable in a number of different formats: as an indicator for whether the individual experienced any corporal punishment, by converting the categorical variable into a z-score, or by converting the categorical variable into a measure of the number of incidences of corporal punishment per year. The same is true for many other questions in the survey, which were asked in the same format – including the questions about classroom disruptions, fights, bullying, detentions, suspensions, and expulsions. Table PPSUMSTAT contains some descriptive statistics on the PureProfile survey sample and their responses to these questions. Notably, almost exactly half of the sample reports that they never became aware of other students experiencing corporal punishment during their schooling; the other half reports at least some exposure.

[Table PPSUMSTAT about here]

Turning to inference, Table PPCORPPUN reveals that there is no significant evidence of the corporal punishment bans being associated with a reduction in reported corporal punishment. Each column of the table corresponds to our static regression specification. Column (1) features

as an outcome the indicator variable for whether the respondent ever became aware of any other students experiencing corporal punishment in the schools they attended. Column (2) features an outcome variable that instead converts the categorical variable corresponding to the frequency corporal punishment was observed into a z-score. Column (3) features the number of incidences per year as an outcome variable (e.g., Once a year would correspond to 1, Once a month would correspond to 30, etc.). Columns (4) through (6) repeat this cycle, albeit with the underlying question being whether the respondent *themselves* experienced corporal punishment. However, the result is clear across columns of the table: if anything, these simple specifications suggest an increase in corporal punishment being associated with bans. Figure PPCORPPUNFIG reveals, however, that these results are quite noisy, and there is no consistent evidence of anything other than a null effect in the more sophisticated event-study specifications.

[Table PPCORPPUN about here]

[Figure PPCORPPUNFIG about here]

Table PPOTHER examines the effects of the bans on a variety of other relevant outcomes. Columns (1) through (3) focus on effects on the frequency with which other forms of discipline are practiced. Are the corporal punishment bans associated with increased use of expulsions, suspensions, and detentions? The answer to this question appears to be yes – though the effect is not quite statistically significant at traditional levels in the case of detentions. This suggests an alternative mechanism behind the effects induced by the corporal punishment bans. A growing literature in the economics of education finds important negative effects of school disciplinary methods like suspensions and expulsions (see, for example, Bacher-Hicks, Billings, and Deming 2019). Instead of being in-class and under supervision, misbehaving children are sent away from school – potentially to interact in an unsupervised setting with the other negatively-selected children who have also been so disciplined.

[Table PPOTHER about here]

Again, however, the noisiness of these results in the more sophisticated event-study specifications is worth noting. There is limited evidence of any effects in that context, so once again, these results should be regarded as suggestive at best.

[Figure PPOTHERFIG about here]

Columns (4) through (6) of Table PPOTHER focus on effects on disruptions to the school educational environment – classroom disruptions in column (4), bullying in column (5), and fights at school in column (6). In two of these three cases (classroom disruptions and fights), corporal punishment is associated with decreased incidence of disruptions. All columns in this table used an indicator variable as their outcome variable; using the z-score or incidence per-year version of each variable does not change the conclusions. Again the result is not altogether robust to the event-study specifications.

While we appear to lack power for running satisfying cohort difference-in-differences specifications in the context of our PureProfile survey, it is possible to run simpler OLS fixed-effect regressions investigating the effects of own exposure and exposure of others to corporal punishment on our main outcomes – education, social trust, authoritarian attitudes, etc.⁷ While these specifications should again be regarded as suggestive, they provide some additional evidence as to whether the channel of the effect of corporal punishment is through those who actually receive it or spillovers to others. Table PPSPILLOVER shows that exposure of one’s schoolmates to corporal punishment is associated significantly with higher educational attainment, more social trust, more trust in institutions, and more pro-free-speech attitudes – consistent with our main results in the ACS and GSS. Meanwhile, the coefficients on own exposure to corporal punishment have the opposite sign in most cases. This can be interpreted as additional evidence that the effects of corporal punishment are mostly systemic; they appear to reflect spillovers on children not receiving corporal punishment. The children who receive it tend to have, if anything, somewhat worse outcomes.

