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The Effects of a Statewide Ban on School Suspensions

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The Effects of a Statewide Ban on School Suspensions

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Abstract

This research uses the implementation of a school suspension ban in Maryland to test whether a top-down state-initiated ban on suspensions in early primary grades can influence school behavior regarding school discipline. Beginning in the fall of 2017, the State of Maryland banned the use of out-of-school suspensions for grades PK-2, unless a student posed an "imminent threat" to staff or students. This research investigates (1) what was the effect of the ban on discipline outcomes for students in both treated grades and upper elementary grades not subject to the ban? (2) did schools bypass the ban by coding more events as threatening or increasing the use of in-school suspensions? and (3) were there differential effects for students in groups that are historically suspended more often? Using a comparative interrupted time series strategy, we find that the ban is associated with a substantial reduction in, but not a total elimination of, out-of-school suspensions for targeted grades without substitution of in-school suspensions. Disproportionalities by race and other characteristics remain after the ban. Grades not subject to the ban experienced few effects, suggesting the ban did not trigger a schoolwide response that reduced exclusionary discipline.

JEL Classification: I21, J15, J18.
Keywords: school discipline, school exclusion, exclusionary discipline, discipline disparities, educational equity, suspensions.

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I. INTRODUCTION

Out-of-school suspension is an exclusionary discipline policy frequently employed in schools in the US. An estimated one-third of students are suspended at some point during their K-12 education (Shollenberger, 2015; Skiba et al., 2014; Fabelo et al., 2011). While widely accepted for many decades, suspension policies have come under scrutiny based on growing evidence of negative educational outcomes for students (Losen and Whitaker, 2017; Rumberger and Losen, 2017; Noltemeyer, Ward, and Mcloughlin, 2015; Balfanz, Byrnes, and Fox, 2015; Fabelo et al., 2011). Further criticism comes from mounting evidence that suspension is disproportionally used to discipline certain groups, including students with developmental disabilities, students with mental health conditions, Black and Latino students, and Black males in particular (Anderson and Ritter, 2020; Losen and Martínez, 2020; Steinberg and Lacoe, 2018; Anyon et al., 2014; Kinsler, 2011; Skiba et al., 2014, 2011, 2002).

In response to mounting evidence of both equity and efficacy issues, advocates and state policy makers have encouraged schools to develop and implement new discipline strategies that keep students in the classroom and focus on behavior improvement (Adukia, Feigenberg, and Momeni, 2023; Minow, 2022; Acosta et al., 2019). A small number of states have taken a harder line by severely limiting allowable use of out-of-school suspensions (which we refer to from this point forward simply as "suspensions"). This top-down approach has the advantage of standardizing discipline practices to eliminate a tool that is used disproportionately. However, a top-down policy may conflict with local needs and does not provide educators with the tools to implement alternative strategies. Curran (2019) identifies school discipline as a policy area where co-production by different actors and organizations in decentralized education systems is required, raising the question of whether top-down restrictions are compatible with the context, beliefs, and practices of teachers, administrators, and other actors.

While no states have fully banned suspensions, a growing number of states have been limiting allowable uses of exclusionary discipline. Partial bans might focus on restricted consequences for students in specific grade levels or for specific student behaviors. As of May 2021, at least 15 states and Washington, DC, have imposed limitations on the use of exclusionary discipline in schools, and at least 37 states and Washington, DC, promote nonpunitive alternatives to suspensions. Additionally, at least 22 states and Washington, DC, mandate the reporting and disaggregation of exclusionary data to highlight disparities (Leung-Gagné et al., 2022). School districts have also implemented local policy reforms to reduce the use of suspensions and emphasize less punitive disciplinary measures (Leung-Gagné et al., 2022; Steinberg and Lacoe, 2017, 2018). Yet, evidence of the effects of these reforms continues to be mixed (Cleveland, 2023; Payne, 2022; Craigie, 2022; Wang, 2022; Craig and Martin, 2023; Anderson, 2018; Baker-Smith, 2018; Hashim, Strunk, and Dhaliwal, 2018; O'Connor, 2015), often because school actors found ways to continue to use suspension despite the policy change.

In this study, we investigate the efficacy of the first statewide ban that is focused on suspensions in early primary grades. Beginning in Fall 2017, the Maryland legislature outlawed the use of out-of-school suspensions in grades PK-2. The Maryland policy serves as an excellent test case for top-down discipline policies for several reasons. First, the Maryland ban is very broad, applies to all public schools in the state, and compared to other state bans, allows only a narrow range of exceptions in cases of an imminent threat of violence. Second, while intended to reduce disproportionality by race, gender, and disability status, the ban is neutral regarding student characteristics. The Maryland context offers an opportunity to test whether states can eliminate inequities in suspensions simply by outlawing suspensions for all. Finally, the ban focuses on early primary grades where suspensions are least common, class sizes are smaller, and students are generally less defiant than older children (Leung-Gagné et al., 2022), but the expectation was that an early grade ban would have schoolwide impacts on all elementary students. Since PK-2 students are typically in schools with upper elementary grades, we are able to test whether there are schoolwide effects beyond the targeted grades. This offers an excellent test of the theory that a top-down suspension ban that targets primary grades can trigger schoolwide changes that will improve equity and benefit all students, without specifically addressing disproportionally or mandating specific schoolwide discipline reforms.

We follow the population of Maryland public elementary school students from 2014 to 2019 to test the effects of the 2017 ban. We isolate the direct effects of the ban over simultaneous efforts to change discipline policies schoolwide through a difference-in-difference approach that compares changes across both targeted grades and upper elementary grades where suspensions were still allowed. We also test for persistent effects on third graders as they exit the suspension ban and enter grades where suspension is allowed. Our results suggest that the state ban substantially reduced but did not fully eliminate suspensions in treated grades across all student subgroups and all types of offenses. While all student subgroups saw substantial declines in suspension frequencies and rates, disproportionalities by race, income, and gender remain, and disproportionalities by disability status increased. We find no evidence that schools engaged in strategic behavior to continue exclusionary discipline after the ban. Despite the large reduction in suspensions in K-2, we find little evidence of improved student attendance or changes in the use of suspensions in non-targeted grades.

II. POLICY CONTEXT

The State of Maryland PK-12 public education system serves a racially and economically diverse student body of approximately 900,000 students. Overall, student enrollment is 33% White, 33% Black, 22% Hispanic, 7% Asian, and 5% multi-race and 40% identified as "economically disadvantaged" (MSDE, 2023). Since 2004, the state has been working to reduce high rates of exclusionary discipline and disparities by race and disability status in discipline policies (Maryland Advisory Committee to the U.S. Commission on Civil Rights, 2019). This included publicizing data on racial disproportionalities by district, promoting the implementation of alternative practices in local districts, and funding pilot programs in positive behavior supports and restorative practices. Overall, these efforts had little or no impact on suspension rates and disparities (Maryland Advisory Committee to the U.S. Commission on Civil Rights, 2019). In 2017, the state legislature took the more definitive step of banning out-of-school suspensions for grades PK-2 beginning in the fall of 2017 (MSDE, 2018).

In July 2017 Maryland's General Assembly enacted a bill prohibiting a child enrolled in pre-kindergarten (PK), kindergarten (k), first, or second grade from being suspended or expelled from school, subject to exceptions. The bill permits the suspension of a PK-2 student for up to five school days per incident only in cases where the school administration, in consultation with a school psychologist or other mental health professional, determines there is an imminent threat of serious harm to other students or staff that cannot be mitigated through interventions and supports. This severely reduced the range of behavior that led to suspensions and raised the administrative burden of using suspensions by requiring additional expert consultation. Additionally, the bill outlines interventions and educational supports that must be provided to students in PK-2 who are suspended, as well as to other students in those grades who are disruptive or commit acts that would typically warrant suspension (MSDE, 2018).

We use the case of Maryland to study whether a broad state-initiated ban on suspensions can influence district and school behavior, including reducing disproportional use of exclusionary discipline, and whether a ban in early primary grades, where suspensions are infrequent, can have spillover effects in grades where suspensions are more common. To that end, we investigate (1) what was the effect of the ban on discipline outcomes for students in grades with and without the ban? (2) did schools bypass the ban by coding more events as threatening or increasing the use of in-school suspensions? and (3) did the ban, which does not target specific groups, reduce or eliminate disparities in the use of exclusionary discipline?

Data for this study come from the Maryland Longitudinal Data System Center (MLDSC), which includes the full population of Maryland public school students from 2008 to the present. We include in this study approximately 300,000 students per year who enrolled in grades K-5 at a Maryland public school. We follow school discipline outcomes from 2014-15 (3 years before the ban) through the 2018-19 school year (2 years after the ban). During this period, all students in grades K-2 were directly treated by the suspension ban beginning in fall 2017. Students in grades 4-5 faced no change in suspension policy but might have benefited from schoolwide changes in disciplinary approaches that were triggered by the ban. To separate the direct effects of the ban itself and the schoolwide effects of new approaches, we employ comparative interrupted time series and event study models that differentiate time trends and policy effects by grade level.

III. LITERATURE REVIEW

Exclusionary discipline remains an important component of discipline practices used in schools in the United States. The overall suspension rate in the U.S. increased from four percent in 1973 to seven percent by the 2009-10 school year and then decreased to five percent in 2017-18 (Leung-Gagné et al., 2022). Prior to the current period of reform, it was estimated that at least a third of students in the U.S. are suspended from school at some point between kindergarten and twelfth grade (Shollenberger, 2015; Skiba et al., 2014; Fabelo et al., 2011). The use of suspensions starts early in the educational system and increases across grades. In 2017-18, about two percent of students in elementary school were suspended, and this figure grows to seven percent among students in secondary school (Leung-Gagné et al., 2022). The reduction in suspension rates observed during the last decade was concentrated in secondary schools while changes in early grades were smaller and less consistent (Leung-Gagné et al., 2022).

Disparities in suspensions by race, disability status, and gender have persisted over time. Black students are disproportionately suspended (Leung-Gagné et al., 2022; Anderson and Ritter, 2020; Ayoub et al., 2019; Anyon et al., 2014; Skiba et al., 2014, 2011, 2002; Kinsler, 2011) with an estimated 12 percent of Black students receiving one or more out-of-school suspensions in 2017-2018, compared to the five percent national average (Leung-Gagné et al., 2022). Race disproportionality has increased with school integration (Chin, 2021) and state "zero-tolerance" policies (Curran, 2019, 2016). This has meant that Black students are more likely to be suspended and for more days relative to White peers even when the suspensions are the results of the same incidents (Barrett et al., 2023).

Other groups that face disproportional use of suspensions are students with disabilities, boys, and Black boys. Students with disabilities are suspended at higher rates than their nondisabled peers (Welsh and Little, 2018; Losen et al., 2014; Achilles, McLaughlin, and Croninger, 2007). In 2017-2018, about nine percent of students with disabilities were suspended, compared to four percent of students without disabilities (Leung-Gagné et al., 2022). Brobbey (2018) reported that students with learning disabilities accounted for 20 percent of all suspensions despite representing 11 percent of the population. Similarly, a study in New York City found that boys were involved in 61 percent of incidents subject to formal disciplinary response but represented half of the overall student population (Ayoub et al., 2019). Students with multiple risk factors also face a higher probability of suspension. For example, in 2017-18, more than 1 in 4 Black boys with disabilities in secondary schools were suspended (Leung-Gagné et al., 2022). This is a 27 percent suspension rate for this specific population compared to a five percent national rate in the same year.

President Obama's administration recognized the federal government's role in protecting students' civil rights and addressing educational inequities. Soon after taking office in 2009, his administration began focusing on reforming school discipline. In 2011, the U.S. Departments of Education and Justice launched the Support School Discipline Initiative. The goal was to reduce the use of exclusionary discipline in schools by developing consensus-based recommendations and investing in research and data collection. In 2014, the Department of Education issued a guidance package on civil rights and school discipline. The guidance provided information to support states, districts, and schools in their efforts to move away from zero-tolerance policies and toward research-based, restorative practices. Racial and other disparities in exclusionary discipline could lead to a federal investigation into whether a district had violated civil rights laws, regardless of whether the disparities were caused by discriminatory practices or by the disparate impacts of a neutral school discipline policy on students (Leung-Gagné et al., 2022).

Since then, a growing number of state and local reforms have emerged to limit the use of out-of-school suspensions. As of May 2021, at least 15 states and Washington, DC, have implemented restrictions on the use of exclusionary discipline in schools, and at least 37 states and Washington, DC, promote the use of nonpunitive alternatives to suspensions (Leung-Gagné et al., 2022). There is still, however, no consensus on a singular approach to implementation, and the results of various levels (e.g., state, local) and different policy designs (e.g., total ban, limited ban, alternative practices) remain mixed (Cleveland, 2023; Payne, 2022; Craigie, 2022; Wang, 2022; Craig and Martin, 2023; Anderson, 2018; Baker-Smith, 2018; Hashim, Strunk, and Dhaliwal, 2018; O'Connor, 2015).

In 2012, Rhode Island prohibited the use of out-of-school suspensions for attendance-specific infractions such as truancy and absenteeism. In 2016, the state further restricted suspensions for certain disruptive behaviors (e.g., insubordination, disorderly conduct, and obscene language) unless the student persistently threatens or harms others, repeatedly obstructs learning, and/or other corrective measures have failed (Craigie, 2022). The first reform reduced out-of-school suspensions for attendance-related infractions and decreased racial-ethnic disparities. However, the second reform did not impact either the out-of-school suspensions or racial-ethnic disparities (Craigie, 2022).

Similarly, in 2013, the Arkansas state legislature passed a bill prohibiting out-of-school suspensions as a consequence of truancy. In 2017, the state further banned out-of-school suspensions for students in grades K-5 for additional infractions (Anderson, 2018; Arkansas Act 1059, 2017). An evaluation of this top-down policy found that schools most likely targeted by the policy—those serving more minority students, with higher rates of truancy and out-of-school suspensions—were also the ones less likely to comply with the new regulations (Anderson, 2018).

Instead of imposing a ban on suspensions, Massachusetts sought to reduce suspensions indirectly by encouraging alternate strategies, mandating education resources for suspended students, and increasing reporting requirements regarding the reasons for suspensions. Recent evidence indicates that this 2014 policy change has led to significant reductions in reported incidents and disciplinary actions, especially benefiting students with disabilities, as well as Black, Hispanic, and low-income students (Cleveland, 2023).

Reforms are also occurring at the local level, with many districts imposing bans on suspensions for certain infractions or implementing alternative disciplinary practices to reduce the use of exclusionary discipline. By the 2015–16 school year, 23 of the nation's 100 largest school districts had enacted policy reforms to reduce suspensions and emphasize less punitive disciplinary measures (Steinberg and Lacoe, 2017, 2018). In 2012, New York City implemented targeted high school reforms that eliminated suspensions for low-level infractions among ninth graders. Research indicates a notable decline in first-time suspensions following this policy change. However, the risk of experiencing a second or subsequent suspension rose. This pattern persisted across racial and gender demographics, showing a slight reduction in disproportionality for first suspensions but an increase in disproportionality for subsequent suspensions (Baker-Smith, 2018).

Philadelphia reformed its discipline policy to limit suspensions for nonviolent student misconduct and grant principals greater discretion in responding to more serious occurrences of student misconduct. In 2012, Philadelphia revised its code of conduct and students were no longer to be removed from school for failing to follow classroom rules or using profane or obscene language or gestures; instead, the maximum allowable punishment changed from out-of-school suspension for 1–3 days to in-school intervention. Other changes included in-school suspensions for offenses that were out-of-school before and out-of-school suspensions for offenses that were expulsion before (Lacoe and Steinberg, 2019, 2018). An evaluation of this policy has shown a modest decline in suspensions for nonviolent infractions in the year of reform; however, total suspensions remained unchanged while serious incidents of student misconduct increased (Lacoe and Steinberg, 2019, 2018).

The Los Angeles Unified School District implemented a suspension ban for willful defiance in the 2011-12 school year, followed by the introduction of Restorative Justice Practices in 2014-15. Results reported by Hashim, Strunk, and Dhaliwal (2018) reveal significant declines in suspension rates in the years following the ban compared to preceding years, accompanied by a narrowing of suspension disparities between students frequently disciplined and those with fewer disciplinary incidents. Moreover, the district's targeted identification of schools requiring ongoing reform efforts, coupled with the provision of restorative justice training, contributed to additional reductions in suspensions. Despite these advancements, their results indicate persistent suspension gaps between Black and non-Black students, as well as between students in special education (SPED) and those not in SPED programs.

Wang (2022) examines the impact of the ban on issuing out-of-school suspensions for willful defiance in four California school districts (San Francisco Unified School District, Pasadena Unified School District, Azusa Unified School District, and Oakland Unified School District). Wang's study reveals that while the ban significantly reduced willful defiance out-of-school suspension rates by approximately 69 percent, it did not lead to an overall reduction in out-of-school suspension rates; rather, schools changed the stated reasons for issuing suspensions. Importantly, the study highlights that Black students were disproportionately affected by this policy shift. Supplemental analysis using data from the Youth Risk Behavior Surveillance System indicates that the increase in out-of-school suspension cannot be attributed to an increase in student infractions. Wang concludes that these findings suggest the ban on willful defiance suspensions failed to effectively address biases against Black students in California schools.

