Suspended from Work and School? Impacts of Layoff Events and Unemployment Insurance on Student Disciplinary Incidence*

Riley K. Acton
Miami University and IZA

Jo R. King
Boston University

Austin C. Smith
Bates College and IZA

December 3, 2025

Abstract

We examine the impact of local labor market shocks and state unemployment insurance (UI) policies on student discipline in U.S. public schools. Analyzing school-level discipline data and firm-level layoffs in 23 states, we find that layoffs have little effect on discipline rates on average. However, effects differ across the UI benefit distribution. At the lowest benefit level ($265/week), a mass layoff increases out-of-school suspensions by 5.1%, with effects dissipating as UI benefits increase. Effects are consistently largest for Black students —especially in predominantly White schools —resulting in increased racial disproportionality in school discipline following layoffs in low-UI states.

JEL Codes: I24, J63, J65
Keywords: school discipline, layoffs, unemployment insurance

*We thank Lucie Schmidt for providing state-level SSI data and Jim Flynn for providing Medicaid expansion data. In addition, we thank conference participants at the Association for Education Finance and Policy (AEFP) Annual Conference, Association for Public Policy Analysis & Management (APPAM) Fall Research Conference, Allied Social Sciences Association (ASSA) Annual Meeting, Liberal Arts College Public & Labor (LAC-PAL) Conference, and the Western Economic Association Annual Conference, and seminar participants at the RAND Corporation, Macalester College, New York University, and the University of New Hampshire for their helpful comments and suggestions on this project.

1 Introduction

Exclusionary discipline practices, such as suspensions and expulsions, are commonly used in the U.S. and around the globe (Arum and Ford, 2012; Deakin et al., 2018) to manage student behavior in schools. However, these practices have been linked to a variety of negative long-run outcomes, including reduced academic achievement and high school graduation rates (Lacoe and Steinberg, 2018; Holt et al., 2022; Sorensen et al., 2022), increased rates of incarceration (Bacher-Hicks et al., 2024), and lower rates of adult employment and lower earnings (Davison et al., 2021). Moreover, historically marginalized groups, including Black students, students with disabilities, and LGBTQ+ students, tend to be exposed to these practices —and their potential negative consequences —at higher rates than their peers (Rumberger and Losen, 2016; Welsh and Little, 2018). Given the prevalence of exclusionary discipline practices and their potential role in exacerbating inequality, understanding the determinants of student exposure to exclusionary discipline is of high importance to education policymakers and practitioners.

In this paper, we study how local labor market shocks and unemployment insurance (UI) benefits affect students' exposure to exclusionary discipline practices. Family and community economic stability are important factors for children's social, emotional, and academic development (Hardy et al., 2019), but existing research has not considered the relationship between local economic conditions, stabilizing labor market policies, and school disciplinary outcomes. This lack of prior literature is surprising given the multiple channels through which destabilizing economic events —such as mass layoffs —could potentially impact discipline outcomes in schools.

The primary mechanism by which we expect layoff events to influence disciplinary outcomes is at the family level. If a student's exposure to exclusionary discipline results from misbehavior exhibited in the classroom, we may suspect that destabilizing events in a student's life, such as a parent or guardian losing a job, trigger or exacerbate this misbehavior. We may also expect that local labor market shocks increase stress and uncertainty in households where parents maintain their jobs, generating changes in behavior for children whose families are not directly impacted by a layoff. Simultaneously, it could also be the case that teachers and/or principals respond to these stressful community-wide events by reducing their tolerance for classroom misbehavior and increasing their use of suspensions and expulsions as classroom management tools. This type of response is possible in the U.S. context because teachers and administrators typically have some latitude in employing these measures and can use exclusionary discipline practices in response to both low-level (e.g., infractions such as talking out of turn, tardiness, use of profanity, or making excessive noise) and severe (e.g., fighting, bringing a weapon to school, use of illicit substances) instances of misbehavior (Perera and Diliberti, 2023; Sorensen et al., 2022).

At the same time, we may expect that parents spend more time in the home and with their children following a loss of employment (Becker and Tomes, 1986), which could potentially lead to improved behavior in school. Thus, more generous UI benefits that stabilize family income and increase parental time investments could counteract the effects of labor market shocks on student behavior. We may also expect that more generous UI benefits would stabilize economic conditions and stress levels in the broader community, potentially mitigating effects at the community level as well. As such, our analysis not only examines the direct impact of local labor market shocks on disciplinary outcomes, but also considers the moderating effect of UI generosity.

We rely on school-level data on disciplinary incidence from the U.S. Department of Education's Civil Rights Data Collection, combined with detailed information on firm-level layoffs filed to state employment agencies under the federal Worker Adjustment and Retraining Notification (WARN) Act. Together, these data sources allow us to construct a school-level panel dataset on school discipline and local layoff prevalence for over 5000 schools across 23 U.S. states. Our empirical approach relies on school fixed effects to leverage plausibly exogenous variation in exposure to local labor market shocks within a geographic area (e.g., a city or a school district) over time. We further control for unobservable changes over time at the state level by including state-by-year fixed effects. We operationalize this approach using a two-stage estimator proposed by Borusyak et al. (2024) and Gardner et al. (2024), which is robust to heterogeneous treatment effects with staggered timing. Additionally, we interact our measure of local layoffs with state-level UI benefits to understand the moderating effects of labor market policies on responses to labor market shocks.

We find that, on average, exposure to a mass layoff event has a limited impact on discipline outcomes. However, this average effect masks important heterogeneity across states with varying levels of UI benefits: layoffs lead to an increase in rates of exclusionary discipline when UI benefits are low, but this effect fades as UI benefits become more generous. At the lowest level of UI benefits in our sample ($265/week), a mass layoff event in a school's city increases the number of students receiving in-school suspensions by 2.6 per 1000 students, out-of-school suspensions by 3.5 per 1000 students, and expulsions by 0.27 per 1000 students—increases of 2.5%, 5.1%, and 13% relative to their respective means of 103, 67, and 2.1 per 1000 students. These effects dissipate when UI benefits reach approximately $500/week, or about the top quartile of benefits in our sample. These effects are similar if we instead measure layoff events at the school district or county level, rather than the city level, or if we define our treatment using only layoff events that are large relative to the local population. We further show that our results are not driven by changes in student characteristics induced by layoffs, nor differential trends between areas that do and do not experience layoffs, and are robust to including interactions between layoff exposure and other social safety net programs.

Our estimated effects are consistently larger for Black students than for White students. For example, at the lowest UI benefit level in our sample, layoffs increase the number of students receiving out-of-school suspensions by 10.5 per 1000 students (7.8% of the mean rate of 133 per 1000 students) for Black students, compared to 2.5 per 100 students (4.5% of the mean rate of 54.6 per 1000 students) for White students. These heterogeneous effects by race are particularly large in majority-White schools: in schools with a below-median share of non-White students at baseline, a layoff event occurring when UI benefits are at their lowest level increases the number of Black students receiving out-of-school suspensions by 15.8 per 1000 students —an effect that is about 50% larger than our estimates for Black students in the full sample. We further find that, in the absence of generous UI benefits, layoffs increase *within-school* racial disparities in disciplinary incidence, particularly in majority-White schools. However, this effect dissipates as UI becomes more generous, indicating that stabilizing labor market policies can play an important role in limiting racial disparities in exposure to exclusionary discipline practices.

Our study contributes to several related strands of literature on local labor markets, childhood outcomes, and school disciplinary practices. First, we build on a growing body of work that considers the relationships between local labor market shocks and educational outcomes. Prior work has documented that community-level mass layoff events influence student test scores (Ananat et al., 2017), college attendance (Foote and Grosz, 2020; Hubbard, 2018), and field of study choices (Acton, 2021; Weinstein, 2020). A related line of literature documents the direct negative effect of parental job loss on childhood outcomes, including infant (Lindo, 2011) and child (Page et al., 2019; Schaller and Zerpa, 2019; Ubaldi and Picchio, 2023) health, as well as academic achievement (Stevens and Schaller, 2011; Ruiz-Valenzuela, 2020). We contribute new evidence to this literature that local labor market shocks also affect youth's exposure to exclusionary discipline practices in schools, which may contribute to the documented declines in academic achievement, as suspensions and expulsions remove students from standard instructional settings.

Second, we contribute to the literature on the stabilizing effects of unemployment benefits for workers and families. Generous unemployment benefits allow households to smooth their consumption (Gruber, 1997), which, in turn, improves their health (Kuka, 2020) and reduces suicide rates (Cylus et al., 2014), opioid and antidepressant prescriptions among women (Ahammer and Packham, 2020), the probability of divorce (Swensen et al., 2023), and the likelihood that a child repeats a grade (Regmi, 2019). We provide evidence that this stabilizing mechanism also limits students' exposure to exclusionary discipline practices, as they are no more likely to be suspended or expelled after a mass layoff event if state UI benefits are sufficiently generous.

Finally, we provide new evidence on the determinants of school disciplinary practices and, in particular, racial disparities in suspension and expulsion rates. Prior research shows that Black students are suspended and expelled from U.S. schools at over twice the rate of White students (CRDC, 2021), and are more likely to experience exclusionary discipline practices even when they are involved in the same incidents as White students (Barrett et al., 2019; Shi and Zhu, 2021; Liu et al., 2021). We find that these racial gaps in suspensions may be exacerbated by local labor market shocks, especially in majority-White schools, but can be lessened by generous UI policies. This finding suggests that labor market policy not only plays an important role in stabilizing household resources following unemployment events, but also in limiting racial disparities in formative childhood educational experiences.

2 Conceptual Framework

Ex ante, the impact of a local labor market shock on student disciplinary incidence is ambiguous. There are several mechanisms by which student disciplinary incidence may increase in response to layoff events. First, at the family level, we would expect that students in families directly affected by a mass layoff event may experience adverse effects. For example, reduced parental labor market opportunities may negatively impact the behavioral outcomes of a child, as parents will have fewer resources to invest in their child's human capital development (Becker and Tomes, 1986). In line with this hypothesis, prior literature shows that parental unemployment increases stress within a familial unit, and can have a negative impact on a child's mental and physical health (Page et al., 2019; Lindo et al., 2018; Nikolova and Nikolaev, 2018; Kalil and Ziol-Guest, 2008).

From a broader, community perspective, we would also expect that community-level shocks could have a negative effect on the behavior of children whose parents are *not* directly affected by a layoff event, either by increasing general community-level stress related to poor economic conditions or by generating negative peer effects within schools (Carrell and Hoekstra, 2010; Carrell et al., 2018). Indeed, prior evidence shows that economic downturns are associated with worse adult mental health outcomes (Charles and DeCicca, 2008), increased opioid abuse (Hollingsworth et al., 2017), and higher levels of "deaths of despair" (Lowenstein, 2024). Moreover, in the wake of simply announcing community-level layoff events, racial animus increases (Bestenbostel and Peralta, 2021) and birth outcomes worsen (Carlson, 2015), suggesting the presence of diffuse, negative impacts of stressful community events on individuals.

Similarly, we may expect that teachers and principals could also respond to poor economic conditions and increased household or community stress by reducing their tolerance for misbehavior, which can determine whether this behavior is managed within the classroom or through exclusionary strategies (Welsh and Little, 2018; McIntosh et al., 2014). Such a mechanism would be consistent with prior work showing that economic distress and anxiety make teachers less effective (Strunk et al., 2018) and more likely to be absent, and more likely to leave their position (Dizon-Ross et al., 2019).[1]

Conversely, parents may spend more time with their children when local labor market opportunities decline (Jones, 1991; Kalil and Ziol-Guest, 2008), potentially offsetting the effects of reduced financial investments. This type of offsetting effect, however, is likely heterogeneous across families and dependent upon both the quality of the time parents are able to spend with their children, as well as the characteristics of the parent that experiences an unemployment event. For example, prior work shows that child health tends to improve in times of strong male employment growth and decline in times of strong female employment growth (Page et al., 2019), while child maltreatment incidence increases with male unemployment, but decreases with female unemployment (Lindo et al., 2018), suggesting that there may be different effects on children depending on which parent experiences a layoff.[2]

Finally, the effect of a local labor market shock on children's behavior —both via direct effects from parental unemployment and indirect effects from community stress, peer effects, and changes to teachers' classroom management practices —is likely to depend on the ability of unemployed workers to smooth consumption and find new labor market opportunities. Because unemployment benefit programs can help smooth consumption (Gruber, 1997), stabilize families (Swensen et al., 2023), and mitigate negative effects on health (Kuka, 2020), we expect more generous UI to lessen the effects of negative employment shocks on student behavior.

We also expect the effects of layoffs and UI benefits to vary across demographic groups. The large racial wealth gap in the United States suggests that programs like UI may be even more important for Black families who, on average, have less wealth from which to draw during labor shocks (Aliprantis et al., 2022; Derenoncourt et al., 2023).[3] Previous findings also suggest that Black students are subjected to disciplinary action more often than their White peers (Losen

[1]Local economic downturns could affect the composition of the teacher workforce (see, e.g., Deneault 2025). However, it is unclear whether such a resorting of individuals into teaching professions would increase or decrease the use of exclusionary discipline practices.

[2]While we are unable to examine heterogeneity by parental gender in our setting, layoffs in our sample tend to be associated with larger reductions in male (vs. female) employment (see Appendix Table A.3), which could contribute to our findings. However, developing a better understanding of the relationships between paternal and maternal employment and school discipline outcomes is an important area for future research to address.

[3]It is also possible that, within a given community, Black parents are more likely to be directly affected by layoffs than White parents. However, our results in Appendix Table A.3 suggest that this is not the case for Black and White workers generally in our sample, as layoffs are associated with larger absolute and relative reductions in White (vs. Black) employment.

and Skiba 2010; Terriquez et al. 2013; CRDC, 2021), even when they are involved in the same incident (Barrett et al., 2019; Liu et al., 2021), and that teachers are more likely to surveil the behavior of Black students (Okonofua and Eberhardt, 2015). These prior findings suggest that even if the behavioral change of Black and White students in response to a local economic shock is the same, Black students may be punished more harshly than their White peers. In addition, prior research that finds that racial animus increases after the announcement of a community layoff event (Bestenbostel and Peralta, 2021) suggests that Black households may disproportionately experience increased stress following a local labor market shock, which could have downstream effects on the behavior of their children in school. Thus, we examine the differential effects of layoffs and UI on discipline outcomes for Black and White students and consider whether UI generosity can reduce racial gaps in discipline following layoff events.

3 Data and Sample Construction

3.1 School Data

We obtain school-level data on suspensions and expulsions —both in the aggregate and for key demographic subgroups —from the U.S. Civil Rights Data Collection (CRDC), which also contains information on a school's enrollment, geographic location, grades offered, and a variety of other student demographic characteristics. CRDC surveys are mandatory for all U.S. public schools and school districts and are administered every other year, with data available in academic years 2011, 2013, 2015, and 2017. We limit our data to schools with a full panel of discipline and enrollment data in these years and that are located in 23 states with available layoff information (see Section 3.2 and Appendix Table A.1). We focus our analysis on middle and high schools as the incidence of exclusionary discipline in grades K-5 is relatively low (National Center for Education Statistics, 2022), potentially due to recent policy pushes against exclusionary discipline in grades PK-5 (Rafa, 2018; US Department of Health and Human Services, 2016).[4]

We narrow our sample to schools that are most representative of traditional U.S. public schools

[4]We define middle school as grades 6-8 and high school as grades 9-12. We do not include combined elementary and middle/high schools (i.e., K-12 or K-8) in our sample.

in three ways. First, we only include schools classified by the National Center for Education Statistics (NCES) as "regular" schools, excluding virtual, charter, and alternative schools from our analysis.[5] Second, we omit schools governed by regional, state, or federal agencies, as well as charter, specialized, and "other" districts.[6] These restrictions narrow our sample to districts where students both live and attend school in the same geographic area, allowing for a more precise match between layoffs and schools.[7] Third, we limit our sample to schools for which we observe discipline outcomes separately for White, Black, male, and female students and that have a complete panel of non-missing data.[8]

We match all schools in our sample to (1) the city and county in which they are located, and (2) the city or cities from which their district draws students using the Missouri Census Data Center (MCDC) geographic correspondence engine.[9] We define cities using the U.S. Census Bureau's place codes, which are designed to capture concentrations of population that are named, locally recognized, and mutually exclusive (U.S. Census Bureau, 1994).[10] In our sample, about 60% of schools are located in places that are recognized and referred to as cities, while 40% are recognized and referred to as towns, villages, or other Census-designated places (CDPs). However, for clarity and consistency, we refer to these areas as cities throughout the text.

