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

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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 overall. However, effects differ across the UI benefit distribution. At the lowest benefit level ($265/week), a mass layoff increases out-of-school suspensions by 4.5%, 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.

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1 Introduction

Exclusionary discipline practices such as suspensions and expulsions are widely used in U.S. public schools to manage student behavior. 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., 2019), 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, are disproportionately more likely to experience these practices —and their negative consequences (Rumberger and Losen, 2016; Welsh and Little, 2018). Given the prevalence of these 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.

At the family level, if a student's involvement with 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. Concurrently, at the community level, stressful community-wide events like large employment shocks could potentially heighten stress levels in families and among disciplinarians (e.g., teachers and principals) and thereby influence their likelihood of employing exclusionary measures. At the same time, however, 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 local labor market shocks within a geographic area (e.g., a city or a school district) over time. We further control for unobservable changes at the national and state levels by including year or state-by-year fixed effects. We operationalize this approach using Gardner (2022)'s two-stage estimator, 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 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 annual in-school suspensions by 0.4 per 100 students, out-of-school suspensions by 0.3 per 100 students, and expulsions by 0.037 per 100 students—increases of 4%, 4.5%, and 17% relative to their respective means of 10.3, 6.7, and 0.21 per 100 students. These effects dissipate when UI benefits reach $480-$600, approximately the top quartile of benefits in our sample. These effects are similar if we instead measure layoff events at the school district level, rather than the city level, or if we use a continuous measure of layoff exposure. We further show that our results are not driven by changes in student characteristics induced by layoffs and are robust to including interactions of layoff exposure and other social safety net programs, such as TANF and EITC benefits.

Our estimated effects are consistently larger for Black students than for White students. For example, at the lowest UI benefit level, layoffs increase out-of-school suspensions 0.98 per 100 students (7.3% of the mean rate of 13.3 per 100 students) for Black students, relative to 0.25 per 100 students (4.5% of the mean rate of 5.46 per 100 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 out-of-school suspensions for Black students by 1.79 per 100 students —nearly twice the effect 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.

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 mass layoff events influence student test scores (Ananat et al., 2011), 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, and test scores (Stevens and Schaller, 2011). We contribute new evidence to this literature that local labor market shocks also affect children's exposure to exclusionary discipline practices in schools, which may contribute to the documented declines in academic achievement by removing students from instructional settings during suspensions and expulsions.

Second, we contribute to a 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 substantially higher rates than White students, even when they are involved in the same incidents (Barrett et al., 2019; Shi and Zhu, 2021; Liu et al., 2021). We find that these racial gaps in suspensions are 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 does not only play 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. 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). Indeed, 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 diffuse 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). In addition, we may expect that disciplinarians themselves, such as teachers and principals, respond to poor economic conditions and increased household or community stress by reducing their tolerance for misbehavior, which can determine whether misbehavior is managed internally within the classroom or through exclusionary strategies (Welsh and Little, 2018). Thus, in the wake of community-wide layoffs, teachers may increase their use of exclusionary discipline even if student behavior does not change.

On the other hand, it is possible that parents spend more time with their children when local labor market opportunities decline (Jones, 1991; Kalil and Ziol-Guest, 2008), potentially offsetting effects from 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 and the gender 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).

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 disciplinarians' 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 that the effects of layoffs and UI benefits will 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). Previous findings also suggest that Black students are subjected to disciplinary action more often than their White peers (Losen 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). In addition, evidence suggests that teachers are more likely to surveil the behavior of Black students than White 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. 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

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 characteristics (free and reduced-price lunch, student demographics, etc.). 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 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).[1]

We narrow our sample to schools that are most representative of traditional U.S. public schools 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. Second, we omit schools governed by regional, state, or federal agencies, as well as

[1] 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.

charter, specialized, and “other” districts. 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.[2] Third, we limit our sample to schools that report discipline outcomes separately for White, Black, male and female students. We then match all school districts in this sample to county subdivisions and Census place codes using the Missouri Census Data Center (MCDC) geographic correspondence engine.[3] For the less than 1% of school districts that are not matched with the MCDC, we proceed to manually link to place codes.[4]

Ultimately, our final sample consists of over 5,000 schools in 2,758 cities across 23 US states for the 2011, 2013, 2015, and 2017 school years. 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 our without 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.[5] Table 1 provides summary statistics on these outcomes.[6] 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 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

[2]For example, in a regional (e.g., county), state, or federally operated school, we would expect students 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.

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

[4]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.

[5]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.2, we consider these outcomes separately.

[6]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.

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.