Despite an ostensible agreement among researchers and educators that out-of-school suspensions do not work as a strategy to improve student behavior and outcomes, students are still frequently suspended, and disproportionality persists (Koon et al., 2021). A nationally representative survey of principals shows that the puzzle is far from being solved. Most principals do not believe that suspensions and expulsions serve the intended purposes, but many continue to use these practices—especially with students of color—and appear reluctant to give them up entirely (Perera and Diliberti, 2023). Perera and Diliberti (2023) hypothesize, based on the survey of principals, that schools' reluctance to stop using exclusionary discipline could be a response to, (1) zero-tolerance policies that limit principals' options; (2) insufficiency of resources to adopt non-punitive alternatives to suspensions, particularly among high-suspending schools; and (3) a reluctance to implement new strategies even when resources exist.

For instance, in 2015, the Miami-Dade District in Florida implemented a suspension ban aimed at reducing out-of-school suspensions. However, this ban has since been lifted. During its enforcement, some administrators circumvented the policy by sending students home for misbehavior without officially recording them as suspended. According to reports, this practice led to a significant increase in unexplained absences. Despite the formal directive to end out-of-school suspensions, "off-the-books" suspensions persisted, undermining the intended impact of the policy reform (Payne, 2022; O'Connor, 2015).

Overall, these results suggest that suspension bans that target a limited set of infractions reduce suspensions in these targeted areas, while also leading to unintended shifts in school behavior that limit policy impacts. Using the state-wide policy in Maryland, we explore whether a broader statewide ban targeted in early primary grades has a stronger influence on schools' behavior regarding exclusionary discipline. The Maryland case allows us to investigate new behavioral responses of school administrators. First, we test whether a broad ban with very narrow exceptions still leads to strategic behavior. Second, we test the effects of the ban for grades directly affected and those not included in the ban. This provides new information on the impacts of a grade-level specific ban on schoolwide behavior. Finally, we estimate the unique effects on historical disparities in the application of exclusionary discipline by race, income, gender, and disability status.

V. EMPIRICAL STRATEGY

Our empirical strategy exploits both the timing and design of the Maryland suspension ban. Beginning in the 2017-18 school year, Maryland law prohibits a child enrolled in PK-2 in a Maryland public school from being suspended or expelled from school in most circumstances.

Under the ban, students in PK-2 can be suspended for up to five school days per incident only in cases of imminent threat of serious harm to other students or staff that cannot be reduced or eliminated through interventions and supports (MSDE, 2018)[1]. Threats of harm are determined by administrators and must be confirmed by a school psychologist before an out-of-school suspension can occur. Schools can employ in-school suspension as an alternative consequence in all grades, and students in grades 3-12 can still be suspended for lesser infractions such as disobedience, disruption, or academic dishonesty.

In Maryland, elementary schools typically serve either K-5 or K-8, so under the ban, most elementary school buildings are subject to two distinct policies, the ban for grades K-2 and no ban for grades 3 and up. As part of the suspension ban legislation, schools were directed to implement less punitive discipline strategies overall and to address socioeconomic and racial disparities in discipline practices. In particular, the bill established that the school system, to the best of its ability, shall remedy the impact of a student's behavior on school climate through appropriate intervention methods including restorative practices.[2] However, the legislation did not include funding for implementation of new discipline strategies. The Maryland State Department of Education (MSDE) continued pre-existing programs to support pilot programs, professional development, and other efforts to reform punitive discipline strategies (Maryland Advisory Committee to the U.S. Commission on Civil Rights, 2019).

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[1] A student can be determined to pose an "imminent threat" to staff or students for actions such as fighting, threats of violence, or bringing weapons or drugs to school. Maryland law specifies that interventions and supports that must be provided to students who are suspended during their time out of school for suspension (MSDE, 2018) Intervention and support provided includes positive behavior intervention and supports, a behavior intervention plan, a referral to a student support team, a referral to an individualized education program team, and/or a referral for appropriate community-based services.

[2] According to the bill, "restorative practices" means practices conducted in a whole-school ethos or culture that supports peacemaking and solves conflict by building a community and addressing harm in a school setting and that: (a) Are conducted by trained staff; (b) Focus on repairing the harm to the community through dialogue that emphasizes individual accountability; and (c) Help build a sense of belonging, safety, and social responsibility in the school community. (MSDE, 2018)

Our empirical objective is to measure the effects of the suspension ban on student discipline outcomes and access to classroom instruction. We consider multiple ways that Maryland's implementation could affect schools and students in both treated and untreated grades. The legally mandated response of schools was to stop out-of-school suspensions for students prior to third grade, as long as the student did not pose a threat of harm to others. The larger objective was for schools to take steps to teach and reward positive behavior in all grades, and those efforts might have schoolwide effects above the effects of the ban. We also consider unintended consequences that might be triggered by the ban.

To consider the holistic effects of the ban on schools and students, we hypothesize potential effects for students based on the timing of their initial exposure to the policy change. Students in grades PK-2 when the ban took effect were directly affected, with the intended effect of nearly eliminating suspensions at these grade levels. A direct effect of the ban would be reflected in administrators applying alternative consequences for most disruptive behaviors that would have triggered a suspension prior to the ban. Suspensions could also be reduced indirectly in PK-2 if the ban triggered new classroom approaches that prevented disruptive behaviors from occurring in the first place. Finally, exceptions allowed under the ban might trigger unintended effects. To maintain an exclusionary approach to suspensions in affected grades, schools might apply violent codes to behavior that would have been coded differently prior to the ban or substitute in-school suspensions in cases where out-of-school suspension would be applied before the ban.

While upper grades were not subject to the suspension ban, we cannot assume that these students are unaffected. Students in upper grades at schools with PK-2 might also experience the benefits of changes in administrator responses to nonviolent offenses and implementation of alternative discipline strategies. Since suspension decisions ultimately fall on administrators who supervise multiple grades, it is possible that the ban will have effects on how infractions are coded and how teachers are trained to respond to student behavior in all grades. It is likely that discipline reforms implemented to mitigate the need for PK-2 suspensions would be implemented more broadly to maintain consistency and continuity within a school. Thus, any schoolwide responses could trigger indirect effects that result in reduced use of suspensions in grades not subject to the ban.

Finally, if the absence of suspensions in early grades leads to meaningful changes in student behavior, students who are subject to the ban might experience follow-up effects as they age out of treated grades. For example, if schools respond by emphasizing socio-emotional skills in treated grades, treated students might have better behavior when they reach third grade than they would have had in the absence of the ban.

A longitudinal study that follows students from kindergarten through untreated grades would be ideal to fully observe direct, indirect, and follow-up effects, but we are only able to study school discipline outcomes through the 2018-2019 school year. In the spring of 2020, all Maryland public schools shifted to online learning as part of Maryland's statewide response to the COVID-19 pandemic. The timing of returns to in-person learning is not documented at the school or student level, making it impossible to accurately track suspensions in the 2019-20 and 2020-21 school years. Figure 1 illustrates how students in different grade cohorts experienced the suspension ban during pre-COVID years. Since Maryland does not have universal PK and access to PK is often contingent on socioeconomic or ability measures, we focus our empirical analysis on grades K-5. In the observable post-ban years of 2017-18 and 2018-19, all students in grades K-2 were directly treated by the suspension ban. Students in grades 4-5 in 2017-18 and 2018-19 and students in grade 3 in 2017-18 never directly experienced the ban. Students entering grade 3 in 2018-19 are a unique cohort in our data. These students experienced the ban in second grade in 2017-18 and then aged out in 2018-19. For this unique cohort, we can cleanly estimate follow-up effects.

Estimating Direct and Indirect Effects

If we consider the mechanisms described above, students in both treated and untreated grades might experience schoolwide efforts to improve behavior. Any difference in outcomes before and after implementation for grades 4-5 would point to schoolwide effects only. Any differences in outcomes before and after implementation for grades K-2 would capture both schoolwide effects and direct effects of the ban itself. The difference-in-differences across treated and untreated grade levels provides the best estimate of the unique effect of banning suspensions from the coincidental effects of schoolwide reforms. Third graders might have experienced follow-up effects in year two only. Because the ban is experienced differently across grades, our empirical models estimate unique pre- and post-ban outcomes by grade level. To estimate direct and indirect effects of the ban, we employ a comparative interrupted time series model (CITS) for outcome *Y* for student *i* who is enrolled at school *j* in grade *g* at time *t*:

$$Y_{igjt} = \alpha + \sum_{g=\{k,1,2,3,4]} \left(\beta_g \cdot \ grade_{igt}\right) + \gamma \cdot post_t + \sum_{g=\{k,1,2,34\}} (\delta_g \cdot \ grade_{igt} \cdot post_t) + \vartheta_1 t + \vartheta_2(t \cdot kto2) + \theta_j + \varepsilon_{igjt} \quad (1)$$

Here, unique β coefficients for grades K-4 indicate pre-ban differences compared to fifth grade, which is the omitted reference group. γ measures the overall post-ban difference, and a significant γ would indicate that a school-wide effect occurred. δ coefficients for grades K-4 measure the difference in the post-policy change for each grade, compared to grade 5 (i.e., the difference in differences). For treated grades, δ measures the direct effect of the ban over any schoolwide effects that are captured in γ. For untreated grades, δ provides a comparison of schoolwide effects across grades that were not affected by the ban. Thus, significant δ's for treated grades but not untreated grades would provide evidence of direct effects of the ban. Significant δ's in untreated grades would indicate that indirect effects vary by grade. To isolate policy effects from other statewide influences, equation (1) also includes two time trends to reflect the pre-ban annual growth in use of suspensions, where ϑ_1 is the slope of the trendline for untreated grades, and ϑ_2 is a differential slope for treated grades. Because school discipline is implemented at the school level, we also include θ_j in equation (1) to reflect schoolwide differences in outcomes that might be influenced by administrator preferences, local socioeconomics conditions, or other school-level characteristics. We operationalize θ_j with either clustered standard errors or school fixed effects, and ε is the remaining random error.

Estimating Follow-up Effects

As noted above, some grades might also have follow-up effects in year two, and grade 3 is unique for having one cohort that aged out of the suspension ban during the study period. Our second model is an event study with unique indicators for the two post-ban years. We continue to control for differential trends in treated and untreated grades but also add interaction terms for post years and each grade level. The model tests specifically for follow-up effects by estimating unique grade-level effects in year 1, where there would be no follow-up effects, and year 2, where follow-up effects might emerge. This allows us to directly estimate the difference in effects of the ban for third graders who did and did not experience the ban in second grade.

$$Y_{igjt} = \alpha + \sum_{g=\{k,1,2,3,4\}}\left(\beta_{1g} \cdot\ grade_{igt}\right) + \gamma \cdot post_t + \sum_{g=\{k,1,2,3,4\}}\left(\delta_{1g} \cdot\ grade_{igt} \cdot post\ year1_t\right)\ + \sum_{g=\{k,1,2,3,4\}}\left(\delta_{2g} \cdot\ grade_{igt} \cdot post\ year2_t\right) + \vartheta_1 t + \vartheta_2(t \cdot kto2) + \theta_j + \varepsilon_{igjt} \quad (2)$$

Illustrating Impacts by Grade Level

Post-estimation, we use the coefficients from eq (1) or eq (2) to generate fitted y-values for each grade in post-ban years if the ban had not occurred (i.e., *post=0*). This provides the best counterfactual for comparison to the actual outcomes, as it allows us to include continuation of the pre-ban time trends in our expectations of where post-ban values would have been if the ban had not occurred. For example, if we set *t=0* in the first post-ban year, the estimated counterfactual for post-ban year 1 using eq (1) is simply:

$$\hat{Y}_{without\ ban} = \alpha + \beta_{1g} \quad (3)$$

In our results, we illustrate the effect of the ban by grade level by graphing these expected counterfactuals (with robust confidence intervals) against the actual values.

Estimating Effects on Disproportional Use of Exclusionary Discipline

Our final research question asks whether the ban decreased disproportionality in exposure to exclusionary discipline by income, race, gender, and disability status. To reduce disproportionality, the policy must not just have similar effects on a subgroup, but the subgroup effect must be larger than the overall effect. Rather than including control variables that could mask demographic changes over time, we generate individual estimates by subgroup, with a focus on groups that faced disproportionality in Maryland prior to the ban. Because there are very few suspensions for some subgroups, we use a variation of eq (1) that replaces grade-level indicators with an aggregate indicator for all treated grades. We use these subgroups estimates to output expected outcomes in post-ban years if the ban had not been enacted and use this as the counterfactual to compare to actual post-ban rates. From there, we estimate the effects of the policy on disproportionality by calculating a risk ratio that compares across groups as follows:

$$Disproportionality_g = \frac{suspension\ rate_{group\ member=1}}{suspension\ rate_{group\ member=0}} \quad (4)$$

where *g* indicates key subgroups. A ratio of 1.0 would indicate that subgroup members and non-members were suspended at an equal rate (i.e., no disproportionality), while a ratio greater than one would indicate that subgroup members are suspended more often (e.g., a ratio of 4.0 means that subgroup members are four times more likely to be suspended than non-members). Evidence of reduced disproportionality after the ban would require the actual risk ratio to be substantively closer to equality than our estimated counterfactual if the ban had not occurred.

VI. DATA

Data for this study come from the Maryland Longitudinal Data System Center (MLDSC), which houses mergeable administrative data sets from education, labor, and other state departments for use in educational research. Because pre-k is not universal in Maryland and seats are often selected based on student attributes, our study population includes all students enrolled in grades K-5 at a Maryland public elementary school from 2014-15 to 2018-19, approximately 300,000 total students[3].

The MLDSC discipline files are recorded at the student x event level and include whether the student received a typical suspension or an in-school alternative.[4] Typical suspensions are served out-of-school from a half-day to multiple days, during which the student is not allowed to enter the school building. Throughout our discussion, we refer to out-of-school suspensions simply as "suspensions." In-school suspensions remove the student from the classroom but are served elsewhere in the school building, such as a detention room or study hall. Events that led to milder responses, such as office referrals or warnings, are not recorded in Maryland data, so our discipline outcomes relate exclusively to the school's use of exclusionary discipline, which we measure in several ways: 1) a student's probability of ever being suspended during the school year, 2) the number of suspensions per student, 3) total days of suspension per student, and 4) the number of in-school suspensions.

[3] In Maryland, middle school begins in grade 6.
[4] A third option of expulsions was used in a very small number of cases in grade K-5. These students (<10 over the five-year period studied) were excluded from the study.

Discipline data also include an infraction code that identifies the type of behavior that led to each recorded event. To test whether infraction coding practices changed in response to the exceptions allowed in Maryland's suspension ban, we collapsed several dozen infraction codes into an aggregate category of codes used to justify a K-2 suspension under the ban. This includes several categories of threats or acts of violence against students or adults. The policy allowed suspensions for these infractions if a student was deemed to pose an "imminent threat" to others.[5] We refer to this second category as "violent codes," and the increased use of these codes after the ban is a potential unintended consequence of including code-based exceptions in the law. Infraction coding is subjective under the discretion of administrators who record the data at the school level. Behavior coded as "willful disobedience" by one individual might be coded as "violent threat" by another. Evidence suggests that differences in coding occur both across and within schools, often in ways that impose racial and gender bias (Barrett et al., 2023). We use infraction codes cautiously to draw conclusions only about changes in the use of codes rather than changes in student behavior. We cannot directly observe student behavior changes in the data, nor can we identify individuals who were responsible for coding decisions.

As a final outcome measure, we estimate the effects of the ban on total days of school attended to see if the ban increased students' time in the classroom. This might happen directly for students who are in class instead of suspended. Indirect effects on students who would not have been suspended regardless of the ban are ambiguous. School discipline reforms that improve the classroom environment and reduce student stressors overall might increase attendance, while the

[5] Infractions were coded with guidance from MSDE's Division of Student Support, Academic Enrichment & Educational Policy and reflect how school leaders were advised on implementing the suspension ban.

presence of badly-behaved students in the classroom could lead some students to stay home more often.

To investigate disproportionality, we used indicators from the MLDSC to identify students who are historically at higher risk of exclusionary discipline due to demographic characteristics or challenging circumstances. Potential risk factors included in the data include race, gender, economic disadvantage, special education status due to disabilities, homelessness, migrant status, and participation in foster care.

VII. RESULTS

Changes in Suspension Use over Time

As descriptive evidence of the effects of the ban, Figure 2 illustrates the number of students suspended over time by grade level. Values on the x-axis indicate spring for each academic year. After trending upward before the ban, we see a sudden 58% drop in suspensions in K-2 (solid line) from 3,318 in 2016-17 to 1,409 in 2017-18. In untreated grades 3-5 (dotted line), we see a slowing of the pre-ban upward trend but a decrease of less than 1% from 5,545 suspensions in 2016-17 to 5,499 in 2017-18. Figure 3 shows the same trend displayed as the probability of suspension for students in treated (solid line) and untreated (dotted line) grades. A child's probability of suspension in treated grades increased from 1.4% to 1.9% from 2015 to 2017, and then declined to less than 1% after the ban. In untreated grades, the rate remained steady at 3.1% from 2017 into the post-ban years.