[Table PPSPILLOVER about here]

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

⁷ We did not ask survey participants a question about whether they have committed crimes.

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 abolition reforms led to significant and robust decrease 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 abolition reforms induced 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 event-study 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 spillovers of corporal punishment are likely to be at least as important as any direct effects. Furthermore, we find that the corporal punishment bans are associated with increases in the use of suspensions and expulsions as disciplinary strategies, suggesting that a large component of the effects we find may simply reflect the large negative effects of suspensions and expulsions that have been uncovered elsewhere in the literature on the economics of education.

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. These findings parallel the “dark side of social capital”, as identified by Voth et al. (2019). Finally, we find that when individuals’ children are exposed to corporal punishment, the effect is 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

State	Year of Ban
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 CRIMEREFS: 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 Importance
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	FreeThght 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

Table PPSUMSTAT: Descriptive Statistics on PureProfile Survey Sample

Statistic	Value
Median Age	45
Median Household Income	\$50,000
Share Male	0.467
Share White, non-Hispanic	0.763
Share Public School Attendees	0.779
Share with B.A. or Greater Education	0.423
Share Raised in 2-Parent HH	0.740
Share In Labor Force	0.699
Observations	10,874

Table PPCORPPUN: Effects on Reported Experience of Corporal Punishment

	(1)	(2)	(3)	(4)	(5)	(6)
	Schoolmates: Any Corp. Pun. Experience	Schoolmates: Corp. Pun. Experience	Schoolmates: Corp. Pun. Experience	You: Any Corp. Pun. Experience	You: Corp. Pun. Experience	You: Corp. Pun. Experience
Outcome Type:	Indicator	Z-Score	Incidents/year	Indicator	Z-Score	Incidents/year
CP Indicator	-0.105*** (0.039)	-0.376*** (0.127)	-9.842*** (3.107)	-0.092*** (0.028)	-0.183** (0.075)	-1.462 (2.395)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State	State
Observations	10,240	10,238	10,238	10,238	10,238	10,238

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

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

Table PPOTHER: Effects on Reported Use of Other Disciplinary Methods and Disruptive Behaviors

	(1)	(2)	(3)	(4)	(5)	(6)
	Schoolmates: Any Expulsion Experience	Schoolmates: Any Suspension Experience	Schoolmates: Any Detention Experience	Any Classroom Disruptions at School	Any Bullying at School	Any Fights at School
Outcome Type:	Indicator	Indicator	Indicator	Indicator	Indicator	Indicator
CP Indicator	-0.038* (0.018)	-0.030** (0.013)	-0.015 (0.014)	-0.036*** (0.009)	-0.000 (0.015)	-0.028** (0.011)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State	State
Observations	10,240	10,240	10,240	10,226	10,234	10,234

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

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

Table PPSPILLOVER: Effects on Main Outcomes – Direct Effects vs. Spillovers

	(1)	(2)	(3)	(4)	(5)
	Years of Education	Social Trust	Institutional Confidence Index	Free Speech Index	Free Thought in Children Importance
Outcome Type:	Linear	Indicator	Sum of Indics	Z-Score Index	Z-Score
CP Indicator (You)	-0.180*** (0.062)	0.058*** (0.011)	1.133*** (0.242)	-0.607*** (0.156)	-0.017 (0.036)
CP Indicator (Schoolmates)	0.139** (0.058)	0.039*** (0.011)	0.571** (0.228)	0.488*** (0.147)	0.011 (0.034)
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Home-State FEs	Yes	Yes	Yes	Yes	Yes
Standard Errors	Robust	Robust	Robust	Robust	Robust
Observations	10,325	10,125	8,178	10,315	6,353