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 with 100 or more employees to provide at least 60 days' notice to employees ahead of a mass layoff or plant closing affecting 50 or more employees at a single employment site (U.S. Department of Labor, 2023). These announcements are public information and allow us to construct a measure for layoff prevalence in specific locations, such as cities, school districts, and counties. Additionally, some states within our sample have passed "mini-WARN" acts, which can enforce reporting requirements for smaller employers and/or smaller layoff events.[7]

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, a number of 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, 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.[8] To do so, we match place codes provided by the MCDC to the location names in each WARN noticed filed. Due to concerns about use of two-way fixed effects with continuous variables (Callaway et al., 2021), our primary specification uses an indicator treatment variable

[7]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.

[8]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.

equal to one if the school was exposed to a layoff in that academic year. However, we also present results using a continuous measure of layoffs per 10,000 working-age population. To create the continuous measure, we collect data on the working-age population (aged 15-65) for each location included in the sample from the U.S. Census Bureau's population tables to normalize the layoff data relative to the size of the location in which the event occurred. We construct city-level layoff exposure measures as:

$$\text{Layoffs}_{ct} = \frac{\Sigma_n \text{Layoffs}_{ct}}{\text{Population}_{ct}} \times 10000 \tag{1}$$

where c denotes a city, t denotes an academic year (which we define as July 1 of year t to June 30 of year $t+1$), and n denotes the number of WARN notices filed in city c in academic year t. As such, Layoffs_{ct} provides the per capita number of workers laid off in city c in year t, based on the WARN notices filed. We construct school district layoffs in an analogous manner after 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. We also sum the layoffs and populations of all cities within a county to construct county-level layoff measures.

Panel C of Table 1 provides summary statistics on the layoffs observed in our sample using these measures. Because schools that experience layoffs in every year of our data do not contribute identifying variation, we restrict our sample to schools that experience layoffs in 0-3 of the academic years 2011, 2013, 2015, and 2017.[9] 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 job over the course of an academic year. Figure 1 plots the mean school-level layoff prevalence over our sample, illustrating how layoff exposure varies across states.

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

[9]Similar to our discipline data, we also exclude schools that experience per capita layoffs that are greater than 4 standard deviations above the mean layoff event, then further windsorize our data to exclude the top 1% of layoff events.

state and year, as reported by the U.S. Department of Labor in July of each year. Maximum UI benefits is a commonly used summary measure of UI generosity in the literature (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. Figure 2 provides a visual depiction of the variation in mean UI generosity across states.

4 Empirical Strategy

We estimate the impacts of local layoff events on student disciplinary outcomes by first estimating baseline equations of the following form:

$$\text{Discipline}_{ist} = \beta \text{Layoff}_{it} + \lambda_i + \theta_t + \mathbf{X}_{it}\mathbf{\Gamma} + \varepsilon_{ist} \tag{2}$$

where Discipline_{st} is the number of students disciplined (i.e., suspended or expelled) per 100 students enrolled in school i, located in state s, during academic year t. Layoff_{it} is an indicator variable equal to one if a layoff event occurred in the same city or school district as school i during academic year t. 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 grade levels. θ_t is a school-invariant time fixed effect that accounts for unobserved time trends in discipline rates. $\mathbf{X}_{it}$ contains time-varying school-level control variables that plausibly affect disciplinary prevalence in a school, such as the percentage of students who qualify for free and reduced price lunch (FRPL) and the percent of non-White students. ε_{ist} is an idiosyncratic error term.

The fixed effects capture two important sources of unobserved heterogeneity within our data: differences in school discipline rates across schools and over time. Specifically, the school fixed effects allow us to control for differences in school climate and culture surrounding discipline across schools, as well as location-specific effects, such as varying state laws on allowable disciplinary actions or city-wide initiatives to place police officers in schools. Likewise, the time fixed effects allow us to control for unobserved heterogeneity across time, which may encompass national shifts in discipline culture over years, specifically in response to growing research and evidence regarding the negative relationship between discipline and academic outcomes. Thus, the identifying assumption for β to represent the causal effect of layoffs on student disciplinary outcomes is that there are no changes in unobserved determinants of student discipline at the school-level that are correlated with labor market shocks. While this assumption is inherently untestable, we provide suggestive evidence that it is likely to hold by (1) accounting for state-specific changes in labor markets and discipline practices by including state-by-year fixed effects, (2) testing for whether student composition or school inputs change in response to layoffs, and (3) controlling for generosity in other state-level social programs.

To investigate the potential mitigating effect of UI following a layoff event, we augment the baseline equation with an interaction between Layoff_{ist} and UI_{st}:

$$\text{Discipline}_{ist} = \beta \text{Layoff}_{it} + \gamma(\text{Layoff}_{it} \times \text{UI}_{st}) + \lambda_i + \theta_t + \mathbf{X}_{it}\mathbf{\Gamma} + \varepsilon_{ist}$$

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. In interacted specifications, β is the effect of a layoff event with no UI benefits and γ is the change in effect due to a \$100 increase in maximum weekly UI benefits.