Table 1 provides descriptive statistics from the final pre-ban year (2016-17) for outcomes, demographics, and contextual student risk factors for grade K-2 (columns 1-3) and 3-5 (columns 4-6), including the full population of Maryland public school students. Columns 1 and 4 include all students, columns 2-3 and columns 4-5 show disaggregated means comparing students who were never suspended to those who were suspended. In that final pre-ban year, 1.1% of K-2 students and 2.3% of grades 3-5 students were suspended one or more times for an average of 3 days per year. K-2 and 3-5 students are demographically similar, but K-2 students are slightly more likely to be identified as English learners (EL) and slightly less likely to be assigned into special education (SPED).

We observe substantial differences in demographics of students who were and were not suspended prior to the ban. Groups that are overrepresented in the suspended group include Black students, males, economically disadvantaged students, SPED students, and students with home risk factors such as foster care or homelessness. We note that some subgroups that might be disproportionately suspended in other settings are under-represented among suspended students in Maryland. These include Hispanic students, multi-race students, and English learners. Our subgroup analyses below will focus on four groups with observed inequalities in Maryland: Black students, males, economically disadvantaged students, and SPED students. Students with home risk factors are also disproportionately suspended but the number of students in this group is too small to power a disaggregated analysis.

Figure 4 illustrates the probability of suspensions over time for students in these four subgroups. All groups have higher rates of suspensions than the statewide rates, but all groups also see substantial declines after the ban in treated grades. Figure 5 presents this data as risk ratio of subgroup members compared to non-subgroup members. Subgroup members are three to six times more likely to be suspended than non-members, depending on the group and grade level. The trend graphs show no clear effects of the ban on disproportionality. A flat trend for economically disadvantaged students in treated grades starts to decline after the ban, but this matches a negative pre-trend for untreated grades. Male and SPED students see an increase in disproportionality after the ban in treated grades only, and disproportionality for Black students appears to continue a decline that started prior to the ban.

Direct and Indirect Effects of the Ban

For regression analysis, we make two restrictions in the analytic sample from the full census of students described above. Because we are relying on time-trends in policies implemented at the school level, we exclude 214 elementary schools (23%) that never suspended K-2 students in the pre-ban period[6]. Practically, schools should be considered "untreated" by the ban if their internal practice already precluded suspensions for K-2 students. Mathematically, the exclusion of these schools avoids downward bias in estimates due to the fact that suspensions levels cannot fall below zero. Second, because we are attempting in part to measure schoolwide responses and to include school fixed effects, we restrict our analytic sample to students who attended only one school per year and thus were only subject to one school's discipline policies and practices. This exclusion applies to only 0.3% of K-5 students statewide[7].

Table 2 reports CITS regression estimates for the six outcomes described above at the student level: 1) probability of ever being suspended during a school year, 2) number of suspensions, 3) days of suspension, 4) number of suspensions coded as threats or acts of violence, 5) number of in-school suspensions, and 6) days attended. The first three outcomes measure the effects of the ban on the use of suspension overall. As depicted in Figure 2, we confirm a positive and significant time trend in these suspension outcomes in untreated grades, with a significantly smaller (but still

[6] In 2017, 19.3% of students in grades K-2 and 22.9% in grades 3-5 attended schools that never suspended K-2 students in the pre-ban period. Estimates including all schools are provided in the Appendix.
[7] Approximately 300,000 students were enrolled in K-5 each year. The rate of within-year school transfer overall is <0.3%. Students who were suspended during the year had a higher rate of transfer at 0.7%, and transfer and suspensions are potentially related in many cases (in either direction). This omission rule leads to potentially underestimating effects of the ban by omitting students who are more frequently suspended. However, replications of models without school fixed effects that include all students produced identical results to what is presented here (available on request), suggesting that the omission does not introduce bias.

positive) trend for treated grades. Prior to the ban, kindergarteners are the least likely to experience suspensions with incremental increases up to grade 5. The overall post-ban indicators are negative but not statistically significant, suggesting that the ban did not significantly reduce suspensions use in untreated 5th grade. Post x grade indicators for untreated grades 3 and 4 show that suspensions probability was slightly reduced in 4th grade, and all three suspension outcomes were reduced in 3rd grade, relative to 5th. We see substantially larger, negative coefficients for post x grade indicators for all three treated grades (K-2) that are significant for all three grades and suspension outcomes.

Figure 6 illustrates the magnitude of estimated effects of the ban for each grade level, showing the actual value and the estimated counterfactual derived from eq (1). The counterfactual is illustrated with a 95% confidence interval based on robust standard errors clustered at the school level[8]. We see that the probability, number, and duration of suspensions declined substantially and significantly in all three treated grades. In second grade, for example, the number of actual suspension events per 100 students was 1.2 compared to an expectation of 3.0 (60% lower than expected), and suspension days per 100 students were 2.1 compared to an expectation of 5.9 (64% lower than expected). Smaller declines in 4th and 5th grade do not vary much from the counterfactuals. If any schoolwide effects occurred, they are seen only in 3rd grade where the number and duration of suspensions are significantly lower than the expectation.

The next two outcomes in Table 2 and Figure 6 test for perverse effects of the suspension ban in terms of increased use of violent codes to facilitate out-of-school suspensions during the ban (column 4) or increased use of in-school suspension (column 5). Instead of increases, we see substantial and significant decreases in the use of violent codes in treated grades only. This suggests that schools reduced suspensions of both allowable and banned infraction codes. In-school

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[8] The actual value is based on all students in the regression sample. Confidence intervals around these values as estimation of the full population means are too small to illustrate.

suspension was rare prior to the ban and no more likely to occur after the ban in treated or untreated grades. Thus, we find no evidence overall of behavior changes among school staff to produce these perverse effects.

Finally, we look at the ban's effects on days of schools attended (column 6). We find no evidence that the ban contributed to more time in school on average. On the contrary, days attended was slightly lower than the expectation in all grades, with the biggest decline in second grade, which was subject to the ban. All estimated differences amount to less than one day of learning, so we find that the suspension ban did not meaningfully impact the average student's time spent in the classroom either positively or negatively.

As robustness checks for these results, we include additional specifications in Appendices. Appendix 1 replicates the results in Table 2 for the full sample of Maryland public schools. While 23% of these schools did not suspend any K-2 students prior to the ban, there might still be indirect effects in upper grades. Appendix 2 replicates Table 2 with school fixed effects in addition to clustered standard errors. This addition transforms coefficients to estimates of within-school effects of the ban. Both robustness checks report similar coefficients and lead to comparable conclusions as the core results. Coefficients are predictably smaller in the full school sample, where the average effect includes a null effect at schools that never used suspensions prior to the ban.

Event Study

Our second model estimates unique policy effects by year and grade level. Coefficients are displayed in Table 3. We use model (2) first to observe if immediate effects persisted into year 2, and second, to see if any follow-up effects occur in grade 3 for students who experienced the ban in grade 2. Figure 7 illustrates the actual trend over time, compared to the counterfactual expectation for two post-ban years for our three suspension measures and days attended. Estimated policy effects on suspension probability, number, and days, are similar in years 1 and 2 for treated grades. Smaller reductions in suspension outcomes do emerge for grade 3 in year 2, but since this also occurs in grade 4 and sometimes even grade 5, we cannot attribute it solely to follow-up effects. It is more plausible that indirect effects of changing classroom practices take more than one year to influence suspension outcomes in grades that are outside the ban. We also display event study results for days attended. Here, regression coefficients show no effect in year 1 for grade 3, but a positive, significant effect in year 2 that does not appear for other grades. The illustration in Figure 7, however, shows that when combined with other relevant coefficients, all grades have slightly lower attendance than predicted in year 2, and there was no net gain for grade 3 students who had aged out of experiencing the ban. Overall, the event study results suggest that year 1 effects were sustained in year 2 in treated grades, but there are no follow-up effects for the cohort that aged out of the ban within the short period that we are able to study. Event study results including all schools and with school fixed effects are provided in Appendix 3 and 4, respectively, with similar coefficients to Table 3.

Effects on Frequently Suspended Subgroups

Next, we estimate equation (1) for subgroups that are disproportionately suspended in Maryland within schools that used K-2 suspensions prior to the ban. Coefficients are displayed in Table 4 and actual and expected values are displayed in Figure 8. For all four subgroups (Black, male, economic disadvantage, and SPED), we find that the suspension ban significantly and substantially reduced the probability, number, and duration of suspensions in treated grades, with no increases in violent codes or in-school suspensions. In many cases, the absolute magnitude of reductions is larger within subgroups. For example, 2nd graders identified as SPED experienced 5 days of suspension per 100 students in 2017-18, compared to an expectation of over 15 days (a 68%

reduction). Black, male, and economically disadvantaged students have no significant effects in untreated 4th or 5th grade. We do see a small reduction in the number of suspensions in 3rd grade for Black students, and SPED students have lower-than-expected suspension outcomes in both 3rd and 4th grade.

To end disproportionality, the ban would need to both reduce suspensions in high-risk subgroups and have larger relative effects within these subgroups. To assess the effects on disproportionality, we constructed counterfactual risk ratios based on a simplified version of eq (1) that includes a single indicator for treated grades (K-2) and an interaction between treated x post-ban period in place of grade-specific indicators. The counterfactual, in this case, is the expected level of disproportionality in the first post-ban year, had the ban not been implemented. We derive this expectation by estimating suspension outcomes separately for subgroup members and non-subgroup members, outputting an expected value for each group, and calculating disproportionality as the ratio of the in-group expected value to the out-of-group expected value. These results are illustrated in Figure 9, in which disproportionality is based on actual values in 2017-18, compared to the estimated counterfactuals for Black vs. White students, male vs. female students, FRPL vs. non-FRPL students, and SPED vs. non-SPED students in treated grades. For the estimates, we include all elementary schools in the state, and estimate risk ratios for the probability and number of suspensions, as well as potential unintended outcomes of violent coding and in-school suspensions.

The relative effect of the ban is not consistent across subgroups. Actual disproportionality is at or near the expectation for Black students for suspension outcomes and higher for in-school suspensions relative to White students. Males also show no change in disproportionality compared to females, except here the disproportionality for in-school suspensions declined. Since in-school suspensions were very rare for both groups and changed little, these changes in ratios do not reflect profound changes for students. For economically disadvantaged students, there are small reductions in disproportionality across all four outcomes, but for SPED students, there are increases across all four outcomes. For example, SPED students went from an expectation of 5 times as many suspensions per 100 students as non-SPED students to 6 times as many with the ban, while that number fell from an expectation of 3.3 times as many suspensions for economically disadvantaged students to 2.9.

It is unclear why the Maryland ban would have impacted students living in poverty more equitably than SPED students. SPED students cannot legally be suspended for behavior related to identified disabilities, and the policy requires that all K-2 suspensions be approved by a school psychologist or other professional who is familiar with the child's disabilities and accommodations. SPED students did experience considerable reductions after the ban, including in the use of violent coding. The increase in disproportionality suggests that this is an area where a blanket ban requires more specific guidance and training to avoid an unfair perception of the disabled as needing to be separated from others, as well as additional training for teachers to prevent and address potentially violent behavior within this subgroup. Overall, mixed results for changes in disproportionality suggest that a blanket ban on suspensions is not sufficient to erase inequity in the use of suspensions, as long as some suspensions are allowed to continue.

VIII. DISCUSSION AND CONCLUSION

While there is general agreement that exclusionary discipline does not achieve the desired goals of improving behavior and educational outcomes, variations in state responses suggest that first, many policymakers still believe suspensions are necessary and useful, and second, states have many policy design options when they move to restrict the use of suspensions, including options in which grades levels and which behaviors are targeted. Maryland's strategy was broad by including all schools in the state, but also narrow in its focus on early primary grades, where suspension rates were already relatively low. Further, Maryland provided a narrow but critical exception for cases where students are perceived to be a threat to other students or the school, with no additional support for implementing discipline reforms, and no explicit sanctions for violating the ban.

We find that Maryland's version of a suspension ban substantially reduced, but did not fully eliminate, the use of out-of-school suspensions in targeted grades only. Our results show significant and substantial decreases in the probability of being suspended, the number of events, and the number of days suspended in treated grades. The magnitude of reductions is typically a reduction of 60% or more in the suspension rates both overall and for groups that have been historically more likely to be suspended. We find that these are primarily direct effects of banning out-of-school suspension in grades under the ban. There is little evidence of schoolwide spillovers on other grades or persistent effects as students age out of the ban.

While this provides promising evidence regarding the direct goals of the policy, the Maryland example provides several broader insights regarding policy design of suspension bans that take a top-down approach. First, the policy provided limited opportunities for schools to continue to suspend in targeted grades, while also raising the administrative burden to justify a suspension. Based on our conversations with policymakers, these exceptions were added to gain support of teachers who feared that other students would suffer in cases where a truly disruptive student could not be removed from schools. This concession, which was likely necessary for broad political support in Maryland, created the possibility to continue suspensions through strategic use of infraction codes. While we find that the exceptions led to over 1,000 suspensions per year in targeted grades *after* the ban, we find no evidence that this was because schools strategically changed their coding. In fact, we estimate that the ban substantially reduced the incidence of events coded as violent or threatening, and there was no strategic shift to in-school suspensions as a replacement after the ban. This means that schools must likely still occasionally used suspension for some non-violent infractions, despite the ban being in place. Since suspensions occur at the school level, it might require extra oversight and accountability to eliminate suspensions in practice. However, it is notable that Maryland's policy design, in which suspensions were only allowed for violent infractions and required approval of a school psychologist, prevented perverse strategic behavior that was observed in studies of less-restrictive bans such as those in Miami-Dade County (Payne, 2022; O'Connor, 2015) and Arkansas (Anderson, 2018).

Second, the Maryland ban is unique in its focus only on early primary grades where suspensions are less common than upper grades. This allows us to observe students within the same schools who are and are not subject to the ban. Our results suggest that policymakers seeking to reduce suspensions for all students will likely need to target all grades. We find progressively smaller reductions in untreated grades and no effects by fifth grade. It is clear that Maryland schools continued to suspend older students for offenses for which K-2 students in the same building could not be suspended. Further, we find no evidence that third graders who could not be suspended in second grade were less likely to be suspended in third grade. Given the short timeframe we could study, follow-up effects of the ban need more investigation and might be larger for students who experience the ban for the full period from PK-2. However, early evidence suggests that it is likely that limited bans also have limited impacts.

Third, we find this ban also failed to eliminate disproportionalities in suspension and even increased some inequity, particularly for SPED students. The ban is a blunt policy instrument that does not target any specific student subgroups for additional supports or treatment. Likely, eliminating entrenched disparities will require more tailored interventions (such as those taught in restorative practices programs) and a focus on students' needs and risks both inside and outside of schools. Importantly, the safety net of requiring a school psychologist to approve K-2 suspensions was not adequate to achieve equity in suspension rates for SPED students, suggesting that more advocacy, research, and training are required for this subpopulation.

Finally, we note that Maryland's ban resulted, in the short-term, in no meaningful gains in school attendance, and therefore did not increase the quantity of education time for students. It is beyond the scope of this study to assess whether education quality was impacted, as educational outcome measures, such as standardized test scores, are not available in Maryland for students in affected grades. Any reversals of documented negative effects of exclusionary discipline will likely take much longer to emerge and observe. With more states taking policy action to limit suspensions, future research will provide greater insight into the mechanisms of policy design through which alternative strategies shape student behavior and outcomes.

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Table 1. Pre-Treatment Student Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Grades K-2			**Grades 3-5**		
	All	Never Suspended	Suspended	All	Never Suspended	Suspended
Suspensions						
Ever	0.011	0.000	1.000	0.023	0.000	1.000
Frequency	0.019	0.000	1.775	0.036	0.000	1.546
Number of days	0.032	0.000	3.033	0.069	0.000	2.979
Characteristics						
Black	0.321	0.317	0.679	0.326	0.317	0.681
Hispanic	0.177	0.179	0.065	0.166	0.168	0.080
Asian	0.061	0.062	<0.01	0.058	0.059	<0.01
Multiple or Other	0.119	0.119	0.055	0.110	0.111	0.059
Male	0.512	0.509	0.835	0.515	0.509	0.765
FRPL eligible	0.467	0.464	0.745	0.453	0.446	0.776
Extra risk factors	0.015	0.015	0.045	0.014	0.014	0.041
Special education	0.137	0.134	0.388	0.179	0.174	0.393
Engish language learners	0.160	0.161	0.052	0.105	0.107	0.049
Attended multiple schools	0.003	0.003	0.007	0.003	0.003	0.006
N students	176,986	175,108	1,878	109,272	106,748	2,524

Source: Author calculations from MSDE enrollment and student discipline records.
Notes: Extra risk factors include homelessness, foster child, and migrant status. FRPL stands for free-or-reduced-price lunch. Values less than 1% are masked.