† 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

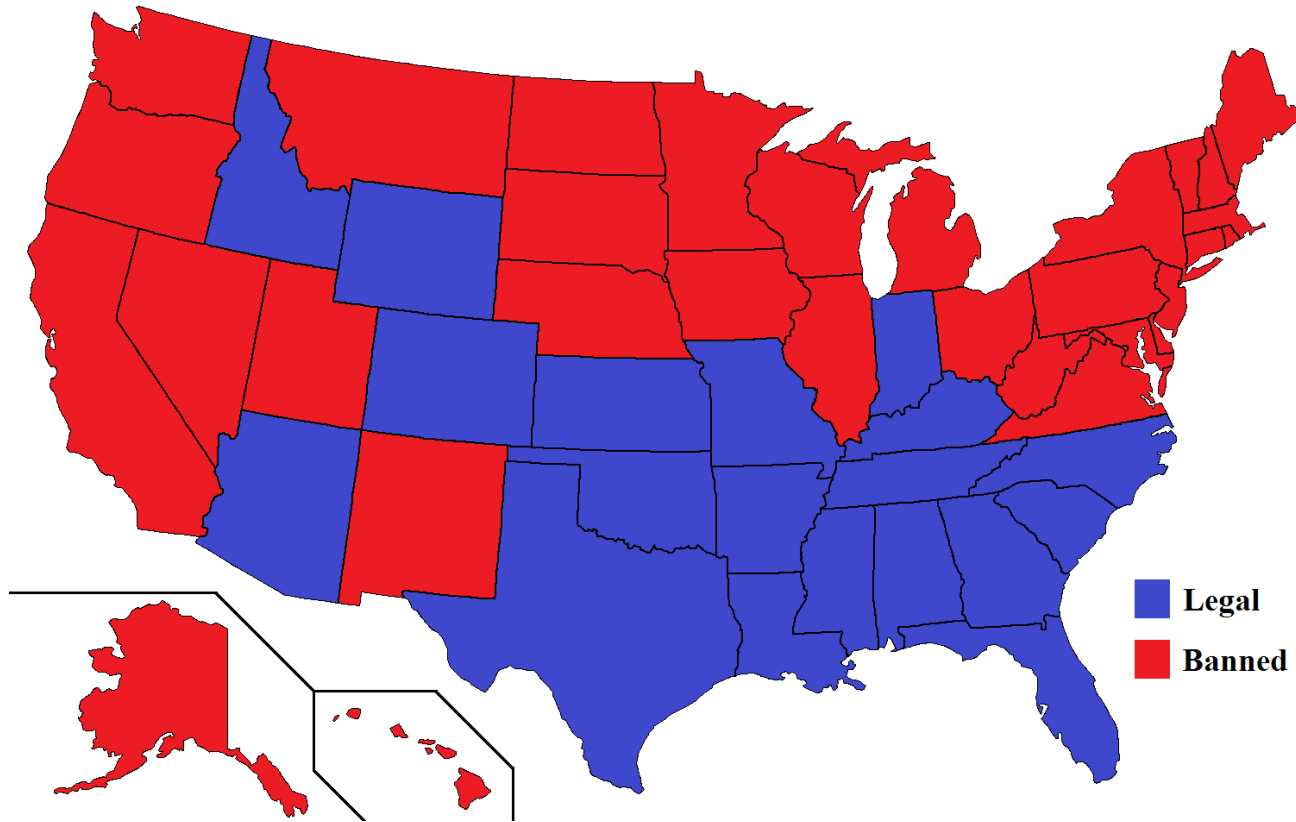


Figure ROBPERMFIG: Permutation Tests

