EdWorkingPaper No. 24-1008

# Priceless Benefits: Effects of School Spending on Child Mortality

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The academic and economic benefits of school spending are well-established, but focusing on these outcomes may underestimate the full social benefits of school spending. Recent increases in U.S. child mortality are driven by injuries and raise questions about what types of social investments could reduce child deaths. We use close school district tax elections and negative binomial regression models to estimate effects of a quasi-random increase in school spending on county child mortality. We find consistent evidence that increased school spending from passing a tax election reduces child mortality. Districts that narrowly passed a proposed tax increase spent an additional $243 per pupil, mostly on instruction and salaries, and had 4% lower child mortality after spending increased (6-10 years after the election). This increased spending also reduced child deaths of despair (due to drugs, alcohol, or suicide) by 5% and child deaths due to accidents or motor vehicle accidents by 7%. Estimates predicting potential mechanisms suggest that lower child mortality could partly reflect increases in the number of teachers and counselors, higher teacher salaries, and improved student engagement.

VERSION: July 2024

Suggested citation: Rauscher, Emily, Greer Mellon, and Susanna Loeb. (2024). Priceless Benefits: Effects of School Spending on Child Mortality. (EdWorkingPaper: 24-1008). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/s7t7-j992

**Priceless Benefits: Effects of School Spending on Child Mortality**

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**Abstract**
The academic and economic benefits of school spending are well-established, but focusing on these outcomes may underestimate the full social benefits of school spending. Recent increases in U.S. child mortality are driven by injuries and raise questions about what types of social investments could reduce child deaths. We use close school district tax elections and negative binomial regression models to estimate effects of a quasi-random increase in school spending on county child mortality. We find consistent evidence that increased school spending from passing a tax election reduces child mortality. Districts that narrowly passed a proposed tax increase spent an additional $243 per pupil, mostly on instruction and salaries, and had 4% lower child mortality after spending increased (6-10 years after the election). This increased spending also reduced child deaths of despair (due to drugs, alcohol, or suicide) by 5% and child deaths due to accidents or motor vehicle accidents by 7%. Estimates predicting potential mechanisms suggest that lower child mortality could partly reflect increases in the number of teachers and counselors, higher teacher salaries, and improved student engagement.

**Acknowledgements**
This research was supported by a research grant from the Gilead Foundation Creating Possible Fund (#17154). The project benefited from support from the Population Studies and Training Center at Brown University, which receives funding from the NIH (P2C HD041020). The authors are grateful for feedback from colleagues at the Annenberg Institute for School Reform at Brown University.

## **Introduction**

Does school spending reduce child mortality? The academic and economic benefits of school spending are well-established (Candelaria and Shores 2019; Jackson, Johnson, and Persico 2016; Jackson and Mackevicius 2021), but focusing on these outcomes may underestimate the full social benefits of school spending. Recent evidence shows that increased school funding reduces crime (Baron, Hyman, and Vasquez 2024), and school funding increases may have other important benefits for children's life chances, including improvement in child health and well-being.

After decades of improvement, child mortality increased by 20% from 2019 to 2021, driven by injuries – including suicide, homicide, drug overdoses, and accidents – not the COVID-19 pandemic (Woolf, Wolf, and Rivara 2023). This increase predated the pandemic and reflects social factors including mental health and inequality, with larger increases in mortality among males and non-Hispanic Black children (Woolf et al. 2023:975). While there is growing research on the social determinants of mortality (e.g., Denney and He 2014; Miech et al. 2011; Torche and Rauf 2021), we know less about the factors that may reduce child mortality, specifically. Learning whether community investments, including school funding to support student engagement and relationships, can reduce child mortality could help counteract the troubling rise in child deaths.

We use close school district tax elections to estimate effects of a quasi-random increase in school spending on county child mortality. To preview our results, we find consistent evidence that passing a tax election increases school spending and reduces child mortality. Estimates predicting potential mechanisms suggest that lower child mortality could partly reflect increases in the number of teachers and counselors, higher teacher salaries, and improved student engagement, including lower absenteeism, suspension, and arrest rates. This study contributes to evidence that the benefits of school spending go beyond academic and economic outcomes to improve child well-being (Baron et al. 2024). It also contributes to research on the relationship between education and mortality, which has focused on adult mortality (Balaj et al. 2024; Galama, Lleras-Muney, and Kippersluis 2018; Lleras-Muney 2005; Miech et al. 2011), by presenting new evidence on the relationship between school resources and child mortality. Results suggest that school spending could help counteract the recent increase in child mortality.

## Theoretical and Empirical Background

### Children and Adolescent Mortality

Each year, over 20,000 children and adolescents die in the United States (Centers for Disease Control and Prevention 2021). Most of these deaths are preventable, due to injuries including motor vehicle crashes, suffocation, and firearms (Cunningham, Walton, and Carter 2018), and affect school-age children (ages 5-19), particularly teenagers aged 15-19 (CDC 2021). In 2021, the mortality rate among children ages 5-19 was 30.6 per 100,000, compared to 62.2 per 100,000 among those ages 15-19 (CDC 2021). Despite reductions in the number of deaths from other causes, injury-related deaths have increased among children and adolescents in recent years (Cunningham et al. 2018).

While many of these "accidental" categories of deaths have traditionally been viewed as random and mostly unpredictable (Ludwig and Miller 2007), there is growing evidence that these types of deaths have a social determinant. Among adults, "accidental" deaths have been found to be more common among low-SES individuals (Denney and He 2014) and in low-SES communities (Karb, Subramanian, and Fleegler 2016). While there is comparatively less research on the social determinants of child and adolescent mortality, there is growing evidence that social environments may affect mortality rates among younger age groups, by influencing their likelihood of participating in risky behaviors that can lead to injury or death (Huang, Cheng, and Theise 2013).

Learning whether and in which contexts social policy interventions can reduce child and adolescent mortality is important for extending longevity and improving child well-being. Interventions that aim to reduce youth involvement in risky behaviors may play a role in reducing mortality from accidental causes (Catalano et al. 2012; Cunningham et al. 2018; Fletcher, Bonell, and Hargreaves 2008). Broader interventions aimed at improving children and adolescents' social environments also have the potential to improve health and reduce mortality for these age groups (Viner et al. 2012).

**School Resources and Child Health and Mortality**

A large literature has examined the relationship between education and adult mortality (Balaj et al. 2024; Lleras-Muney 2005; Miech et al. 2011). A recent meta-analysis of research in 59 countries found a reduction in adult mortality risk of 1.9%, with each additional year of educational attainment ( Balaj et al. 2024). Researchers have examined whether this association is likely to be causal, and have found evidence of a plausible effect of higher educational attainment on reduced adult mortality, though the findings differ across labor market and educational contexts (Galama et al. 2018; Lleras-Muney 2005).

We know much less about the relationship between education and child mortality. Given that children and adolescents spend a significant portion of their time in school, policies that improve local educational environments may play an important role in reducing child and adolescent mortality. Yet there has been limited direct research in this area. While past research has shown that school health policies that provide vaccinations and directly target treatable health conditions reduce child mortality (Ludwig and Miller 2007), we know less about the role that other types of school resources and educational policy interventions may have on reducing child and youth mortality from other causes of death, including accidental or injury-related deaths.

In this paper, we consider whether school funding increases have the potential to reduce child and adolescent mortality. Over the past several decades, a strong consensus has emerged that increased school funding has beneficial outcomes for students. Most research on the impact of school funding has focused on estimating the academic benefits of these policies. A recent meta-analysis shows that on average, a $1,000 per pupil increase in school spending leads to a 0.03 standard deviation increase in test scores (Jackson and Mackevicius 2024). However, school funding increases have also been found to affect measures of student well-being. For instance, a recent study shows that investments in schools early in children's lives reduces their participation in future adult crime (Baron et al. 2024). The authors further find that funding increases have an intermediate effect on reducing absenteeism in middle school and also reduce juvenile detention, which are mechanisms that may explain the drop in crime (Baron et al. 2024). Comparing interventions, investing in operating expenditures, rather than capital expenditures, has a stronger effect on reducing adult crime (Baron et al. 2024). To our knowledge, past work has not examined if school funding increases also play a role in reducing child and adolescent mortality. However, school funding increases could reduce youth mortality rates by decreasing children's likelihood of participating in risky behaviors with the potential to lead to injury and death.

School funding increases could reduce child and youth mortality by increasing children's time in school. More time in school translates to less time in out-of-school contexts that could lead to dangerous behaviors (Bell, Costa, and Machin 2022). While past research has not directly examined the link between school time and child mortality, existing studies point to a possible inverse relationship. Children who spend more time in school, due to compulsory schooling laws or other similar interventions, have been shown to have better health outcomes and reduced risky health behaviors. A randomized control trial in the Dominican Republic found that an intervention that provided information to teenage boys on the long-term benefits of more schooling led them to stay in school longer and had a strong causal effect on reducing the onset of drinking (Jensen and Lleras-Muney 2012). More time spent in school has also been shown to affect other health outcomes. Using data from the Canadian census and variation in compulsory schooling laws, scholars find that education decreases teenage births at ages 17 and 18 (DeCicca and Krashinsky 2020). Investments in local education systems could also have positive externalities for later-life outcomes, given that there is a plausible causal effect of increased education on adult mortality (Galama et al. 2018; Lleras-Muney 2005).

In addition to time in school, funding increases could reduce child mortality by increasing student engagement and school connectedness. Past research provides suggestive, but not conclusive, evidence that school funding increases may affect students' connection to school, given its effect on reducing absenteeism and juvenile delinquency (Baron et al. 2024). In longitudinal survey research in Australia, children with low self-reported connectedness to school had higher risks of developing anxiety and depression, smoking, alcohol and drug use in their later teenage years and one year after secondary schooling (Bond et al. 2007). Analysis using data from the National Longitudinal Study of Adolescent to Adult Health, shows a correlation between school connectedness and fewer risky behaviors (Resnick et al. 1997).

School funding could improve student engagement by improving school climate and relationships with teachers or school staff. Interventions aimed at improving school climate have been found to reduce student drug use and improve students' subjective well-being and mental health, which could in turn, reduce deaths of despair or suicide (Case and Deaton 2015, 2020; Fletcher et al. 2008; Hong and Sullivan 2013; Marraccini and Brier 2017). Perceptions of support from teachers and school environment are connected with student engagement and feelings of hope (Van Ryzin, Gravely, and Roseth 2009). There is also an association between student ratings of teacher support and school connectedness and emotional health (Kidger et al. 2012). In addition, teachers have been found to play an important role in the early detection and reporting of child maltreatment outside of school, which can help improve child well-being (Fitzpatrick, Benson, and Bondurant 2020).

We anticipate that effects will emerge with a delay. It takes time for districts to raise and spend funds, hire new staff, and for staff to develop relationships with students and improve engagement in school. Children who experience multiple years of higher spending in a school are likely to benefit most and form stronger relationships at school. Based on previous research (Biasi 2023; Cellini, Ferreira, and Rothstein 2010; Rauscher 2020a), we expect to find delayed effects of school funding, with effects emerging several years after spending increases.

In summary, higher school funding could reduce child mortality by improving student engagement and connections at school, which reduce both the opportunity and desire to participate in risky behaviors. This suggests larger reductions in child mortality due to external causes and accidents. School funding could improve student engagement and connections by increasing the number and salaries of teachers, staff, and counselors. Because teachers play a key role in student engagement, connections, and outcomes (Lin 2010; Pedler, Hudson, and Yeigh 2020; Tao et al. 2022; Yontz and Wilson 2021), increasing the number or average salary of teachers could be two ways that funding reduces student risky behaviors and mortality. The evidence above suggests the following hypotheses, with effects emerging several years after the increase due to delays in spending:

> Hypothesis 1: School funding reduces mortality among school-age children, especially for mortality due to accidents and external causes.
>
> Hypothesis 2: School funding increases spending on potential mechanisms, including the number and average salaries of teachers, staff, and counselors.

**Heterogeneity: Where Might School Funding Matter Most?**

Child and adolescent mortality rates differ by local community contexts and individual characteristics (Cunningham et al. 2018). Black children consistently die at the highest rate throughout childhood, with mortality rates often double those of white children (CDC 2021), and poverty is related to higher mortality rates at all ages (Karb et al. 2016; Lee et al. 2013; Mannocci et al. 2019). Child mortality is also higher in rural areas compared to urban or suburban settings (Cunningham et al. 2018). Thus, children face unequal risk of mortality by race/ethnicity, income, and geography.

At the same time, growing evidence indicates that low-resource students and communities benefit more from school funding (Biasi 2023; Lafortune, Rothstein, and Schanzenbach 2018; Rauscher 2020a, 2020b; Rauscher and Shen 2022). However, most of this research examines academic outcomes and funding could provide larger benefits for marginalized students and communities partly by reducing risky behaviors or improving mental health. Given higher risk of mortality and higher potential benefits of school funding, we anticipate differences in the relationship between school funding and mortality by individual race/ethnicity and by community characteristics, including income, race/ethnicity, and geography.