Table 2. Comparative Interrupted Time Series Estimates

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions	(6) days attended
time to ban	0.0023**	0.0048**	0.0080**	0.0033**	0.0001	0.2119**
	(0.0005)	(0.0009)	(0.0019)	(0.0006)	(0.0004)	(0.0360)
time to ban x treated grade (K-2)	-0.0011*	-0.0022*	-0.0036	-0.0015*	0.0003	0.0713**
	(0.0005)	(0.0009)	(0.0020)	(0.0007)	(0.0004)	(0.0255)
Grade dummies (grade 5 is reference)						
kindergarten+	-0.0221**	-0.0310**	-0.0670**	-0.0201**	-0.0071**	-1.9217**
	(0.0015)	(0.0029)	(0.0065)	(0.0019)	(0.0013)	(0.0844)
grade1+	-0.0177**	-0.0236**	-0.0519**	-0.0154**	-0.0054**	-0.7839**
	(0.0013)	(0.0025)	(0.0057)	(0.0017)	(0.0012)	(0.0734)
grade2+	-0.0149**	-0.0201**	-0.0431**	-0.0135**	-0.0032**	-0.2694**
	(0.0012)	(0.0024)	(0.0056)	(0.0017)	(0.0011)	(0.0692)
grade3	-0.0089**	-0.0102**	-0.0245**	-0.0063**	-0.0021*	-0.1151*
	(0.0008)	(0.0015)	(0.0033)	(0.0010)	(0.0010)	(0.0466)
grade4	-0.0037**	-0.0040**	-0.0127**	-0.0021	0.0004	0.0204
	(0.0008)	(0.0016)	(0.0035)	(0.0011)	(0.0008)	(0.0414)
Post-ban indicators						
post	-0.0012	-0.0048	-0.0070	-0.0017	-0.0011	-1.2566**
	(0.0016)	(0.0030)	(0.0067)	(0.0021)	(0.0012)	(0.1003)
post x kindergarten+	-0.0057**	-0.0084*	-0.0151*	-0.0072**	-0.0010	0.2140*
	(0.0016)	(0.0033)	(0.0072)	(0.0023)	(0.0013)	(0.0870)
post x grade 1+	-0.0081**	-0.0139**	-0.0260**	-0.0103**	-0.0016	0.2356**
	(0.0016)	(0.0032)	(0.0070)	(0.0023)	(0.0014)	(0.0792)
post x grade 2+	-0.0084**	-0.0136**	-0.0263**	-0.0095**	-0.0024	0.1298
	(0.0016)	(0.0032)	(0.0073)	(0.0023)	(0.0014)	(0.0846)
post x grade 3	-0.0039**	-0.0068**	-0.0117*	-0.0052**	-0.0021	0.2054**
	(0.0013)	(0.0025)	(0.0055)	(0.0017)	(0.0016)	(0.0701)
post x grade 4	-0.0030*	-0.0042	-0.0065	-0.0037*	-0.0020	0.0755
	(0.0012)	(0.0023)	(0.0055)	(0.0015)	(0.0015)	(0.0621)
Constant	0.0323**	0.0505**	0.0988**	0.0333**	0.0106**	171.5643**
	(0.0016)	(0.0031)	(0.0068)	(0.0020)	(0.0013)	(0.1098)
Observations	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703
Pre-Ban untreated mean	.023	.036	.07	.024	0.010	171.11
Pre-ban treated mean	.012	0.020	.036	.013	.005	169.994

Source: Authors calculations from MSDE enrollment and student discipline records.
Notes: + indicates grade was subject to the suspension ban. The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** p<0.01, * p<0.05

Table 3. Event Study Effects by Year and Grade Level

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions	(6) days attended
time to ban	0.0029**	0.0061**	0.0104**	0.0038**	0.0000	0.3147**
	(0.0006)	(0.0011)	(0.0023)	(0.0008)	(0.0005)	(0.0416)
time to ban x treated grade (k-2)	-0.0014*	-0.0032**	-0.0055*	-0.0018*	0.0004	0.0857**
	(0.0006)	(0.0012)	(0.0025)	(0.0008)	(0.0005)	(0.0298)
Grade dummies (grade 5 is reference)						
kindergarten	-0.0227**	-0.0330**	-0.0708**	-0.0206**	-0.0069**	-1.8906**
	(0.0016)	(0.0033)	(0.0073)	(0.0022)	(0.0014)	(0.0918)
grade1	-0.0183**	-0.0256**	-0.0557**	-0.0159**	-0.0052**	-0.7521**
	(0.0015)	(0.0029)	(0.0064)	(0.0019)	(0.0014)	(0.0802)
grade2	-0.0155**	-0.0220**	-0.0468**	-0.0139**	-0.0030*	-0.2391**
	(0.0014)	(0.0028)	(0.0063)	(0.0019)	(0.0013)	(0.0746)
grade3	-0.0089**	-0.0102**	-0.0245**	-0.0063**	-0.0021*	-0.1154*
	(0.0008)	(0.0015)	(0.0033)	(0.0010)	(0.0010)	(0.0466)
grade4	-0.0037**	-0.0040**	-0.0127**	-0.0021	0.0004	0.0198
	(0.0008)	(0.0016)	(0.0035)	(0.0011)	(0.0008)	(0.0414)
Post-ban indicators						
post year 1	-0.0017	-0.0053	-0.0060	-0.0021	-0.0018	-1.2234**
	(0.0017)	(0.0032)	(0.0073)	(0.0022)	(0.0012)	(0.1027)
post year 2	-0.0040	-0.0108*	-0.0198*	-0.0037	-0.0000	-1.7995**
	(0.0023)	(0.0045)	(0.0095)	(0.0031)	(0.0022)	(0.1482)
post year 1 x kindergarten	-0.0055**	-0.0087*	-0.0173*	-0.0072**	-0.0005	0.2038*
	(0.0018)	(0.0035)	(0.0078)	(0.0024)	(0.0013)	(0.0940)
post year 2 x kindergarten	-0.0042	-0.0032	-0.0036	-0.0060	-0.0019	0.1448
	(0.0023)	(0.0049)	(0.0102)	(0.0035)	(0.0023)	(0.1339)
post year 1 x grade 1	-0.0079**	-0.0139**	-0.0279**	-0.0102**	-0.0009	0.2020*
	(0.0018)	(0.0034)	(0.0077)	(0.0024)	(0.0013)	(0.0866)
post year 2 x grade 1	-0.0067**	-0.0089	-0.0146	-0.0092**	-0.0028	0.1859
	(0.0023)	(0.0047)	(0.0098)	(0.0035)	(0.0024)	(0.1240)
post year 1 x grade 2	-0.0071**	-0.0117**	-0.0249**	-0.0082**	-0.0014	0.0892
	(0.0018)	(0.0034)	(0.0078)	(0.0024)	(0.0013)	(0.0916)
post year 2 x grade 2	-0.0082**	-0.0106*	-0.0183	-0.0097**	-0.0038	0.0922
	(0.0022)	(0.0046)	(0.0100)	(0.0034)	(0.0024)	(0.1201)
post year 1 x grade 3	-0.0030	-0.0067*	-0.0150*	-0.0047*	-0.0012	0.1258
	(0.0017)	(0.0030)	(0.0069)	(0.0020)	(0.0013)	(0.0830)
post year 2 x grade 3	-0.0048**	-0.0070*	-0.0085	-0.0057*	-0.0031	0.2799**
	(0.0017)	(0.0032)	(0.0068)	(0.0023)	(0.0022)	(0.0926)
post year 1 x grade 4	-0.0023	-0.0030	-0.0065	-0.0029	-0.0007	0.0433
	(0.0017)	(0.0030)	(0.0072)	(0.0020)	(0.0015)	(0.0858)
post year 2 x grade 4	-0.0037*	-0.0055	-0.0067	-0.0046*	-0.0032	0.1023
	(0.0016)	(0.0030)	(0.0072)	(0.0020)	(0.0020)	(0.0838)
Constant	0.0336**	0.0532**	0.1035**	0.0343**	0.0105**	171.7685**
	(0.0018)	(0.0035)	(0.0075)	(0.0022)	(0.0014)	(0.1191)
Observations	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703
Pre-Ban untreated mean	.023	.036	0.070	.024	.010	171.11
Pre-ban treated mean	.012	.020	.036	.013	.005	169.994

Source: Authors calculations from MSDE enrollment and student discipline records.

Notes: The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** p<0.01, * p<0.05

Table 4A. Comparative Interrupted Time Series Estimates, Black Students

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions
time to ban	0.0040**	0.0092**	0.0170**	0.0068**	0.0001
	(0.0011)	(0.0022)	(0.0047)	(0.0016)	(0.0007)
time to ban x treated grade (k-2)	-0.0016	-0.0039	-0.0065	-0.0034*	0.0003
	(0.0011)	(0.0023)	(0.0052)	(0.0016)	(0.0007)
Grade dummies (grade 5 is reference)					
kindergarten+	-0.0425**	-0.0619**	-0.1352**	-0.0431**	-0.0132**
	(0.0031)	(0.0066)	(0.0154)	(0.0042)	(0.0027)
grade1+	-0.0318**	-0.0432**	-0.0974**	-0.0300**	-0.0096**
	(0.0028)	(0.0059)	(0.0140)	(0.0039)	(0.0026)
grade2+	-0.0261**	-0.0363**	-0.0783**	-0.0268**	-0.0058*
	(0.0026)	(0.0056)	(0.0140)	(0.0037)	(0.0023)
grade3	-0.0149**	-0.0158**	-0.0410**	-0.0102**	-0.0034
	(0.0018)	(0.0035)	(0.0077)	(0.0022)	(0.0025)
grade4	-0.0053**	-0.0058	-0.0207*	-0.0028	-0.0004
	(0.0019)	(0.0036)	(0.0081)	(0.0026)	(0.0016)
Post-ban indicators					
post	-0.0035	-0.0124	-0.0188	-0.0066	-0.0041
	(0.0038)	(0.0074)	(0.0169)	(0.0052)	(0.0026)
post x kindergarten+	-0.0090*	-0.0134	-0.0304	-0.0096	0.0012
	(0.0038)	(0.0082)	(0.0185)	(0.0057)	(0.0030)
post x grade 1+	-0.0157**	-0.0270**	-0.0581**	-0.0187**	0.0005
	(0.0038)	(0.0081)	(0.0182)	(0.0058)	(0.0032)
post x grade 2+	-0.0156**	-0.0249**	-0.0560**	-0.0153**	-0.0002
	(0.0038)	(0.0080)	(0.0187)	(0.0059)	(0.0032)
post x grade 3	-0.0051	-0.0106	-0.0236	-0.0071	-0.0018
	(0.0030)	(0.0059)	(0.0131)	(0.0040)	(0.0039)
post x grade 4	-0.0057*	-0.0077	-0.0142	-0.0067	-0.0020
	(0.0028)	(0.0054)	(0.0133)	(0.0036)	(0.0030)
Constant	0.0604**	0.0977**	0.2007**	0.0656**	0.0177**
	(0.0034)	(0.0068)	(0.0154)	(0.0044)	(0.0028)
Observations	482,448	482,448	482,448	482,448	482,448
Pre-Ban untreated mean	.046	.072	.146	.048	.016
Pre-ban treated mean	.022	.04	.076	.026	.007

Source: Authors calculations from MSDE enrollment and student discipline records.

Notes: + indicates grade was subject to the suspension ban. The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** $p<0.01$, * $p<0.05$

Table 4B. Comparative Interrupted Time Series Estimates, Male Students

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions
time to ban	0.0028**	0.0068**	0.0115**	0.0050**	-0.0001
	(0.0007)	(0.0014)	(0.0031)	(0.0010)	(0.0008)
time to ban x treated grade (k-2)	-0.0010	-0.0029	-0.0048	-0.0022	0.0009
	(0.0007)	(0.0016)	(0.0034)	(0.0011)	(0.0008)
Grade dummies (grade 5 is reference)					
kindergarten+	-0.0304**	-0.0446**	-0.0968**	-0.0294**	-0.0098**
	(0.0022)	(0.0045)	(0.0104)	(0.0031)	(0.0016)
grade1+	-0.0233**	-0.0321**	-0.0716**	-0.0215**	-0.0071**
	(0.0020)	(0.0040)	(0.0091)	(0.0028)	(0.0016)
grade2+	-0.0190**	-0.0271**	-0.0581**	-0.0186**	-0.0041**
	(0.0019)	(0.0038)	(0.0092)	(0.0027)	(0.0016)
grade3	-0.0110**	-0.0121**	-0.0308**	-0.0076**	-0.0028
	(0.0013)	(0.0025)	(0.0054)	(0.0017)	(0.0016)
grade4	-0.0048**	-0.0059*	-0.0172**	-0.0028	0.0014
	(0.0013)	(0.0025)	(0.0057)	(0.0019)	(0.0014)
Post-ban indicators					
post	-0.0020	-0.0082	-0.0151	-0.0045	-0.0014
	(0.0024)	(0.0047)	(0.0105)	(0.0033)	(0.0018)
post x kindergarten+	-0.0092**	-0.0129*	-0.0203	-0.0102**	-0.0024
	(0.0025)	(0.0053)	(0.0117)	(0.0038)	(0.0019)
post x grade 1+	-0.0130**	-0.0222**	-0.0390**	-0.0154**	-0.0033
	(0.0025)	(0.0052)	(0.0113)	(0.0037)	(0.0020)
post x grade 2+	-0.0134**	-0.0212**	-0.0388**	-0.0138**	-0.0046*
	(0.0025)	(0.0050)	(0.0116)	(0.0037)	(0.0021)
post x grade 3	-0.0045*	-0.0087*	-0.0115	-0.0060*	-0.0030
	(0.0021)	(0.0041)	(0.0088)	(0.0028)	(0.0023)
post x grade 4	-0.0023	-0.0033	-0.0031	-0.0037	-0.0028
	(0.0020)	(0.0038)	(0.0089)	(0.0027)	(0.0023)
Constant	0.0472**	0.0763**	0.1487**	0.0513**	0.0158**
	(0.0024)	(0.0046)	(0.0105)	(0.0032)	(0.0018)
Observations	689,748	689,748	689,748	689,748	689,748
Pre-Ban untreated mean	.036	.057	.109	.038	.015
Pre-ban treated mean	.019	.034	.06	.022	.007

Source: Authors calculations from MSDE enrollment and student discipline records.
Notes: + indicates grade was subject to the suspension ban. The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** $p<0.01$, * $p<0.05$

Table 4C. Comparative Interrupted Time Series Estimates, Economically Disadvantaged Students

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions
time to ban	0.0034**	0.0076**	0.0128**	0.0051**	0.0002
	(0.0009)	(0.0016)	(0.0035)	(0.0012)	(0.0007)
time to ban x treated grade (k-2)	-0.0013	-0.0031	-0.0052	-0.0022	0.0006
	(0.0008)	(0.0017)	(0.0038)	(0.0012)	(0.0006)
Grade dummies (grade 5 is reference)					
kindergarten+	-0.0374**	-0.0549**	-0.1198**	-0.0354**	-0.0119**
	(0.0024)	(0.0049)	(0.0116)	(0.0033)	(0.0023)
grade1+	-0.0303**	-0.0424**	-0.0950**	-0.0272**	-0.0093**
	(0.0022)	(0.0044)	(0.0103)	(0.0030)	(0.0023)
grade2+	-0.0253**	-0.0359**	-0.0794**	-0.0240**	-0.0059**
	(0.0021)	(0.0042)	(0.0102)	(0.0029)	(0.0019)
grade3	-0.0161**	-0.0193**	-0.0479**	-0.0118**	-0.0044*
	(0.0014)	(0.0027)	(0.0058)	(0.0017)	(0.0018)
grade4	-0.0068**	-0.0077**	-0.0241**	-0.0042*	0.0004
	(0.0014)	(0.0028)	(0.0065)	(0.0020)	(0.0013)
Post-ban indicators					
post	-0.0034	-0.0103	-0.0188	-0.0043	-0.0039
	(0.0027)	(0.0053)	(0.0122)	(0.0037)	(0.0020)
post x kindergarten+	-0.0074**	-0.0113	-0.0185	-0.0097*	0.0003
	(0.0028)	(0.0058)	(0.0131)	(0.0042)	(0.0023)
post x grade 1+	-0.0113**	-0.0198**	-0.0350**	-0.0150**	-0.0004
	(0.0028)	(0.0057)	(0.0128)	(0.0041)	(0.0024)
post x grade 2+	-0.0125**	-0.0199**	-0.0365**	-0.0134**	-0.0017
	(0.0028)	(0.0057)	(0.0132)	(0.0042)	(0.0024)
post x grade 3	-0.0036	-0.0078	-0.0110	-0.0061*	-0.0007
	(0.0022)	(0.0044)	(0.0097)	(0.0029)	(0.0024)
post x grade 4	-0.0042	-0.0060	-0.0074	-0.0049	-0.0027
	(0.0023)	(0.0042)	(0.0101)	(0.0028)	(0.0021)
Constant	0.0525**	0.0842**	0.1692**	0.0547**	0.0172**
	(0.0026)	(0.0052)	(0.0118)	(0.0034)	(0.0024)
Observations	662,638	662,638	662,638	662,638	662,638
Pre-Ban untreated mean	.038	.059	.118	.039	.015
Pre-ban treated mean	.017	.031	.056	.02	.007

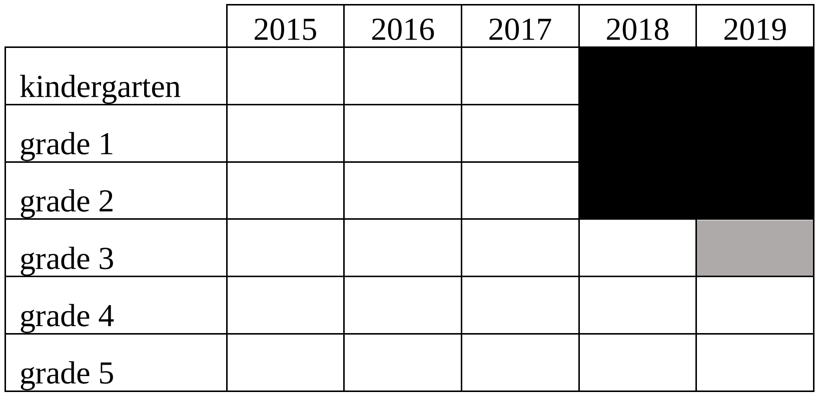
Source: Authors calculations from MSDE enrollment and student discipline records.
Notes: + indicates grade was subject to the suspension ban. The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** $p<0.01$, * $p<0.05$