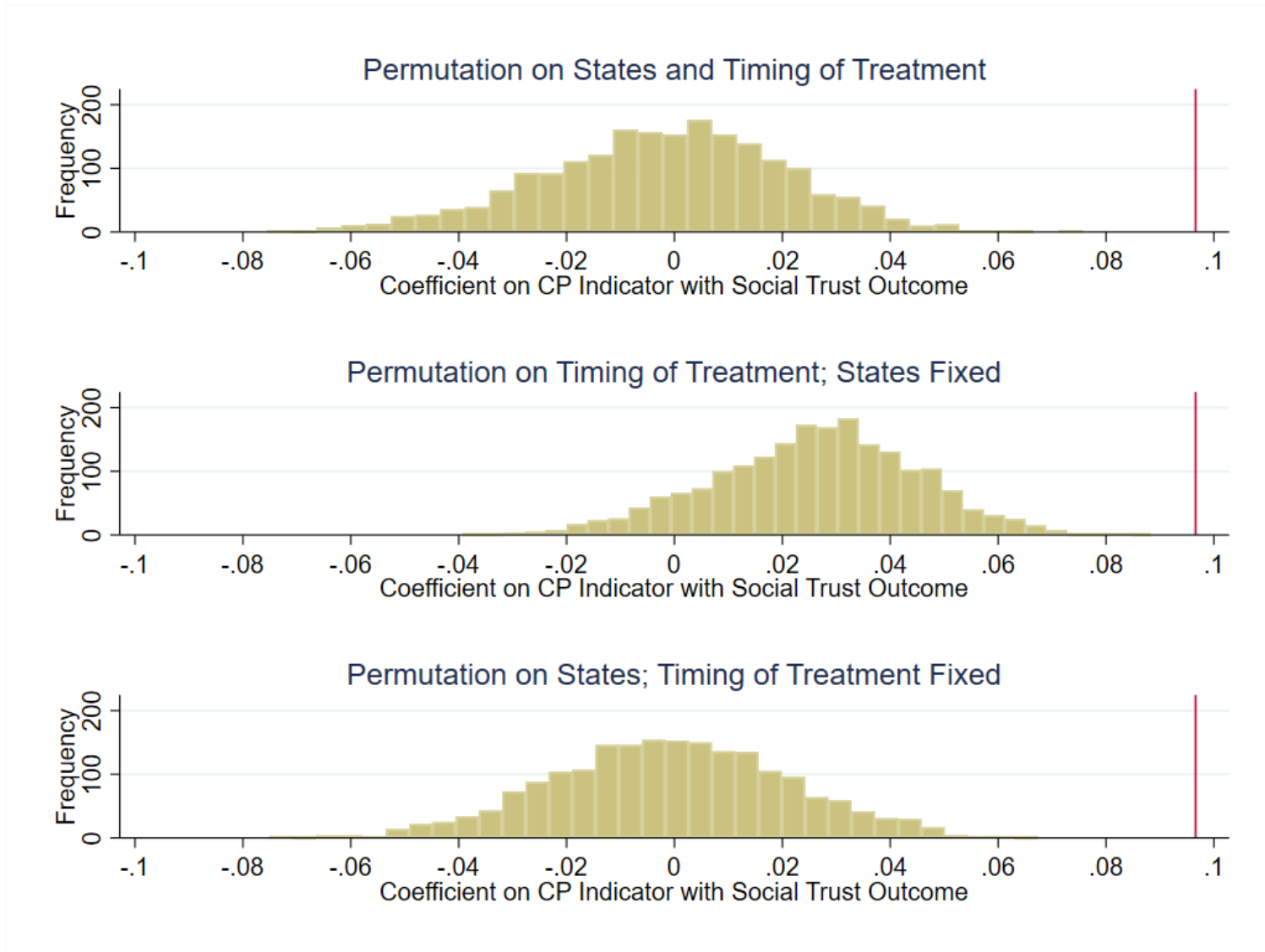