Children and adolescents from lower-SES families have higher death rates, particularly accidental death rates (Mare 1982). Adolescents who have higher subjective-SES levels have better health outcomes (Quon and McGrath 2014). Existing inequalities in educational environments by socio-economic status could lead spending increases to be particularly beneficial for low-SES students (Huang et al. 2013). We examine if the effects of school funding increases on mortality rates differ between higher and lower-SES school districts.

Mortality is consistently higher for Black youth, relative to other racial/ethnic groups, and the extent of inequality varies (Cunningham et al. 2018). For example, in 2021, the county-level mortality rate by assault was over 10 times higher among Black children than white children (21.7 vs. 1.6 per 100,000 children ages 5-19; based on NVSS mortality data and SEER population estimates weighted by county population). In comparison, the mortality rate for all causes of death was 3.3 times higher for Black children than white children (83.2 vs. 24.9 per 100,000 children ages 5-19). Therefore, we also include variation by cause of death when examining heterogeneous effects of school funding on child mortality. Mortality rates are also higher among Latinx children and the ratios differ by cause of death, but the disparities and variation are not as large as those for Black children. School funding increases that improve school resources and teacher connections may be particularly beneficial for reducing Black and Latinx child mortality, especially for deaths by assault or external causes.

School funding increases may also have differential effects on reducing child and adolescent mortality rates by gender. Girls often have a stronger connection to school and higher academic performance than boys (DiPrete and Buchmann 2013), so efforts to improve boys' school engagement could be particularly beneficial to reducing risky behavior and decreasing accidental deaths. Studies examining the effects of school-level interventions on child health often focus on boys, given their higher propensity to engage in risky behaviors (e.g. Jensen and Lleras-Muney 2012). Similarly, we anticipate larger effects for boys than girls.

Finally, there may be important differences in the effect of school funding on mortality by urbanicity. Children in rural areas have higher mortality, especially from motor vehicle crashes (Cunningham et al. 2018). School funding reductions have been shown to have larger impacts on achievement in rural areas (Rauscher 2020b). A similar relationship may exist between school funding increases and youth well-being and health outcomes, such as reductions in mortality.

Hypothesis 3: School funding reduces child mortality more among students and districts with higher risk of child mortality (Black, Latinx, and male students; low-income, high-poverty, high-minority, and rural districts).

## Methods

We use a regression discontinuity (RD) approach to estimate effects of school spending on child mortality. Building on Abott and colleagues' (2020) study on school funding and student achievement, we compare districts that narrowly passed or failed a tax election to increase local revenue for operating expenses in 9 states (AR, CA, LA, MI, MO, OH, PA, TX, WI). We estimate effects of narrowly passing a tax election on county-level child mortality, as well as multiple potential mechanisms at the district-level, including spending, staffing, and instructional salaries. We test for heterogenous effects using stratified models and conduct sensitivity analyses to assess validity of the RD approach.

### Data

Mortality data are from the National Vital Statistics All County Mortality Files 1995-2020, which include information from all U.S. death certificates. The restricted-use data include county of residence, in addition to age, race, and cause of death (NCHS 1995-2020). The racial/ethnic identification is "highly accurate" with low rates of misclassification for all groups except for American Indians (Arias et al. 2008; Arias, Heron, and Hakes 2016). As the most inclusive data on mortality in the U.S., the vital statistics data provide the most accurate information about child mortality. Annual county population estimates by age and race are gathered from the U.S. Census Bureau 1995-2020 to calculate mortality rates. Sensitivity analyses use population estimates from the Surveillance, Epidemiology, and End Results Program (SEER).

Data on district tax elections are from Abott et al. (2020) and from the California Elections Data Archive (CEDA) and include elections in years 1995-2018. Abott et al. (2020:3) include elections in Arkansas, Louisiana, Michigan, Missouri, Ohio, Pennsylvania, Texas, and Wisconsin, excluding 2007-08 elections in Pennsylvania about a state tax policy change and excluding elections in districts that include multiple counties due to difficulty in accurately measuring election outcomes from county records. We add California elections from CEDA, which includes accurate results from multi-county districts. Elections data include the date, percent of votes in favor of the tax increase, the cutoff required to pass, and outcome. Main analyses estimate effects of elections in years 1995-2018. We repeat analyses limited to 2000-2015 for consistency with Abott et al.(2020) and results are similar.

Data on district characteristics are gathered for years 1995-2020 from multiple sources, including the Census Finance Survey (F-33) on spending, National Center for Education Statistics (NCES) data on district enrollment and composition, and Census Small Area Income and Poverty Estimates (SAIPE). Data on student engagement (chronic absenteeism, suspension, expulsion, arrest) are gathered from the Civil Rights Data Center (CRDC), but are only available for 6 years (every other year from 2010-2021).

**Measures**

Child mortality: For each county and year, we calculate the total number of deaths among children ages 5-19. This age range is comparable with other research and age categories used in CDC reports (CDC 2021; IOM 2003). While district-level mortality information would be preferable, county is the finest level of geography where these statistics are produced. We link county-level child mortality with district-level information, which reduces the precision of our estimates. Using ICD-10 codes (ICD-9 before 2003) and cause of death categories, we create separate measures of child mortality due to all causes, external causes (excluding mortality due to internal health-related causes), deaths of despair (drug, alcohol, or suicide; Case and Deaton 2015, 2020), motor vehicle accidents, and all other accidents. We calculate child mortality separately by race/ethnicity using the following mutually exclusive categories: non-Hispanic white, non-Hispanic Black, and Hispanic. In sensitivity analyses, we also examine deaths among school-age children ages 5-18 and among high-school-age children ages 14-18, who are involved in more risky behaviors (Cunningham et al. 2018). County child mortality rates are calculated using annual county-level Census population estimates for all children and by age and race/ethnicity (U.S. Census Bureau 1995-2020; https://www2.census.gov/programs-surveys/popest/datasets/2010-2020/counties/). We prefer the count measures of child mortality because the denominators to calculate mortality rates are estimates and add measurement error.

Tax elections: In the tax election data, the treatment is an indicator for passing a proposed tax increase. We use the proportion of votes in favor of the proposed tax increase to calculate voteshare centered at the pass cutoff, which is the running variable in our RD analysis.

District spending: Annual spending measures per pupil are calculated from F-33 data for total current spending and for spending on instruction, salaries, instructional salaries, benefits, support services, and capital. All currency is adjusted for inflation to 2020 dollars based on the academic fiscal year using Consumer Price Index data from the St. Louis Federal Reserve Bank (FRED) data repository (Candelaria and Shores 2020).

District staff: From NCES data, we measure the number of full-time equivalent teachers, total staff, counselors, instructional aides, support staff, and administrators. For each of these measures, we calculate the number of students per staff member (e.g., the number of students per counselor). We also calculate average salary per staff person and average instructional salary per teacher.

Student engagement: Using school-level CRDC data, we calculate the proportion of students who are chronically absent, suspended out-of-school multiple times, expelled, and arrested in each district and year. These measures provide information about potential mechanisms, but analyses of these measures are based on fewer elections and observations because CRDC data are only available for six years (2010, 2012, 2014, 2016, 2018, 2021).

Controls: To address potential variation in district characteristics before the election, we include the same controls used by Abott et al. (2020): the proportions of students who are Latinx, Black, and eligible for free/reduced price lunch in the year before the election, current spending per pupil in years 1 and 3 before the election (logged), and enrollment in years 1 and 3 before the election (logged). To increase precision, we also control for child mortality (the dependent variable) in year 3 before the election. Year indicates spring of the academic year throughout the paper.

**Analyses**

As in previous research (Abott et al. 2020; Cellini et al. 2010; Rauscher 2020a), we use a dynamic regression discontinuity (RD) approach to examine effects of narrowly passing a proposed tax increase over time since the election. Comparing districts that narrowly pass or fail a tax election, RD provides a causal estimate of the treatment effect among otherwise similar districts (Lee and Lemieux 2010). Limiting analyses to a narrow window on either side of the pass cutoff, this approach assumes that meaningful unobserved differences between districts are eliminated and that other covariates related to the outcome vary continuously over the forcing variable, which is controlled in the regression (Lee and Lemieux 2010:287). Districts that pass and fail tax elections may differ, but we limit analyses to a narrow window on both sides of the cutoff to include districts that should be similar, except for observed (and controlled) differences in the forcing variable.

Based on evidence of delayed spending benefits and following previous research (Rauscher 2020; Cellini et al. 2010), we estimate effects of passing a tax increase in years before and after the election. We create stacked panel data around each individual election, including 5 years before and 10 years after the election (*t*-5 to *t*+10), into one dataset. These data can include multiple observations of the same district-years if districts hold multiple elections. To allow for delayed effects that emerge after multiple years of increased spending, we estimate separate effects before the election (where effects should be null) and in 5-year periods after the election. Specifically, we fit separate models to estimate effects of passing an election on child mortality in years 0-5 before the election, years 1-5 after the election, and years 6-10 after the election.

We use the same bandwidth as Abott et al. (2020) and limit the sample to districts within 10 percentage points of the cutoff required to pass an election. We assess robustness to other bandwidths from one to 15 percentage points on either side the cutoff. We use negative binomial regression models because the dependent variable (child mortality) is an over-dispersed count variable where the variance exceeds the mean (Browning et al. 2021; Long and Freese 2006; Olzak 2021).

Equation 1 predicts the number of child deaths in the county for district *i* in calendar year *t* with an indicator for whether the election passed, voteshare centered at the pass cutoff, an interaction between pass and voteshare to allow the relationship to vary, fixed effects for each calendar year ($\mu_t$), controls ($X_{it}$) for pre-election characteristics (percent Latinx, Black, and eligible for free/reduced price lunch, log spending per pupil, log enrollment, child mortality), and log of the county child population (to address unequal exposure):

$$\text{Child Mortality}_{it} = exp(\beta_1 \text{Pass}_{it} + \beta_2 \text{Voteshare}_{it} + \beta_3 \text{Pass*Voteshare}_{it} + \mu_t + \beta_k \text{X}_{it} + ln(\text{Population}_{it}) + \varepsilon_{it}) \quad (1)$$

Standard errors are robust to heteroskedasticity and adjusted for clustering at the election level. Tests of overdispersion are significant and suggest that negative binomial models improve model fit compared to poisson models, which would underestimate the standard errors (Cameron and Trivedi 1986; Long and Freese 2006).

The coefficient of interest ($\beta_1$) estimates the effect of narrowly passing a tax election on child mortality, accounting for differences in voteshare, exposure (child population), pre-election differences between districts on multiple characteristics, and changes over time. We report estimates from models limited to years 0-5 before the election and to years 1-5 and 6-10 after the election. Estimates should be null before the election. Negative $\beta_1$ coefficients in years after the election would support *Hypothesis 1* and suggest higher school funding reduced child mortality.

We use a similar approach to test *Hypothesis 2* and estimate effects on potential mechanisms, including the number and average salaries of teachers and staff. Because these outcome measures have a distribution that is closer to normal, we use OLS models to predict potential mechanisms. To examine heterogeneous effects, we create indicators for whether districts are above the median values of spending per pupil, enrollment, poverty rate, and percent of students who are Black, Latinx, white, and eligible for free/reduced-price lunch in the year before the election. To test *Hypothesis 3*, that school funding reduces child mortality more among students and districts with higher risk of child mortality, we repeat analyses when limiting the sample to districts with high and low pre-election values for each of those measures and test for significant differences between $\beta_1$ coefficients from those separate models (Clogg, Petkova, and Haritou 1995). For example, to test for different effects of passing a tax election on child mortality 6-10 years after the election, we calculate z statistics ($z = (\beta_H - \beta_L)/\sqrt{SE_H^2 + SE_L^2}$), where $\beta_H$ indicates $\beta_1$ when predicting mortality in high-poverty districts and $\beta_L$ indicates $\beta_1$ from Equation 1 when predicting mortality in low-poverty districts. Negative and significant z statistics would support *Hypothesis 3* and suggest that school funding reduces child mortality more in high-poverty districts with higher risk of child mortality.

**Sensitivity Analyses**

We conduct sensitivity analyses to assess robustness and validity of the RD approach.

<u>Robustness</u>: First, results may differ by the specific measure of child mortality. We repeat analyses using alternative measures, including deaths among children ages 5-18, 14-18, and rate measures. We also examine variation by various causes of death, including all external causes,

accidents, and deaths of despair. Second, results may differ depending on the specific model used. We repeat analyses using zero-inflated negative binomial models and using ordinary least squares regression models to predict child mortality rates. We also repeat analyses without including pre-treatment controls and varying the bandwidth of voteshare around the cutoff included in analyses.

Validity of the Regression Discontinuity Approach: We assess the extent to which this application satisfies the RD standards developed by What Works Clearinghouse (2022), including integrity of the forcing variable, low sample attrition, continuity of the relationship between the forcing variable and the outcome, and functional form and bandwidth selection. We conduct conventional and robust density tests, which are both not statistically significant and consistent with the statistical integrity of the forcing variable (see Figure A1; McCrary 2008). Consistent with institutional integrity, district tax elections require support from a fixed proportion of voters to pass, the pass cutoff is fixed prior to the election, votes are collected and recorded by county election offices, and districts have no opportunity to change votes.