Our outcomes of interest from the CRDC data are the proportion of students in each school that receive (1) in-school suspensions, (2) out-of-school suspensions, and (3) expulsions in a given academic year. In-school suspensions refer to actions that result in a student being removed from the classroom environment, generally for a day or less, but are still supervised by school personnel. Out-of-school suspensions differ in that they not only remove the student from the classroom, but also temporarily remove them from school supervision, e.g., to home either with or without

[5]92% of schools in the CRDC data are classified as "regular" schools.

[6]Only 1.5% of regular schools are governed by these agencies.

[7]For example, in a regional (e.g., county), state, or federally operated school, we would expect students to attend from areas outside of the place where the school itself is located. This means that layoff events that would affect a student's family and home community would not be attributed to the school a student is attending and layoff events that do not affect a student's family and home community, that is, they occur in the same place as the school's location, would be (incorrectly) attributed to that student.

[8]Schools for which we do not observe outcomes separately by subgroup are typically small and/or do not enroll a sufficient number of students across racial groups to disaggregate their discipline data and maintain confidentiality standards. 66% of "regular" schools in traditional districts, representing 80% of total enrollment, have non-missing data in all years.

[9]https://mcdc.missouri.edu/applications/geocorr.html

[10]For the less than 1% of school districts that are not matched to a place code with the MCDC, we proceed to manually link to place codes. In addition, we exclude schools in places that are unidentified (i.e., place code of 99999 or missing place names), which accounts for less than 0.1% of schools.

educational services being provided. Expulsions are the most punitive action that we examine, and remove the student from their school for the remainder of the school year and, in some cases, permanently. Expulsions may or may not include educational services to be provided to the expelled student.[11]

3.2 Layoff and UI Data

We construct measures of local job loss exposure using records of all mass layoffs and plant closures reported under the Worker Adjustment and Retraining Notification (WARN) Act of 1988. The WARN Act requires private employers (both for-profit and non-profit) with 100 or more employees to provide at least 60 days written notice to employees ahead of a mass layoff or plant closing affecting 50 or more full-time employees at a single employment site (U.S. Department of Labor, 2023).[12] The penalty for non-compliance with the act—that is, if an employer fails to give 60 days notice for a qualifying WARN event—holds employers liable to pay each laid-off employee wages and benefits for the period of violation, up to 60 days. In addition to the federal requirements, individual states can and have passed "mini-WARN" acts, which can enforce reporting requirements for smaller employers and/or smaller layoff events.[13]

We collect data on all available layoff events from the WARN Database (Arain, 2021), which has consolidated layoff information for the majority of U.S. states and includes the number of workers laid off by each employer and the location of the layoff event. At the time of collection, however, several states do not regularly publish data at the county or sub-county levels and others are still pending public information requests for this data. To construct a more representative sample, we contacted all remaining states without data available from the WARN Database via email, requesting their data on layoffs pursuant to the WARN Act from 2010 onward. Additionally,

[11]The CRDC data reports out-of-school suspensions as single and multiple out-of-school suspensions, which are mutually exclusive. They also report expulsions with and without services, which are also mutually exclusive. Our final measure of out-of-school suspensions is the sum of single and multiple out-of-school suspensions and our final measure of expulsions is the sum of expulsions with and without services. In Appendix Table A.4, we consider these outcomes separately.

[12]Specifically, a plant closing is defined as an employment site shutting down and at least 50 full-time workers losing their jobs. A mass layoff is defined as employment being reduced by 500 or more full-time workers at a given site *or* by 50-499 full-time workers if they make up at least one third of the employer's workforce.

[13]For example, Wisconsin's mini-WARN Act applies to employers with 50+ workers and layoffs of either 25 workers or 25% of the workforce, whichever is greater. The states in our sample that have mini-WARN acts are Illinois, Kansas, Michigan, New Hampshire, New Jersey, Oregon, Rhode Island, and Wisconsin.

we rely on the data from Michigan that was used in Acton (2021) to complete our sample. Our final sample consists of 23 states with complete information on both layoff locations and dates.

We use the WARN data to construct measures of local job loss at the city, school district, and county levels. To do so, we first match the place codes provided by the MCDC to the location names in each WARN notice filed. We define a city as experiencing a layoff in year t if at least one WARN notice filed between July 1 of year t and June 30 of year $t+1$ listed the city by name. We then match these place codes to schools' place codes to determine which layoffs occurred in the same city as a school in our sample. To construct our school district and county measures of layoffs, we match place codes to school districts and counties again using the MCDC geographic correspondence engine.[14] We define a school district or a county as experiencing a layoff in academic year t if a city contained within its boundaries experienced a layoff in academic year t.

Due to concerns about the use of two-way fixed effects with continuous variables (Callaway et al., 2021), our empirical specification uses an indicator treatment variable equal to one if the school was exposed to a layoff —at either the city, school district, or county level —in a given academic year. However, we also present results using an indicator for experiencing a "large" (above-median or top-quartile) layoff. To determine exposure to large layoffs, we scale the number of jobs affected by WARN notices by a city's, school district's, or county's working-age (age 15-65) population, which we collect from the U.S. Census Bureau's population tables. Specifically, we construct the annual, per capita number of jobs affected by WARN notice layoffs in city c and academic year t as:

$$\text{Layoffs}_{ct} = \frac{\Sigma_{W_{ct}}\text{Layoffs}_{wct}}{\text{Population}_{ct}} \times 10000 \qquad (1)$$

where w denotes a WARN notice filing, Layoffs_{wct} is the number of jobs affected by a single WARN notice, and W_{ct} denotes the total number of WARN notices filed in city c in academic year t. We analogously construct school district measures by apportioning the proportion of a city's population that attends a school district and/or summing the layoffs and populations of adjacent cities that share a school district, and county measures by summing the affected jobs and popula-

[14] School districts can either be made up of multiple cities or can contain a fraction of one city. In our sample, the average school district contains 1.04 cities, while the average county contains 2.82 cities.

tions of all cities within a county.

While WARN notices do not capture the universe of job losses —for example, those from public employers, small firms, or small layoff events —they are advantageous for our measurement of local labor market shocks for several reasons. First, WARN notices allow us to construct measures of local layoff prevalence in specific, and sometimes small, geographic locations, including cities, school districts, and counties, which can be difficult to obtain in other publicly available measures of local employment (e.g., the Quarterly Census of Employment and Wages or the County Business Patterns) due to data suppression. Second, WARN notices capture large layoffs that are likely to be covered by media outlets and may generate stress and uncertainty among community members. Prior research has shown that the *announcement* of layoffs under the WARN Act —above and beyond job losses themselves —can worsen birth outcomes Carlson (2015) and increase racial animus Bestenbostel and Peralta (2021), which supports the idea that WARN notices can increase community-level stress.

WARN notices also tend to be one of the most timely measures of local employment changes, as they predict future changes in UI claims and unemployment rates. For example, Krolikowski and Lunsford (2022) estimate that, at the state level, an increase in WARN layoffs of 1,000 jobs in one month increases UI claims by 270 and the unemployment rate by 0.005 percentage points (pp) in the following two months. In Appendix Table A.3, we regress county-level changes in employment from the Quarterly Workforce Indicators (QWI) from the U.S. Census Bureau on an indicator for whether we observe a layoff in the prior year in a given county.[15] We find that, on average, exposure to a layoff in the WARN data is associated with a year-over-year employment reduction of 120 workers, or a 1pp reduction in YOY employment growth. These employment reductions are largest for male and white workers, but occur across all subgroups.

Finally, because we are interested in the potential moderating effects of UI on school discipline following a layoff event, we obtain information on the maximum weekly UI benefits allotted by state and year, as reported by the U.S. Department of Labor (USDOL) in July of each year. Maximum UI benefits is a commonly used summary measure of UI generosity in the literature

[15] We aggregate the quarterly data to academic years to match the timing of our layoff indicators.

(Krueger and Mueller, 2010; Swensen et al., 2023) and is a strong predictor of benefits received (Hsu et al., 2018). The states within our sample provide anywhere from \$265 and \$707 per week in maximum UI benefits. As a robustness check, we also collect information on states' average UI recipiency rates —the share of unemployed workers who receive UI benefits —for each year from the USDOL as in O'Leary et al. (2022). Figure 1 provides a visual depiction of the variation in mean UI generosity across states. Our sample has substantial heterogeneity in the value of benefits and recipiency rates across different parts of the country. Some regions, i.e., the Southeast, tend to provide less generous benefits than other regions. Generally, states with higher recipiency rates also tend to have more generous benefits.

3.3 Summary Statistics

Table 1 provides summary statistics on our analysis sample. In addition to the school-level sample restrictions we describe in Section 3.1, we further restrict our sample to schools that experience layoffs in 0-3 of the academic years 2011, 2013, 2015, and 2017, because schools that experience layoffs in every year of our data neither contribute identifying variation, nor serve as comparison units, in our analysis. Our resulting final analysis sample consists of 5,847 unique schools in 3,248 cities across 23 states.

Panel A provides information on the demographic characteristics of the schools in our sample. The average school in our sample enrolls approximately 830 students, 42% of whom qualify for free or reduced-price lunch and 34% of whom are non-White. Our sample contains schools located in a variety of geographic settings, including cities (8.4%), suburbs (36%), towns (21%), and rural areas (35%). However, due to our restriction to schools with year-to-year variation in layoffs, we note that very large urban areas are less likely to be included in our sample as they are more likely to have layoffs reported in each year of our sample period.

Panel B then provides summary statistics on our discipline outcomes of interest.[16] Within our sample, more punitive actions (i.e., expulsions and out-of-school suspensions) are used less often than less punitive actions (in-school suspensions). On average, 0.21% of students are expelled

[16] We exclude outliers that may be caused by reporting errors, which we define by each type of suspension and expulsion per 100 pupils greater than 4 standard deviations above the mean, and also exclude schools in the bottom 1% of enrollment.

each year, with approximately 6.7% experiencing an out-of-school suspension, and nearly 10.3% experiencing an in-school suspension. Suspensions occur more often for both male and Black students, although expulsions do not vary substantially across subgroups, likely due to their rarity. As such, we examine heterogeneity by both race and gender in our analysis.

Finally, Panel C of Table 1 provides summary statistics regarding schools' exposure to mass layoffs. In a given year, approximately 11.7% of schools in our sample experience a layoff in their city, and approximately 20.1% experience a layoff in their school district. Over the four years of discipline data, 42% of the schools in our sample experience a layoff in their city, and 46.9% experience a layoff in their school district. When a layoff occurs in either a city or a school district, about 45 workers per 10,000 working-age residents lose their jobs over the course of an academic year. Figure 2 plots the mean school-level layoff prevalence over our sample, illustrating how layoff exposure varies across states. We have not only substantial variation in layoff size, but also across space —while we are unable to examine all 50 states, the 23 states in which we have layoff data are dispersed across the country and in all nine Census divisions. Appendix Table A.1 provides a list of these states and the number of schools and layoffs we observe in each state.

To better understand how schools in these 23 states compare to the nation as a whole, Panels A and B of Table A.2 presents summary statistics from the CRDC data for both the analysis sample and all schools in the U.S. meeting our school-level sample restrictions (traditional middle and high schools, non-missing data, etc.). We cannot restrict the national sample to schools that do not experience layoffs every year of our sample period, as we do not observe layoffs for the 27 states that are not in our sample. Nevertheless, in general, the analysis sample is very similar to U.S. schools nationally, although the schools in our sample, on average, are less likely to be located in cities and enroll fewer economically disadvantaged and non-White students. Exclusionary discipline practices are used similarly —and more often with Black and male students —in our sample and the national sample, though out-of-school suspensions and expulsions are used less frequently overall in our sample. Panel C of Table A.2 further shows that state labor market conditions and policies are similar in our sample and the national sample: the average maximum weekly UI benefit in our sample is $434, compared to $442 nationally, and the average state unemployment rate in our sample is 5.92%, compared to 6.05% nationally. Taken together, these summary statistics suggest that our results are likely to be broadly representative of U.S. middle and high schools as a whole.

4 Empirical Strategy

4.1 Estimation

We are first interested in estimating the contemporaneous impacts of local layoff events on student disciplinary outcomes, as specified by the following functional form:

$$\text{Discipline}_{ist} = \beta \text{Layoff}_{it} + \lambda_i + \theta_{st} + \varepsilon_{ist} \tag{2}$$

where Discipline_{ist} is the number of students disciplined (i.e., suspended or expelled) per 100 students in school i, located in state s, during academic year t, scaled per 100 students enrolled. Layoff_{it} is an indicator variable equal to one if a mass layoff event occurred in the same city (or alternative geographic unit, such as school district or county) as school i during academic year t.[17] λ_i is a time-invariant school fixed effect that is used to control for unobserved differences across schools that may affect disciplinary incidence, such as the school's location or the grade levels it serves. θ_{st} is a school-invariant state-year fixed effect that accounts for unobserved, state-level time trends in discipline rates, local labor market conditions, and state policies across states in our sample, over time. Importantly, this term absorbs the direct effects of any state-level changes in social safety net programs, including changes to UI generosity, on school discipline outcomes. Finally, ε_{ist} is an idiosyncratic error term.

Our parameter of interest in this equation is β, which represents the effect of a layoff event in school year t on contemporaneous discipline outcomes in year t. The model implicitly assumes that large layoffs in year t only impact student disciplinary outcomes during school year t. To the extent that there are carryover effects from layoffs in prior years, our methodology will provide conservative estimates attenuated toward zero, because these partially treated subsequent years

[17] We also provide estimates using different definitions of treatment in Section 5.3.4.

are considered untreated by the model. In later specifications, we explore the dynamic effects of layoffs directly using an event-study framework. The largest effects on school discipline are indeed during the year of large layoffs and effects taper towards zero by two periods after large layoffs.

In addition to the specification in equation (1), we are interested in understanding the potential moderating effect of UI following a layoff event, which we specify as:

$$\text{Discipline}_{ist} = \beta \text{Layoff}_{it} + \gamma(\text{Layoff}_{it} \times \text{UI}_{st}) + \lambda_i + \theta_{st} + \varepsilon_{ist} \tag{3}$$

where UI_{st} is maximum weekly UI benefits, measured in \$100s, in state s and year t and all other variables retain their definitions from the baseline model. We omit the non-interacted UI_{st} term in this specification, as it is absorbed by our state-year fixed effects, θ_{st}. Our parameters of interest in this equation are β, the effect of a layoff event with no UI benefits, and γ, the change in the effect of a layoff event due to a \$100 increase in maximum weekly UI benefits.

Both equations (1) and (2) represent variations of a difference-in-differences empirical specification, where treatment (layoff) timing occurs at different times for different units (schools), may "reverse" (i.e., may occur in one period and not the next), and may occur multiple times for a given unit throughout our sample period. It is now well-established that estimating such equations with standard two-way fixed effects (TWFE) may result in biased estimates if there are heterogeneous treatment effects across treatment timing and/or time periods (see Baker et al., 2022; De Chaisemartin and d'Haultfoeuille, 2023; Roth et al., 2023, for recent reviews of the literature). To address this concern, we estimate our parameters of interest —β and γ in equations (1) and (2) —using a two-stage approach developed independently by Borusyak et al. (2024) and Gardner et al. (2024), the latter of whom shows that this approach can be extended to settings where treatment is reversible and/or occurs multiple times. Gardner et al. (2024) further show that this approach is the most efficient of the myriad of alternative difference-in-differences estimators that have recently been proposed in the literature.

The two-stage approach proceeds as follows. In the first stage, we estimate our school and state-year fixed effects (and time-varying, school-level covariates, when we include them) using only untreated observations, i.e., observations where $\text{Layoff}_{it} = 0$:

$$\text{Discipline}_{ist} = \lambda_i + \theta_{st} + \varepsilon_{ist} \tag{4}$$

Then, in the second stage, we estimate either:

$$\text{Discipline}_{ist} - \widehat{\text{Discipline}}_{ist} = \beta \text{Layoff}_{it} + \nu_{ist} \tag{5}$$

or:

$$\text{Discipline}_{ist} - \widehat{\text{Discipline}}_{ist} = \beta \text{Layoff}_{it} + \gamma(\text{Layoff}_{it} \times \text{UI}_{st}) + u_{ist} \tag{6}$$

where $\text{Discipline}_{ist} - \widehat{\text{Discipline}}_{ist}$ are the residualized outcomes for all school-year observations in our sample, generated using the coefficients we estimate in equation (4). As Gardner et al. (2024) show, this two-step procedure avoids contaminating the school and state-year fixed effects with the true values of β and γ we aim to estimate, ensuring our estimates are robust to heterogeneous treatment effects across units and time.