Table 4D. Comparative Interrupted Time Series Estimates, SPED students

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions
time to ban	0.0051**	0.0131**	0.0214**	0.0089**	0.0003
	(0.0011)	(0.0023)	(0.0056)	(0.0017)	(0.0009)
time to ban x treated grade (k-2)	-0.0028*	-0.0069*	-0.0094	-0.0042	0.0008
	(0.0014)	(0.0031)	(0.0067)	(0.0024)	(0.0011)
Grade dummies (grade 5 is reference)					
kindergarten+	-0.0367**	-0.0527**	-0.1249**	-0.0298**	-0.0077**
	(0.0038)	(0.0086)	(0.0189)	(0.0063)	(0.0030)
grade1+	-0.0273**	-0.0345**	-0.0879**	-0.0179**	-0.0049
	(0.0035)	(0.0082)	(0.0176)	(0.0061)	(0.0026)
grade2+	-0.0239**	-0.0319**	-0.0713**	-0.0187**	-0.0027
	(0.0035)	(0.0078)	(0.0177)	(0.0057)	(0.0028)
grade3	-0.0129**	-0.0130**	-0.0420**	-0.0067*	-0.0003
	(0.0023)	(0.0047)	(0.0121)	(0.0033)	(0.0020)
grade4	-0.0038	-0.0000	-0.0155	0.0028	0.0011
	(0.0022)	(0.0046)	(0.0117)	(0.0035)	(0.0018)
Post-ban indicators					
post	-0.0090*	-0.0194*	-0.0359	-0.0107	-0.0006
	(0.0037)	(0.0085)	(0.0204)	(0.0062)	(0.0024)
post x kindergarten+	-0.0097*	-0.0231*	-0.0409	-0.0208*	-0.0049
	(0.0048)	(0.0114)	(0.0246)	(0.0085)	(0.0036)
post x grade 1+	-0.0147**	-0.0365**	-0.0631*	-0.0275**	-0.0056
	(0.0047)	(0.0112)	(0.0245)	(0.0086)	(0.0034)
post x grade 2+	-0.0125**	-0.0294**	-0.0559*	-0.0206*	-0.0067
	(0.0045)	(0.0106)	(0.0246)	(0.0080)	(0.0034)
post x grade 3	-0.0059	-0.0148	-0.0187	-0.0101	-0.0068*
	(0.0035)	(0.0079)	(0.0184)	(0.0056)	(0.0034)
post x grade 4	-0.0068*	-0.0184*	-0.0253	-0.0153**	-0.0033
	(0.0033)	(0.0076)	(0.0177)	(0.0054)	(0.0031)
Constant	0.0681**	0.1198**	0.2358**	0.0781**	0.0181**
	(0.0034)	(0.0074)	(0.0173)	(0.0051)	(0.0024)
Observations	216,562	216,562	216,562	216,562	216,562
Pre-Ban untreated mean	.052	.09	.174	.059	.018
Pre-ban treated mean	.035	.069	.119	.047	.011

Source: Authors calculations from MSDE enrollment and student discipline records.
Notes: + indicates grade was subject to the suspension ban. The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** p<0.01, * p<0.05

Figure 1. Grade-Level Cohorts' Experience of the Maryland Suspension Ban



White boxes = no ban
Black boxes = ban
Grey boxes = aged out of ban

Figure 2. Number of Suspensions in Maryland Elementary Grades before and after K-2 Suspension Ban

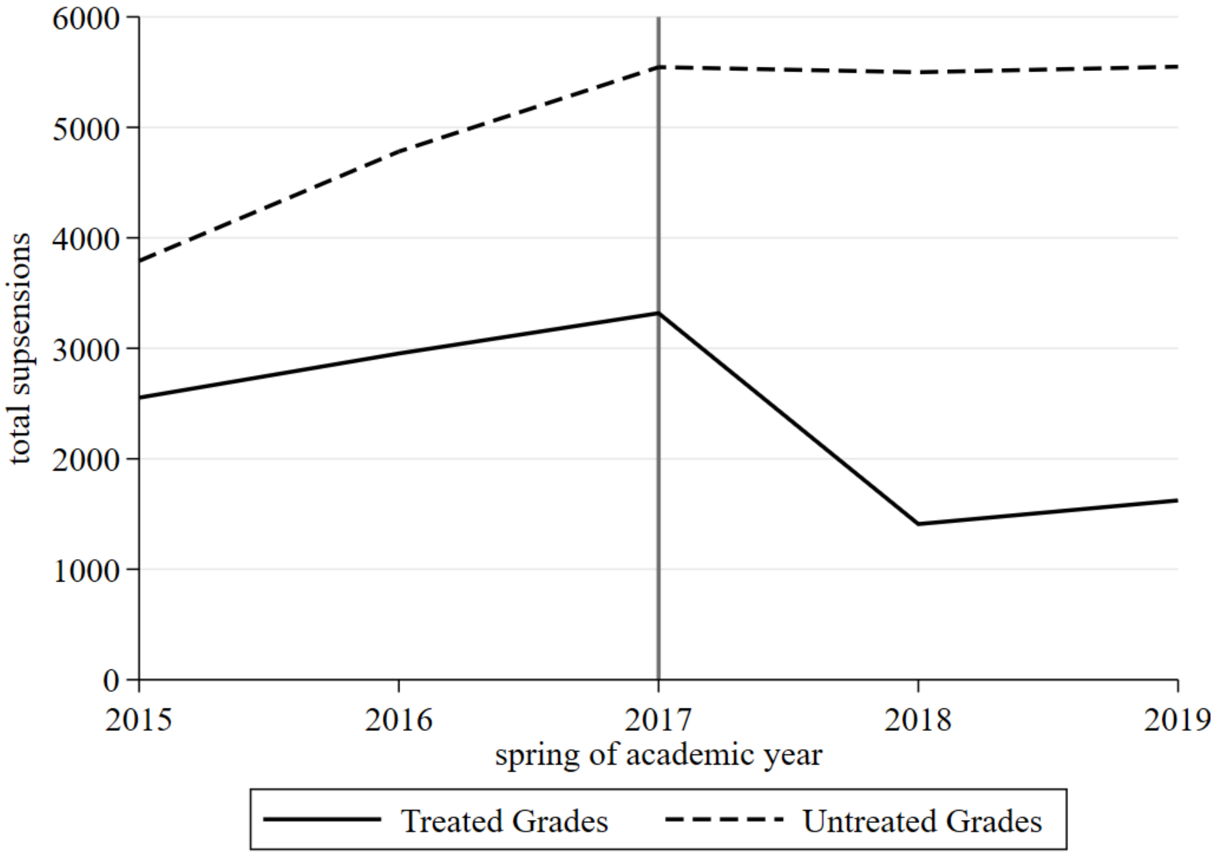


Figure 3. Probability of Getting Suspended in Maryland Elementary Grades before and after K-2 Suspension Ban

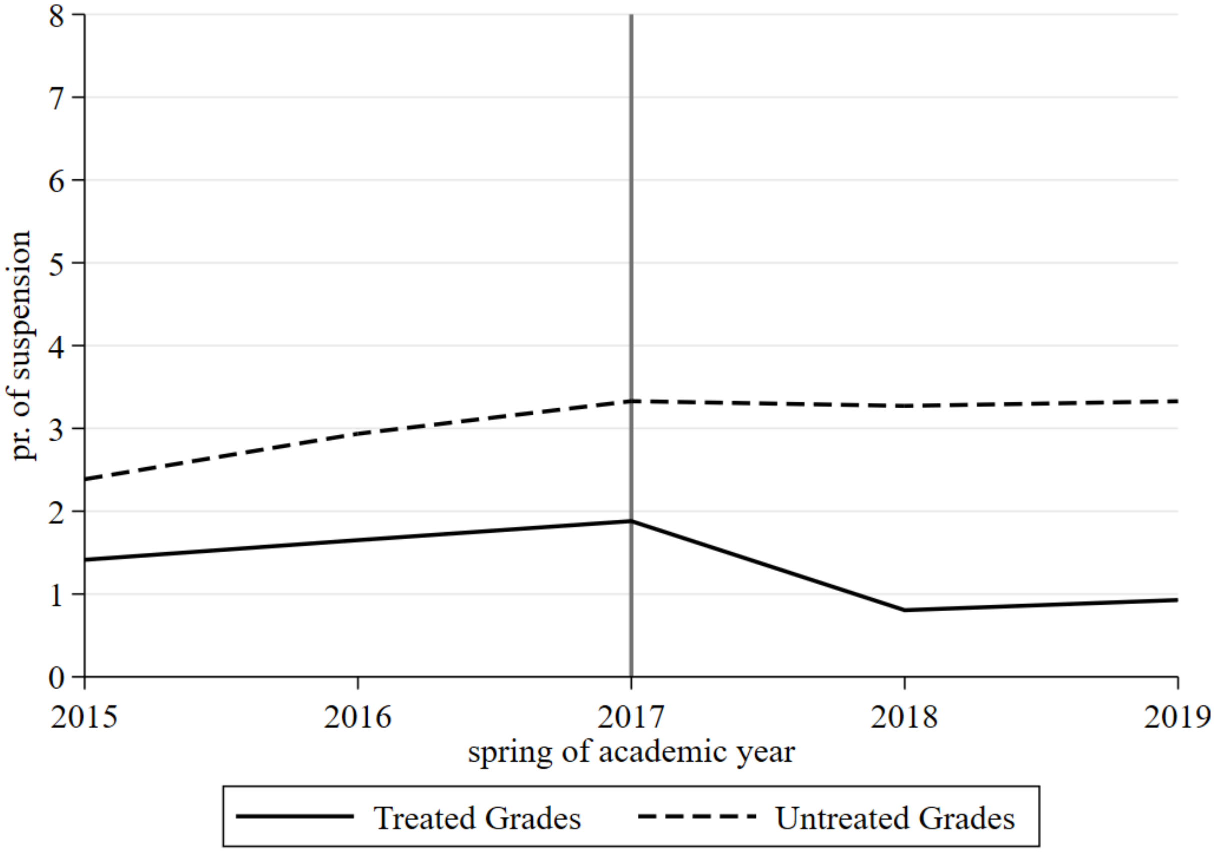


Figure 4. Probability of Suspension for Frequently Suspended Subgroups before and after K-2 Suspension Ban

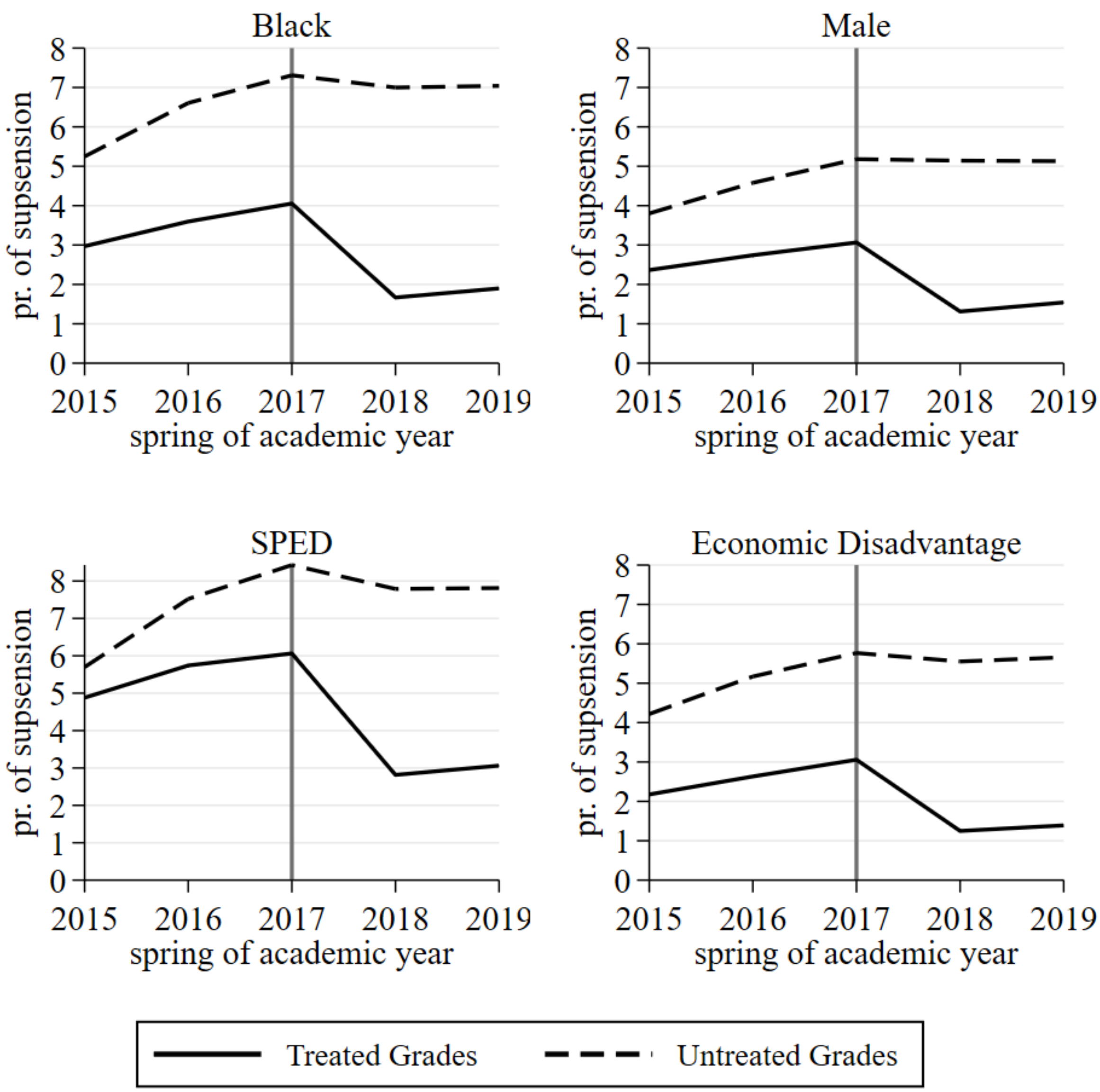


Figure 5. Disproportionality in Suspensions for Frequently Suspended Subgroups before and after K-2 Suspension Ban