Given the use of district-level data, sample attrition is low. The total attrition rate for district-year observations missing any variable in the analysis in any year post-election is 3.2%, ranging from 2.8%-4.0% for observations in any year from 1-10 years post-election. Attrition rates are slightly higher in districts that passed an election (3.6% vs 2.6%). We examine the consistency of our results in the full panel and find substantively similar results.

We examine continuity of the outcome across the distribution of the forcing variable. Figure A2 does not indicate discontinuities in the outcome-forcing variable relationship at values of the forcing variable other than at the cutoff. We also examine and test for differences in pre-election covariates at the cutoff of the forcing variable. Table A1 shows estimated differences between district that passed or failed the tax election on multiple district measures one and three years before the election. Estimates show limited pre-election differences by election outcome. The only significant difference is in salary spending per pupil 3 years before the election (one out of 82 tests of difference).

The main analyses use a sample within a predetermined bandwidth of voteshare (following Abott et al. 2020). We assess robustness to varying the bandwidth and choice of functional form for the forcing variable. We also conduct placebo checks by assigning false pass cutoffs at points above and below the actual cutoff required to pass. Specifically, we estimate effects at the median value of voteshare above and below the true cutoff. These estimates (shown in Appendix Table A5) are null and provide further evidence of continuity in the outcome-forcing variable relationship at values of the forcing variable other than the pass cutoff.

## Results

Descriptive statistics for district-year observations after the election are provided in Table 1. Estimates in the first two columns are weighted by county child population (ages 5-19), while estimates in the other columns are unweighted. The mean child mortality rate (ages 5-19) in the sample is 23.9 deaths per 100,000. For comparison, the U.S. child mortality rate among children (ages 5-19) in 2019 was 25.4 per 100,000 (CDC 2024). Comparing mean values by election outcome, districts that passed have a larger number of child deaths in the county, but also have larger county populations, on average. County child mortality rates are slightly lower among districts that passed the election, but this difference is only statistically significant for deaths of despair (suicide, drug, or alcohol related deaths). District demographics are relatively similar by election outcome. The share of students who are Black and total spending per pupil are slightly higher in districts that passed the election ($p<0.05$), but the proportion of students who are Latinx and the proportion of students who are eligible for free/reduced-price lunch are not significantly different by election outcome. We control for pre-election district characteristics to address potential variation.

Table 2 shows estimated effects of narrowly passing a tax election on child mortality by time since the election. Using negative binomial regression models to predict the number of child deaths in the county, estimates consistently suggest no significant differences between districts that narrowly passed or failed a tax election in years 0-5 before the election or in years 1-5 after the election. However, in years 6-10 after the election, estimates show significantly lower child mortality in districts that narrowly passed a tax election to increase school funding (see Figures 1 and 2). Compared to districts that failed an election, districts that passed had 4% fewer child deaths in the county. At the mean of 16.8 annual deaths per county-year, this amounts to nearly 1 less child death per county each year (16.8*0.04=0.67). The coefficient predicting deaths due to all external causes is negative but not significant; however, reductions in deaths of despair, in deaths due to accidents, and in deaths due to motor vehicle accidents are statistically significant, with reductions of 5.2%, 7.1%, and 7.6% respectively. These estimates are consistent with *Hypothesis 1* and suggest lower child mortality with increased school funding. The timing is consistent with prior evidence of delayed benefits of school spending for children (Rauscher 2020).

The main analyses are limited to districts within 10% of the voteshare required to pass. We repeat analyses when varying the bandwidth of voteshare around the cutoff from one to 15 percentage points. We also repeat analyses when using the optimal bandwidth identified for each dependent variable by rdbwselect in Stata (Calonico, Cattaneo, and Farrell 2021). These estimates are shown in Appendix Figures A3 and A4 and consistently suggest lower child mortality from passing a proposed tax increase.

Mechanisms: Table 3 shows estimated effects of narrowly passing a tax election on potential mechanisms. OLS regression models predict each potential mechanism with the same controls as estimates in Table 2. Narrowly passing a tax election increased total current spending by about $243 per pupil in years 1-5 post-election. Examining categories of spending, we find significant increases in per pupil spending on instruction ($105), salaries ($149), instructional salaries ($77), and support services ($137). In contrast, we find no effects on capital or benefits spending.

Consistent with *Hypothesis 2*, estimates in Table 3 also show that narrowly passing a tax election increased the number of teachers, counselors, and administrative staff, and increased average teacher salaries. On average, passing a tax election increased the average number of full-time teachers and administrative staff by 3 and the number of counselors by almost 1. The average teacher salary increased by $952. Despite an increase in number of staff, we find no effects on staff per pupil. The number of students per counselor decreased after passing a tax election (-32.7 in years 6-10 after the election), but the decline was not significant at the 95% level.

Comparing estimates by time from the election, effects on staffing and spending emerge in years 1-5 after the election, before the effects on child mortality. These results suggest that investments in these school resources have delayed effects on child mortality, as children develop relationships and attachment to school over time. Estimates in years before the election suggest few pre-election differences in staffing or spending, supporting the effectiveness of the

RD approach in this setting. The number of counselors is slightly higher before the election in districts that passed a tax election, but this is just one significant difference out of 19 tests and could have happened by chance.

To better understand mechanisms, we conduct mediation estimates using paramed in Stata (Emsley and Hanhua 2013). Mediation estimates indicate that spending on instructional salaries and average teacher and staff salaries are mechanisms for effects of school spending on child mortality. Figure A5 shows that spending on instructional salaries mediates approximately 4% of the effect on child mortality, and average teacher and staff salaries mediate approximately 5% and 7% of the effect on child mortality, respectively.

The bottom rows of Table 3 show estimated effects on measures of student engagement based on data from CRDC for limited years. Narrowly passing a tax election reduced the rate of chronic absenteeism by 1.4 percentage points and the rate of multiple out-of-school suspension by 0.3 percentage points in years 6-10 after the election. The rate of student arrests also declined slightly, by 0.05 percentage points, in years 1-5 after the election. These estimates should be interpreted with caution, because they are based on limited years and smaller sample sizes. However, the estimates are consistent with school funding impacting child mortality partly through student engagement at school.

<u>Heterogeneous Effects</u>: Table 4 provides estimates by district and child characteristics. We fit models separately among districts that were above and below the median value for each district measure in the year before the election. The estimates for the subgroups show many similar effects to the overall sample, with negative point estimates for all the categories. We test for significant differences between coefficients using z-tests (Clogg et al. 1995). Due to higher

risk of child mortality, we expected to find larger effects in districts with high proportions of low-income, Black, or Latinx students. In fact, almost no estimates differ significantly by district income or racial/ethnic composition. Figure 3 shows estimates by district poverty rate. While the point estimates for the effects of spending on accidental deaths and motor vehicle deaths are somewhat greater in magnitude in high poverty vs. low poverty districts, these differences are not significantly different at traditional levels. Among all of the tests for heterogeneity of effects, none differ significantly at the 95% level. In only one case do estimates differ significantly at the 90% level: when predicting child deaths due to motor vehicle accidents, passing a tax election reduced motor vehicle deaths by about 12% in high-Latinx districts, compared to 1% in low-Latinx districts. Coefficients also do not differ by pre-election spending or by rural/non-rural geography. Figure A6 shows estimates by district free/reduced-price lunch eligibility, racial and ethnic composition, spending, and geography.

We also expected to find larger effects on Black, Latinx, and male child mortality. However, we find no significant differences between coefficients predicting mortality by child race or sex. Results are not consistent with *Hypothesis 3* and suggest consistent effects on child mortality by district and child characteristics.

Sensitivity Analyses: To assess robustness to alternative measures of child mortality, we repeat analyses predicting deaths among children ages 5-18 and 14-18 (Appendix Table A2) and find consistent patterns of results across these age groups. To assess robustness to the model, we repeat analyses using zero-inflated negative binomial models, using ordinary least squares regression models to predict child mortality rates, and without including pre-treatment controls. We also allow the functional form of the forcing variable to vary. Specifically, we repeat the main analyses when controlling for voteshare squared and cubed, and their interactions with the pass indicator. These estimates are shown in Appendix Table A3. In some cases, the precision of the estimates is lower, especially when predicting mortality rates, which is expected because the denominators are estimated and introduce additional error and because the distribution is skewed. Despite lower precision, the results are consistent using these alternative measures and models. We also find consistent results when excluding districts with low enrollment (≤400), elections with few votes (≤200), and districts observed fewer than 15 years in the data (Appendix Table A4). Placebo checks at false pass cutoffs (above and below the actual cutoff) indicate null effects (Appendix Table A5), which suggests continuity in child mortality at values other than the pass cutoff and increases confidence in our approach.

## Conclusion

Child mortality has grown in recent years. Yet we have little evidence on whether school spending can affect this key measure of child wellbeing. We use close school district tax elections to estimate effects of a quasi-random increase in school spending on county child mortality. We find consistent evidence that increased school spending, from passing a tax election, reduces child mortality. Districts that narrowly passed a proposed tax increase spent an additional $243 per pupil, mostly on instruction and salaries, and had 4% lower child mortality. This increased spending reduced child deaths of despair (due to suicide, drugs, or alcohol) by 5% and child deaths due to accidents and motor vehicle accidents by 7%. If spending were increased more, by $1,000 per pupil, this implies a 16% reduction in child mortality (0.04*4=0.16) or nearly 3 fewer child deaths per county at the mean of 16.8 annual child deaths per county

(16.8*0.16=2.69). Saving even one child can have substantial benefits to that child and their family, as well as community spillover effects.

We find that these effects on child mortality emerge 6-10 years after an election. These results are consistent with prior research that finds delayed effects of school funding increases on student-level outcomes (Rauscher 2020a). These results speak to the importance of evaluating the effects of school funding increases over a longer time frame, to build in time for new expenditures to take effect.

Estimates predicting potential mechanisms suggest that lower child mortality could partly reflect increases in the number of teachers, counselors, administrative staff, and teacher salaries. Passing a tax election increased the average number of full-time teachers, administrative staff, and counselors, and increased average teacher salary by $952 in the first 5 years after the election. The growth in staffing and salaries are reflected in increases in per pupil spending on instruction, salaries, instructional salaries, and support services. Mediation estimates are consistent with this evidence and indicate that teacher and staff salaries are mechanisms for effects of school spending on child mortality. Together, this evidence suggests the importance of relationships at school for reducing child mortality.

Despite limited years of available data, estimates also suggest that the effects on child mortality may reflect improved student engagement at school. Passing a tax election reduced the rate of chronic absenteeism, out-of-school suspension, and student arrests. Combined with the results predicting staffing and salaries, these results are consistent with the theory that school spending reduces child mortality by improving student relationships and engagement at school, which reduces the opportunity for risky behaviors (Baron et al. 2024; Bond et al. 2007; Resnick et al. 1997). Student relationships and engagement could also improve student mental health and explain the reduction in deaths of despair (Case and Deaton 2015, 2020; Fletcher et al. 2008; Hong and Sullivan 2013; Marraccini and Brier 2017).

We expected to find larger benefits of school spending for children and districts with higher risk of child mortality. Instead, we find generally consistent results by child race, by district geography, and by district student income and racial composition. This consistency suggests that school spending has similar benefits for child mortality by student race and across district characteristics. Because mortality is higher and recently increased more among Black children (Cunningham et al. 2018; Woolf et al. 2023), our results suggest that targeted investments to improve school spending for Black children and in areas with high proportions of Black children could most effectively reduce child mortality overall and for Black children in particular.

Limitations of this study include reliance on county-level mortality data and limited measures of student engagement or other potential mechanisms. Predicting county-level child mortality reduces precision and could underestimate district-level effects of school spending. In addition, we examine child mortality, but school spending may also have longer-term effects on mortality. Future research could usefully examine district-level child mortality and longer-term effects of school spending on adult mortality (Galama et al. 2018; Lleras-Muney 2005). Qualitative or mixed methods research would be valuable to provide more detailed information about how school spending reduces child mortality. This study includes data from nine states, which improves on single-state analyses, but future work examining effects of school spending in all states would improve external validity.

This study contributes to evidence that the benefits of school spending go beyond academic and economic outcomes to improve child well-being (Baron et al. 2024). By reducing child mortality, school spending provides important individual and social benefits. If state and local policymakers focus only on educational or economic returns, they underestimate the full benefits of school spending.