Throughout our analysis, we cluster standard errors at the city level, which is the geographic unit at which we observe layoff events.[18] Given the two-stage estimation we outline above, we do so using a Bayesian bootstrapping procedure with 500 iterations per specification (Rubin, 1981).[19] We additionally present standard two-way fixed effects (TWFE) estimates for comparison, which are of similar magnitude to, but less precise than, our estimates using our preferred two-stage procedure.

4.2 Identification Assumptions

The school and state-year fixed effects we include in our estimation approach capture two important sources of unobserved heterogeneity within our data: differences in school discipline rates and layoff exposure (1) across schools and (2) over time, within a given state. Specifically, the

[18] When we provide alternative specifications that define layoffs at the school district or county levels, we analogously cluster our standard errors at these levels. Our results are also robust to clustering standard errors at the state level. These results are available upon request.

[19] The Bayesian bootstrap approach smooths bootstrap samples by reweighting, rather than resampling, observations, which ensures we estimate all school and year fixed effects —and, therefore, residuals for all observations —in all iterations. For recent examples of this approach, see Angrist et al. (2017), Finkelstein et al. (2021), and Gilpin et al. (2024).

school fixed effects allow us to control for time-invariant differences in school climate and culture surrounding discipline across schools, as well as location-specific effects, such as local demographic characteristics and industry composition. Likewise, the state-year fixed effects allow us to control for unobserved heterogeneity within states across time, which may encompass state-level shifts in discipline culture over time, specifically in response to growing research and evidence regarding the negative relationship between discipline and academic outcomes, and general cyclical trends in both layoffs and discipline outcomes. The state-year fixed effects further control for any direct effects of changes in social safety net programs, including changes in UI generosity, on school discipline outcomes.

The identifying assumption for β to represent the causal effect of layoffs on student disciplinary outcomes is that, after accounting for the school-year fixed effects, there are no within-school changes in unobserved determinants of student discipline that are correlated with labor market shocks. This assumption may be threatened if, for example, families differentially exit public schools in response to a local mass layoff or if inputs into the education production function (e.g., per-pupil spending or student-teacher ratios) change as a result of a local labor market downturn. We limit the potential for these responses by only considering contemporaneous changes in school discipline outcomes (those occurring within the same academic year as a layoff event), and by testing for whether observable characteristics of students and schools change contemporaneously with layoffs. We further estimate specifications where we control directly for time-varying student and school characteristics.

As an additional test of our identifying assumption, we extend our main estimating equation, equation (6) in Section 4.1, to an event study specification that tests for differential changes in discipline outcomes prior to layoff events, across the UI benefit distribution. Specifically, we estimate:

$$\text{Discipline}_{ist} - \widehat{\text{Discipline}}_{ist} = \sum_{k=-3}^{2} \beta_k 1[t - t_i^* = k] + \sum_{k=-3}^{2} \gamma_k (1[t - t_i^* = k] \times \text{UI}_{st}) + u_{ist} \qquad (7)$$

where t_i^* is the year where a layoff first occurs in city i. The β_k coefficients trace out how discipline outcomes were evolving prior to, and following, a layoff event, net of any moderating effects of UI generosity. The γ_k coefficients then trace out how these outcomes evolve differentially across the UI benefit distribution. For both sets of coefficients, we omit the period $k = -1$, the year before we first observe a layoff. As we discuss in Section 5.1, these specifications provide no evidence of differential pre-trends between locations that do and do not experience layoffs, nor in states with high and low UI benefits, strengthening our identifying assumption.[20]

Finally, in all specifications where we include an interaction term between Layoff_{it} and UI_{st}, we note that we additionally assume that within-state changes in unemployment insurance benefits are not correlated with other changes that would differentially affect schools that experience a mass layoff relative to schools that do not experience a layoff. We believe this assumption is reasonable, as changes in state UI benefit levels are generally not correlated with changes in state economic conditions nor other social safety net programs (see, for example, Kuka, 2020; Swensen et al., 2023). Nevertheless, we show that our results are robust to interacting our layoff measure with a variety of other social safety net programs including TANF, EITC, SSI, and SNAP generosity.

5 Results

5.1 Main Results

Table 2 reports regression results for our baseline and UI-augmented specifications. Columns (1) and (2) first report results from our baseline specification using city-level layoffs as our key dependent variable. Column (1) includes our baseline school and year FEs, while column 2 adds our preferred state-by-year fixed effects to account for changes in state labor markets, policies, and discipline practices over time. Each panel reports results for one of our three outcomes of interest: in-school suspensions, out-of-school suspensions, and expulsions. The results in both columns (1) and (2) reveal that, on average, layoff exposure has little effect on school discipline. Our estimated effects are consistently small and close to zero and are not statistically significant at conventional levels.

[20] We do note, however, that while we do not find any statistical evidence of pre-trends, this is largely due to large confidence intervals on our pre-period coefficients.

In column (3), we report results for the UI-augmented specification. The coefficient on the layoff indicator now represents the impact of a layoff event with zero UI —an out-of-sample parameter —and the interaction term represents the change in the impact of layoff events due to a \$100 increase in maximum weekly UI benefits. For each type of school discipline outcome we consider, the implied impact of a layoff without benefits is an *increase* in discipline rates, while additional UI benefit generosity *decreases* the disciplinary response to layoffs. These effects are statistically significant for both out-of-school suspensions and expulsions —the most severe discipline outcomes we can observe. At the lowest level of UI benefits in our sample (\$265), layoff exposure increases the number of students receiving in-school suspensions by 0.26pp, or 2.6 per 1,000 students enrolled. At this level, layoff exposure increases out-of-school suspensions by 3.5 per 1,000 students, and expulsions by 0.27 per 1,000 —increases of 2.5%, 5.1%, and 13% relative to their means, respectively. As UI generosity increases, these negative impacts shrink and eventually reverse. For example, an additional \$100 of UI reduces out-of-school suspensions induced by a mass layoff event by 1.5 per 1000 students, or approximately 2.2% relative to the mean.

To better understand the magnitude of these effects, we use estimates from Appendix Table A.3, where we show that exposure to a layoff event in the WARN data is associated with a year-over-year reduction of approximately 120 workers at the county level. Given that the typical county in our sample has roughly 4,200 middle and high school students, our results in Table 2 indicate that a layoff event at the lowest level of UI generosity increases out-of-school suspensions in the typical county by approximately 14.7 students.[21] Or, for every 100-worker employment reduction due to a mass layoff event, 12.3 more students would be suspended.[22] Since about half of the prime-age labor force are parents (Federal Reserve Bank of Chicago, 2024), our results suggest that, for every 50 parents who lose a job due to a mass layoff at the bottom of the UI generosity distribution, roughly one-quarter will have a child suspended —if we abstract away from any potential spillover effects of layoffs on still-employed parents, teachers, or classroom peers.[23] This magnitude is in line with prior research on the direct effects of parental job loss on academic achievement, which

[21] Calculated as 4,200 students × 3.5 additional suspensions per 1,000 students = 14.7 additional out-of-school suspensions.

[22] Calculated as 14.7 additional suspensions $\div$ 120-worker employment reduction ≈ 0.123 additional suspensions per worker, or 12.3 additional suspensions per 100 workers.

[23] Calculated as $12.3 \div 50 = 0.246$.

find that a parent losing a job can increase the probability of repeating a grade by 15% (Stevens and Schaller, 2011) and reduce grades during compulsory schooling by about 15% of a standard deviation (Ruiz-Valenzuela, 2020).

As a whole, the results in column (3) of Table 2 indicate that layoff events have heterogeneous effects on school discipline outcomes across the UI generosity distribution. To further illustrate these heterogeneous responses, we estimate the effects of a layoff event separately for each state in our sample. From these state-specific estimates, we can compare the effect that layoff events have on suspensions and expulsions in a given state to the state's 2010-2017 average UI benefit generosity. Figure 3 presents these results, which again suggest that the greater UI benefits a state has, the lesser effect that a layoff event has on suspensions and expulsions.

To further understand these results, Appendix Table A.4 segments the analysis by the type of disciplinary action students experience. The CRDC data reports out-of-school suspensions as single (meaning a student was suspended once during the year) and multiple (meaning a student was suspended multiple times during the year) out-of-school suspensions, which are mutually exclusive. They also report expulsions with and without services, which are also mutually exclusive. Our primary measure of out-of-school suspensions is the sum of single and multiple out-of-school suspensions. Columns (2) and (3) explore the single and multiple out-of-school suspension outcomes separately, and reveal that increases in suspensions at low levels of UI generosity are driven by increases in both students receiving one suspension and students receiving multiple suspensions throughout the year. Columns (5) and (6) then show results for expulsions with and without services. For expulsions, the results are imprecise but larger effects occur for expulsions with educational services provided.

5.2 Heterogeneous Effects

Because rates of exclusionary discipline are much higher for Black and male students —both in our sample (see Table 1) and in the U.S. generally (CRDC, 2021) —we consider whether layoffs and UI generosity have differential effects across race and gender. Table 3 reports our results from the UI-augmented specification, stratified by student subgroup. In Panel A, we find that the increases in in-school suspensions due to layoffs at low levels of UI generosity, as well as the offsetting effects of more generous UI, are driven by larger effects for male students, but our estimates across gender and racial subgroups are not generally statistically significant at conventional levels. The results, while imprecise, also suggest that layoffs may *decrease* in-school suspensions for Black students when UI benefits are low, which may be indicative of substitution away from less-severe disciplinary practices and towards out-of-school suspensions and expulsions.

Our results for out-of-school suspensions in Panel B are much more precise and we find that the effects of layoffs and UI are larger for Black students and male students.[24] At the lowest UI benefit level in our sample ($265), layoffs increase the number of students receiving out-of-school suspensions by 10.5 per 1000 for Black students (7.8% of mean) but only by 2.5 per 1000 for White students (4.5% of mean). By gender, at the lowest benefit levels, layoffs increase the number of students receiving out-of-school suspensions by 4.5 per 1000 (4.9% of mean) for male students and by 3.1 per 1000 (7.4% of mean) for female students. In Panel C, we additionally see that the effects on expulsions are almost entirely driven by Black students, but are similar in magnitude for male and female students.

We further explore heterogeneity by race and gender in Table 4, where we estimate effects for out-of-school suspensions across different school contexts for all students (Panel A), Black students (Panel B), and male students (Panel C). Column (1) repeats our main estimates from Table 3. Columns (2) and (3) then divide the sample by schools' baseline out-of-school suspension rate. For the sample as a whole, as well as for Black and male students specifically, we find that our effects are entirely driven by schools with above-median discipline rates at baseline. These schools, which, as documented by prior literature, tend to contain teachers with higher propensities to refer a student, may thus be more susceptible to stressful contexts, generating more "vulnerable decision points," which may increase racial disproportionality in suspension (Liu et al., 2023; McIntosh et al., 2014).

In columns (4) and (5), we split the sample by schools' baseline FRPL percentage. The es-

[24]It is not surprising that the effects are generally more precise for out-of-school suspensions, as there is more variation in out-of-school suspensions in our data. In 11% of observations, schools report not using any in-school suspensions in a given year, while only 4.7% report not using any out-of-school suspensions.

timates in these specifications are somewhat imprecise and not statistically different from one another, but we find some evidence that our effects are higher in high-poverty schools, particularly for male students. Columns (6) and (7) then estimate effects separately for middle (grades 6-8) and high (grades 9-12) schools. The point estimates are consistently larger for middle schools, but are generally not statistically indistinguishable between the two settings.

Columns (8) and (9) estimate effects separately for schools located in rural and non-rural areas, while columns (10) and (11) divide the sample by their baseline non-White enrollment share. We find that our overall effects and, especially, our effects for male students are larger in rural areas than in non-rural areas. We also see that the effects, particularly for Black students, are larger in schools with low baseline percentages of non-White students.

Taken together, our results in Tables 3 and 4 suggest that, in the absence of UI benefits, layoffs may increase racial disparities in school discipline rates by affecting Black students more than White students. This phenomenon may be particularly pronounced in schools with low shares of non-White students, but could be offset by more generous UI policies. We more directly explore how layoffs and UI policies impact racial disparities in out-of-school suspension rates *within* schools in Appendix Table A.5, where we consider effects of layoffs and UI benefits on a school's Absolute Risk Difference (ARD) in out-of-school suspensions, defined as $\text{SuspensionRate}_{Black} - \text{SuspensionRate}_{White}$.[25] Column (1) reports effects for our full sample of schools. We see that, in the absence of UI benefits, layoffs increase within-school racial disproportionality in suspensions. However, this effect is offset by more generous UI benefits. In Panels B and C, we again separate schools by their baseline non-White enrollment share. We see that layoff-induced increases in within-school racial disproportionality when UI benefits are low are larger for schools with higher shares of White students.

While prior literature shows that exclusionary discipline practices tend to be used more often in schools with a higher proportion of non-White students (Chin, 2021; Welsh and Little, 2018), layoffs may have a larger effect on Black students in predominantly White schools for at least

[25] See Rodriguez and Welsh (2022) for a complete discussion of the different metrics for measuring disproportionality and disparities in discipline. We prefer the ARD measure over alternatives, such as the Relative Risk Ratio (RRR), because it allows for the inclusion of schools in which no suspensions are given to a group.

two reasons. First, it may be the case that layoffs that affect predominantly White schools are concentrated among Black families, in turn generating larger effects on discipline rates of Black students. Second, it may be the case that predominantly White schools (with predominantly White teaching and administrative staff) perceive a Black student's behavioral response to a layoff as more severe than that of a White student due to a cultural mismatch between White teachers and administrators and Black students (Welsh and Little, 2018). Similarly, educators are more likely to have empathy for misbehaved students with a turbulent home life (i.e., experiencing a household labor shock) if the student is of the same race (Gilliam et al., 2016). The corollary is that schools with a higher non-White population may have staff with a greater capacity to understand changes in behavior, even if the baseline suspension rate is higher than that in predominantly White schools.

5.3 Robustness of Results

Our results in sections 5.1 and 5.2 rely on the assumption that, after accounting for unobservables at the year or state-by-year level, within-school variation in layoff exposure is unrelated to within-school variation in unobservable determinants of discipline outcomes. While this assumption is inherently untestable, we now present several pieces of evidence that suggest it is likely to hold in our setting and further test alternative specifications of our main results. We concentrate these robustness checks on out-of-school suspensions, which provide the most variation in our data and which have been studied most in prior literature.

5.3.1 Changes in Student and School Characteristics

First, in Table 5, we test whether layoffs are associated with changes in student and school characteristics and, if so, whether these effects vary by state UI generosity. Columns (1)-(3) present specifications analogous to those in Table 2, first adding state-by-year fixed effects and then augmenting the estimating equation with the layoff-UI interaction term. In Panel A, we find that layoffs do not change school enrollment in the year that they occur, nor are there differential effects across the UI distribution.[26] In Panels B and C, we find little evidence that student demo-

[26]This finding is consistent with Foote et al. (2018), who find that, during and following the Great Recession, non-participation the labor force —rather than out-migration —accounts for the majority of labor force exits following a mass layoff event. Thus, it is unlikely that families would

graphic characteristics (% FRPL and % non-White) change in response to layoffs, nor are there heterogeneous effects of UI generosity. Finally, in Panel D we find some evidence of a decline in student-teacher ratios when layoffs occur, but there is not a heterogeneous response across low and high UI states, making it unlikely that this change is driving our results.

The results in Table 5 provide little evidence that layoffs induce changes in student or school characteristics that may be driving our results. Nevertheless, we also estimate specifications that control for demographic characteristics directly. In column (2) of Appendix Table A.6, we estimate our preferred, UI-augmented specification for out-of-school suspensions, while controlling for a school's log-enrollment, % FRPL, and % non-White students. In columns (3) and (4), we also include year fixed effects interacted with indicators for commuting zones (CZs) —collections of counties that reflect where people live and work —to account for within-state changes in local labor markets and educational practices across different geographic areas. Across our specifications with demographic controls and/or CZ-by-year FEs, our estimates remain close to our main specification and statistically significant, indicating that neither demographic changes nor within-state regional trends are driving our results.

5.3.2 Differential Trends

Figure 4 provides event study estimates of how layoffs affect out-of-school suspensions across the UI benefit distribution, as outlined in equation (7).[27] First, in Panel A, we present the β_k coefficients that show how out-of-school suspensions evolve before and after a city's first layoff, net of any moderating UI benefit effects. We see limited evidence of differential pre-trends between cities that do and do not experience a layoff, and then see an increase in suspensions in the year where a layoff occurs —consistent with our main finding that layoffs increase suspensions when UI benefits are not sufficiently generous. We further see that this initial jump in out-of-school suspensions fades quickly in the years following a layoff event, supporting our functional form decision to concentrate on the contemporaneous effects of layoffs on discipline outcomes.

systematically move out of local public schools in response to layoffs during our sample period.