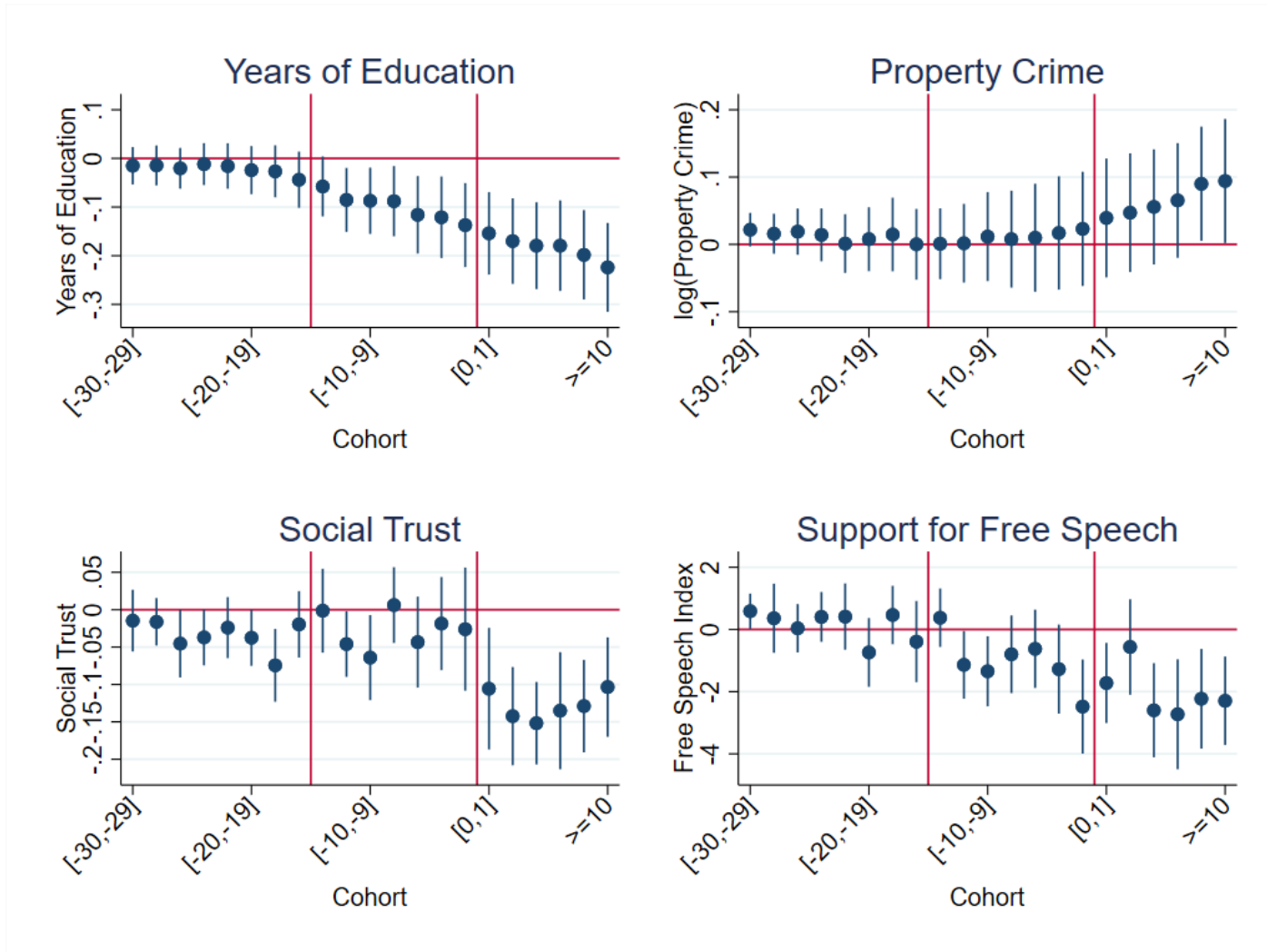
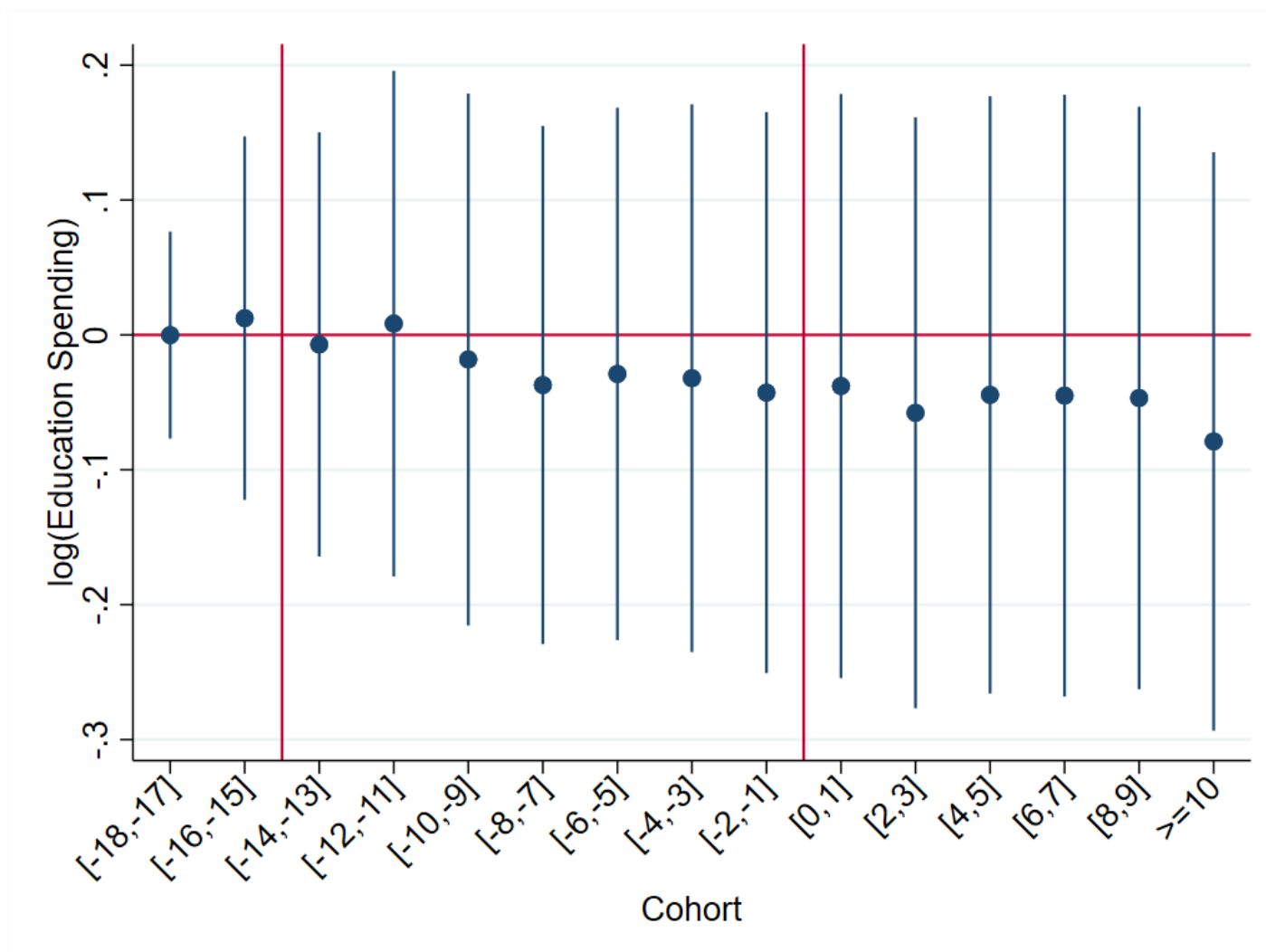