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## Tables and Figures

Table 1: Descriptive Statistics

| | Weighted | | Unweighted | | Election Outcome | | |
|---|---|---|---|---|---|---|---|
| Variable | Mean | Std Dev | Mean | Std Dev | Failed | Passed | Difference |
| County | | | | | | | |
| Child Deaths (ages 5-19) | 107.91 | 153.56 | 16.82 | 38.51 | 16.16 | 17.34 | * |
| Child Deaths of Despair | 15.89 | 20.77 | 3.19 | 5.94 | 3.07 | 3.29 | * |
| Child External Deaths | 65.56 | 92.84 | 10.68 | 23.69 | 10.28 | 10.99 | * |
| Child Accidental Deaths | 10.96 | 15.20 | 2.04 | 4.23 | 1.95 | 2.11 | * |
| Child Motor Vehicle Acc Deaths | 17.88 | 26.30 | 3.05 | 6.58 | 2.96 | 3.13 | * |
| Child Death Rate (ages 5-19, per 100,000) | 23.93 | 8.43 | 27.07 | 17.56 | 27.17 | 26.98 | |
| Child Deaths of Despair Rate | 4.54 | 3.31 | 5.69 | 6.89 | 5.77 | 5.62 | * |
| Child External Death Rate | 15.19 | 6.98 | 17.84 | 15.40 | 17.94 | 17.77 | |
| Child Accidental Death Rate | 2.90 | 2.70 | 3.79 | 7.15 | 3.81 | 3.77 | |
| Child Motor Vehicle Acc Death Rate | 4.35 | 3.99 | 7.01 | 10.81 | 7.10 | 6.94 | |
| Child Population (ages 5-19) | 490272.30 | 697491.70 | 70281.93 | 171809.50 | 66237.24 | 73496.11 | * |
| District | | | | | | | |
| % Free/Reduced-Price Lunch | 0.32 | 0.24 | 0.37 | 0.21 | 0.37 | 0.37 | |
| % Black Students | 0.12 | 0.21 | 0.08 | 0.17 | 0.07 | 0.08 | * |
| % Latinx Students | 0.13 | 0.19 | 0.06 | 0.12 | 0.06 | 0.07 | |
| Total Current Spending/Pupil (1,000s) | 12.01 | 2.48 | 11.68 | 2.23 | 11.54 | 11.79 | * |
| Enrollment | 6585.21 | 7886.42 | 3334.61 | 5138.25 | 3301.30 | 3361.05 | |
| Year | 2013.24 | 4.37 | 2013.13 | 4.32 | 2012.86 | 2013.33 | * |
| Passed | 0.58 | 0.49 | 0.56 | 0.50 | 0.00 | 1.00 | n/a |
| Voteshare Centered at Pass Cutoff (%) | 0.95 | 5.43 | 0.76 | 5.44 | -4.44 | 4.89 | * |
| N District-Years | 38,108 | | 38,108 | | 16,874 | 21,234 | |

Summary statistics of district-year observations 1-10 years after a tax election held in years 1995-2018 with voteshare within 10 percentage points of the pass cutoff. The first two columns are weighted by county child population ages 5-19; other columns are unweighted. Two-tailed t-tests indicate whether the mean difference between observations by election outcome is significant: * p<0.05.

Table 2: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality

| | Before Election | After Election | |
|---|---|---|---|
| Dependent Variable | Year -5-0 | Years 1-5 | Years 6-10 |
| Deaths (ages 5-19) | -0.0200 | -0.0100 | -0.0379* |
| | (0.0130) | (0.0145) | (0.0175) |
| alpha | 0.0300 | 0.0335 | 0.0404 |
| Deaths of Despair | -0.0174 | -0.0199 | -0.0516* |
| | (0.0241) | (0.0232) | (0.0256) |
| alpha | 0.0565 | 0.0552 | 0.0512 |
| External Deaths | -0.0208 | -0.0183 | -0.0408 |
| | (0.0172) | (0.0181) | (0.0210) |
| alpha | 0.0584 | 0.0601 | 0.0670 |
| Accidental Deaths | -0.0337 | 0.0059 | -0.0708* |
| | (0.0259) | (0.0280) | (0.0313) |
| alpha | 0.1033 | 0.0908 | 0.0999 |
| Motor Vehicle Accidental Deaths | -0.0257 | -0.0229 | -0.0757* |
| | (0.0280) | (0.0295) | (0.0342) |
| alpha | 0.1666 | 0.1658 | 0.1665 |
| N District-Years | 26478 | 21603 | 16505 |

Coefficients are from negative binomial regression models predicting county child mortality. Sample includes district-year observations with a tax election 1995-2018 and voteshare within 10 percentage points of the pass cutoff. Models are fit separately among observations 0-5 years before the election (column 1), 1-5 years after the election (column 2), and 6-10 years after the election (column 3). All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year 3 before election).

Robust standard errors adjusted for district election clustering in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Alpha measures overdispersion compared to poisson distribution and likelihood-ratio tests of all models indicate overdispersion ($p<0.01$).

Table 3: Estimated Effect of Narrowly Passing a Tax Election on Potential Mechanisms

| | Before Election | After Election | |
|---|---|---|---|
| Dependent Variable | Years -5-0 | Years 1-5 | Years 6-10 |
| FTE Teachers | -0.2196 | 3.1861* | 3.0872 |
| | (0.5692) | (1.4467) | (2.1556) |
| Total Staff | -0.9886 | 6.6399 | 8.3296 |
| | (1.2838) | (3.4599) | (5.0376) |
| Counselors | 0.1778* | 0.7085*** | 0.3063 |
| | (0.0717) | (0.1745) | (0.2810) |
| Administrators | 0.0229 | 0.5518 | 0.1978 |
| | (0.1860) | (0.4052) | (0.5882) |
| Administrative Staff | -0.1472 | 2.9513* | 0.8811 |
| | (0.7862) | (1.3356) | (1.7086) |
| Students per Teacher | 0.0106 | -0.0077 | -0.0527 |
| | (0.1131) | (0.1086) | (0.1249) |
| Students per Staff | 0.0221 | -0.0530 | -0.0393 |
| | (0.0579) | (0.0700) | (0.0938) |
| Students per Counselor | -4.3602 | -8.2247 | -32.7408 |
| | (9.7806) | (16.0462) | (22.4326) |
| Students per Administrator | 0.9931 | 2.3002 | -1.6034 |
| | (2.1021) | (7.4085) | (6.3042) |
| Students per Admin Staff | -0.3980 | 4.5813 | 11.4329 |
| | (2.3515) | (2.8859) | (13.3049) |
| Instructional Salary per Teacher | 0.2085 | 0.9515** | 0.9557 |
| | (0.1673) | (0.3639) | (0.5126) |
| Salary per Staff | 0.2850 | 0.3984 | 0.6819 |
| | (0.1619) | (0.3543) | (0.4998) |
| Total Current Spending/Pupil (1,000s) | 0.0211 | 0.2428*** | 0.0314 |
| | (0.0154) | (0.0607) | (0.0777) |
| Instructional Spending/Pupil | 0.0168 | 0.1054** | 0.0240 |
| | (0.0112) | (0.0332) | (0.0498) |
| Salary Spending/Pupil | 0.0107 | 0.1487*** | 0.0725 |
| | (0.0102) | (0.0299) | (0.0455) |
| Instructional Salary Spending/Pupil | 0.0038 | 0.0771*** | 0.0494 |
| | (0.0071) | (0.0200) | (0.0299) |
| Benefits Spending/Pupil | -0.0067 | 0.0232 | -0.0379 |
| | (0.0069) | (0.0192) | (0.0280) |
| Support Services Spending/Pupil | 0.0043 | 0.1365** | 0.0229 |
| | (0.0096) | (0.0418) | (0.0403) |
| Capital Spending/Pupil | 0.0943 | -0.0445 | 0.0948 |
| | (0.0648) | (0.1009) | (0.1186) |
| N District-Years | 26424 | 21526 | 16490 |
| CRDC Measures | | | |

| | | | |
|---|---|---|---|
| Chronic Absenteeism/Pupil | -0.0021 | -0.0041 | -0.0142* |
| | (0.0030) | (0.0056) | (0.0062) |
| Multiple Out-of-School Suspension/Pupil | 0.0012 | -0.0019 | -0.0030* |
| | (0.0013) | (0.0012) | (0.0013) |
| Expulsion/Pupil | 0.0003 | -0.0003 | -0.0002 |
| | (0.0004) | (0.0002) | (0.0002) |
| Arrest/Pupil | 0.0005 | -0.0005* | 0.0003 |
| | (0.0006) | (0.0002) | (0.0002) |
| N District-Years | 3398 | 6442 | 6433 |

Coefficients are from OLS regression models predicting each dependent variable. Sample includes district-year observations with a tax election 1995-2018 and voteshare within 10 percentage points of the pass cutoff. Models are fit separately among observations 0-5 years before the election (column 1), 1-5 years after the election (column 2), and 6-10 years after the election (column 3). All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and (except for CRDC measures) the dependent variable (year 3 before election). Models predicting CRDC measures do not control for the lagged dependent variable due to limited years available.

Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table 4: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality by District and Child Characteristics

| Variation by: | All Deaths | Deaths due to: Despair | External | Accident | Motor Vehicle Acc |
|---|---|---|---|---|---|
| Income | | | | | |
| Low FRL (High-Income) | -0.0461* | -0.0369 | -0.0426 | -0.0585 | -0.0251 |
| | (0.0225) | (0.0335) | (0.0279) | (0.0391) | (0.0425) |
| High FRL (Low-Income) | -0.0296 | -0.0713 | -0.0450 | -0.0957 | -0.1335* |
| | (0.0255) | (0.0378) | (0.0297) | (0.0493) | (0.0533) |
| z-test of difference | -0.49 | 0.68 | 0.06 | 0.59 | 1.59 |
| Poverty | | | | | |
| Low Poverty | -0.0427* | -0.0485 | -0.0429 | -0.0564 | -0.0427 |
| | (0.0213) | (0.0303) | (0.0260) | (0.0387) | (0.0421) |
| High Poverty | -0.0413 | -0.0656 | -0.0483 | -0.1054* | -0.1256* |
| | (0.0252) | (0.0379) | (0.0293) | (0.0475) | (0.0504) |
| z-test of difference | -0.04 | 0.35 | 0.14 | 0.80 | 1.26 |
| % Black | | | | | |
| Low % Black | -0.0363 | -0.0060 | -0.0637* | -0.0132 | -0.0852 |
| | (0.0245) | (0.0401) | (0.0276) | (0.0502) | (0.0519) |
| High % Black | -0.0343 | -0.0725* | -0.0293 | -0.1071** | -0.0599 |
| | (0.0235) | (0.0324) | (0.0279) | (0.0389) | (0.0446) |
| z-test of difference | -0.06 | 1.29 | -0.88 | 1.48 | -0.37 |
| % Latinx | | | | | |
| Low % Latinx | -0.0307 | -0.0351 | -0.0277 | -0.0808 | -0.0134 |
| | (0.0209) | (0.0305) | (0.0245) | (0.0423) | (0.0425) |
| High % Latinx | -0.0382 | -0.0604 | -0.0455 | -0.0674 | -0.1226* |
| | (0.0261) | (0.0354) | (0.0317) | (0.0421) | (0.0485) |
| z-test of difference | 0.22 | 0.54 | 0.44 | -0.22 | 1.69 |
| Spending | | | | | |
| Low Spending | -0.0330 | -0.0466 | -0.0332 | -0.0525 | -0.0649 |
| | (0.0241) | (0.0411) | (0.0297) | (0.0461) | (0.0485) |
| High Spending | -0.0585* | -0.0614* | -0.0637* | -0.1048** | -0.0970* |
| | (0.0237) | (0.0306) | (0.0276) | (0.0404) | (0.0462) |
| z-test of difference | 0.75 | 0.29 | 0.75 | 0.85 | 0.48 |
| Geography | | | | | |
| Rural | -0.0256 | -0.0200 | -0.0427 | 0.0137 | -0.0279 |
| | (0.0270) | (0.0450) | (0.0339) | (0.0623) | (0.0583) |
| Non-Rural | -0.0417* | -0.0530 | -0.0391 | -0.0838* | -0.0845* |
| | (0.0210) | (0.0296) | (0.0252) | (0.0356) | (0.0399) |
| z-test of difference | 0.47 | 0.61 | -0.09 | 1.36 | 0.80 |
| Child Race | | | | | |
| Black | -0.0231 | -0.0713 | -0.0042 | -0.1306* | -0.0643 |

| | | | | | |
|---|---|---|---|---|---|
| | (0.0255) | (0.0479) | (0.0308) | (0.0550) | (0.0389) |
| Latinx | -0.0438 | | | -0.0654 | -0.0791 |
| | (0.0312) | | | (0.0651) | (0.0629) |
| White | -0.0359* | -0.0341 | -0.0529** | -0.0455 | -0.0757* |
| | (0.0170) | (0.0230) | (0.0204) | (0.0320) | (0.0342) |
| B-W z-test of difference | 0.42 | -0.70 | 1.32 | -1.34 | 0.22 |
| L-W z-test of difference | -0.22 | | | -0.27 | -0.05 |
| Child Sex | | | | | |
| Male | -0.0408* | -0.0550 | -0.0397 | -0.0811* | -0.0925* |
| | (0.0192) | (0.0292) | (0.0224) | (0.0344) | (0.0376) |
| Female | -0.0338 | -0.0488 | -0.0406 | -0.0578 | -0.0730 |
| | (0.0205) | (0.0361) | (0.0267) | (0.0463) | (0.0410) |
| z-test of difference | -0.25 | -0.13 | 0.03 | -0.40 | -0.35 |

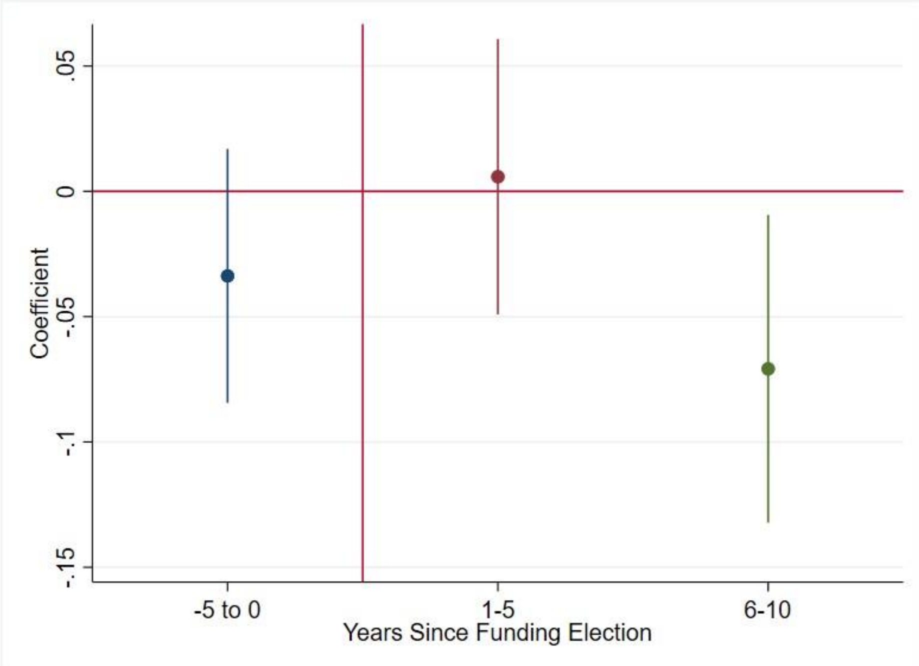
Coefficients are from negative binomial regression models predicting county child mortality. Sample includes district-year observations with a tax election 1995-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year 3 before election).