[27] For this event-study analysis, we restrict the sample to cities that are treated once or never treated; we omit cities that experience multiple layoffs in our sample period.

Panel B then shows the interaction effects —the γ_k coefficients in equation (7) —between our pre- and post-layoff indicators and a state's UI benefit generosity. Similar to Panel A, we do not find evidence of differential pre-trends. We then see a negative effect once a layoff has occurred. This finding is again consistent with our main result that the positive effect of layoffs on discipline outcomes is reduced as UI benefits become more generous. As in Panel A, we also see that this effect fades out quickly after a layoff occurs. Taken together, these event study results provide additional evidence that our findings are not driven by differential trends in discipline outcomes between cities that do and do not experience layoffs, further strengthening the validity of our identifying assumption.

Because our effects are consistently larger for Black and male students, we also estimate event study specifications separately for these subgroups. Appendix Figures A.1 and A.2 present these results. While less precisely estimated than our event study estimates of the full sample, these specifications indicate that our stronger effects for these subgroups are unlikely to be driven by differential trends prior to layoffs occurring. We do note, however, that while we fail to detect statistically significant pre-trends in any specification, this is largely due to the fact that these estimates are imprecise—we cannot necessarily rule out the presence of pre-trends. As such, we interpret these estimates with caution.

5.3.3 Other Social Safety Net Programs and Policies

Next, we address the concern that UI benefits may be correlated with other state-level social programs and policies and, thus, our interaction term is capturing heterogeneous effects not only across the UI generosity distribution, but across the distribution of social safety net program generosity more generally. To test whether our results capture generosity in other social programs, we re-estimate our main specification, including interaction terms between our layoff indicator and measures of a state's maximum Temporary Assistance for Needy Families (TANF) monthly benefits for a family of four, maximum supplemental security income (SSI) for individuals, maximum Earned Income Tax Credit (EITC) for a family with two dependents, maximum Supplemental Nutrition Assistance Program (SNAP) monthly benefits for a family of four, minimum wage, and

Medicaid expansion status.[28] We obtain measures of TANF, EITC, SNAP, and minimum wages from the University of Kentucky Center for Poverty Research (UKCPR, 2022) and SSI benefits from Schmidt et al. (2016). To ease the interpretation of the interaction terms, we standardize each of our continuous measures to each have a mean of 0 and a standard deviation of 1, while allowing the Medicaid expansion variable to be a 0/1 indicator.

Table 6 presents our results adding interactions between layoffs and these additional social programs and policies. The main effect of layoffs and the UI-layoff interaction term remain remarkably stable as we add these additional interactions. Moreover, in the final specification that includes interactions for the six measures discussed above, the magnitudes of both the main effect of layoffs and the UI interaction term become larger. Thus, we do not have reason to believe that our main results are contaminated by the generosity of other social programs.

The interaction terms with other benefit programs further reveal that TANF and the EITC may also impact exclusionary discipline following a layoff. The interaction term with TANF is negative, suggesting that, similar to UI, more generous TANF benefits also reduce suspensions following a layoff. However, we note that the TANF results are sensitive to specification. The interaction term with the EITC is positive and statistically significant, suggesting that more generous EITC benefits increase suspensions following a layoff. Because EITC benefits are only earned when working, a layoff could reduce the households' effective income (wage earnings plus EITC benefits) by a larger amount in high EITC states, exacerbating the financial strain of a layoff. These additional results underscore the importance of state-level social support programs in mitigating employment shocks, which is a fruitful area for future research.

While our interactions in Table 6 capture cross-state generosity in the most prominent social safety net programs and policies, it is possible that state policy environments differ in other ways that may correlate with UI generosity. Therefore, in Appendix Table A.7, we further show that our results are robust to interacting our layoff indicator with measures of a state's demographic (percent White, percent of adults with a bachelor's degree, and median household income) and political (Democratic governor and share of state legislature that are Democrats) profile.

[28]These measures all vary across states and years. Our state EITC and SSI measures are a combination of the maximum federal EITC benefits and state-level benefits.

5.3.4 Alternative Treatment Definitions

Next, in Table 7, we test the sensitivity of our main out-of-school suspension results to alternative treatment geographies and measures of layoff exposure. In Panel A, we continue to use a dummy variable for any layoff during an academic year, but consider layoffs at the school district and county levels, as opposed to the city level. These broader measures account for the fact that families may live, and/or parents may work, in different cities than where their children attend school. We find very similar results when measuring layoffs at the school district level and county levels, indicating that the definition of local area used does not meaningfully change our results.

In Panel B, we then define layoff treatment as experiencing above-median per capita job losses due to WARN-reported layoffs in a given year, relative to all observations with non-zero layoffs.[29] With this measure, we find very similar effects at the city level and larger estimates at the school district and county levels, suggesting that our effects may be larger when layoff events affect more members of a community. Lastly, panel C uses a dummy variable for experiencing per capita job losses due to WARN-reported layoffs in the top quartile of the non-zero layoff distribution. The results remain qualitatively similar but less precise for out-of-school suspensions: experiencing a top quartile layoff increases out-of-school suspensions, but the impact is mitigated by more generous UI.

5.3.5 Additional Specifications

In the Appendix, we present three additional sets of specifications to probe the robustness of our main results. First, Appendix Tables A.8 and A.9 re-estimate our primary specification, equation 3, for the overall sample and by subgroup using a traditional two-way fixed effects approach, rather than our preferred two-stage approach from Gardner et al. (2024). These estimates are of the same sign and generally similar magnitude to the main results, but they are less precise. Appendix Table A.10 then uses an alternative measure of UI generosity: the product of maximum weekly benefits and the states' UI recipiency rate. This alternative measure captures variation both in benefit levels and the share of unemployed workers who receive UI benefits. Results using this alternative UI

[29] In this specification and the one that follows, we drop all observations with below-median layoffs in order to compare those with above-median layoffs to those that do not experience layoffs.

measure mirror the core findings: at low benefit levels mass-layoffs increase discipline, with effects dissipating as UI benefits increase.

Next, we conduct a placebo exercise to probe whether our methodology generates spurious results in the absence of layoffs. To do so, we restrict the sample to never-treated schools, which reduces the sample size from 23,388 to 14,124. We then randomly assign treatment at the state-year level in the frequency that the true treatment occurred (e.g. if 18% of Michigan schools experienced layoffs in 2013, we randomly assign placebo treatment to 18% of the never-treated Michigan schools in 2013) and repeat our main specification from equation 3 on out-of-school suspensions. Appendix Figure A.3 displays the distribution of treatment effects from 1,000 iterations of this exercise for both our non-interacted layoff exposure term and our layoff-UI interaction term. Notably, both the main effect and interaction term are centered around zero. The true treatment effects are indicated with the dotted line and occur at the 94th percentile (main effect) and the 7th percentile (interaction effect) of the placebo distributions, consistent with our statistically significant main results.

Finally, to ensure no single state is driving our results, we re-estimate our main specification, dropping one state at a time. Figure 5 presents our results for both the main layoff effect and the layoff-UI interaction term. Across iterations, we consistently see that our results do not meaningfully change when we drop a state and in no iteration do we produce effects that are statistically different from our main specification that uses the full sample.

6 Conclusion

We provide the first analysis in the literature of the relationship between local layoff events, unemployment insurance generosity, and student disciplinary outcomes. By matching U.S. school-level disciplinary incidence data with firm-level local layoff events, we show that local labor market shocks increase out-of-school suspension rates when UI benefits are low, but UI benefits can moderate this effect if they are sufficiently generous. Specifically, our results show that at the lowest level of weekly UI benefits ($265), exposure to a local mass layoff increases the number of students receiving out-of-school suspensions in local middle and high schools by 5.1% from its mean. However, when UI generosity increases to approximately $500 per week, the effects of a layoff event on disciplinary incidence are reduced to zero.

We further find heterogeneous effects of layoffs by student gender, race, and school environment. Male students and Black students drive the documented increases in disciplinary incidence. At the lowest UI benefit level in our sample, layoffs increase out-of-school suspensions for Black students by four times as much as they do for White students, with even larger effects in predominantly White schools. Consequently, at low UI benefit levels, within-school racial disproportionality in out-of-school suspensions increases following local layoff events. However, as with the findings for the sample as a whole, these effects are reduced when UI benefits are sufficiently generous. Given prior research documenting the large long-run costs of suspensions —particularly for Black and male students —our findings suggest that the generosity of UI benefits following layoff events may play an important role in promoting academic achievement (Holt et al., 2022), reducing contact with the juvenile justice system Bacher-Hicks et al. (2024), and keeping students connected to their school communities (Kennedy-Lewis and Murphy, 2016).

More broadly, our results provide evidence that exclusionary discipline practices may change as a result of school and community context. Future research may wish to consider the mechanisms by which UI benefits reduce the impacts of layoff events on student disciplinary incidence. While we suspect that the primary mechanism is through counteracting the negative income shock due to unemployment and in turn allowing parents to spend more time with their children, these effects may be more or less pronounced for paternal or maternal unemployment. These effects may also be due to changes in teacher responses to student behavior in the context of community stress. Examination of these sorts of heterogeneous effects and mechanisms is essential to furthering our understanding of the complementary relationship between education and social policy, as well as policies that can reduce racial and gender disparities in student disciplinary outcomes.

References

Acton, R. (2021). Community College Program Choices in the Wake of Local Job Losses. *Journal of Labor Economics 39*.

Ahammer, A. and A. Packham (2020). Dying to Work: Effects of Unemployment Insurance on Health. Working Paper 27267, National Bureau of Economic Research.

Aliprantis, D., D. Carroll, and E. R. Young (2022). The dynamics of the racial wealth gap. Working Paper 19-18, Federal Reserve Bank of Cleveland.

Ananat, E. O., A. Gassman-Pines, D. V. Francis, and C. M. Gibson-Davis (2017). Linking job loss, inequality, mental health, and education. *Science 356*(6343), 1127–1128.

Angrist, J. D., P. D. Hull, P. A. Pathak, and C. R. Walters (2017). Leveraging Lotteries for School Value-Added: Testing and Estimation. *The Quarterly Journal of Economics 132*(2).

Arain, O. (2021). WARN Database. `https://layoffdata.com/data/`. Accessed May 2021.

Arum, R. and K. Ford (2012). How other countries "do discipline". *Educational Leadership 70*(2), 56–60.

Bacher-Hicks, A., S. B. Billings, and D. J. Deming (2024). The school-to-prison pipeline: Long-run impacts of school suspensions on adult crime. *American Economic Journal: Economic Policy 16*(4), 165–193.

Baker, A. C., D. F. Larcker, and C. C. Wang (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics 144*(2), 370–395.

Barrett, N., A. McEachin, J. N. Mills, and J. Valant (2019). Disparities and Discrimination in Student Discipline by Race and Family Income. *Journal of Human Resources*, forthcoming.

Becker, G. S. and N. Tomes (1986). Human Capital and the Rise and Fall of Families. *Journal of Labor Economics 4*(3, Part 2), S1–S39.

Bestenbostel, A. and A. Peralta (2021). Economic WARNings: The Impact of Negative News on Racial Animus. Working Paper.

Borusyak, K., X. Jaravel, and J. Spiess (2024). Revisiting event-study designs: robust and efficient estimation. *Review of Economic Studies*, rdae007.

Callaway, B., A. Goodman-Bacon, and P. H. Sant'Anna (2021). Difference-in-differences with a continuous treatment. arXiv preprint 2107.02637.

Carlson, K. (2015). Fear itself: The effects of distressing economic news on birth outcomes. *Journal of Health Economics 41*, 117–132.

Carrell, S. E., M. Hoekstra, and E. Kuka (2018). The long-run effects of disruptive peers. *American Economic Review 108*(11), 3377–3415.

Carrell, S. E. and M. L. Hoekstra (2010). Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. *American Economic Journal: Applied Economics 2*(1), 211–28.

Charles, K. K. and P. DeCicca (2008). Local labor market fluctuations and health: is there a connection and for whom? *Journal of health economics 27*(6), 1532–1550.

Chin, M. J. (2021). Jue insights: Desegregated but still separated? the impact of school integration on student suspensions and special education classification. *Journal of Urban Economics*, 103389.

Cylus, J., M. M. Glymour, and M. Avendano (2014). Do Generous Unemployment Benefit Programs Reduce Suicide Rates? A State Fixed-Effect Analysis Covering 1968–2008. *American Journal of Epidemiology 189*(1).

Davison, M., A. M. Penner, E. K. Penner, N. Pharris-Ciurej, S. R. Porter, E. K. Rose, Y. Shem-Tov, and P. Yoo (2021). School Discipline and Racial Disparities in Early Adulthood. *Educational Researcher*, forthcoming.

De Chaisemartin, C. and X. d'Haultfoeuille (2023). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *The Econometrics Journal 26*(3), C1–C30.

Deakin, J., E. Taylor, and A. Kupchik (2018). *The Palgrave international handbook of school discipline, surveillance, and social control*. Springer.

Deneault, C. (2025). Local labor markets and selection into the teaching profession.

Derenoncourt, E., C. H. Kim, M. Kuhn, and M. Schularick (2023, December). Changes in the Distribution of Black and White Wealth since the US Civil War. *Journal of Economic Perspectives 37*(4), 71–90.

Dizon-Ross, E., S. Loeb, E. Penner, and J. Rochmes (2019). Stress in boom times: Understanding teachers' economic anxiety in a high-cost urban district. *Aera Open 5*(4), 2332858419879439.

Federal Reserve Bank of Chicago (2024). National childcare spotlight: Parents and the labor force. `https://www.chicagofed.org/publications/chicago-fed-insights/2024/parents-and-labor-force`.

Finkelstein, A., M. Gentzkow, and H. Williams (2021). Place-Based Drivers of Mortality: Evidence from Migration. *American Economic Review 111*(8).

Foote, A. and M. Grosz (2020). The Effect of Local Labor Market Downturns on Postsecondary Enrollment and Program Choice. *Education Finance and Policy 15*(4).

Foote, A., M. Grosz, and A. H. Stevens (2018). Locate your nearest exit: Mass layoffs and local labor market response. *ILR Review 72*(1).

Gardner, J., N. Thakral, L. T. Tô, and L. Yap (2024). Two-Stage Differences in Differences. Working paper.

Gilliam, W. S., A. N. Maupin, C. R. Reyes, M. Accavitti, and F. Shic (2016). Do early educators' implicit biases regarding sex and race relate to behavior expectations and recommendations of preschool expulsions and suspensions. *Yale University Child Study Center 9*(28), 1–16.

Gilpin, G., E. Karger, and P. Nencka (2024). The returns to public library investment. *American Economic Journal: Economic Policy 16*(2), 78–109.

Gruber, J. (1997). The Consumption Smoothing Benefits of Unemployment Insurance. *American Economic Review 87*(1).

Hardy, B., H. D. Hill, and J. Romich (2019). Strengthening Social Programs to Promote Economic Stability During Childhood. *Social Policy Report 32*(2).

Hollingsworth, A., C. J. Ruhm, and K. Simon (2017). Macroeconomic conditions and opioid abuse. *Journal of health economics 56*, 222–233.

Holt, S. B., K. Vinopal, H. Choi, and L. C. Sorensen (2022). Strictly Speaking: Examining Teacher Use of Punishments and Student Outcomes. EdWorkingPaper 22-563, Annenberg Institute for School Reform at Brown University.

Hsu, J. W., D. A. Matsa, and B. T. Melzer (2018). Unemployment insurance as a housing market stabilizer. *American Economic Review 108*(1), 49–81.

Hubbard, D. (2018). The Impact of Local Labor Market Shocks on College Choice: Evidence from Plant Closings in Michigan. Working Paper.

Jones, L. (1991). Unemployed fathers and their children: Implications for policy and practice. *Child and Adolescent Social Work Journal 8*(2), 101–116.

Kalil, A. and K. M. Ziol-Guest (2008). Parental employment circumstances and children's academic progress. *Social Science Research 37*(2), 500–515.

Kennedy-Lewis, B. L. and A. S. Murphy (2016). Listening to "frequent flyers": What persistently disciplined students have to say about being labeled as "bad". *Teachers College Record 118*(1), 1–40.

Krolikowski, P. M. and K. G. Lunsford (2022). Advance layoff notices and aggregate job loss. *Journal of Applied Econometrics*.

Krueger, A. B. and A. Mueller (2010). Job search and unemployment insurance: New evidence from time use data. *Journal of Public Economics 94*(3-4), 298–307.

Kuka, E. (2020). Quantifying the Benefits of Social Insurance: Unemployment Insurance and Health. *Review of Economics and Statistics 102*(3).