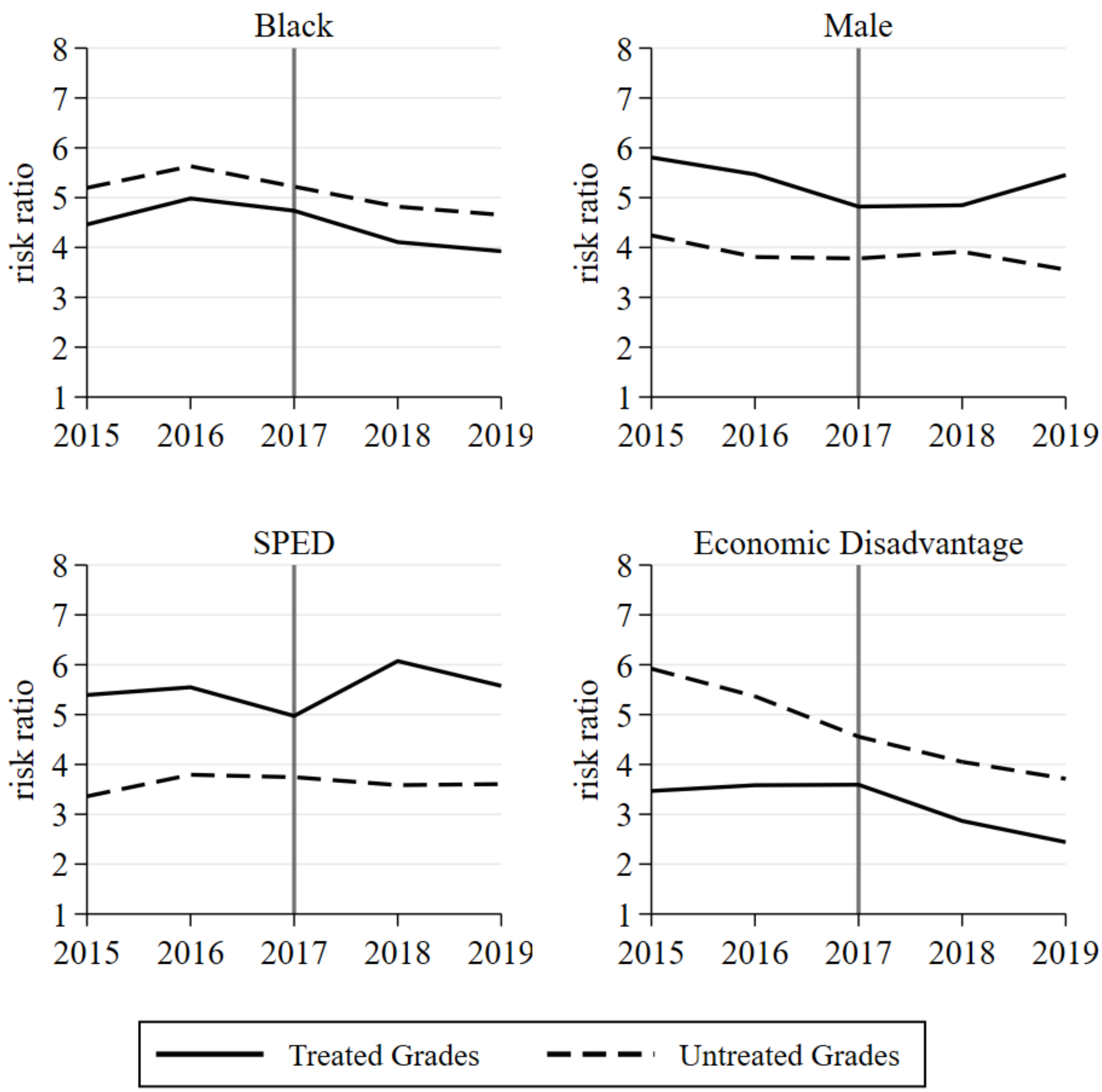
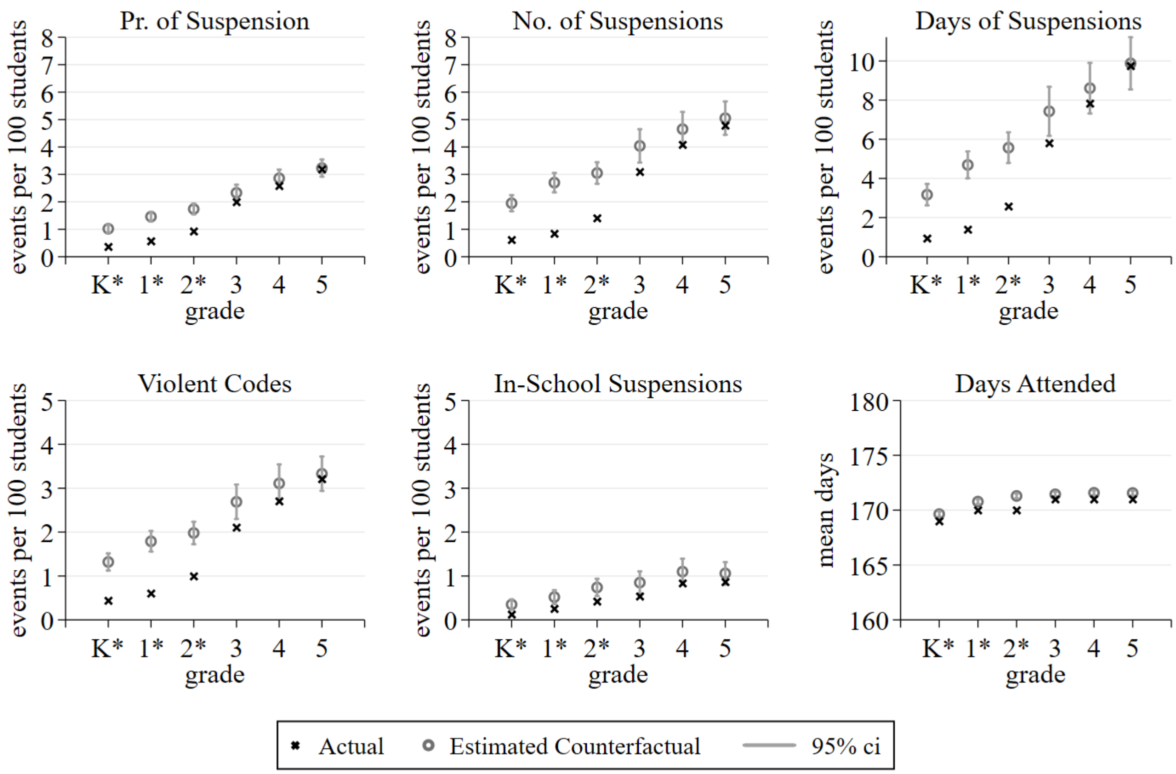


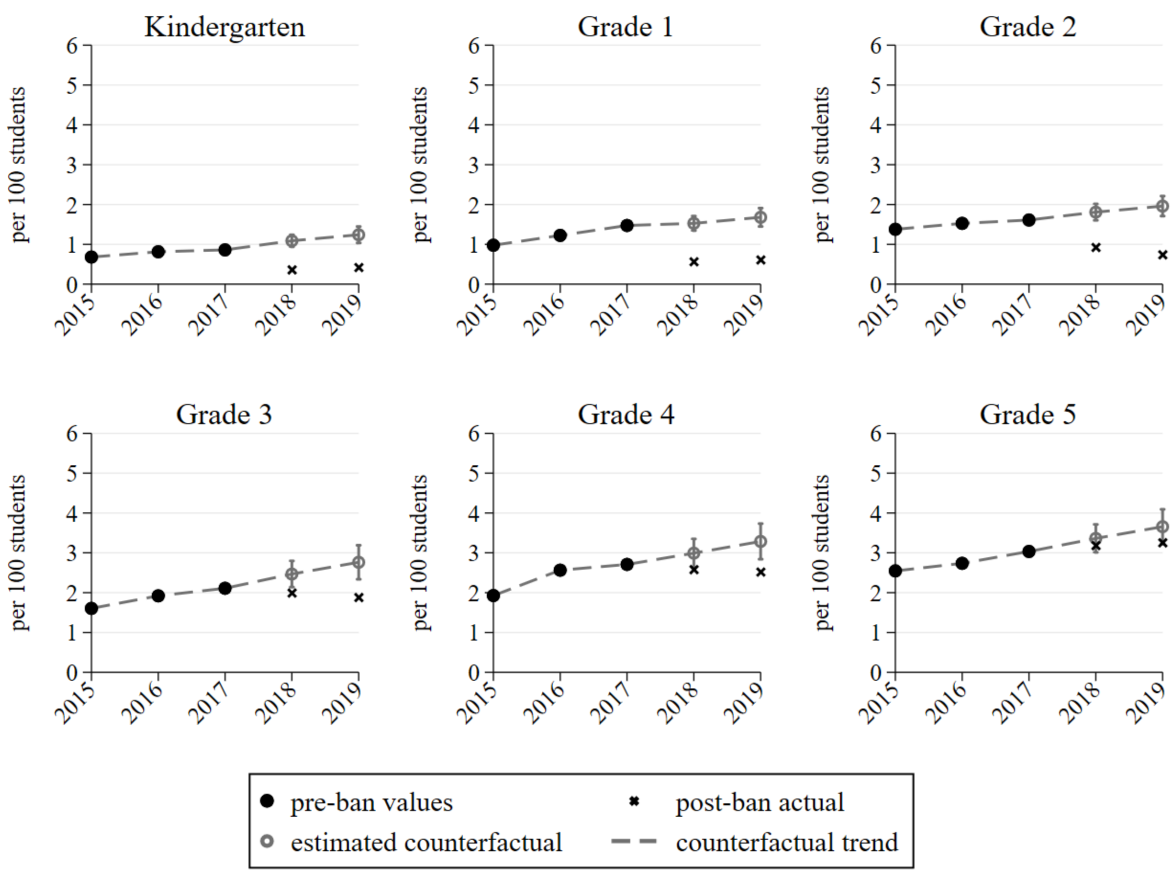
Figure 6. Estimated Effect of Suspension Ban



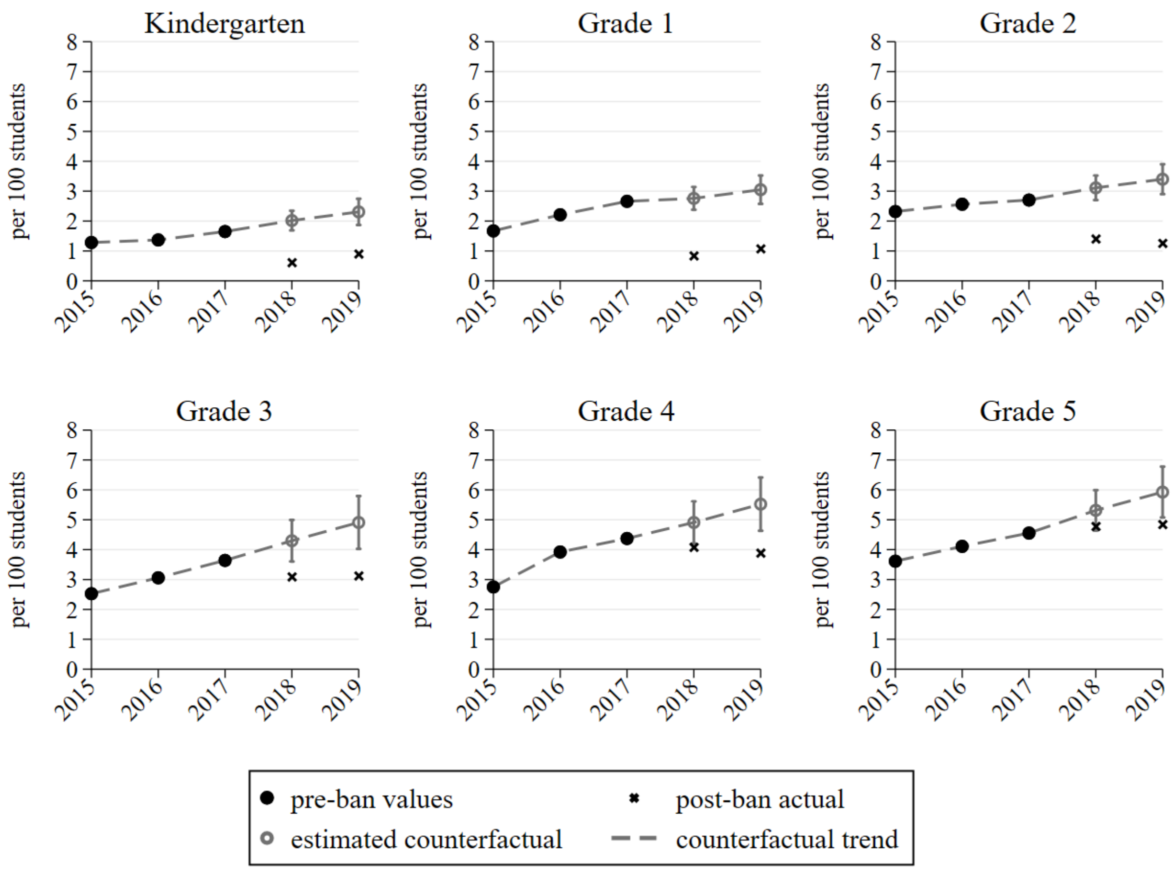
Note: Graphs show estimated and actual values for the 2017-18 school year, which was the first year of the suspension ban. Estimated counterfactual is derived from the regression results in Table 2. * indicates grade was included in the suspension ban. Sample includes schools that used K-2 suspension at least once in the three years prior to the ban.