Models are fit separately among districts above and below the median value of district characteristics in the year before the election. Models by child race and sex predict separate measures of mortality by child race and sex. Models predicting two Latinx child mortality measures do not reach convergence.

Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

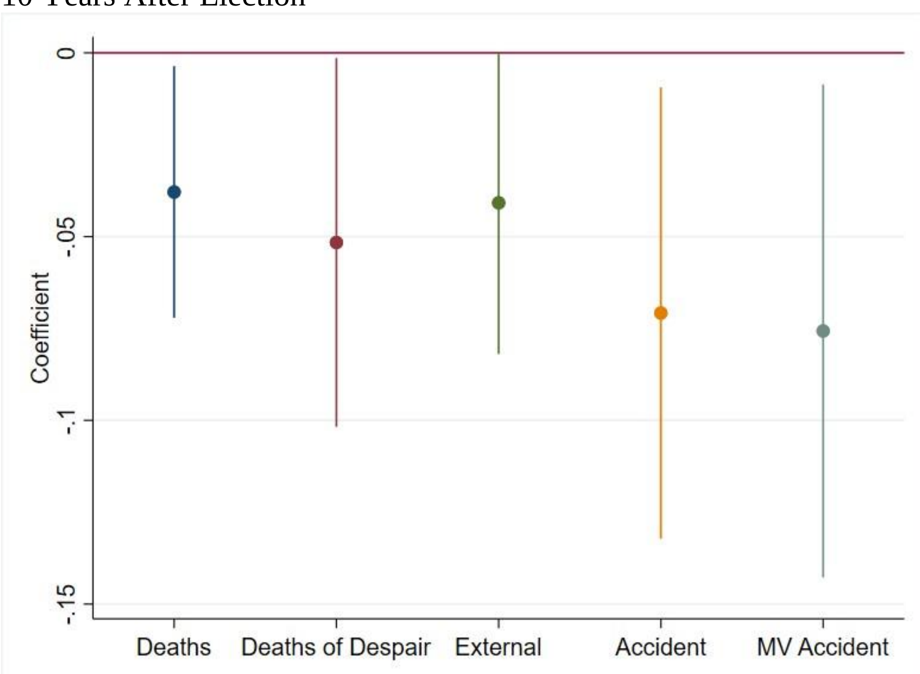
Shaded cells indicate significant difference between coefficients, p<0.10; $z = (\beta_H - \beta_L)/\sqrt{SE_H^2 + SE_L^2}$ (Clogg et al. 1995).

Figure 1: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality Due to Accidents, by Years Since Election



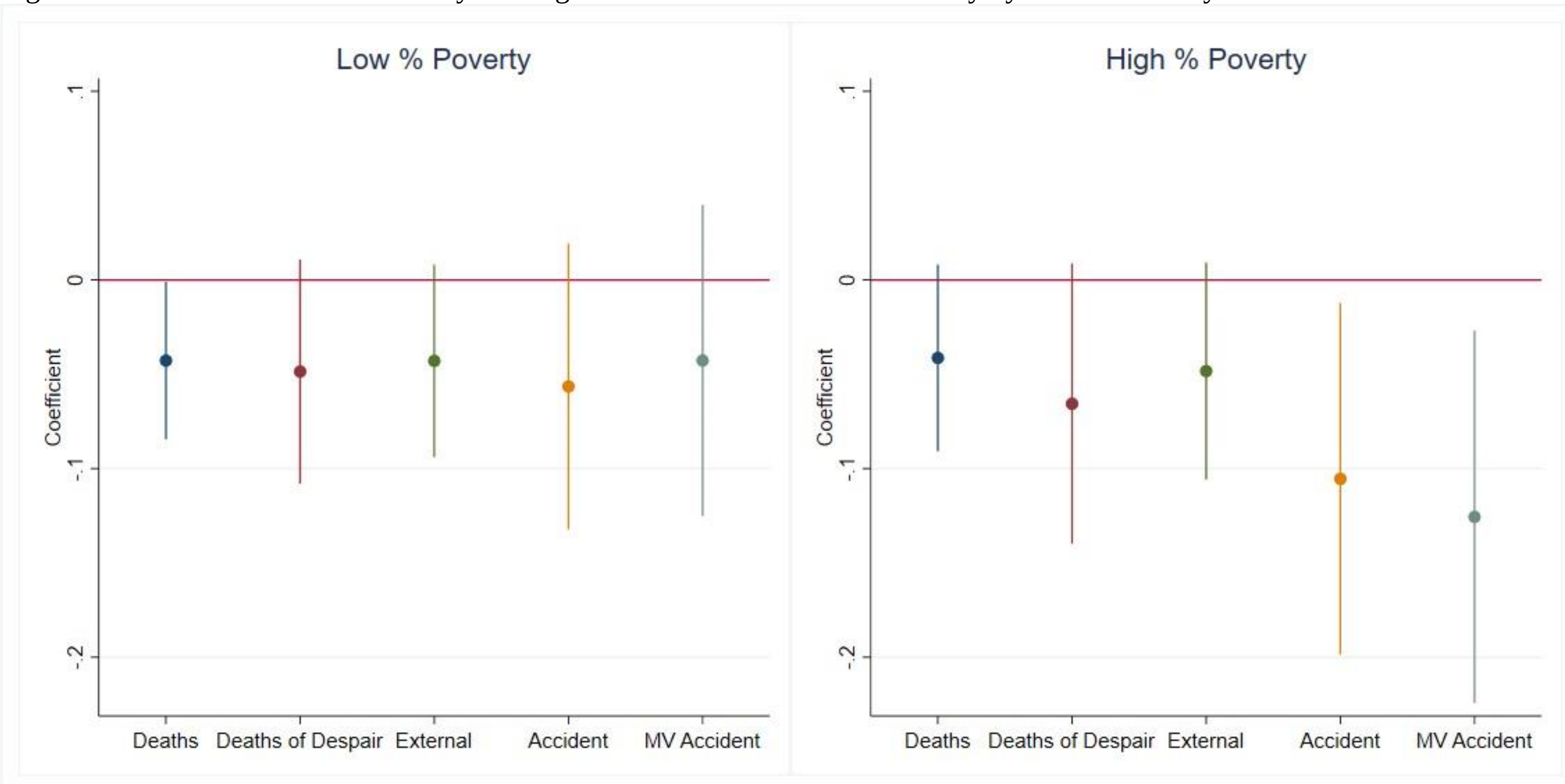

Coefficients shown in Table 2.

Figure 2: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality by Cause, 6-10 Years After Election




Coefficients shown in Table 2.

Figure 3: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality by District Poverty




Coefficients shown in Table 4.

**Appendix**

Table A1: Estimated Pre-Election Differences by Tax Election Outcome

| | 1 Year Before Election | | 3 Years Before Election | |
|---|---|---|---|---|
| Variable | Pass Coefficient | N district-Years | Pass Coefficient | N district-Years |
| % Free/Reduced-Price Lunch | 0.0016 | 4536 | -0.0048 | 4518 |
| | (0.0102) | | (0.0100) | |
| % Black | -0.0031 | 4531 | -0.0038 | 4516 |
| | (0.0083) | | (0.0081) | |
| % Latinx | 0.0094 | 4534 | 0.0066 | 4516 |
| | (0.0066) | | (0.0064) | |
| % White | -0.0081 | 4533 | -0.0062 | 4516 |
| | (0.0117) | | (0.0115) | |
| Total Current Spending (log) | -0.0087 | 4470 | -0.0136 | 4482 |
| | (0.0081) | | (0.0079) | |
| Enrollment (log) | -0.0612 | 4537 | -0.0594 | 4521 |
| | (0.0446) | | (0.0447) | |
| FTE Teachers | -7.2788 | 4229 | -3.3040 | 4306 |
| | (6.7337) | | (6.5657) | |
| Instructional Aides | -4.8085 | 4357 | -4.3223 | 4366 |
| | (3.9810) | | (3.9233) | |
| Administrators | -1.7632 | 4545 | -1.6892 | 4527 |
| | (1.1090) | | (1.2559) | |
| Counselors | -0.2082 | 4321 | -0.6943 | 4360 |
| | (0.6401) | | (0.6369) | |
| Librarians | -0.2610 | 4325 | -0.3054 | 4354 |
| | (0.3041) | | (0.2944) | |
| Total Staff | -19.3901 | 4251 | -16.2921 | 4348 |
| | (13.4245) | | (13.2919) | |
| Students per FTE Teacher | 0.2725 | 4418 | 0.1230 | 4516 |
| | (0.2168) | | (0.1474) | |
| Students per Counselor | 2.4011 | 4237 | 36.0593 | 4282 |
| | (27.8714) | | (24.5239) | |
| Revenue/Pupil | 0.1461 | 4470 | -0.0268 | 4482 |
| | (0.1578) | | (0.1535) | |
| Total Spending/Pupil | 0.0843 | 4470 | -0.1849 | 4482 |
| | (0.2018) | | (0.1930) | |
| Total Current Spending/Pupil | -0.0947 | 4470 | -0.1390 | 4482 |
| | (0.0967) | | (0.0916) | |
| Instructional Spending/Pupil | -0.0241 | 4470 | -0.0581 | 4482 |
| | (0.0569) | | (0.0538) | |
| Support Services Spending/Pupil | -0.0742 | 4470 | -0.0833 | 4482 |
| | (0.0494) | | (0.0461) | |
| Other Spending/Pupil | 0.0037 | 4470 | 0.0024 | 4482 |
| | (0.0067) | | (0.0076) | |

| | | | | |
|---|---|---|---|---|
| Capital Spending/Pupil | 0.1195 | 4470 | -0.0788 | 4482 |
| | (0.1590) | | (0.1504) | |
| Debt Interest Spending/Pupil | -0.0049 | 4470 | -0.0133 | 4482 |
| | (0.0192) | | (0.0189) | |
| Salary Spending/Pupil | -0.0685 | 4470 | -0.1109* | 4482 |
| | (0.0570) | | (0.0540) | |
| Instructional Spending/Pupil | -0.0119 | 4470 | -0.0319 | 4482 |
| | (0.0378) | | (0.0359) | |
| Benefits Spending/Pupil | -0.0582 | 4470 | -0.0593 | 4482 |
| | (0.0372) | | (0.0353) | |
| Child Deaths | 2.8765 | 4545 | 1.4386 | 4527 |
| | (2.1433) | | (2.0533) | |
| Child Deaths of Despair | 0.1304 | 4545 | 0.0605 | 4527 |
| | (0.2203) | | (0.1896) | |
| Deaths Due to External Causes | 1.7193 | 4545 | 0.9808 | 4527 |
| | (1.3527) | | (1.2849) | |
| Accidental Deaths | 0.2030 | 4545 | -0.0427 | 4527 |
| | (0.1885) | | (0.1815) | |
| Motor Vehicle Accidental Deaths | 0.4277 | 4545 | 0.4455 | 4527 |
| | (0.3910) | | (0.3732) | |
| Suicide Deaths | 0.1353 | 4545 | 0.0781 | 4527 |
| | (0.1695) | | (0.1433) | |
| Assault Deaths | 0.9720 | 4545 | 0.4944 | 4527 |
| | (0.6932) | | (0.6826) | |
| Drug-Related Deaths | -0.0160 | 4545 | -0.0337 | 4527 |
| | (0.0695) | | (0.0641) | |
| Alcohol-Related Deaths | 0.0135 | 4545 | 0.0166 | 4527 |
| | (0.0102) | | (0.0128) | |
| Transportation Accidental Deaths | 0.0602 | 4545 | 0.0486 | 4527 |
| | (0.0316) | | (0.0331) | |
| Fall-Related Deaths | 0.0299 | 4545 | 0.0029 | 4527 |
| | (0.0210) | | (0.0184) | |
| Gun/Firearm Deaths | 0.0238 | 4545 | 0.0047 | 4527 |
| | (0.0313) | | (0.0219) | |
| Drowning Deaths | 0.0175 | 4545 | -0.0270 | 4527 |
| | (0.0489) | | (0.0640) | |
| Fire-Related Deaths | 0.0624 | 4545 | -0.0291 | 4527 |
| | (0.0353) | | (0.0359) | |
| Poison-Related Deaths | -0.0042 | 4545 | 0.0034 | 4527 |
| | (0.0707) | | (0.0652) | |
| Other Child Deaths | 0.0204 | 4545 | -0.0377 | 4527 |
| | (0.0478) | | (0.0443) | |

Coefficients are from OLS regression models predicting each variable measured 1 or 3 years before the election. Sample includes district-year observations with a tax election 1995-2018, voteshare within 10 percentage points of the pass cutoff, and in the year of the election. All models include year fixed effects and control for log county child population ages 5-19.