Lacoe, J. and M. P. Steinberg (2018). Do Suspensions Affect Student Outcomes? *Educational Evaluation and Policy Analysis 41*(1).

Lindo, J. M. (2011). Parental job loss and infant health. *Journal of Health Economics 30*(5), 869–879.

Lindo, J. M., J. Schaller, and B. Hansen (2018). Caution! men not at work: Gender-specific labor market conditions and child maltreatment. *Journal of Public Economics 163*, 77–98.

Liu, J., M. S. Hayes, and S. Gershenson (2021). From Referrals to Suspensions: New Evidence on Racial Disparities in Exclusionary Discipline. Discussion Paper 14619, Institute of Labor Economics.

Liu, J., E. K. Penner, and W. Gao (2023). Troublemakers? the role of frequent teacher referrers in expanding racial disciplinary disproportionalities. *Educational Researcher*, 0013189X231179649.

Losen, D. J. and R. J. Skiba (2010). Suspended education: Urban middle schools in crisis. Technical report, Southern Poverty Law Center.

Lowenstein, C. (2024). "deaths of despair" over the business cycle: New estimates from a shift-share instrumental variables approach. *Economics & Human Biology 53*, 101374.

McIntosh, K., E. J. Girvan, R. Horner, and K. Smolkowski (2014). Education not incarceration: A conceptual model for reducing racial and ethnic disproportionality in school discipline. *Kent McIntosh, Erik J. Girvan, Robert H. Horner, & Keith Smolkowski, Education not Incarceration: A Conceptual Model For Reducing Racial and Ethnic Disproportionality in School Discipline 5*.

National Center for Education Statistics (2022). Serious disciplinary actions taken by public schools. `https://nces.ed.gov/programs/coe/indicator/a18/serious-disciplinary-actions?tid=4`.

Nikolova, M. and B. N. Nikolaev (2018). Family matters: The effects of parental unemployment in early childhood and adolescence on subjective well-being later in life. *Journal of Economic Behavior & Organization*.

Okonofua, J. A. and J. L. Eberhardt (2015). Two Strikes: Race and the Disciplining of Young Students. *Psychological Science 26*(5), 617–624. _eprint: https://doi.org/10.1177/0956797615570365.

O'Leary, C. J., W. E. Spriggs, and S. A. Wandner (2022, May). Equity in unemployment insurance benefit access. *AEA Papers and Proceedings 112*, 91–96.

Page, M., J. Schaller, and D. Simon (2019). The effects of aggregate and gender-specific labor demand shocks on child health. *Journal of Human Resources 54*(1), 37–78.

Perera, R. and M. K. Diliberti (2023). Survey: What purpose do suspensions serve? principals don't seem quite sure. Technical report, The Brookings Institution.

Rafa, A. (2018). Suspension and Expulsion: What Is the Issue and Why Does It Matter? Policy Snapshot. *Education Commission of the States*.

Regmi, K. (2019). Examining the Externality of Unemployment Insurance on Student Achievement. *Economic Inquiry 57*(1).

Rodriguez, L. A. and R. O. Welsh (2022). The dimensions of school discipline: Toward a comprehensive framework for measuring discipline patterns and outcomes in schools. *AERA Open 8*.

Roth, J., P. H. Sant'Anna, A. Bilinski, and J. Poe (2023). What's trending in difference-in-differences? a synthesis of the recent econometrics literature. *Journal of Econometrics 235*(2), 2218–2244.

Rubin, D. B. (1981). The Bayesian Bootstrap. *The Annals of Statistics 9*(1).

Ruiz-Valenzuela, J. (2020). Job loss at home: children's school performance during the great recession. *SERIEs 11*(3), 243–286.

Rumberger, R. W. and D. J. Losen (2016). The High Cost of Harsh Discipline and Its Disparate Impact. Technical report, The Civil Rights Project at UCLA.

Schaller, J. and M. Zerpa (2019). Short-run effects of parental job loss on child health. *American Journal of Health Economics 5*(1), 8–41.

Schmidt, L., L. Shore-Sheppard, and T. Watson (2016). The effect of safety-net programs on food insecurity. *Journal of Human Resources 51*(3), 589–614.

Shi, Y. and M. Zhu (2021). Equal Time for Equal Crime? Racial Bias in School Discipline. Discussion Paper 14306, Institute of Labor Economics.

Sorensen, L. C., S. D. Bushway, and E. J. Gifford (2022). Getting Tough? The Effects of Discretionary Principal Discipline on Student Outcomes. *Education Finance and Policy*, forthcoming.

Stevens, A. H. and J. Schaller (2011). Short-run effects of parental job loss on children's academic achievement. *Economics of Education Review 30*(2), 289–299.

Strunk, K. O., D. Goldhaber, D. S. Knight, and N. Brown (2018). Are there hidden costs associated with conducting layoffs? the impact of reduction-in-force and layoff notices on teacher effectiveness. *Journal of Policy Analysis and Management 37*(4), 755–782.

Swensen, I., J. M. Lindo, and K. Regmi (2023). Stable Income, Stable Family. *Review of Economics and Statistics*, forthcoming.

Terriquez, V., R. Chlala, and J. Sacha (2013). The impact of punitive high school discipline policies on the postsecondary trajectories of young men. Technical report, Pathways to Postsecondary Success.

Ubaldi, M. and M. Picchio (2023). Intergenerational scars: The impact of parental unemployment on individual health later in life. Discussion Paper 16103, Institute of Labor Economics.

University of Kentucky Center for Poverty Research (2022). UKCPR National Welfare Data, 1980-2020. `http://ukcpr.org/resources/national-welfare-data`.

U.S. Census Bureau (1994). *Geographic Areas Reference Manual (GARM)*.

U.S. Department of Education, Office for Civil Rights, Civil Rights Data Collection (2021). An overview of exclusionary discipline practices in public schools for the 2017-18 school year. `https://ocrdata.ed.gov/assets/downloads/crdc-exclusionary-school-discipline.pdf`.

US Department of Health and Human Services (2016). Spotlighting progress in policy and supports.

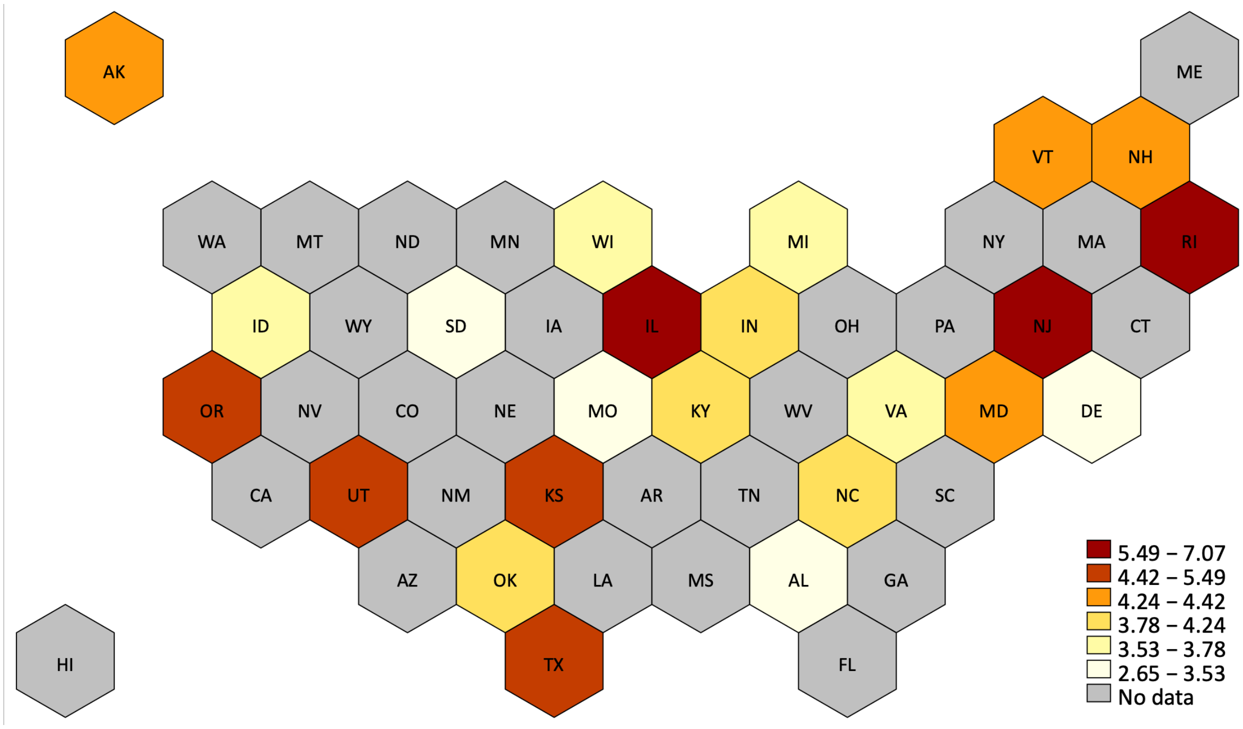
U.S. Department of Labor (2023). Plant closings and layoffs. `https://www.dol.gov/general/topic/termination/plantclosings`.

Weinstein, R. (2020). Local Labor Markets and Human Capital Investments. *Journal of Human Resources*, forthcoming.

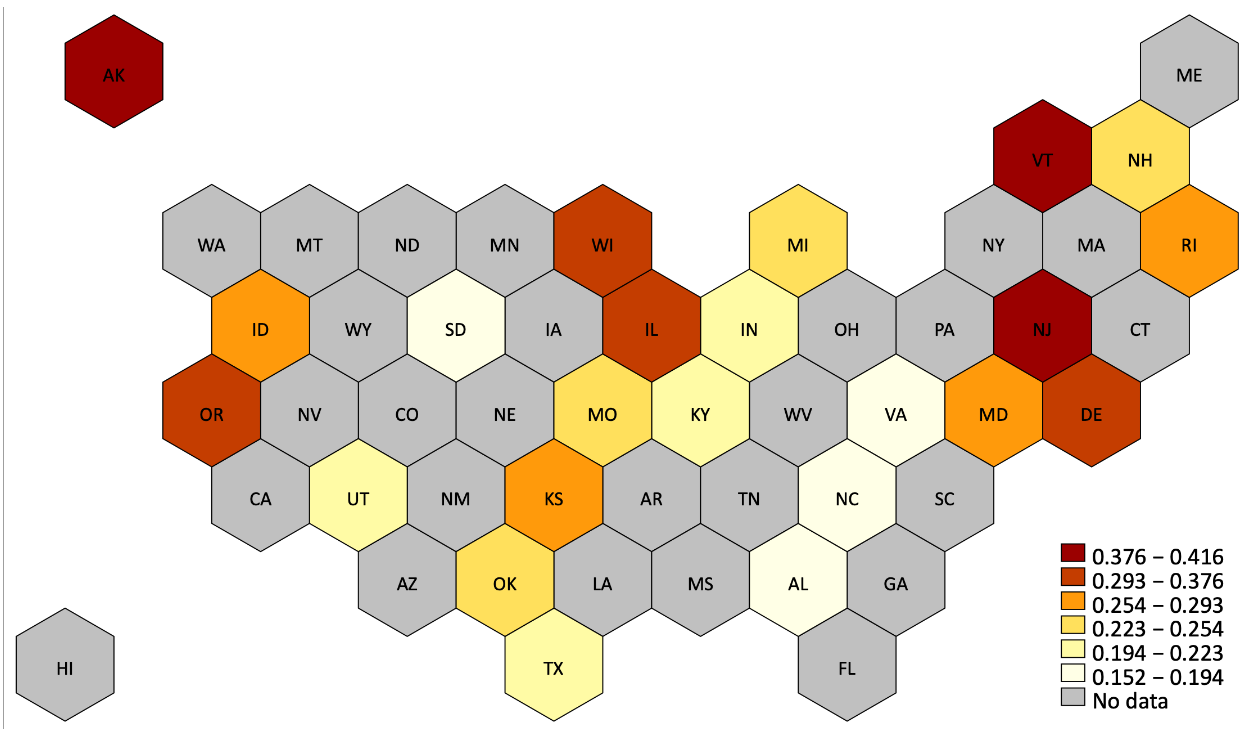
Welsh, R. O. and S. Little (2018). The Shcool Discipline Dilemma: A Comprehensive Review of Disparities and Alternative Aproaches. *Review of Educational Research 88*(5).

Figure 1: Unemployment Insurance Generosity

(a) Maximum Unemployment Insurance Benefits (100s of dollars)



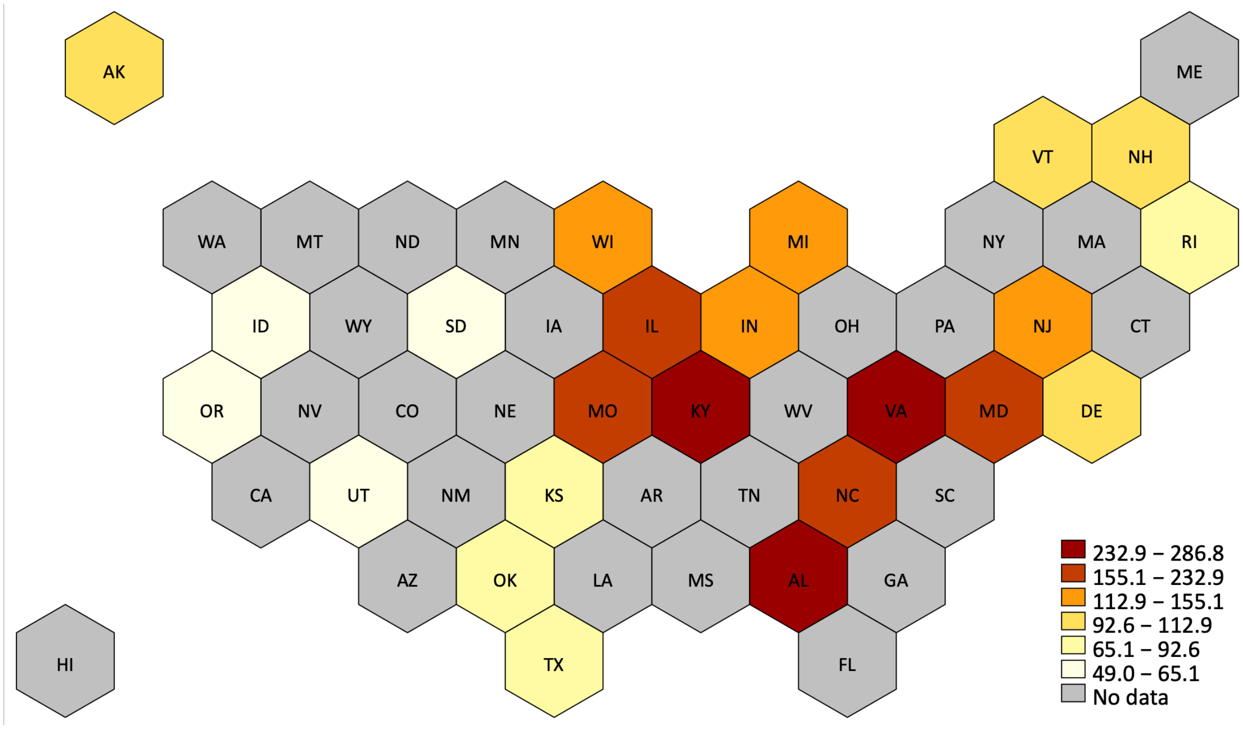
(b) Unemployment Insurance Recipiency Rate