Figure 7. Event Study Results

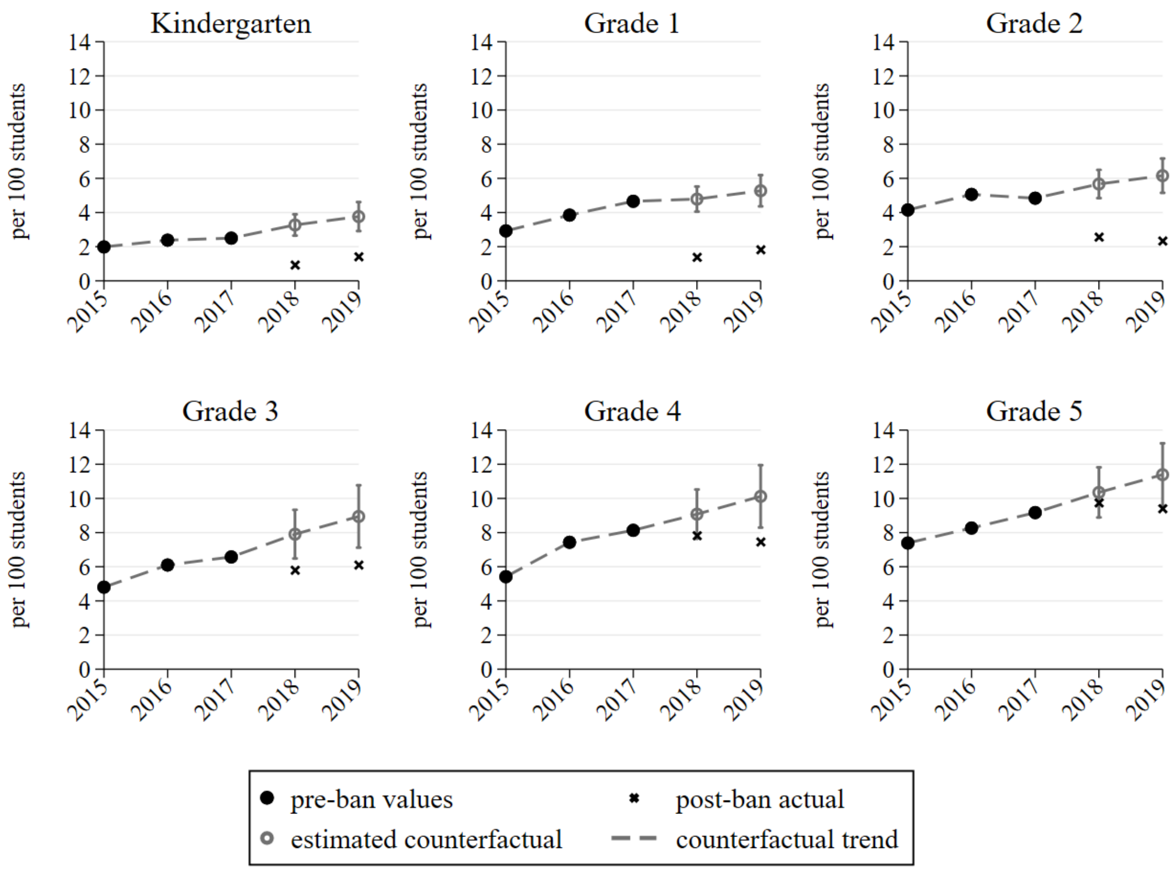
Panel A: Probability of Suspension



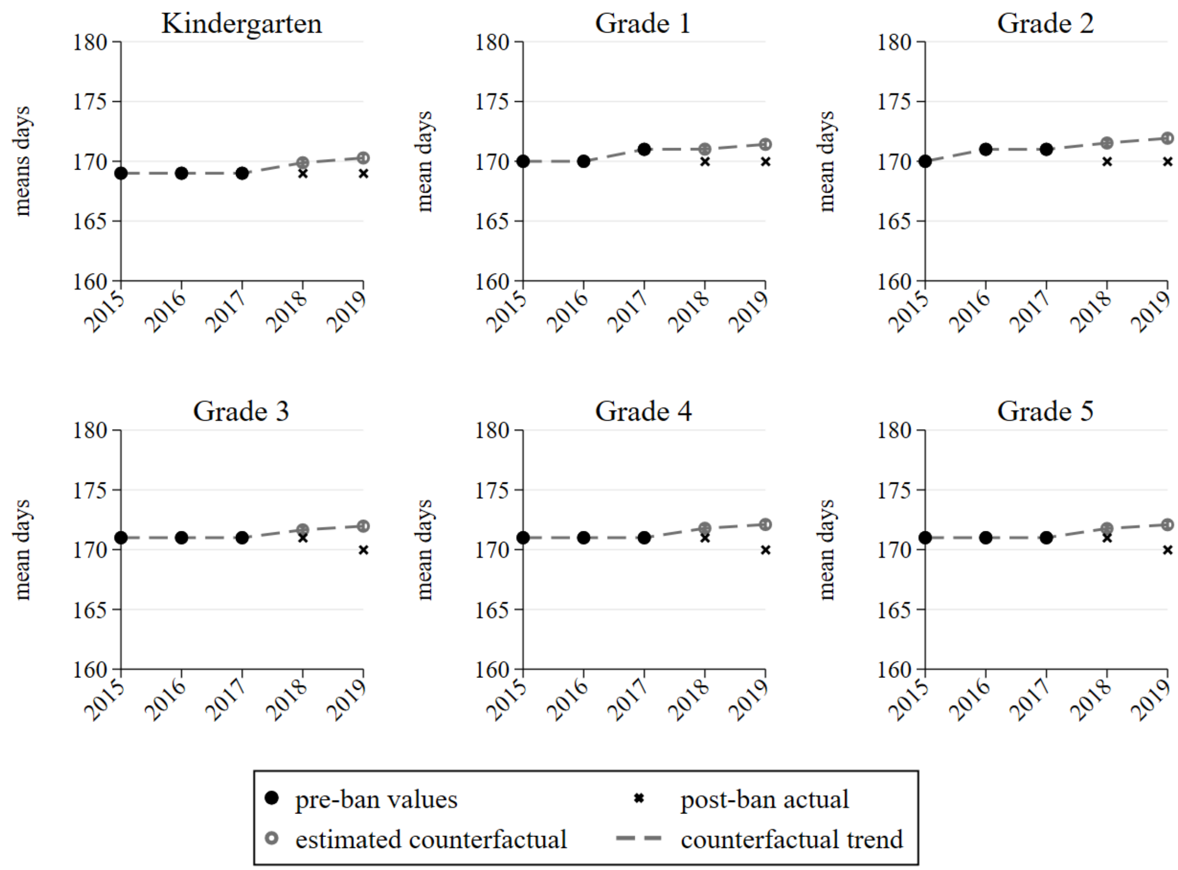
Panel B: Number of Suspensions



Panel C: Days of Suspension



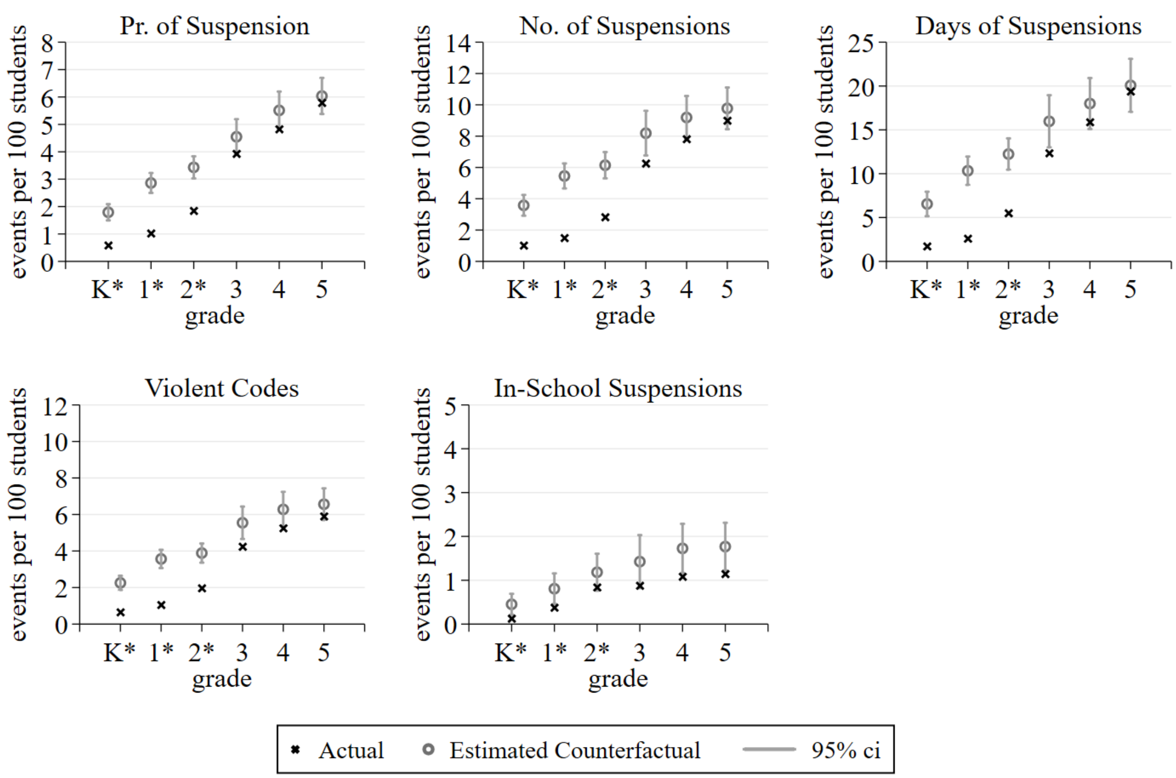
Panel D: Days Attended



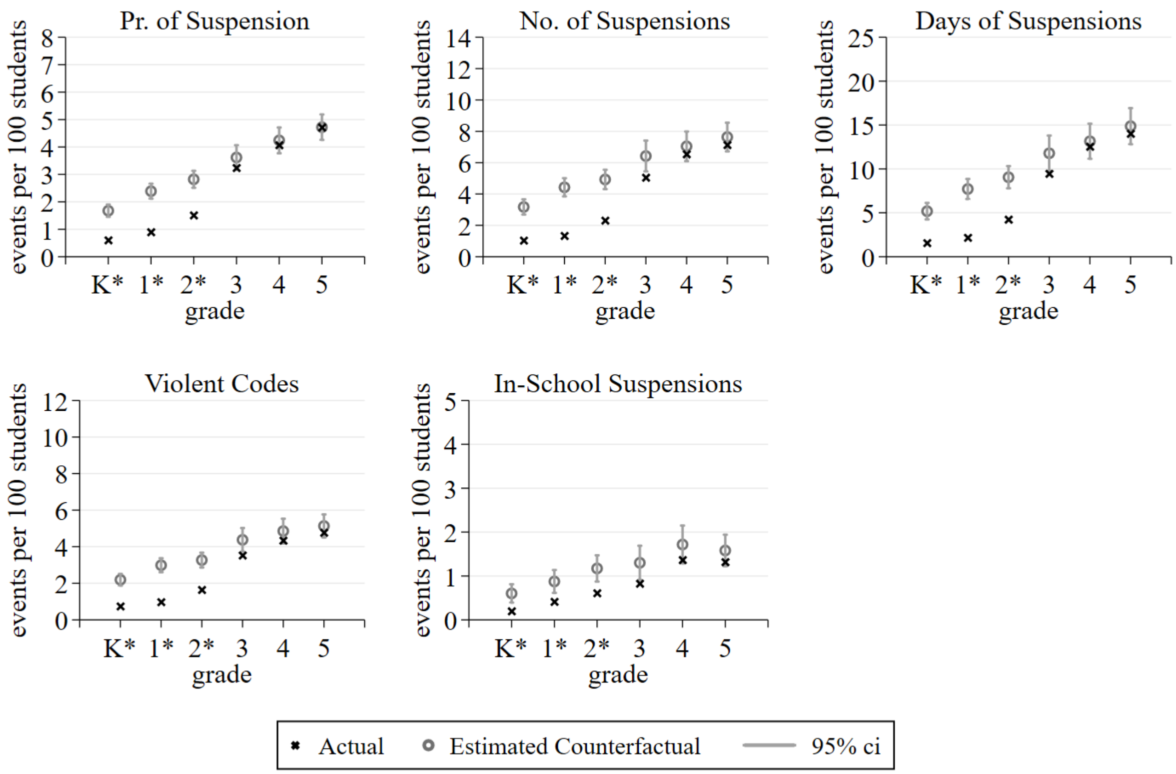
Note: Graphs show estimated and actual values. Estimated counterfactual is derived from the regression results in Table 3. Grades K-2 experienced the suspension ban starting in 2018. Sample includes schools that used K-2 suspension at least once in the three years prior to the ban.

Figure 8. Estimated Effects of Suspension Ban by Grade Level for Frequently Suspended Subgroups

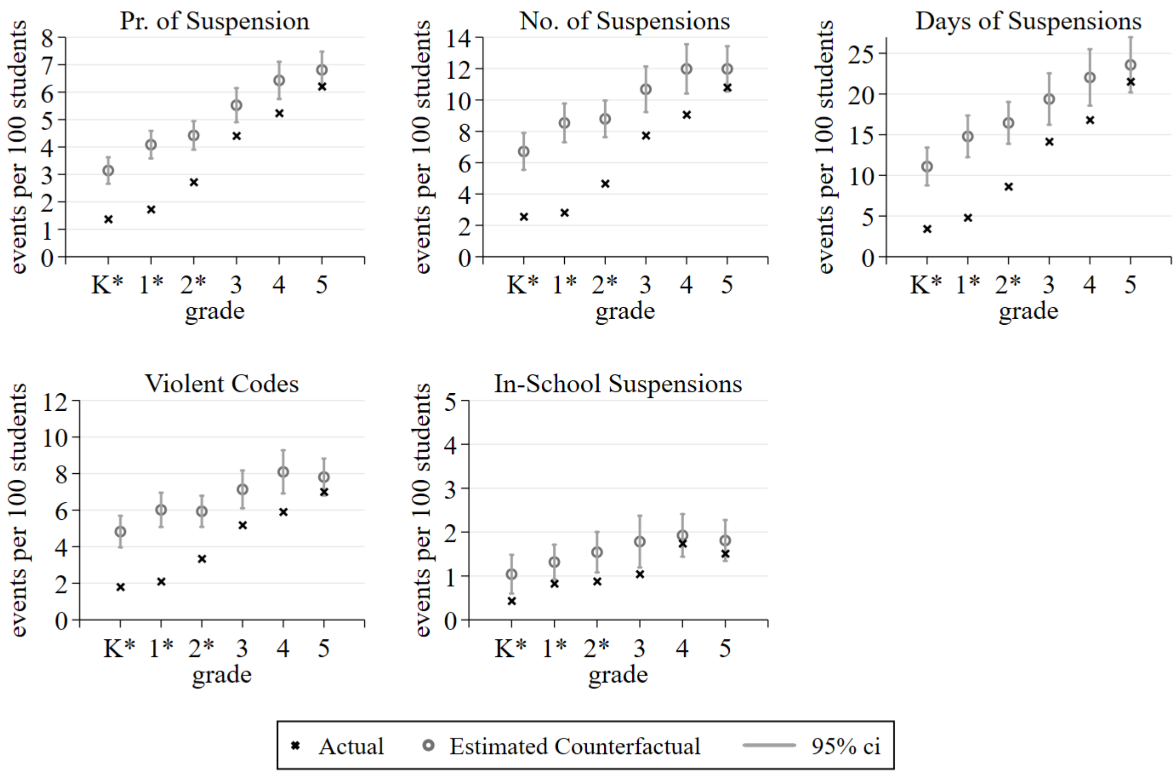
Panel A: Black students



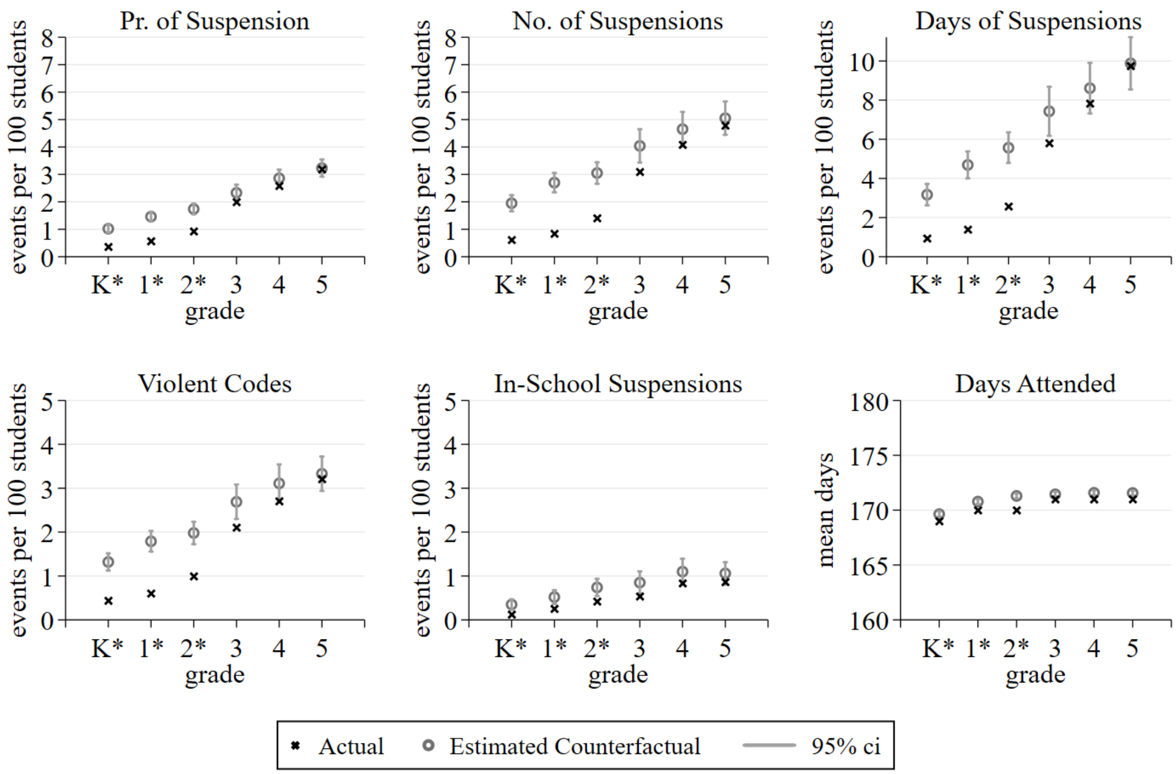
Panel B: Male


Pr. of Suspension
No. of Suspensions
Days of Suspensions
Violent Codes
In-School Suspensions
events per 100 students
grade
K* 1* 2* 3 4 5
Actual
Estimated Counterfactual
95% ci

Panel C: SPED

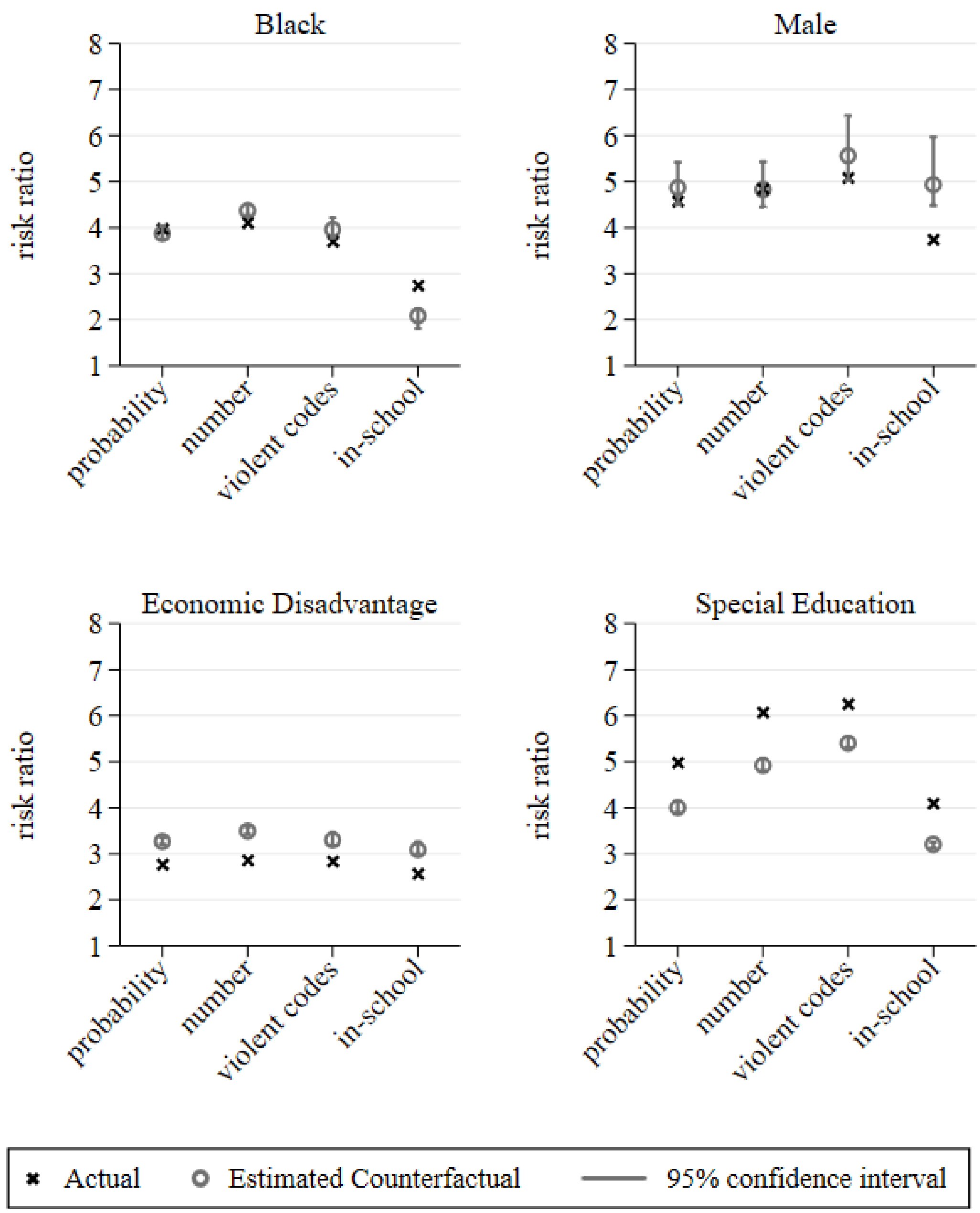

Pr. of Suspension
No. of Suspensions
Days of Suspensions
Violent Codes
In-School Suspensions
events per 100 students
grade
K* 1* 2* 3 4 5
Actual
Estimated Counterfactual
95% ci

Panel D: Economic Disadvantage



Note: Graphs show estimated and actual values for the 2017-18 school year, which was the first year of the suspension ban. Estimated counterfactual is derived from the regression results in Table 4. * indicates grade was included in the suspension ban. Includes schools that used K-2 suspension at least once in the three years prior to the ban.

Figure 9. Estimated Effects of Suspension Ban on Disproportionality in Treated Grades for Frequently Suspended Subgroups



Notes: Graphs show estimated and actual values for aggregate of grades K-2 in the 2017-18 school year, which was the first year of the suspension ban. Includes all elementary schools in Maryland. Risk ratio is equal to (rate for group members)/(rate for non-group members).