Robust standard errors adjusted for district election clustering in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table A2: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality: Alternative Measures

| Dependent Variable | Mortality among Children Ages 14-18 | Ages 5-18 |
|---|---|---|
| Deaths | -0.0384 | -0.0347 |
| | (0.0199) | (0.0180) |
| Deaths of Despair | -0.0365 | -0.0538 |
| | (0.0300) | (0.0307) |
| External Deaths | -0.0444* | -0.0366 |
| | (0.0219) | (0.0216) |
| Accidental Deaths | -0.1298** | -0.0875* |
| | (0.0412) | (0.0364) |
| Motor Vehicle Accidental Deaths | -0.1074** | -0.0903** |
| | (0.0375) | (0.0338) |
| N District-Years | 16505 | 16505 |

Coefficients are from negative binomial regression models predicting county child mortality. Sample includes district-year observations with a tax election 1995-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year 3 before election).
Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table A3: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality: Alternative Models

| | Negative Binomial | | | Zero-Inflated | OLS Predicting |
|---|---|---|---|---|---|
| Dependent Variable | No District Controls | Voteshare Squared | Voteshare Cubed | Negative Binomial | Mortality Rates (per 100k) |
| Deaths | -0.0360* | -0.0595* | -0.0239 | -0.0379* | -1.0057 |
| | (0.0182) | (0.0244) | (0.0317) | (0.0175) | (0.5607) |
| Deaths of Despair | -0.0672* | -0.0669 | -0.0625 | -0.0516* | -0.2884 |
| | (0.0286) | (0.0368) | (0.0474) | (0.0256) | (0.2054) |
| External Deaths | -0.0430 | -0.0693* | -0.0473 | -0.0408 | -0.9769* |
| | (0.0220) | (0.0295) | (0.0383) | (0.0210) | (0.4780) |
| Accidental Deaths | -0.0719* | -0.0889 | -0.0513 | -0.0730* | -0.1189 |
| | (0.0344) | (0.0459) | (0.0611) | (0.0305) | (0.2106) |
| Motor Vehicle Accidental Deaths | -0.0697 | -0.1211* | -0.1430* | -0.0757* | -0.5162 |
| | (0.0377) | (0.0493) | (0.0650) | (0.0342) | (0.3408) |
| N District-Years | 17078 | 16505 | 16505 | 16505 | 16465 |

Coefficients are from the specified model predicting county child mortality. Sample includes district-year observations with a tax election 1995-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and all except column 1 include pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year 3 before election).
Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table A4: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality: Alternative Samples

| | Excluding Districts with | | |
|---|---|---|---|
| Dependent Variable | Low Enrollment | Low Number of Votes | Low Observations |
| Deaths | -0.0376* | -0.0372* | -0.0366 |
| | (0.0175) | (0.0175) | (0.0192) |
| Deaths of Despair | -0.0501 | -0.0505* | -0.0557* |
| | (0.0257) | (0.0257) | (0.0280) |
| External Deaths | -0.0413 | -0.0408 | -0.0376 |
| | (0.0211) | (0.0211) | (0.0231) |
| Accidental Deaths | -0.0716* | -0.0709* | -0.0808* |
| | (0.0314) | (0.0315) | (0.0343) |
| Motor Vehicle Accidental Deaths | -0.0777* | -0.0735* | -0.0594 |
| | (0.0343) | (0.0343) | (0.0365) |
| N District-Years | 16089 | 16305 | 14643 |

Coefficients are from negative binomial regression models predicting county child mortality. Sample includes district-year observations with a tax election 1995-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year 3 before election).

Sample exclusions: column 1 excludes observations with enrollment ≤400, column 2 excludes elections with votes ≤200; column 3 excludes districts observed <15 years.

Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table A5: Placebo Tests: Estimated Effect of False Pass Cutoffs on Child Mortality

| Dependent Variable | Low Placebo (Median Below True Cutoff) | High Placebo (Median Above True Cutoff) |
|---|---|---|
| Deaths (ages 5-19) | -0.0025 | -0.0318 |
| | (0.0176) | (0.0177) |
| Deaths of Despair | -0.0002 | -0.0472 |
| | (0.0259) | (0.0258) |
| External Deaths | 0.0024 | -0.0336 |
| | (0.0211) | (0.0212) |
| Accidental Deaths | -0.0130 | -0.0450 |
| | (0.0309) | (0.0321) |
| Motor Vehicle Accidental Deaths | -0.0028 | -0.0396 |
| | (0.0345) | (0.0348) |
| N District-Years | 16761 | 16256 |

Coefficients are from negative binomial regression models predicting county child mortality. Sample includes district-year observations with a tax election 1995-2018, voteshare within 10 percentage points of the (false) pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year 3 before election).

Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Placebo tests assign false pass cutoffs: the median value of voteshare above and below the true cutoff required to pass. Estimates at these false cutoffs are null and further suggest continuity in the outcome-forcing variable relationship at values of the forcing variable other than the pass cutoff.

Figure A1: Density Plot by Voteshare

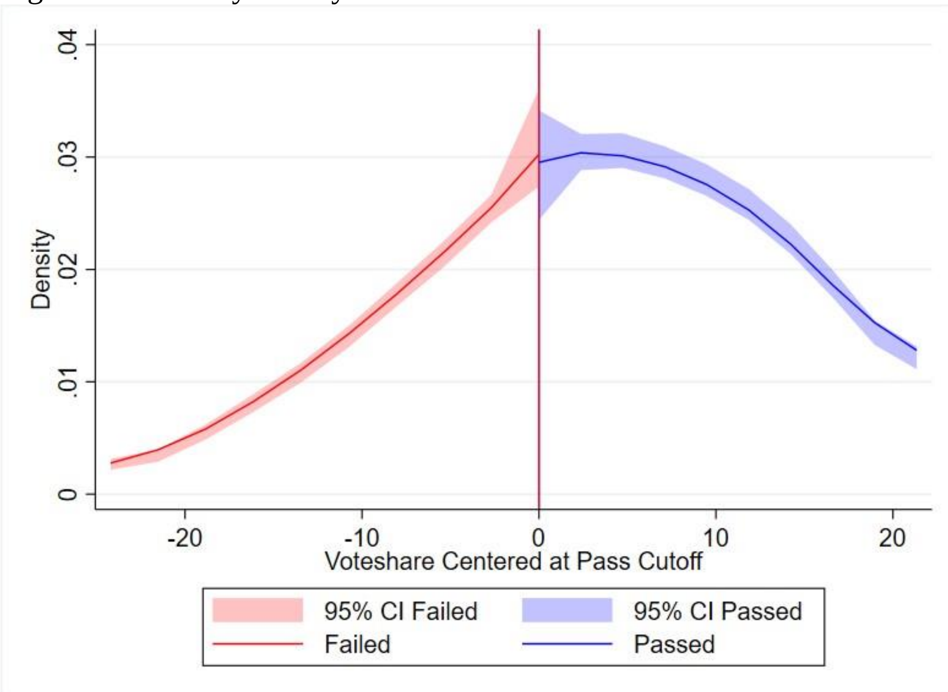



Figure A2: Child Deaths Ages 5-19 by Voteshare

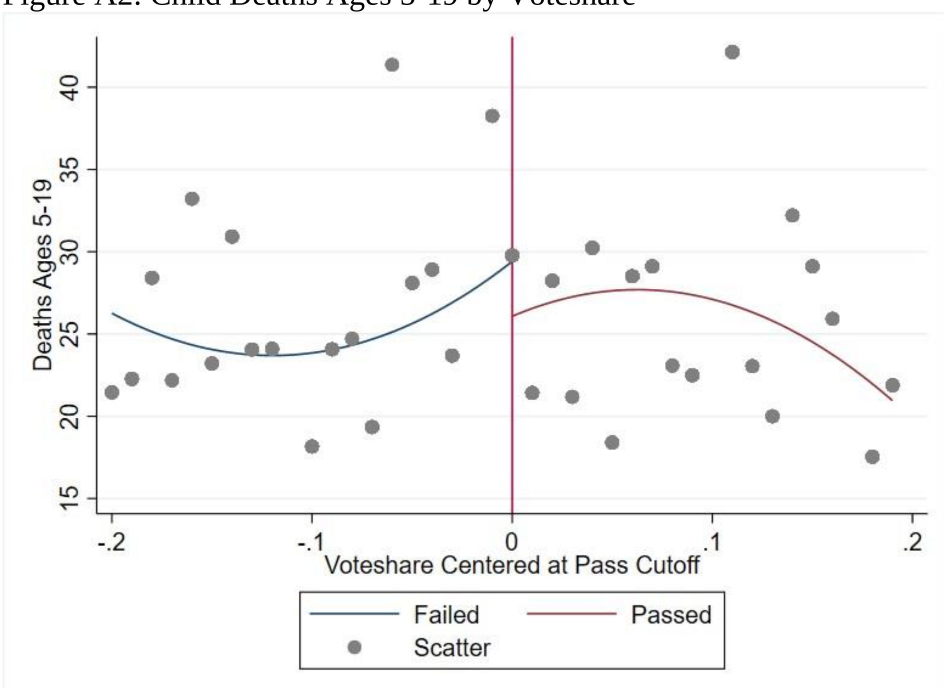
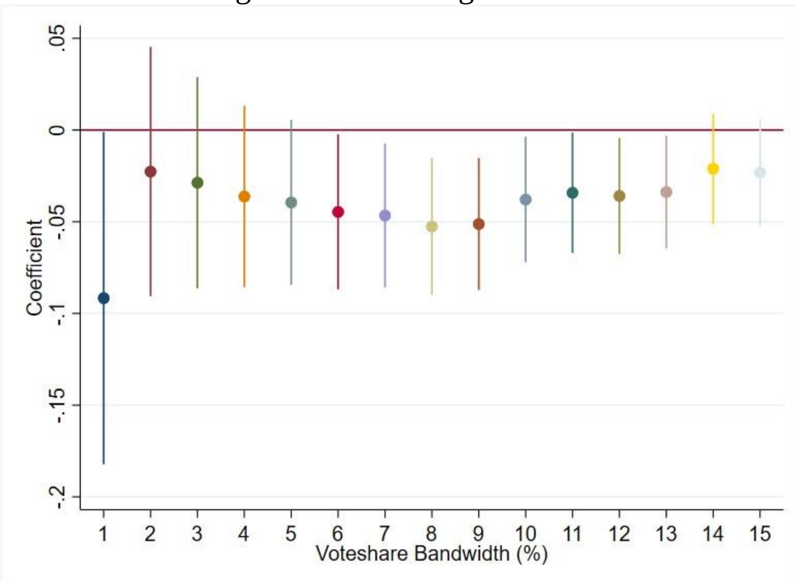
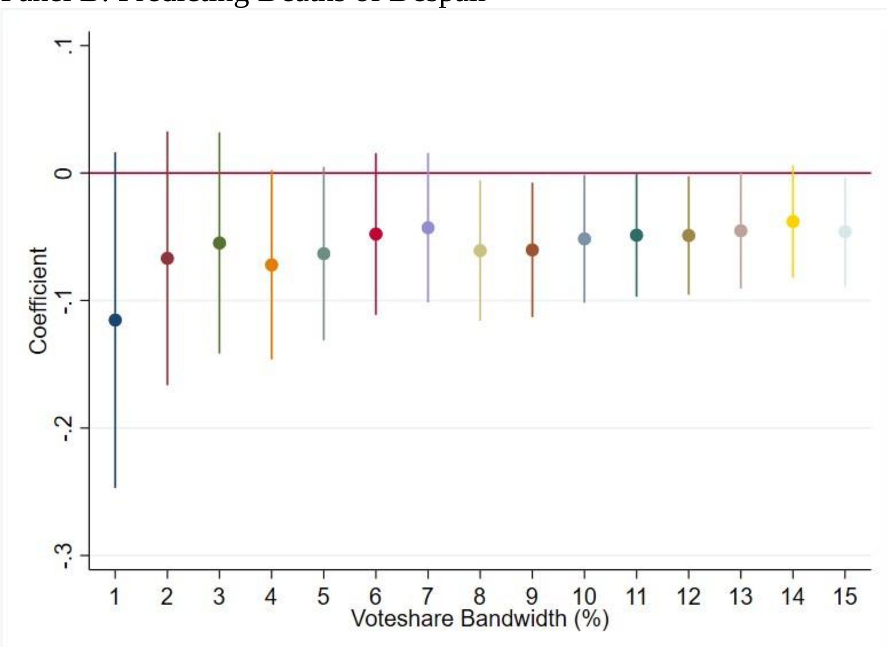