Figure MAINOUTCOMES: Effects on Social Trust – Event-Study Difference-in-Differences



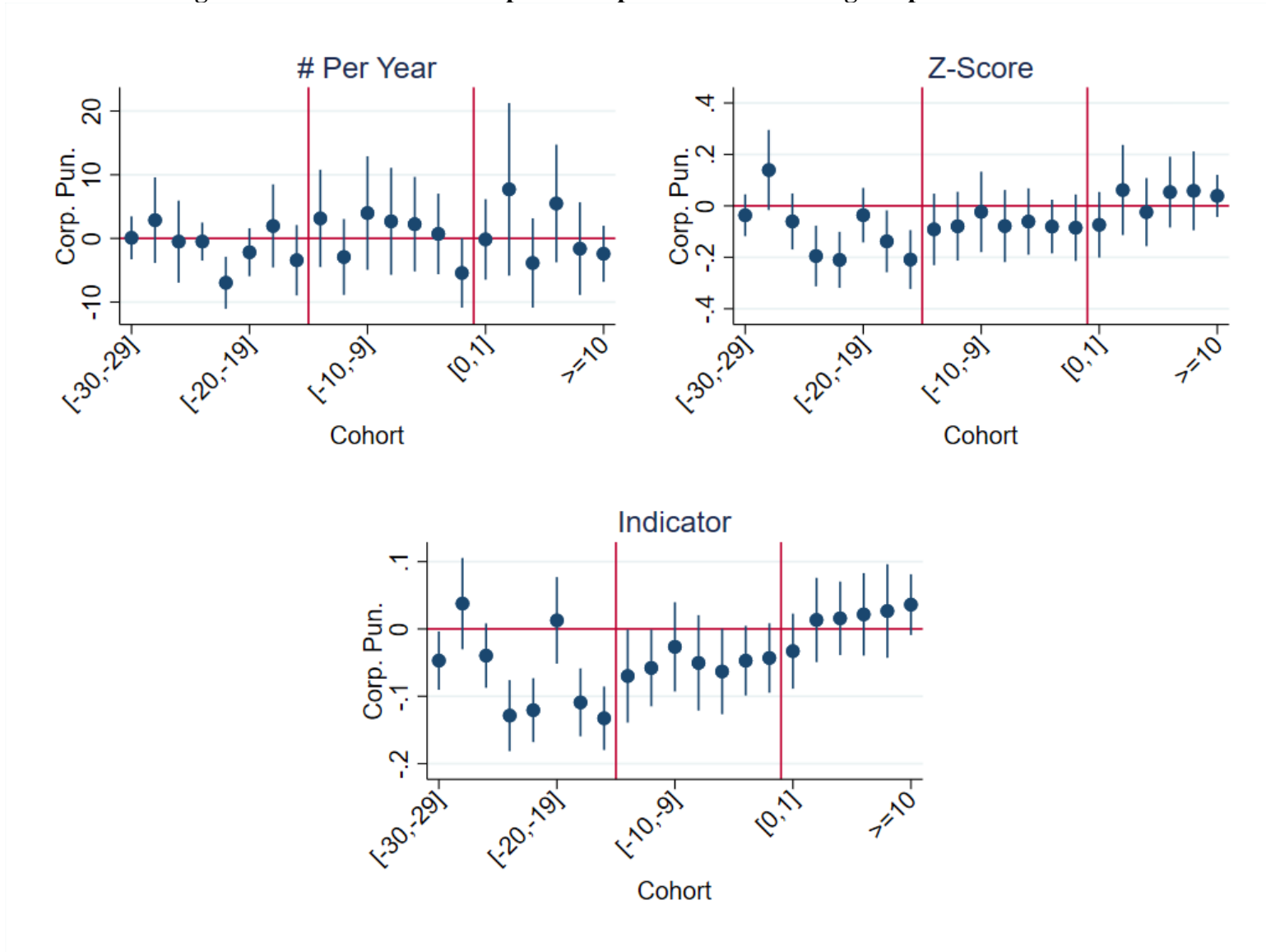
Note: Cohort 0 denotes the first cohort exposed to zero years of legal corporal punishment in schools. 90% confidence intervals are displayed.