Table A1. Comparative Interrupted Time Series Estimates, All Schools

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions	(6) days attended
time to ban	0.0018**	0.0038**	0.0065**	0.0026**	0.0002	0.3317**
	(0.0004)	(0.0007)	(0.0015)	(0.0005)	(0.0003)	(0.0336)
time to ban x treated grade (k-2)	-0.0008*	-0.0017*	-0.0029	-0.0011*	0.0001	0.0270
	(0.0004)	(0.0007)	(0.0016)	(0.0005)	(0.0003)	(0.0241)
Grade dummies (grade 5 is reference)						
kindergarten+	-0.0178**	-0.0248**	-0.0540**	-0.0160**	-0.0070**	-1.9730**
	(0.0012)	(0.0023)	(0.0051)	(0.0015)	(0.0011)	(0.0786)
grade1+	-0.0142**	-0.0188**	-0.0417**	-0.0122**	-0.0056**	-0.8637**
	(0.0011)	(0.0020)	(0.0045)	(0.0014)	(0.0011)	(0.0684)
grade2+	-0.0120**	-0.0160**	-0.0348**	-0.0107**	-0.0036**	-0.3341**
	(0.0010)	(0.0019)	(0.0044)	(0.0013)	(0.0010)	(0.0644)
grade3	-0.0074**	-0.0084**	-0.0204**	-0.0053**	-0.0025**	-0.0769
	(0.0007)	(0.0012)	(0.0026)	(0.0008)	(0.0009)	(0.0419)
grade4	-0.0031**	-0.0035**	-0.0109**	-0.0020*	-0.0001	0.0369
	(0.0007)	(0.0012)	(0.0027)	(0.0009)	(0.0007)	(0.0351)
Post-ban indicators						
post	-0.0002	-0.0026	-0.0040	-0.0005	-0.0012	-1.5136**
	(0.0012)	(0.0024)	(0.0053)	(0.0017)	(0.0010)	(0.0919)
post x kindergarten+	-0.0050**	-0.0076**	-0.0131*	-0.0063**	-0.0005	0.2776**
	(0.0013)	(0.0026)	(0.0057)	(0.0019)	(0.0011)	(0.0794)
post x grade 1+	-0.0071**	-0.0121**	-0.0220**	-0.0089**	-0.0010	0.2823**
	(0.0013)	(0.0026)	(0.0056)	(0.0019)	(0.0012)	(0.0731)
post x grade 2+	-0.0073**	-0.0118**	-0.0222**	-0.0083**	-0.0017	0.1994**
	(0.0013)	(0.0026)	(0.0057)	(0.0019)	(0.0012)	(0.0755)
post x grade 3	-0.0034**	-0.0060**	-0.0098*	-0.0044**	-0.0017	0.1668**
	(0.0010)	(0.0020)	(0.0043)	(0.0013)	(0.0013)	(0.0604)
post x grade 4	-0.0023*	-0.0028	-0.0036	-0.0023	-0.0012	0.0668
	(0.0010)	(0.0018)	(0.0043)	(0.0012)	(0.0012)	(0.0522)
Constant	0.0260**	0.0406**	0.0796**	0.0267**	0.0099**	171.9717**
	(0.0013)	(0.0025)	(0.0054)	(0.0016)	(0.0011)	(0.0967)
Observations	1,709,072	1,709,072	1,709,072	1,709,072	1,709,072	1,709,072
Pre-Ban untreated mean	.019	.029	.056	.019	.009	171.299
Pre-ban treated mean	.009	.016	.029	.011	.004	170.187

Source: Authors calculations from MSDE enrollment and student discipline records.

Notes: + indicates grade was subject to the suspension ban. The sample includes all public schools in Maryland regardless of their suspension practices before the ban. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** p<0.01, * p<0.05

Table A2. Comparative Interrupted Time Series Estimates with School Fixed Effects, K-2 Suspenders School

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions - threats or violence	(5) in-school suspensions	(6) days attended
time to ban	0.0023**	0.0048**	0.0081**	0.0033**	0.0001	0.1968**
	(0.0005)	(0.0009)	(0.0019)	(0.0006)	(0.0004)	(0.0346)
time to ban x treated grade (k-2)	-0.0010*	-0.0020*	-0.0032	-0.0014*	0.0003	0.0469
	(0.0004)	(0.0009)	(0.0019)	(0.0007)	(0.0004)	(0.0240)
Grade dummies (grade 5 is reference)						
kindergarten	-0.0219**	-0.0304**	-0.0660**	-0.0196**	-0.0069**	-1.8800**
	(0.0014)	(0.0028)	(0.0062)	(0.0018)	(0.0012)	(0.0806)
grade1	-0.0177**	-0.0233**	-0.0515**	-0.0151**	-0.0052**	-0.7326**
	(0.0013)	(0.0025)	(0.0056)	(0.0017)	(0.0011)	(0.0699)
grade2	-0.0149**	-0.0199**	-0.0429**	-0.0133**	-0.0032**	-0.2132**
	(0.0012)	(0.0024)	(0.0056)	(0.0016)	(0.0010)	(0.0662)
grade3	-0.0092**	-0.0106**	-0.0254**	-0.0066**	-0.0021*	-0.0276
	(0.0008)	(0.0015)	(0.0032)	(0.0009)	(0.0011)	(0.0450)
grade4	-0.0038**	-0.0042**	-0.0130**	-0.0022*	0.0004	0.0571
	(0.0008)	(0.0015)	(0.0034)	(0.0011)	(0.0009)	(0.0403)
Post-ban indicators						
post	-0.0010	-0.0043	-0.0063	-0.0014	-0.0010	-1.2425**
	(0.0015)	(0.0029)	(0.0066)	(0.0020)	(0.0012)	(0.0967)
post x kindergarten	-0.0056**	-0.0084**	-0.0151*	-0.0071**	-0.0009	0.2052*
	(0.0016)	(0.0032)	(0.0071)	(0.0023)	(0.0013)	(0.0836)
post x grade 1	-0.0078**	-0.0134**	-0.0251**	-0.0100**	-0.0015	0.2004**
	(0.0016)	(0.0032)	(0.0070)	(0.0023)	(0.0014)	(0.0769)
post x grade 2	-0.0083**	-0.0134**	-0.0257**	-0.0093**	-0.0024	0.0932
	(0.0016)	(0.0032)	(0.0073)	(0.0023)	(0.0014)	(0.0819)
post x grade 3	-0.0037**	-0.0064**	-0.0107*	-0.0049**	-0.0022	0.1441*
	(0.0013)	(0.0024)	(0.0054)	(0.0017)	(0.0016)	(0.0684)
post x grade 4	-0.0031*	-0.0043	-0.0067	-0.0038*	-0.0020	0.0697
	(0.0012)	(0.0022)	(0.0054)	(0.0015)	(0.0015)	(0.0607)
Constant	0.0322**	0.0503**	0.0985**	0.0331**	0.0105**	171.4939**
	(0.0011)	(0.0021)	(0.0049)	(0.0014)	(0.0009)	(0.0763)
Observations	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703
R-squared	0.0263	0.0219	0.0186	0.0188	0.0231	0.0932
School FE	x	x	x	x	x	x
N schools	719	719	719	719	719	719

Source: Authors calculations from MSDE enrollment and student discipline records.
Notes: + indicates grade was subject to the suspension ban. The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses) and school fixed effects. Statistical significance: ** $p<0.01$, * $p<0.05$

Table A3. Event Study Effects by Year and Grade Level, All Schools

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions for threats or violence	(5) in-school suspensions	(6) days attended
time to ban	0.0022**	0.0047**	0.0080**	0.0029**	0.0001	0.4621**
	(0.0005)	(0.0009)	(0.0018)	(0.0006)	(0.0004)	(0.0392)
time to ban x treated grade (k-2)	-0.0010*	-0.0024*	-0.0040*	-0.0012	0.0003	0.0438
	(0.0004)	(0.0009)	(0.0020)	(0.0007)	(0.0004)	(0.0287)
Grade dummies (grade 5 is reference)						
kindergarten+	-0.0181**	-0.0261**	-0.0561**	-0.0163**	-0.0067**	-1.9365**
	(0.0013)	(0.0026)	(0.0057)	(0.0017)	(0.0012)	(0.0864)
grade1+	-0.0146**	-0.0201**	-0.0439**	-0.0125**	-0.0053**	-0.8265**
	(0.0012)	(0.0023)	(0.0050)	(0.0015)	(0.0012)	(0.0758)
grade2+	-0.0124**	-0.0173**	-0.0369**	-0.0110**	-0.0033**	-0.2983**
	(0.0011)	(0.0022)	(0.0050)	(0.0015)	(0.0011)	(0.0707)
grade3	-0.0074**	-0.0084**	-0.0204**	-0.0053**	-0.0025**	-0.0770
	(0.0007)	(0.0012)	(0.0026)	(0.0008)	(0.0009)	(0.0419)
grade4	-0.0031**	-0.0035**	-0.0109**	-0.0020*	-0.0001	0.0363
	(0.0007)	(0.0012)	(0.0027)	(0.0009)	(0.0007)	(0.0351)
Post-ban indicators						
post year 1	-0.0010	-0.0036	-0.0039	-0.0012	-0.0020	-1.4776**
	(0.0014)	(0.0025)	(0.0057)	(0.0017)	(0.0010)	(0.0935)
post year 2	-0.0016	-0.0059	-0.0112	-0.0013	0.0002	-2.1942**
	(0.0018)	(0.0037)	(0.0076)	(0.0025)	(0.0018)	(0.1345)
post year 1 x kindergarten+	-0.0046**	-0.0073**	-0.0143*	-0.0059**	0.0001	0.2508**
	(0.0014)	(0.0028)	(0.0061)	(0.0019)	(0.0011)	(0.0848)
post year 2 x kindergarten+	-0.0046*	-0.0047	-0.0066	-0.0061*	-0.0019	0.2107
	(0.0018)	(0.0040)	(0.0083)	(0.0029)	(0.0018)	(0.1220)
post year 1 x grade 1+	-0.0066**	-0.0117**	-0.0230**	-0.0086**	-0.0001	0.2389**
	(0.0014)	(0.0027)	(0.0060)	(0.0019)	(0.0011)	(0.0779)
post year 2 x grade 1++	-0.0067**	-0.0093*	-0.0156*	-0.0085**	-0.0025	0.2295*
	(0.0018)	(0.0038)	(0.0080)	(0.0028)	(0.0019)	(0.1138)
post year 1 x grade 2+	-0.0058**	-0.0096**	-0.0201**	-0.0068**	-0.0008	0.1756*
	(0.0014)	(0.0027)	(0.0062)	(0.0019)	(0.0011)	(0.0808)
post year 2 x grade 2++	-0.0079**	-0.0109**	-0.0190*	-0.0092**	-0.0034	0.1317
	(0.0018)	(0.0038)	(0.0080)	(0.0028)	(0.0019)	(0.1085)
post year 1 x grade 3	-0.0020	-0.0049*	-0.0110*	-0.0035*	-0.0004	0.0828
	(0.0013)	(0.0024)	(0.0054)	(0.0016)	(0.0011)	(0.0706)
post year 2 x grade 3++	-0.0047**	-0.0071**	-0.0085	-0.0054**	-0.0029	0.2438**
	(0.0013)	(0.0025)	(0.0053)	(0.0018)	(0.0018)	(0.0786)
post year 1 x grade 4	-0.0014	-0.0011	-0.0027	-0.0013	0.0000	0.0250
	(0.0013)	(0.0024)	(0.0057)	(0.0016)	(0.0012)	(0.0721)
post year 2 x grade 4	-0.0032*	-0.0046	-0.0046	-0.0034*	-0.0024	0.1014
	(0.0013)	(0.0024)	(0.0058)	(0.0017)	(0.0017)	(0.0710)
Constant	0.0269**	0.0423**	0.0825**	0.0273**	0.0096**	172.2307**
	(0.0014)	(0.0027)	(0.0059)	(0.0018)	(0.0012)	(0.1063)
Observations	1,709,072	1,709,072	1,709,072	1,709,072	1,709,072	1,709,072
Pre-Ban untreated mean	.019	.029	.056	.019	.009	171.299
Pre-ban treated mean	.009	.016	.029	.011	.004	170.187

Source: Authors calculations from MSDE enrollment and student discipline records.
Notes: + treated grade x year, ++ cohort treated in the prior year. The sample includes all public schools in Maryland regardless of their suspension practices before the ban. Robust standard errors clustered at the school level (in parentheses). Statistical significance: ** p<0.01, * p<0.05

Table A4. Event Study Effects by Year and Grade Level with School Fixed Effects, K-2 Suspenders School

VARIABLES	(1) ever suspended	(2) number of suspensions	(3) days of suspensions	(4) suspensions - threats or violence	(5) in-school suspensions	(6) days attended
time to ban	0.0029**	0.0061**	0.0103**	0.0038**	0.0000	0.3055**
	(0.0006)	(0.0011)	(0.0023)	(0.0008)	(0.0005)	(0.0404)
time to ban x treated grade (k-2)	-0.0013*	-0.0030**	-0.0049*	-0.0016*	0.0003	0.0502
	(0.0005)	(0.0011)	(0.0024)	(0.0008)	(0.0005)	(0.0292)
Grade dummies (grade 5 is reference)						
kindergarten	-0.0225**	-0.0322**	-0.0695**	-0.0200**	-0.0068**	-1.8708**
	(0.0016)	(0.0031)	(0.0070)	(0.0021)	(0.0013)	(0.0882)
grade1	-0.0183**	-0.0252**	-0.0550**	-0.0155**	-0.0050**	-0.7229**
	(0.0015)	(0.0028)	(0.0063)	(0.0019)	(0.0013)	(0.0773)
grade2	-0.0155**	-0.0218**	-0.0464**	-0.0137**	-0.0031*	-0.2050**
	(0.0013)	(0.0027)	(0.0062)	(0.0018)	(0.0013)	(0.0721)
grade3	-0.0092**	-0.0106**	-0.0254**	-0.0066**	-0.0021*	-0.0277
	(0.0008)	(0.0015)	(0.0032)	(0.0009)	(0.0011)	(0.0450)
grade4	-0.0038**	-0.0042**	-0.0130**	-0.0022*	0.0004	0.0565
	(0.0008)	(0.0015)	(0.0034)	(0.0011)	(0.0009)	(0.0403)
Post-ban indicators						
post year 1	-0.0015	-0.0048	-0.0052	-0.0019	-0.0017	-1.2150**
	(0.0017)	(0.0031)	(0.0072)	(0.0021)	(0.0012)	(0.0989)
post year 2	-0.0037	-0.0101*	-0.0185*	-0.0033	-0.0000	-1.8084**
	(0.0022)	(0.0044)	(0.0094)	(0.0031)	(0.0022)	(0.1423)
post year 1 x kindergarten	-0.0054**	-0.0085*	-0.0171*	-0.0070**	-0.0003	0.1910*
	(0.0018)	(0.0034)	(0.0076)	(0.0024)	(0.0013)	(0.0906)
post year 2 x kindergarten	-0.0043	-0.0035	-0.0044	-0.0062	-0.0018	0.1947
	(0.0023)	(0.0048)	(0.0100)	(0.0035)	(0.0023)	(0.1195)
post year 1 x grade 1	-0.0077**	-0.0135**	-0.0273**	-0.0100**	-0.0009	0.1740*
	(0.0018)	(0.0034)	(0.0076)	(0.0024)	(0.0013)	(0.0851)
post year 2 x grade 1	-0.0065**	-0.0087	-0.0143	-0.0090**	-0.0025	0.1985
	(0.0023)	(0.0047)	(0.0098)	(0.0034)	(0.0024)	(0.1123)
post year 1 x grade 2	-0.0070**	-0.0115**	-0.0244**	-0.0080**	-0.0014	0.0647
	(0.0018)	(0.0034)	(0.0078)	(0.0024)	(0.0013)	(0.0882)
post year 2 x grade 2	-0.0082**	-0.0106*	-0.0183	-0.0097**	-0.0037	0.0982
	(0.0022)	(0.0046)	(0.0099)	(0.0034)	(0.0024)	(0.1081)
post year 1 x grade 3	-0.0029	-0.0065*	-0.0145*	-0.0045*	-0.0013	0.0871
	(0.0016)	(0.0030)	(0.0067)	(0.0020)	(0.0013)	(0.0821)
post year 2 x grade 3	-0.0044**	-0.0063*	-0.0069	-0.0052*	-0.0031	0.1950*
	(0.0016)	(0.0032)	(0.0068)	(0.0023)	(0.0022)	(0.0890)
post year 1 x grade 4	-0.0023	-0.0029	-0.0063	-0.0028	-0.0008	0.0299
	(0.0016)	(0.0030)	(0.0071)	(0.0020)	(0.0015)	(0.0833)
post year 2 x grade 4	-0.0038*	-0.0058	-0.0073	-0.0048*	-0.0033	0.1038
	(0.0016)	(0.0030)	(0.0071)	(0.0020)	(0.0020)	(0.0801)
Constant	0.0335**	0.0528**	0.1030**	0.0340**	0.0104**	171.7097**
	(0.0013)	(0.0025)	(0.0057)	(0.0016)	(0.0011)	(0.0880)
Observations	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703	1,345,703
Pre-Ban untreated mean						
Pre-ban treated mean						
School FE	x	x	x	x	x	x
N schools	719	719	719	719	719	719

Source: Authors calculations from MSDE enrollment and student discipline records.

Notes: The sample includes only schools that suspended students before the policy change. Robust standard errors clustered at the school level (in parentheses) and school fixed effects. Statistical significance: ** p<0.01, * p<0.05