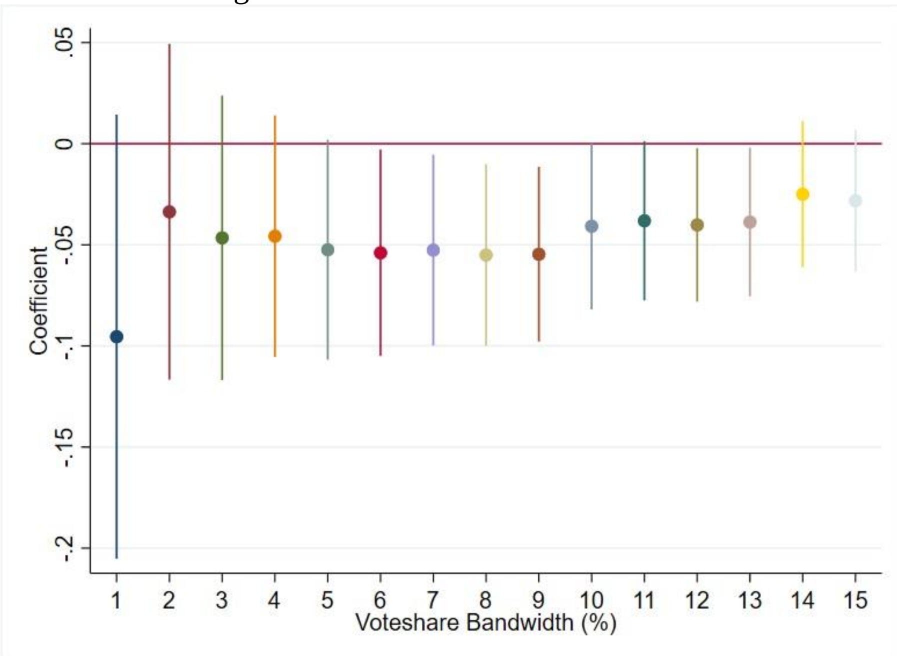
Figure A3: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality by Voteshare Bandwidth from the Pass Cutoff
Panel A: Predicting Child Deaths Ages 5-19



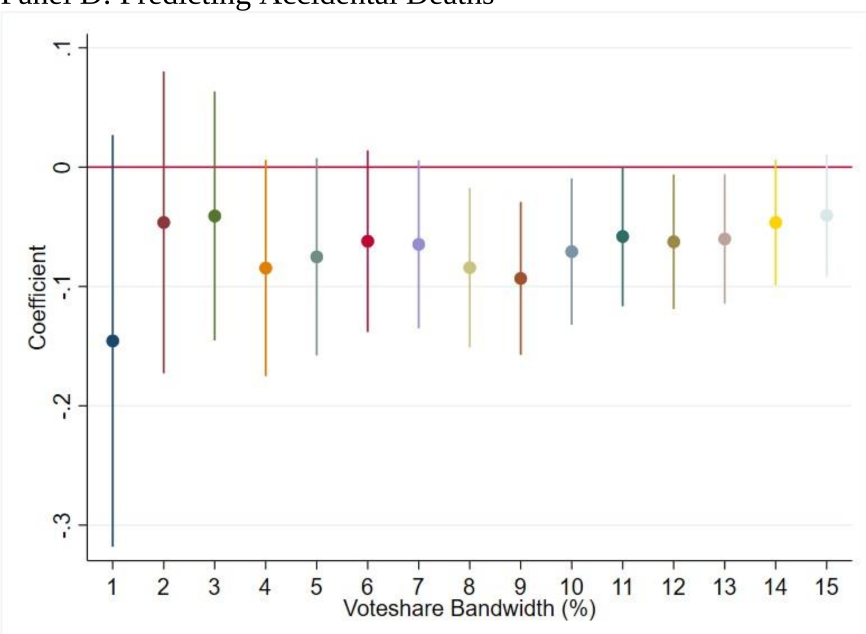

Panel B: Predicting Deaths of Despair



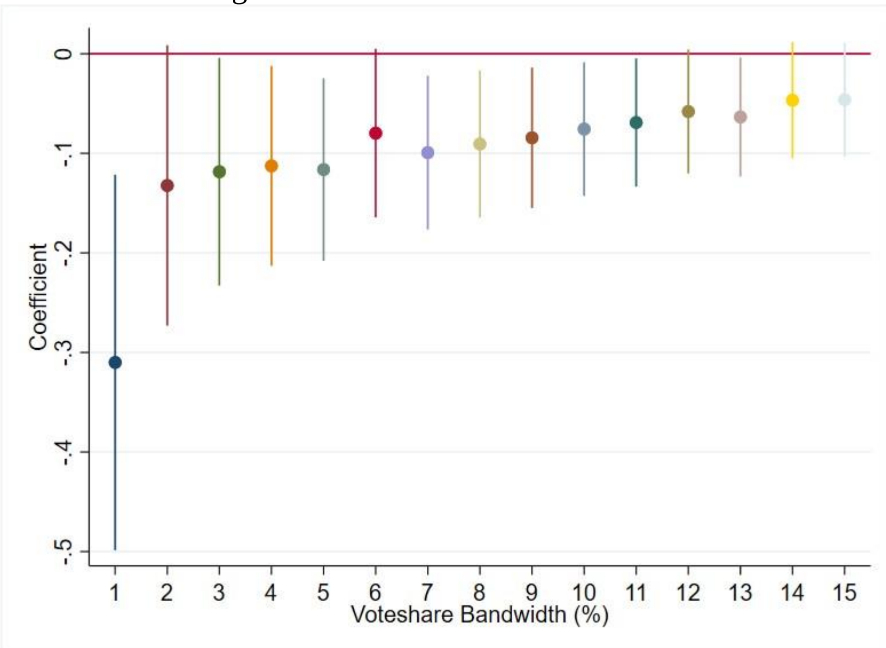
Panel C: Predicting External Deaths




Panel D: Predicting Accidental Deaths

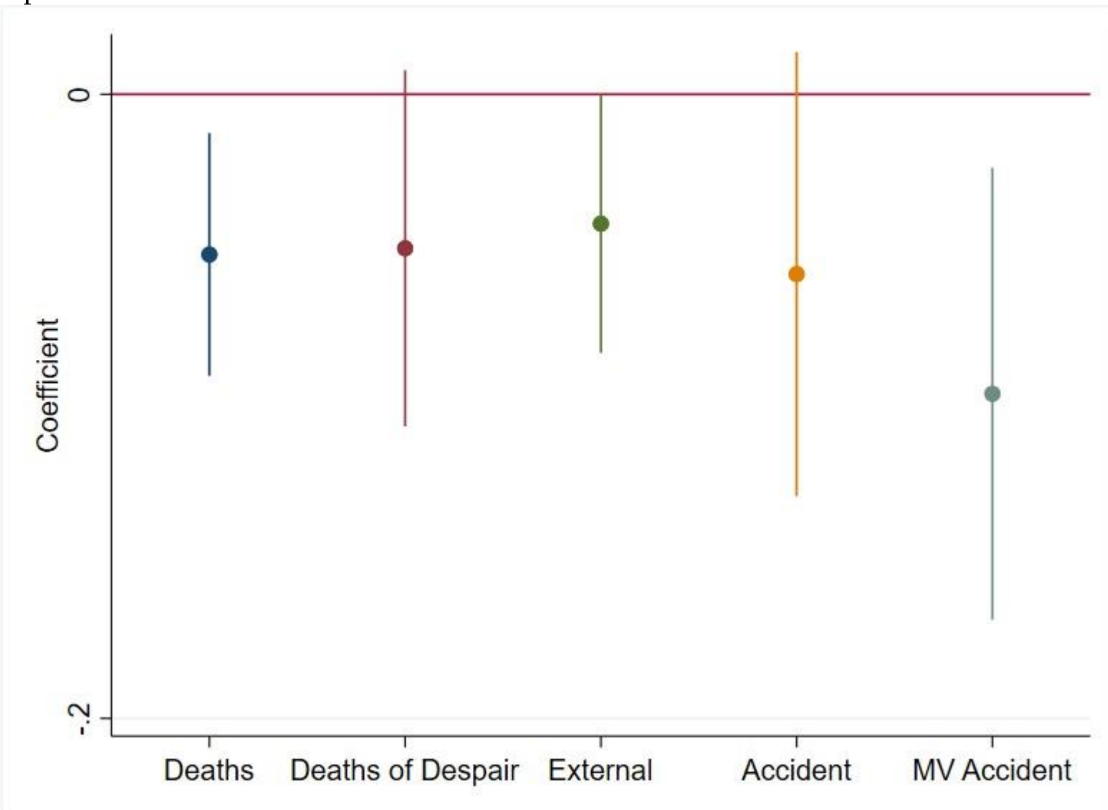


Panel E: Predicting Motor Vehicle Accidental Deaths



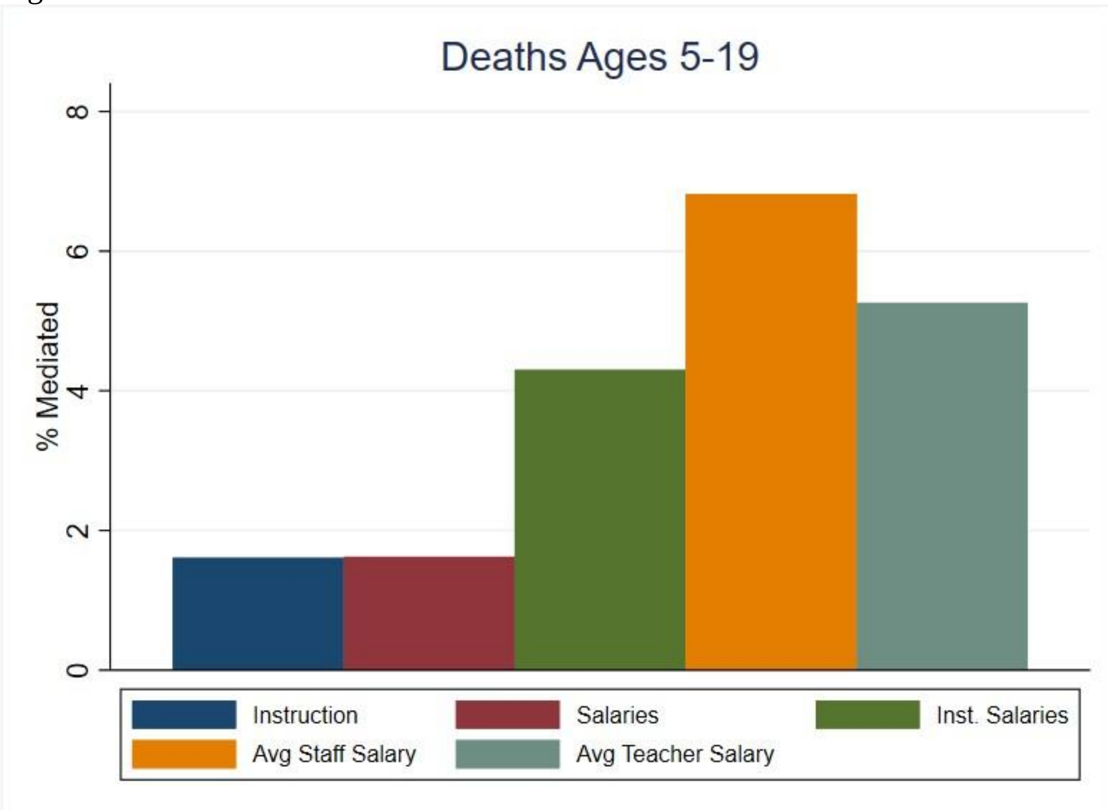

Using the same models used in Table 2 to predict child deaths in years 6-10 after the election, coefficients for passing a tax election are shown from separate models when varying the bandwidth of voteshare from the pass cutoff from 1% to 15%.

Figure A4: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality Using Optimal Bandwidth from the Pass Cutoff




Using the same models used in Table 2 to predict child deaths in years 6-10 after the election, coefficients for passing a tax election are shown from separate models when using the optimal bandwidth of voteshare from the pass cutoff selected using rdbwselect (Calonico et al. 2020). Bandwidths for each dependent variable are: Deaths 7.16%; Deaths of Despair 7.46%; External Deaths 9.90%; Accidental Deaths 6.79%; Motor Vehicle Accidental Deaths 8.31%.