Figure SCHSPEND: School Spending (Placebo test)



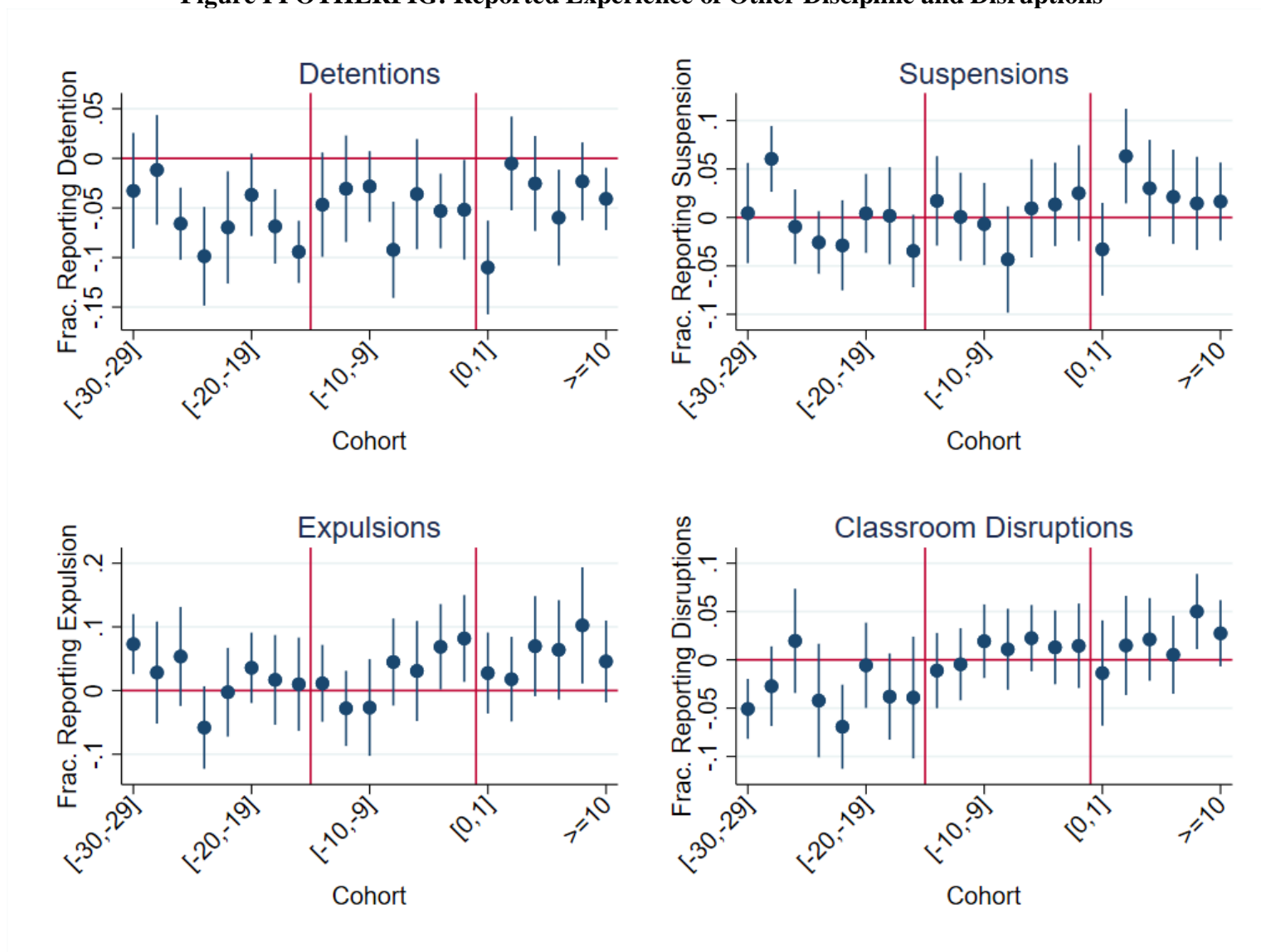
Note: Cohort 0 denotes the first cohort exposed to zero years of legal corporal punishment in schools. 90% confidence intervals are displayed.

Figure PPCORPPUNFIG: Reported Experience of Receiving Corporal Punishment



Note: Cohort 0 denotes the first cohort exposed to zero years of legal corporal punishment in schools. 90% confidence intervals are displayed.

Figure PPOTHERFIG: Reported Experience of Other Discipline and Disruptions



Note: Cohort 0 denotes the first cohort exposed to zero years of legal corporal punishment in schools. 90% confidence intervals are displayed.