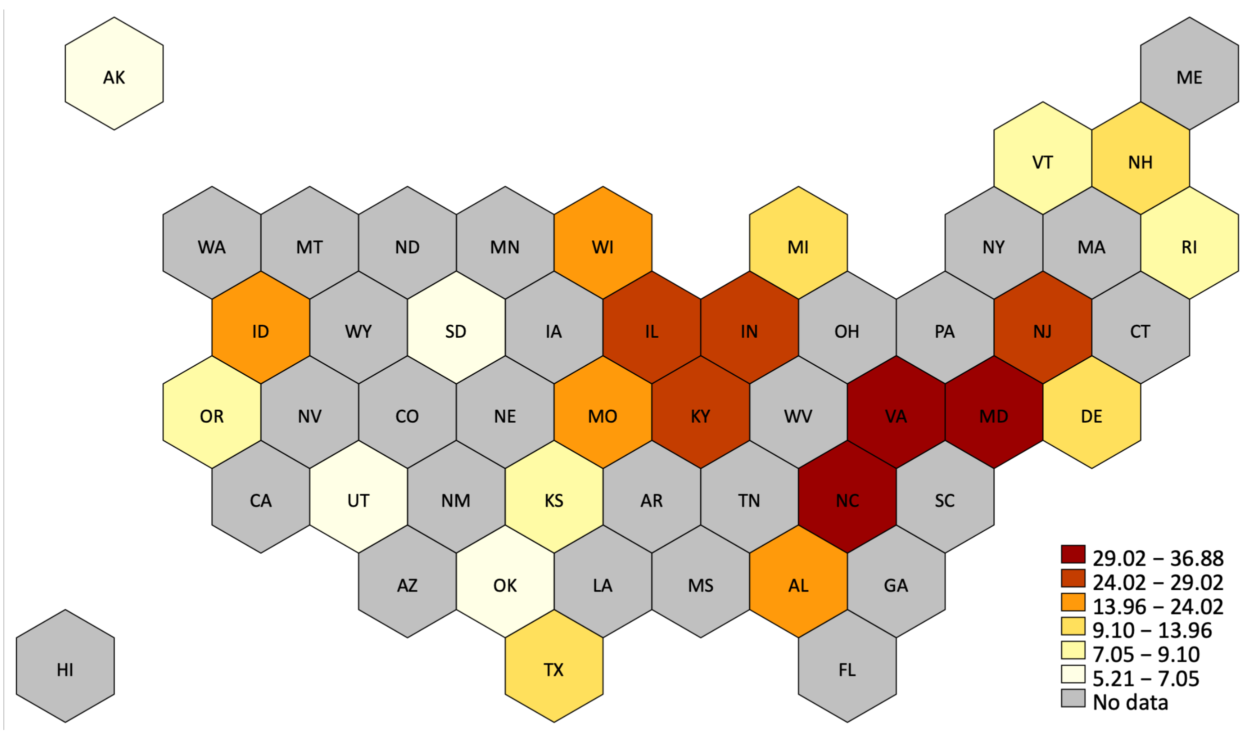
Notes: This figure depicts the average UI benefit generosity and recipiency rate for all states in our sample across all sample years (2011, 2013, 2015, 2017)

Figure 2: Average Layoff Size in Sample Years

(a) Average layoff size per 10,000 workers

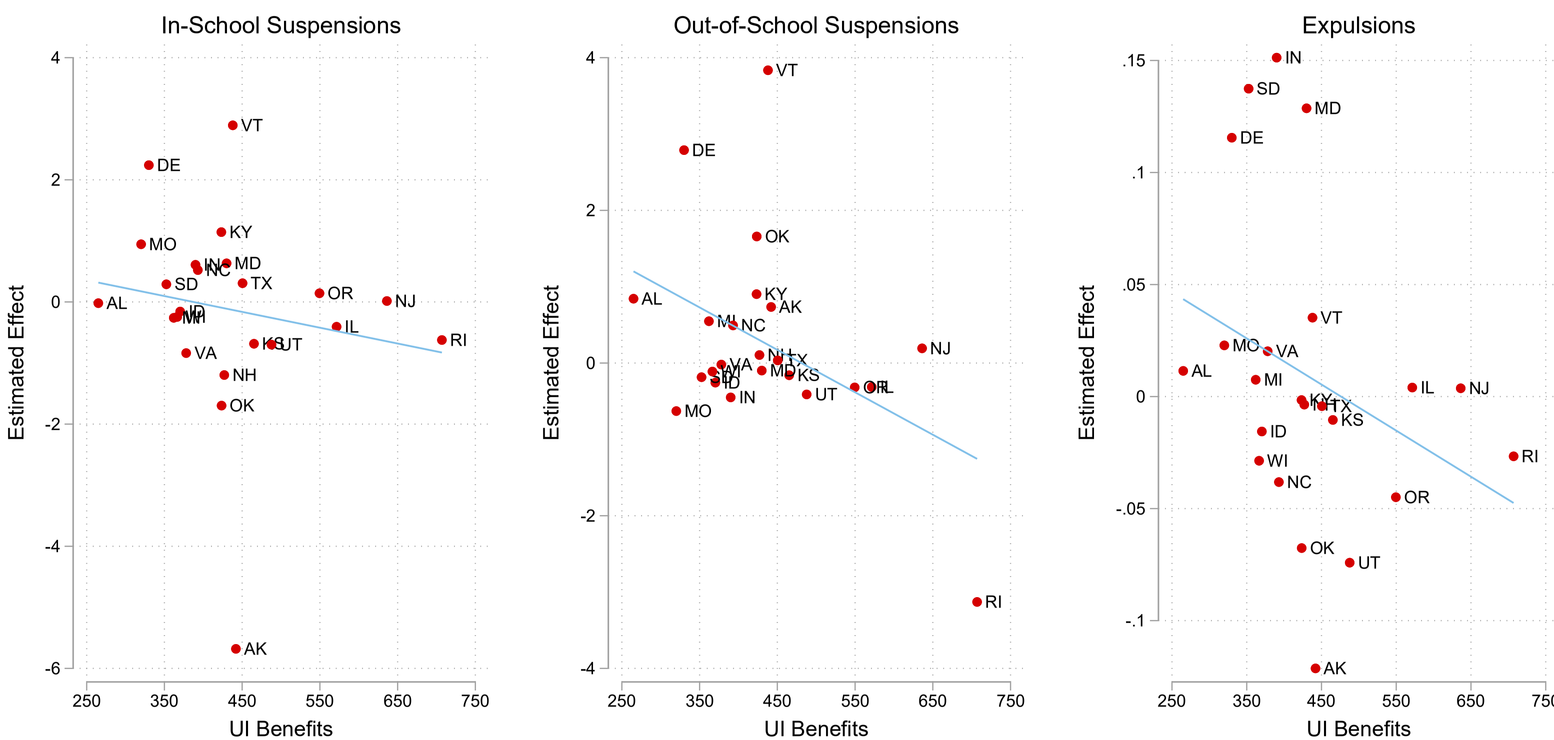


(b) Average layoff size per 10,000 workers (if layoff>0)



Notes: This figure depicts the average layoff size in our sample years for all states in our sample. Panel A shows the average for all observations and Panel B restricts to those places in which layoffs are greater than 0.

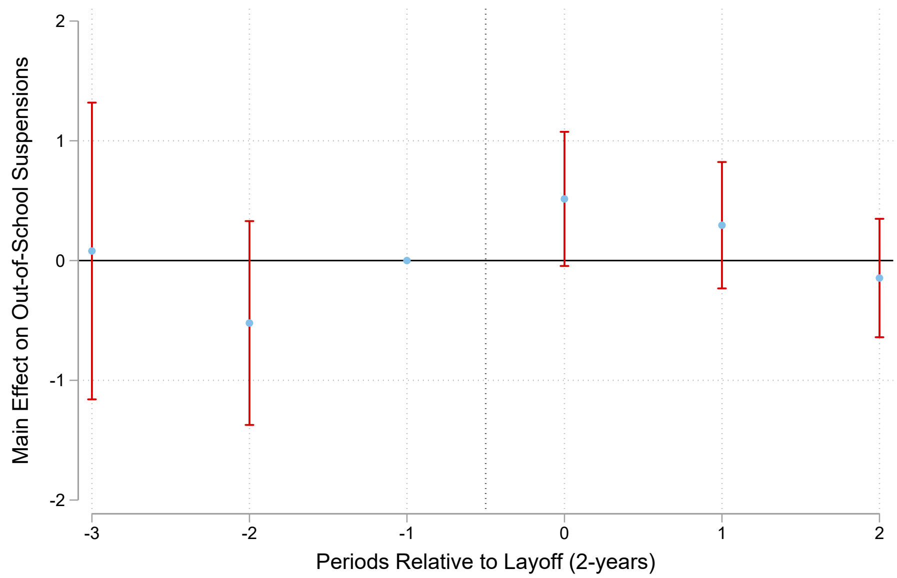
Figure 3: Relationship Between Layoff Effects & State UI Benefits



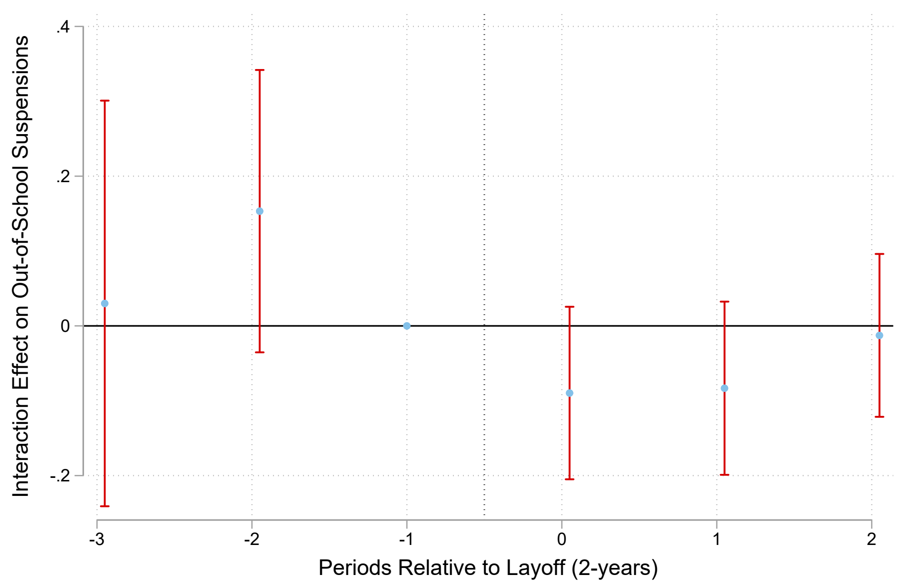
Notes: This figure plots the relationship between state-specific layoff effects and mean state UI benefits across the 23 states in the sample.

Figure 4: Event Studies for Out-of-School Suspensions

(a) Effect of Layoff Without Interaction

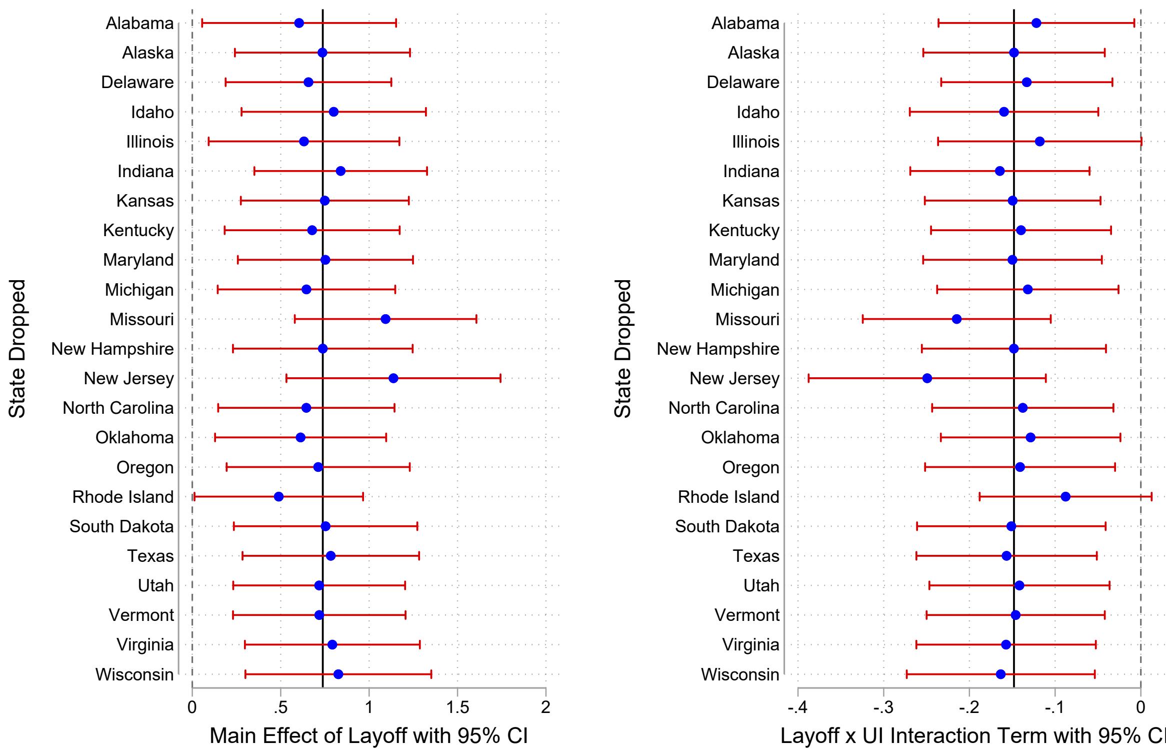


(b) Interacted Effect of Layoff x UI Generosity



Notes: This figure plots the coefficients and corresponding 95% confidence intervals for an event study that includes state-year and school fixed effects. Panel A presents coefficients for the main effect of a layoff, while Panel B shows the coefficients for an interaction between the maximum weekly UI benefits (in \$100s) and a layoff. The sample is restricted to cities that are treated once or never treated yielding 19,768 observations out of the total sample of 23,388 (observations from multi-treated locations are omitted). Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian boostrapping with 500 iterations.

Figure 5: Estimated Effect of Layoffs: Robustness to Omitting Each State



Notes: This figure depicts the main effect of a Layoff and the Layoff x UI interaction term from our preferred specification using samples that iteratively drop a single state.

Table 1: Summary Statistics

	Mean	**Std. Dev.**	**Min.**	**Max.**
	(1)	**(2)**	**(3)**	(4)
Panel A. School Characteristics				
Enrollment	828.2	586.4	16.00	4885
% FRPL	0.419	0.211	0.000	1.000
% Non-White	0.338	0.249	0.002	1.000
City	0.084	0.278	0.000	1.000
Suburb	0.359	0.480	0.000	1.000
Town	0.208	0.406	0.000	1.000
Rural	0.349	0.477	0.000	1.000
Panel B. Discipline Outcomes				
In-school suspensions per 100 (all)	10.31	9.758	0.000	63.89
In-school suspensions per 100 (Black)	18.54	20.29	0.000	100.0
In-school suspensions per 100 (male)	13.63	12.29	0.000	68.57
Out-of-school suspensions per 100 (all)	6.742	5.696	0.000	43.81
Out-of-school suspensions per 100 (Black)	13.41	14.43	0.000	100.000
Out-of-school suspensions per 100 (male)	9.115	7.295	0.000	52.29
Expulsions per 100 (all)	0.212	0.537	0.000	7.110
Expulsions per 100 (Black)	0.290	1.301	0.000	21.62
Expulsions per 100 (male)	0.308	0.771	0.000	9.091
Panel C. Labor Market Characteristics				
Ever experience layoff (city)	0.510	0.500	0.000	1.000
Ever experience layoff (S.D.)	0.553	0.497	0.000	1.000
Experience layoff (city)	0.138	0.345	0.000	1.000
Experience layoff (S.D.)	0.231	0.421	0.000	1.000
Layoffs per 10,000 if layoffs > 0 (city)	147.5	399.5	0.311	8571
Layoffs per 10,000 if layoffs > 0 (S.D.)	49.26	119.1	0.000	2481
Maximum UI weekly benefits	440.0	99.81	265.0	707.0
Unique Schools		5,847		
School-Year Obs.		23,388		

Notes: Summary statistics are displayed for the full analysis sample of school-year observations. In-school suspensions refer to the CRDC in-school suspension variable, out-of-school suspensions aggregate single and multiple out-of-school suspensions reported by the CRDC, and expulsions aggregate those with and without services.