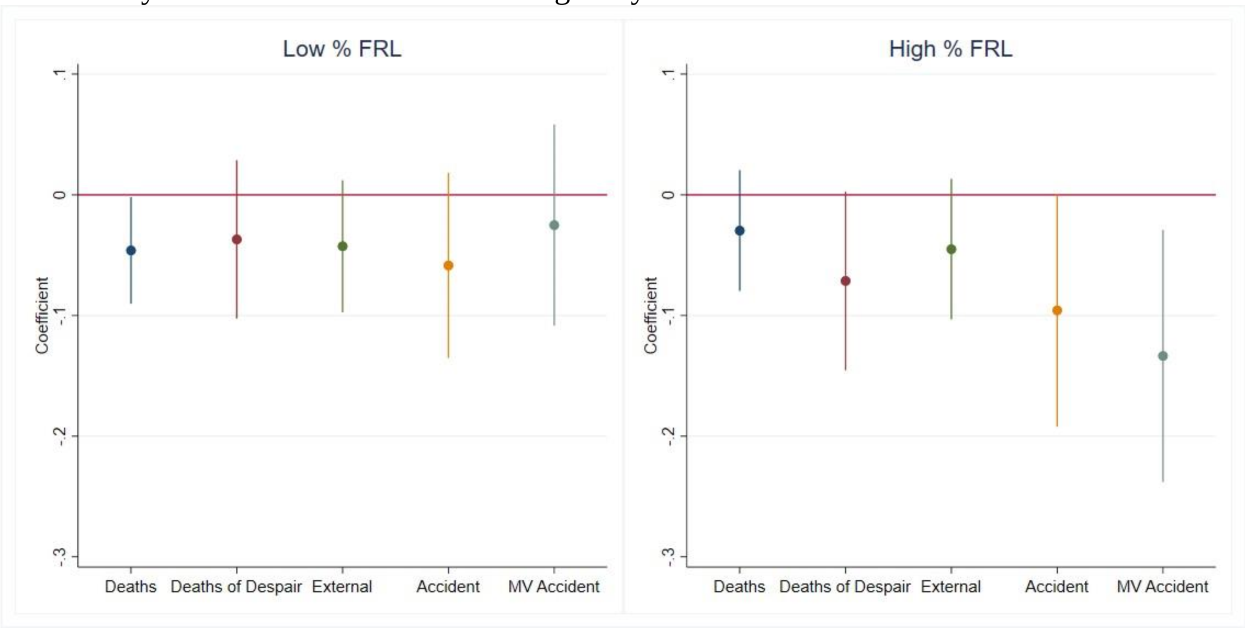
Figure A5: Mediation Estimates




Estimated based on paramed in Stata (Emsley and Liu 2013). Bars indicate the estimated percent of the effect of passing a tax election on child mortality mediated by per pupil spending on instruction, salaries, instructional salaries; average staff salary; and average teacher salary. Paramed estimates use the same negative binomial model in Table 2 and allow for interaction between the treatment (passing an election) and the mediator.

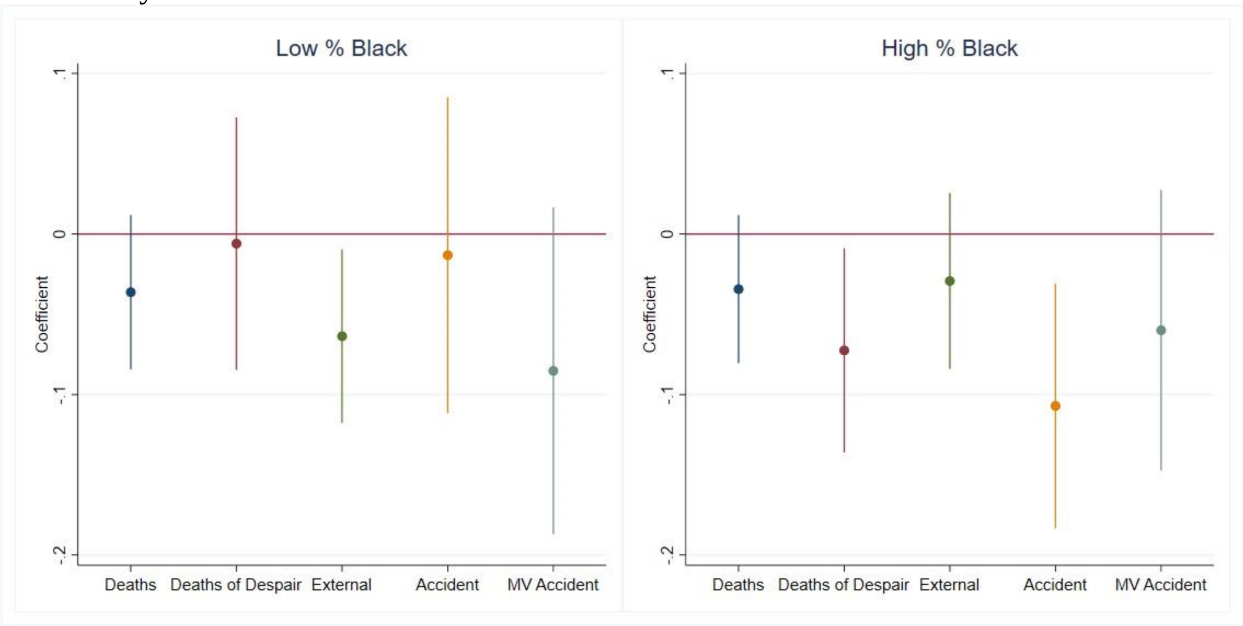
Figure A6: Estimated Effect of Narrowly Passing a Tax Election on Child Mortality by District Characteristics

Panel A: By Free/Reduced-Price Lunch Eligibility



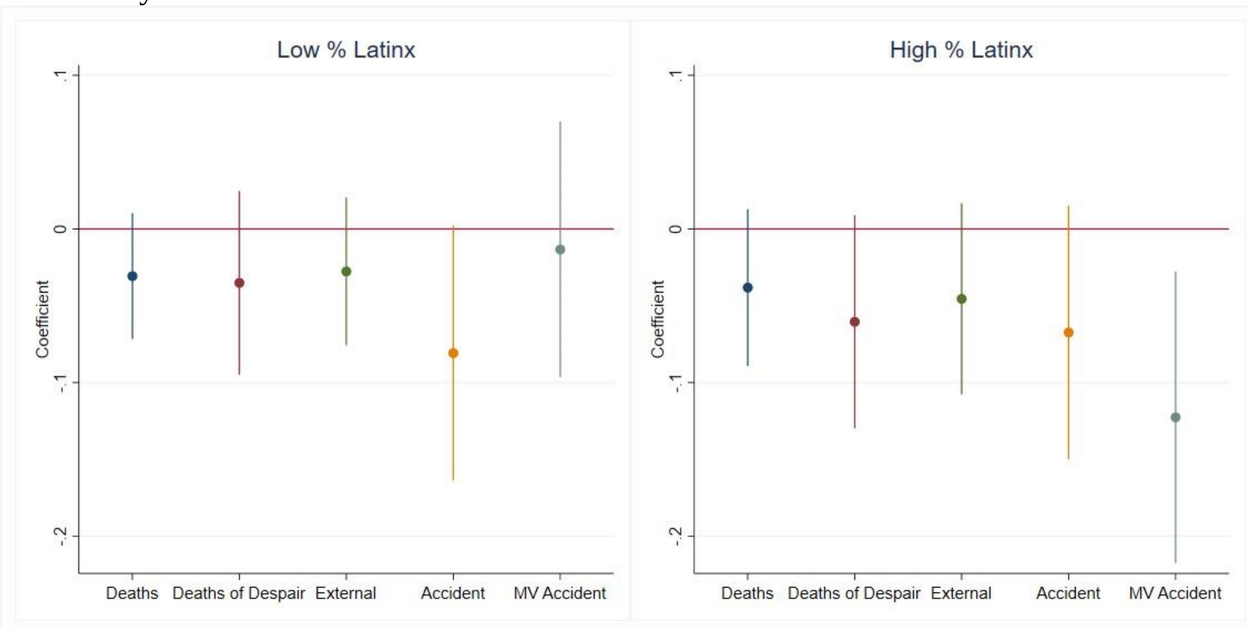

Coefficients shown in Table 4.

Panel B: By Percent Black Enrollment



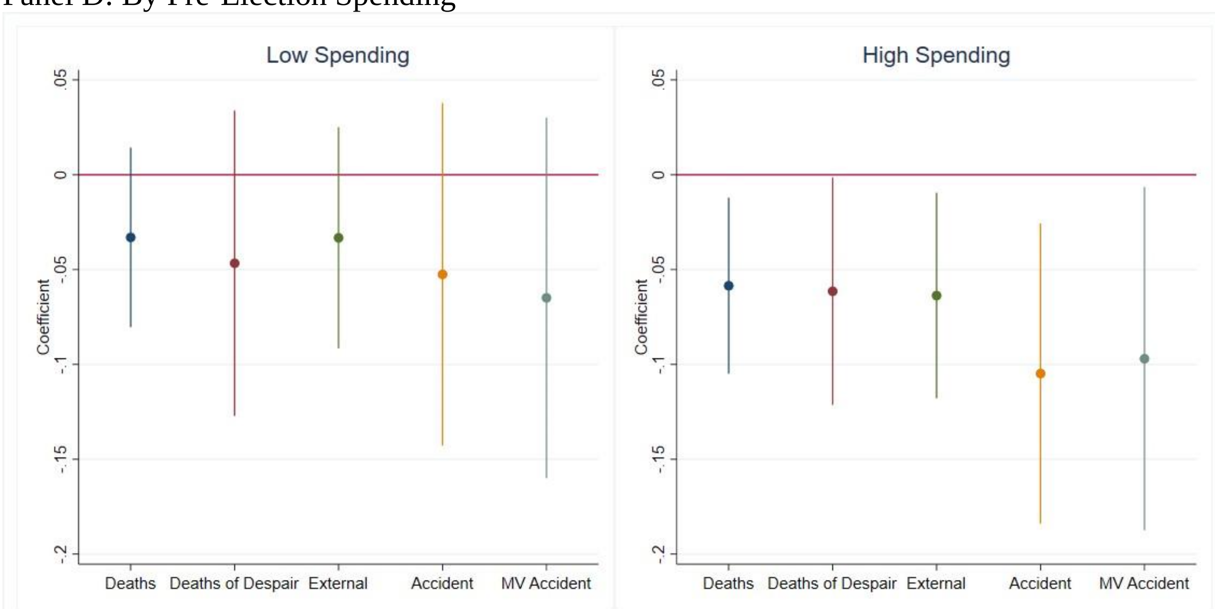

Coefficients shown in Table 4.

Panel C: By Percent Latinx Enrollment



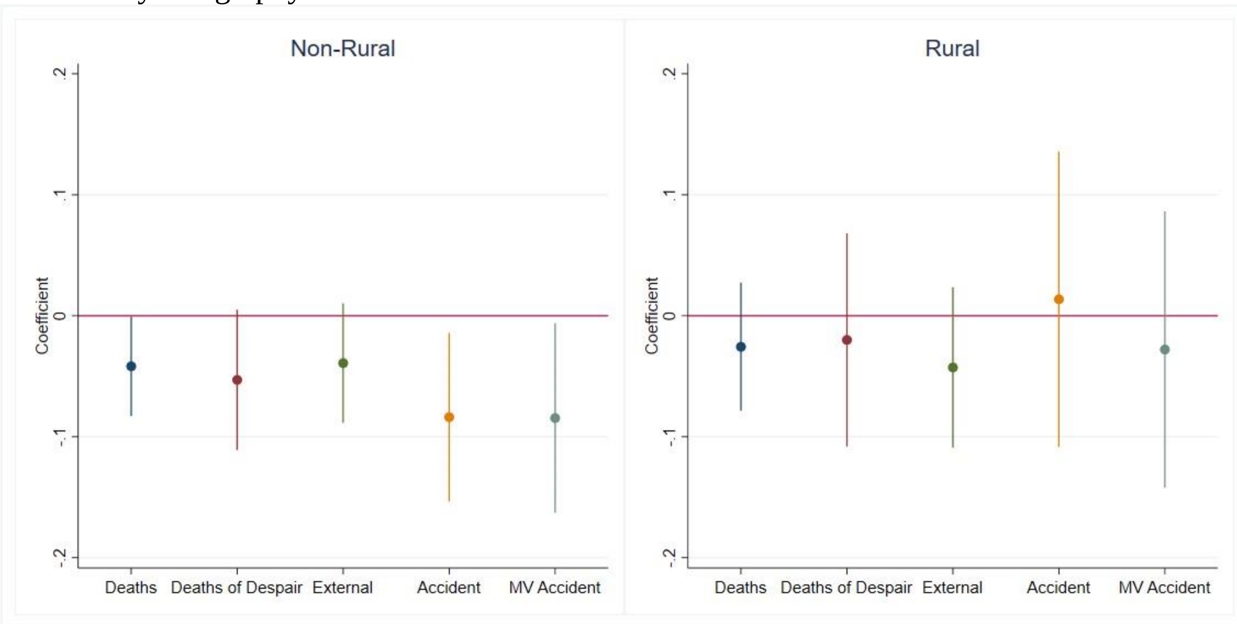

Coefficients shown in Table 4.

Panel D: By Pre-Election Spending




Coefficients shown in Table 4.

Panel E: By Geography




Coefficients shown in Table 4.