Table 2: Effects of Layoffs & UI on Discipline Outcomes

	(1)	(2)	(3)
Panel A. In-School Suspensions			
Exposed to layoff	-0.104	0.046	0.549
	(0.093)	(0.095)	(0.387)
Exposed to layoff X UI			-0.111
			(0.085)
Dependent Var. Mean	10.31	10.31	10.31
Observations	23,388	23,388	23,388
Panel B. Out-of-School Suspensions			
Exposed to layoff	0.074	0.066	0.738***
	(0.053)	(0.057)	(0.244)
Exposed to layoff X UI			-0.148***
			(0.052)
Dependent Var. Mean	6.742	6.742	6.742
Observations	23,388	23,388	23,388
Panel C. Expulsions			
Exposed to layoff	-0.001	0.004	0.056**
	(0.008)	(0.009)	(0.025)
Exposed to layoff X UI			-0.011**
			(0.005)
Dependent Var. Mean	0.212	0.212	0.212
Observations	23,388	23,388	23,388
School FEs	X	X	X
Year FEs	X		
State-Year FEs		X	X

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. Each outcome is scaled to incidence per 100 students. UI is measured in 100s of dollars. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Effects of Layoffs & UI on Discipline Outcomes, by Subgroup

	All (1)	**Black** (2)	**White** (3)	**Male** (4)	**Female** (5)
Panel A. In-School Suspensions					
Exposed to layoff	0.549	-0.518	0.413	0.825*	0.456
	(0.387)	(0.888)	(0.360)	(0.483)	(0.300)
Exposed to layoff X UI	-0.111	0.092	-0.057	-0.169	-0.093
	(0.085)	(0.191)	(0.079)	(0.111)	(0.067)
Dependent Var. Mean	10.32	18.54	8.582	13.63	6.772
Observations	23,388	23,388	23,388	23,388	23,388
Panel B. Out-of-School Suspensions					
Exposed to layoff	0.738***	2.403***	0.505**	1.035***	0.597***
	(0.244)	(0.660)	(0.247)	(0.349)	(0.193)
Exposed to layoff X UI	-0.148***	-0.512***	-0.096*	-0.222***	-0.108***
	(0.052)	(0.138)	(0.054)	(0.075)	(0.040)
Dependent Var. Mean	6.742	13.41	5.464	9.115	4.227
Observations	23,388	23,388	23,388	23,388	23,388
Panel C. Expulsions					
Exposed to layoff	0.056**	0.193***	0.006	0.050	0.062***
	(0.026)	(0.066)	(0.021)	(0.037)	(0.019)
Exposed to layoff X UI	-0.011**	-0.035***	-0.003	-0.011	-0.012***
	(0.005)	(0.013)	(0.004)	(0.008)	(0.004)
Dependent Var. Mean	0.212	0.290	0.175	0.308	0.109
Observations	23,388	23,388	23,388	23,388	23,388

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school-fixed effects are included in all specifications. Each outcome is scaled to incidence per 100 students. UI is measured in 100s of dollars. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Heterogeneous Effects on Out-of-School Suspensions

	All (1)	**Low Disc.** (2)	**High Disc.** (3)	**Low Pov.** (4)	**High Pov.** (5)	**Middle School** (6)	**High School** (7)	**Rural** (8)	**Not Rural** (9)	**Low Non-White** (10)	**High Non-White** (11)
Panel A. All Students											
Exposed to layoff	0.738***	-0.364	1.058**	0.447*	0.856	0.968	0.619	1.807***	0.416	0.727***	0.629
	(0.244)	(0.359)	(0.452)	(0.262)	(0.555)	(0.786)	(0.439)	(0.568)	(0.369)	(0.263)	(0.475)
Exposed to layoff X UI	-0.148***	0.107	-0.234**	-0.108**	-0.153	-0.173	-0.146	-0.370***	-0.076	-0.169***	-0.113
	(0.052)	(0.071)	(0.098)	(0.056)	(0.120)	(0.161)	(0.096)	(0.131)	(0.075)	(0.055)	(0.099)
Dependent Var. Mean	6.742	3.635	9.851	4.737	8.749	6.891	6.635	6.674	6.828	5.254	8.231
Observations	23,388	11,694	11,692	11,696	11,692	9,718	13,230	12,557	10,144	11,692	11,676
Panel B. Out-of-School Suspensions - Black Students											
Exposed to layoff	2.403***	-0.039	3.579***	2.419**	2.534*	3.440*	1.680	3.101*	2.247**	4.196***	1.483*
	(0.666)	(1.402)	(1.052)	(1.030)	(1.330)	(1.895)	(1.388)	(1.646)	(0.910)	(1.234)	(0.873)
Exposed to layoff X UI	-0.512***	-0.006	-0.740***	-0.574***	-0.487*	-0.743*	-0.360	-0.681*	-0.455**	-0.988***	-0.254
	(0.138)	(0.291)	(0.224)	(0.212)	(0.278)	(0.394)	(0.282)	(0.374)	(0.182)	(0.264)	(0.181)
Dependent Var. Mean	13.41	8.561	18.26	11.50	15.32	13.77	13.15	12.50	14.56	12.18	14.65
Observations	23,388	11,694	11,692	11,696	11,692	9,718	13,230	12,557	10,144	11,692	11,676
Panel C. Out-of-School Suspensions - Male Students											
Exposed to layoff	1.035***	-0.444	1.562***	0.594*	1.315*	1.516	0.798	2.345***	0.576	0.680*	1.191*
	(0.349)	(0.460)	(0.593)	(0.358)	(0.761)	(1.021)	(0.575)	(0.773)	(0.472)	(0.375)	(0.611)
Exposed to layoff X UI	-0.222***	0.132	-0.368***	-0.163**	-0.252	-0.295	-0.200	-0.490***	-0.116	-0.184**	-0.235*
	(0.075)	(0.095)	(0.130)	(0.076)	(0.168)	(0.213)	(0.129)	(0.183)	(0.097)	(0.080)	(0.130)
Dependent Var. Mean	9.115	5.153	13.08	6.664	11.57	9.489	8.842	9.149	9.072	7.444	10.79
Observations	23,388	11,694	11,692	11,696	11,692	9,718	13,230	12,557	10,144	11,692	11,676

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school fixed effects are included in all specifications. The outcome variable is out-of-school suspensions, which is measured as incidences per 100 students. UI is measured in 100s of dollars. "Low" and "High" designations are determined a school being above or below the median value for each respective variable in a given year. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effects of Layoffs & UI on Student & School Characteristics

	(1)	(2)	(3)
Panel A. Log(Enrollment)			
Exposed to layoff	0.001	-0.001	-0.003
	(0.001)	(0.001)	(0.007)
Exposed to layoff X UI (100s)			0.001
			(0.002)
Observations	23,388	23,388	23,388
Panel B. % FRPL			
Exposed to layoff	-0.004***	-0.001	0.001
	(0.001)	(0.001)	(0.005)
Exposed to layoff X UI (100s)			-0.000
			(0.001)
Observations	23,388	23,388	23,388
Panel C. % Non-White			
Exposed to layoff	0.001	0.000	0.003
	(0.001)	(0.001)	(0.002)
Exposed to layoff X UI (100s)			-0.001
			(0.000)
Observations	23,388	23,388	23,388
Panel D. Student-Teacher Ratio			
Exposed to layoff	-0.105***	-0.049**	-0.125
	(0.021)	(0.021)	(0.089)
Exposed to layoff X UI (100s)			0.017
			(0.020)
Observations	22,858	22,858	22,858
Year FEs	X	X	X
State-Year FEs		X	X

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. UI is measured in 100s of dollars. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Robustness to Interacting Layoffs with Other Social Safety Net Programs and Policies: Out-of-School Suspensions

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Exposed to layoff	0.738***	0.666***	0.956***	0.758***	0.762***	0.647**	0.823***	0.822**
	(0.244)	(0.243)	(0.275)	(0.246)	(0.259)	(0.293)	(0.258)	(0.321)
Exposed to layoff X UI	-0.148***	-0.133**	-0.198***	-0.152***	-0.154***	-0.127**	-0.173***	-0.179**
	(0.052)	(0.053)	(0.060)	(0.053)	(0.056)	(0.064)	(0.059)	(0.071)
Exposed to layoff X TANF (std.)		-0.076						-0.171**
		(0.058)						(0.071)
Exposed to layoff X EITC (std.)			0.117*					0.157**
			(0.063)					(0.072)
Exposed to layoff X SSI (std.)				0.070				0.061
				(0.052)				(0.062)
Exposed to layoff X SNAP (std.)					-0.061			0.050
					(0.062)			(0.065)
Exposed to layoff X Minimum Wage (std.)						-0.040		-0.087
						(0.061)		(0.083)
Exposed to layoff X Medicaid Expansion (0/1)							0.135	0.239
							(0.155)	(0.247)
Observations	23,388	23,388	23,388	23,388	23,388	23,388	23,388	23,388

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school fixed effects are included in all estimations. UI is measured in 100s of dollars and TANF, EITC, SSI, and SNAP benefits, and minimum wage are standardized to have a mean of 0 and standard deviation of 1. Medicaid Expansion is a dichotomous variable equal to 1 for state-years with expanded Medicaid. The outcome variable is out-of-school suspensions, which is measured as incidences per 100 students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Robustness to Alternative Geographic Areas & Layoff Definitions: Out-of-School Suspensions

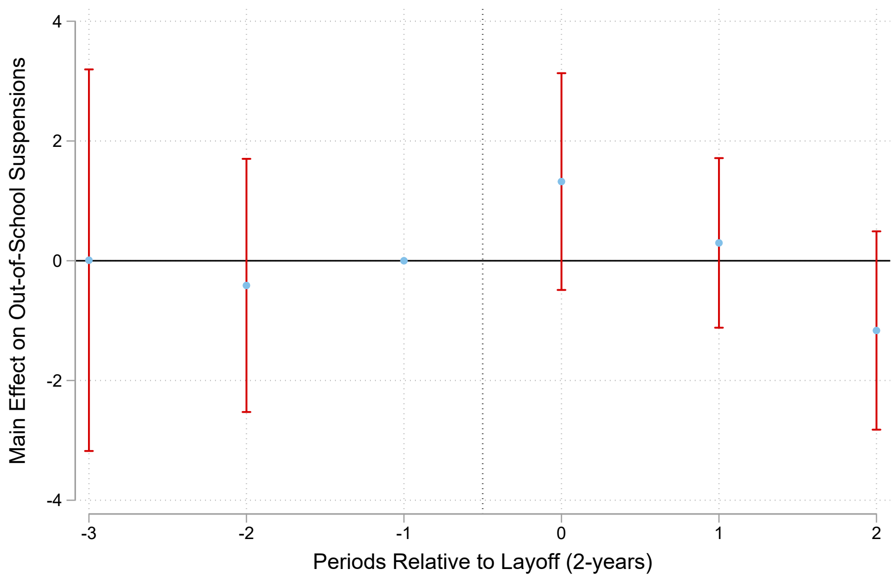
	City Level (1)	**School Dist. Level (2)**	**County Level (3)**
Panel A. Dummy Variable for Any Layoff			
Exposed to layoff	0.738***	0.838***	1.404***
	(0.244)	(0.282)	(0.462)
Exposed to layoff X UI	-0.148***	-0.146**	-0.291**
	(0.052)	(0.062)	(0.115)
Observations	23,388	20,536	12,687
Panel B. Dummy Variable for Above-Median Layoff			
Exposed to layoff	0.718**	1.217***	1.506**
	(0.294)	(0.345)	(0.605)
Exposed to layoff X UI	-0.147**	-0.226***	-0.293**
	(0.062)	(0.073)	(0.149)
Observations	22,365	19,618	11,654
Panel C. Dummy Variable for Top Quartile Layoff			
Exposed to layoff	0.494	1.213***	1.702**
	(0.417)	(0.392)	(0.840)
Exposed to layoff X UI	-0.080	-0.237***	-0.292
	(0.084)	(0.084)	(0.204)
Observations	21,642	18,915	11,008

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school fixed effects are included in all estimations. UI is measured in 100s of dollars. The outcome variable is out-of-school suspensions, which is measured as incidences per 100 students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

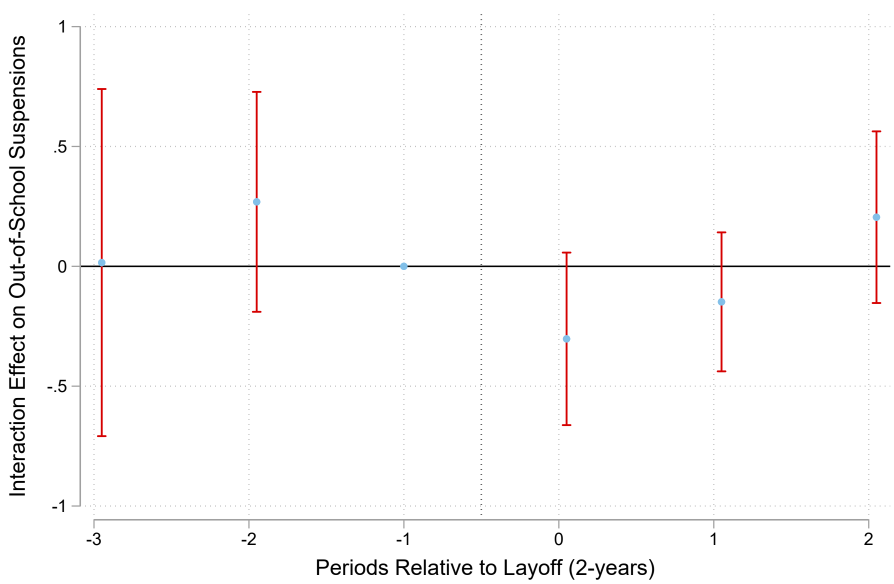
Online Appendix: Not for Publication

Figure A.1: Event Studies for Out-of-School Suspensions: Black Students

(a) Effect of Layoff Without Interaction



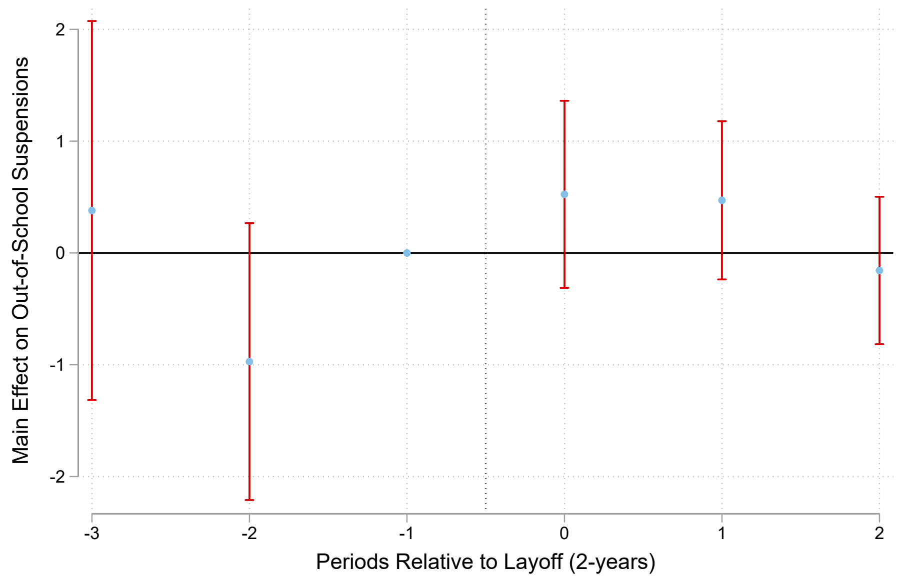
(b) Interacted Effect of Layoff x UI Generosity



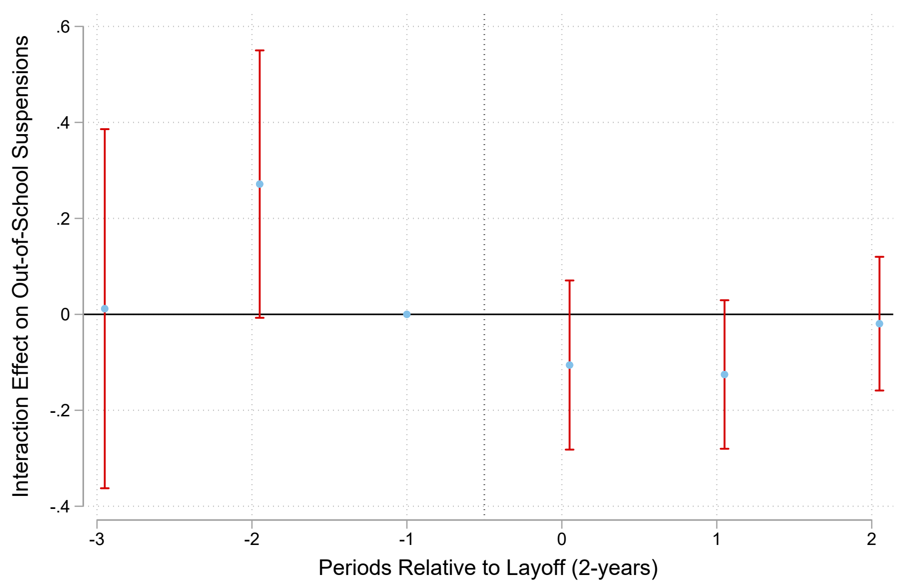
Notes: This figure plots the coefficients and corresponding 95% confidence intervals for an event study that includes state-year and school fixed effects. Panel A presents coefficients for the main effect of a layoff, while Panel B shows the coefficients for an interaction between the maximum weekly UI benefits (in \$100s) and a layoff. The sample is restricted to cities that are treated once or never treated yielding 19,768 observations out of the total sample of 23,388 (observations from multi-treated locations are omitted). Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian boostrapping with 500 iterations.

Figure A.2: Event Studies for Out-of-School Suspensions: Male Students

(a) Effect of Layoff Without Interaction

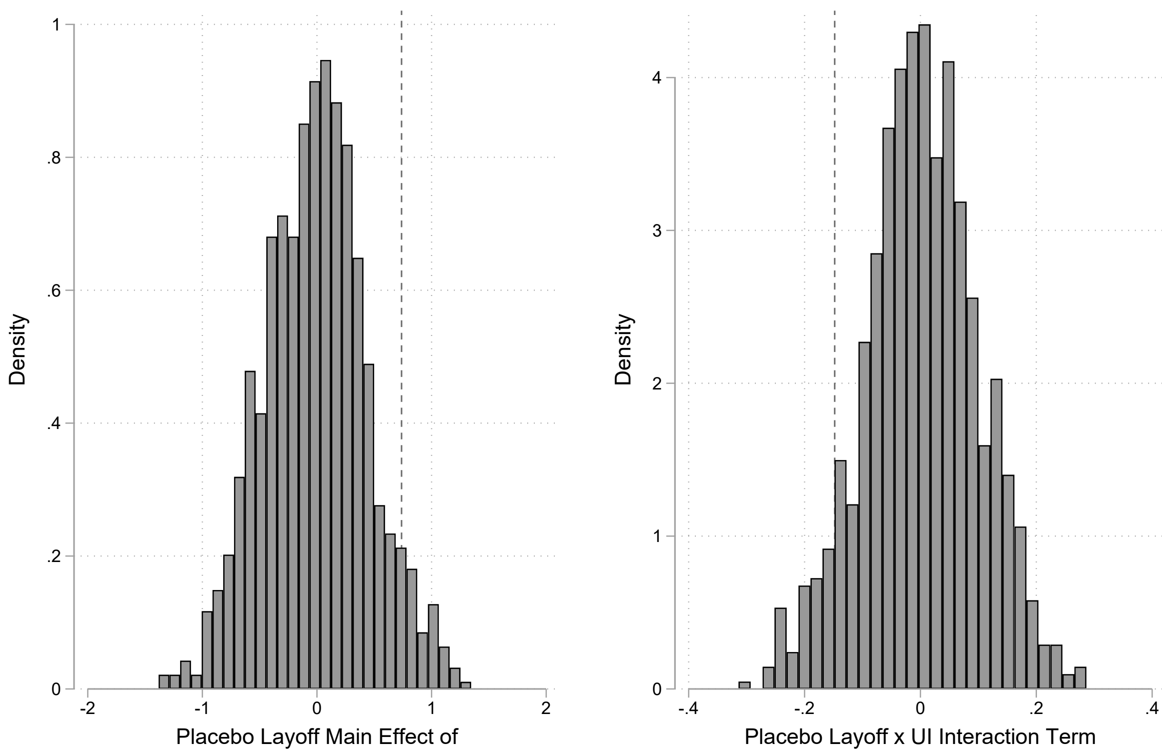


(b) Interacted Effect of Layoff x UI Generosity



Notes: This figure plots the coefficients and corresponding 95% confidence intervals for an event study that includes state-year and school fixed effects. Panel A presents coefficients for the main effect of a layoff, while Panel B shows the coefficients for an interaction between the maximum weekly UI benefits (in $100s) and a layoff. The sample is restricted to cities that are treated once or never treated yielding 19,768 observations out of the total sample of 23,388 (observations from multi-treated locations are omitted). Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian boostrapping with 500 iterations.

Figure A.3: Distribution of Placebo Treatment Effects



Notes: This figure plots the the distribution of treatment effects from 1,000 iterations of a placebo exercise where we randomly assign layoff exposure to a sample of schools that never experience a layoff. See Section 5.3.5 of the main text for details.

Table A.1: States in Sample

State:	Number of Layoff Events (1)	Total Workers Laid Off (2)	School-Years Affected by Layoffs (3)	Total Number of Schools (4)
Alabama	33	6741	84	234
Alaska	2	302	6	30
Delaware	4	642	16	39
Idaho	21	2120	65	69
Illinois	145	27,450	368	525
Indiana	62	7,871	134	215
Kansas	20	7,978	76	177
Kentucky	34	7,927	90	242
Maryland	35	3,857	113	210
Michigan	64	9,932	158	391
Missouri	46	8,986	163	333
New Hampshire	14	1,438	31	65
New Jersey	146	22,605	295	455
North Carolina	101	21,752	339	517
Oklahoma	23	4,825	76	205
Oregon	27	3,397	89	142
Rhode Island	7	982	27	50
South Dakota	5	390	16	50
Texas	135	22,297	637	1,144
Utah	15	3,296	66	138
Vermont	4	319	9	28
Virginia	57	8,844	187	298
Wisconsin	83	7,643	176	290
Total	1,083	181,594	3,221	5,847

Notes: This table presents the number of layoffs that occurred, the total number of workers that were laid off, the school-years affected by layoffs, and the number of schools (regardless of treatment status) in each state in our sample in years 2011, 2013, 2015, and 2017.

Table A.2: Comparison of Analysis Sample to National Sample

	Analysis Sample				National Sample			
	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	Mean (5)	Std. Dev. (6)	Min. (7)	Max. (8)
Panel A. School Characteristics								
Enrollment	828.2	586.4	16.00	4885	839.6	624.2	2.000	5839
% FRPL	0.419	0.211	0.000	1.000	0.493	0.256	0.000	1.000
% Non-White	0.338	0.249	0.002	1.000	0.457	0.313	0.004	1.000
City	0.084	0.278	0.000	1.000	0.261	0.439	0.000	1.000
Suburb	0.359	0.480	0.000	1.000	0.332	0.471	0.000	1.000
Town	0.208	0.406	0.000	1.000	0.149	0.356	0.000	1.000
Rural	0.349	0.477	0.000	1.000	0.258	0.438	0.000	1.000
Panel B. Discipline Outcomes								
In-school Suspensions per 100 (all)	10.31	9.758	0.000	63.89	10.27	11.88	0.000	100.0
In-school Suspensions per 100 (Black)	18.54	20.29	0.000	100.0	17.13	20.59	0.000	100.0
In-school Suspensions per 100 (male)	13.63	12.29	0.000	68.57	13.13	14.21	0.000	100.0
Out-of-school Suspensions per 100 (all)	6.742	5.696	0.000	43.81	9.358	10.45	0.000	100.0
Out-of-school Suspensions per 100 (Black)	13.41	14.43	0.000	100.0	16.66	18.60	0.000	100.0
Out-of-school Suspensions per 100 (male)	9.115	7.295	0.000	52.29	11.99	12.14	0.000	100.0
Expulsions per 100 (all)	0.212	0.537	0.000	7.110	0.480	2.025	0.000	93.55
Expulsions per 100 (Black)	0.290	1.301	0.000	21.62	0.890	4.620	0.000	100.0
Expulsions per 100 (male)	0.308	0.771	0.000	9.091	0.657	2.467	0.000	94.74
Panel C. Labor Market Characteristics								
Ever experience a layoff (city)	0.510	0.500	0.000	1.000				
Ever experience a layoff (school district)	0.553	0.497	0.000	1.000				
Experience layoff (city)	0.138	0.345	0.000	1.000				
Experience layoff (school district)	0.231	0.421	0.000	1.000				
Layoffs per 10,000 if layoffs >0 (city)	147.5	399.5	0.311	8571				
Layoffs per 10,000 if layoffs >0 (school district)	49.26	119.1	0.000	2481				
Maximum UI weekly benefits (state-year level)	434.1	103.4	265.0	707.0	441.8	117.9	235.0	713.0
Unemployment rate (state-year level)	5.92	2.03	2.700	11.00	6.05	2.100	2.400	13.00
Unique Schools		5,847				22,293		
School-Year Obs		23,388				89,172		

Notes: Summary statistics are displayed for the full analysis sample of school-year observations in column (1) and an analogous national sample in column (2). In-school suspensions refer to the CRDC in-school suspension variable, out-of-school suspensions aggregate single and multiple out-of-school suspensions reported by the CRDC, and expulsions aggregate those with and without services.

Table A.3: Relationship between County-Level Employment Changes and Prior Year WARN Layoffs

	All **(1)**	**Male** **(2)**	**Female** **(3)**	**White** **(4)**	**Black** **(5)**
Panel A. YOY Count Change					
Exposed to layoff, t-1	-120.4***	-87.56***	-32.86	-94.71***	-23.77***
	(42.59)	(25.22)	(23.80)	(35.08)	(7.857)
Observations	4,592	4,592	4,592	4,592	4,592
Panel B. YOY Percent Change					
Exposed to layoff, t-1	-0.010***	-0.013***	-0.006***	-0.010***	-0.005
	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)
Observations	4,592	4,592	4,592	4,592	4,592

Notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and are clustered at the county level. County-level employment counts are derived from the Quarterly Workforce Indicators (QWI). County and year fixed effects included in all models.

Table A.4: Effects of Layoffs & UI on Detailed Discipline Outcomes

	Out-of-School Suspensions			**Expulsions**		
	All	**One**	**Mult**	**All**	**With**	**Without**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Exposed to layoff	0.738***	0.344**	0.394***	0.056**	0.037	0.019*
	(0.244)	(0.163)	(0.152)	(0.025)	(0.022)	(0.011)
Exposed to layoff X UI	-0.148***	-0.075**	-0.073**	-0.011**	-0.008*	-0.003
	(0.052)	(0.035)	(0.033)	(0.005)	(0.005)	(0.002)
Observations	23,388	23,388	23,388	23,388	23,388	23,388

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school fixed effects are included in all estimations. UI is measured in 100s of dollars. Outcome variables are measured as incidences per 100 students. Columns (1) and (4) reproduce our main results for out-of-school suspensions and expulsions, respectively. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Effects of Layoffs & UI on Racial Disproportionality in Out-of-School Suspensions

	All	**Low Non-White**	**High Non-White**
	(1)	**(2)**	**(3)**
Exposed to layoff	1.898***	3.508***	1.195*
	(0.627)	(1.131)	(0.688)
Exposed to layoff X UI	-0.416***	-0.831***	-0.211
	(0.135)	(0.244)	(0.146)
Observations	23,388	11,692	11,676

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school fixed effects are included in all estimations. UI is measured in 100s of dollars. Racial disproportionality is measured as within-school Black-white differences in out-of-school suspension rates using the adjusted risk difference (ARD). "High" and "Low" are determined based on a school being above or below the median proportion of non-white students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Robustness to Inclusion of Additional Control Variables

	(1)	**(2)**	**(3)**	**(4)**
Exposed to layoff	0.738***	0.722***	1.049***	1.026***
	(0.244)	(0.256)	(0.292)	(0.282)
Exposed to layoff X UI	-0.148***	-0.145***	-0.201***	-0.196***
	(0.052)	(0.054)	(0.062)	(0.058)
Observations	23,388	23,388	23,123	23,123
State-Year FEs	X	X		
CZ-Year FEs			X	X
Dem. Controls		X		X

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. School fixed effects are included in all estimations. Demographic controls include the proportion of free and reduced-price lunch students, the proportion of non-white students, and the logarithm of enrollment. UI is measured in 100s of dollars. The outcome variable is out-of-school suspensions, which is measured as incidences per 100 students. Minor sample size changes across columns are due to dropping observations with missing demographic controls or commuting zones with only a single school. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Robustness to Interacting Layoffs with State Sociopolitical Factors: Out-of-School Suspensions

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Exposed to layoff	0.738***	0.824***	0.677**	0.762***	0.748***	0.916***
	(0.244)	(0.249)	(0.306)	(0.278)	(0.252)	(0.329)
Exposed to layoff X UI	-0.148***	-0.168***	-0.134**	-0.154**	-0.148***	-0.188***
	(0.052)	(0.053)	(0.068)	(0.062)	(0.054)	(0.072)
Exposed to layoff X Percent White (std.)		-0.185***				
		(0.058)				
Exposed to layoff X Percent Bachelors Degree (std.)			-0.026			
			(0.072)			
Exposed to layoff X Median HH Income (std.)				0.011		
				(0.064)		
Exposed to layoff X Dem Governor (0/1)					-0.043	
					(0.129)	
Exposed to layoff X Dem Legislator Share (std.)						0.062
						(0.081)
Observations	23,388	23,388	23,388	23,388	23,388	23,388

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school fixed effects are included in all estimations. UI is measured in 100s of dollars and Percent White, Percent Bachelors Degree, Median HH Income and Democratic House Share are standardized to have a mean of 0 and standard deviation of 1. Democratic Governor is a dichotomous variable equal to 1 if the state is ruled by a democratic governor in year t. The outcome variable is out-of-school suspensions, which is measured as incidences per 100 students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Effects of Layoffs & UI on Discipline Outcomes, TWFE Approach

	(1)	**(2)**	**(3)**
Panel A. In-School Suspensions			
Exposed to layoff	-0.110	0.0504	0.403
	(0.175)	(0.172)	(0.743)
Exposed to layoff X UI			-0.078
			(0.161)
Observations	23,388	23,388	23,388
Panel B. Out-of-School Suspensions			
Exposed to layoff	0.083	0.074	0.502
	(0.104)	(0.107)	(0.463)
Exposed to layoff X UI			-0.095
			(0.099)
Observations	23,388	23,388	23,388
Panel C. Expulsions			
Exposed to layoff	0.000	0.007	0.069
	(0.017)	(0.016)	(0.051)
Exposed to layoff X UI			-0.014
			(0.010)
Observations	23,388	23,388	23,388
School FEs	X	X	X
Year FEs	X		
State-Year FEs		X	X

Notes: Standard errors are clustered at the city level. UI is measured in 100s of dollars. Outcome variables are measured as incidences per 100 students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: Effects of Layoffs & UI on Discipline Outcomes, by Subgroup, TWFE Approach

	All (1)	**Black** (2)	**White** (3)	**Male** (4)	**Female** (5)
Panel A. In-School Suspensions					
Exposed to layoff	0.403	-0.695	0.348	0.586	0.345
	(0.743)	(1.740)	(0.670)	(0.951)	(0.592)
Exposed to layoff X UI	-0.078	0.117	-0.046	-0.119	-0.066
	(0.161)	(0.369)	(0.145)	(0.205)	(0.129)
Dependent Var. Mean	10.31	18.54	8.582	13.63	6.772
Observations	23,388	23,388	23,388	23,388	23,388
Panel B. Out-of-School Suspensions					
Exposed to layoff	0.502	2.136	0.273	0.692	0.420
	(0.463)	(1.328)	(0.458)	(0.617)	(0.353)
Exposed to layoff X UI	-0.095	-0.459*	-0.042	-0.146	-0.068
	(0.099)	(0.276)	(0.101)	(0.133)	(0.073)
Dependent Var. Mean	6.742	13.41	5.464	9.115	4.227
Observations	23,388	23,388	23,388	23,388	23,388
Panel C. Expulsions					
Exposed to layoff	0.069	0.217	0.010	0.067	0.070*
	(0.051)	(0.140)	(0.042)	(0.074)	(0.038)
Exposed to layoff X UI	-0.014	-0.042	-0.004	-0.014	-0.013*
	(0.010)	(0.027)	(0.008)	(0.015)	(0.008)
Dependent Var. Mean	0.212	0.290	0.175	0.308	0.109
Observations	23,388	23,388	23,388	23,388	23,388

Notes: Standard errors are clustered at the city-level. State-by-year and school-fixed effects are included in all specifications. UI is measured in 100s of dollars. Each outcome is scaled to incidence per 100 students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Effects of Layoffs & UI on Discipline Outcomes, Alternative UI Measure

	Max UI	**Max UI X Recipiency Rate**
	(1)	**(2)**
Panel A. In-School Suspensions		
Exposed to layoff	0.549	0.554***
	(0.387)	(0.215)
Exposed to layoff X UI	-0.111	-0.410***
	(0.085)	(0.152)
Observations	23,388	23,388
Panel B. Out-of-School Suspensions		
Exposed to layoff	0.738***	0.272**
	(0.244)	(0.122)
Exposed to layoff X UI	-0.148***	-0.166**
	(0.052)	(0.084)
Observations	23,388	23,388
Panel C. Expulsions		
Exposed to layoff	0.056**	0.027*
	(0.025)	(0.014)
Exposed to layoff X UI	-0.011**	-0.019**
	(0.005)	(0.008)
Observations	23,388	23,388

Notes: Estimates use Gardner et al. (2024)'s two-stage estimator. City-level clustered standard errors are calculated via Bayesian bootstrapping with 500 iterations for each specification. State-by-year and school-fixed effects are included in all specifications. UI is measured in 100s of dollars in column (1) and 100s of dollars times the state's recipiency rate in column (2). Each outcome is scaled to incidence per 100 students. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.