To address the concern that our school and year fixed effects may be contaminated by the true treatment effect we wish to estimate, we employ a variation of Gardner (2022)'s two-stage estimator, which is robust to heterogeneous treatment effects with staggered timing. In our first stage, we estimate our school and year (or state-by-year) fixed effects using only untreated observations: schools that never experience layoffs across our four years of discipline data and schools that experience layoffs, but in years where layoffs do not occur. We then residualize our outcomes using these estimated fixed effects. In the second stage, we regress the residuals on our layoff and UI variables to obtain estimates of β and γ. We construct our standard errors using a Bayesian bootstrapping procedure with 500 iterations per specification (Rubin, 1981).[10]

[10]The Bayesian bootstrap approach smooths bootstrap samples by reweighting, rather than resampling, observations, which ensures we estimate

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 state-by-year fixed effects to account for changes in state labor markets and state-level discipline practices. 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.

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 a statistically significant *increase* in discipline rates. Additional UI *decreases* this effect by a statistically significant amount. At the lowest level of UI benefits (\$265), layoff exposure increases in-school suspensions by 0.4 per 100 students, out-of-school suspensions by 0.31 per 100 students, and expulsions by 0.037 per 100 students—increases of 4%, 4.5%, and 17% relative 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 by 0.14 per 100 students, or 2.1% relative to the mean.

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 to layoffs based on UI generosity, we estimate the effects of a layoff event separately for each state in our sample. From these state-specific estimates, we are able to compare the effect that layoff events have on suspensions and expulsions in a given state

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) and Finkelstein et al. (2021).

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.2 segments the analysis by the type of disciplinary action students experience. 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 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 students receiving multiple suspensions throughout the school year. Likewise, columns (5) and (6) show results for expulsions with and without services. For expulsions, the significant 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 large effects for male students. The effects are also larger for Black students, but none of the estimates across racial subgroups are statistically significant at conventional levels.

Our results for out-of-school suspensions in Panel B are much more precise and we find that the effects of layoffs and of UI are larger for Black students and male students.[11] At the lowest benefit levels in our sample, layoffs increase out-of-school suspensions by 0.98 per 100 students (7.3% of

[11] 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.

mean) for Black students but only by 0.25 per 100 students (4.5% of mean) for White students. By gender, at the lowest benefit levels, layoffs increase out-of-school suspensions by 0.42 per 100 students (4.6% of mean) for male students and by 0.30 per 100 students (7.2% of mean) for female students. In Panel C, we also see that the effects on expulsions are substantially larger for Black students, but similar 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. Particularly for Black students, and somewhat for male students, we find that our effects are larger in schools with above-median discipline rates at baseline. This finding aligns with our results in Appendix Table A.2 that indicate that the increase in out-of-school suspensions when UI benefits are low are driven by schools suspending more students multiple times during an academic year, which is more likely to happen in schools with higher reliance on suspensions, that is, above-median discipline rates.

In columns (3) and (4), we split the sample by schools' baseline FRPL percentage. We generally find similar effects for schools that are below and above the median FRPL rate, although the effects for Black students are somewhat larger in *low* poverty schools. Columns (5) and (6) 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 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.3, where we consider effects on two measures: (1) a school's Absolute Risk Difference (ARD), defined as $\text{Discipline}_{Black} - \text{Discipline}_{White}$ and (2) a school's Relative Risk Ratio (RRR), defined as $\text{Discipline}_{Black}/\text{Discipline}_{White}$.[12]

Panel A uses the ARD and RRR measures as dependent variables for our full sample of schools. For both measures —and statistically significantly so for the ARD —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 this increase in within-school racial disproportionality in response to layoffs when UI benefits are low is almost entirely driven by schools with low shares of non-White students.

While prior literature shows that exclusionary discipline practices tend to be used more 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 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 the discipline 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 disciplinarians 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.

[12]See Rodriguez and Welsh (2022) for a complete discussion of the different metrics for measuring disproportionality and disparities in discipline. The ARD allows for inclusion of schools in which no suspensions are given to a group, but does not allow for interpretation as based on *relative* magnitudes. The RRR informs relative magnitudes, but does not allow for students in the comparison group to receive 0 suspensions. Thus, we present estimates using both the ARD and RRR.

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 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 and test alternative specifications of our main results.

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.[13] In Panels B and C, we find little evidence that student demographic characteristics (% FRPL and % non-White) change in response to layoffs, nor are there heterogeneous effects. 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.4, 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 niether demographic changes nor within-state regional

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

trends are driving our results.

Next, we address the concern that UI benefits may be correlated with other state-level social programs and, thus, our interaction term is capturing heterogeneous effects not only across the UI generosity distribution, but across states that vary in their generosity of social safety net programs more generally. To test for whether our results capture generosity in other social programs, we re-estimate our main specification, including interaction terms with a state's maximum Temporary Assistance for Needy Families (TANF) monthly benefits for a family of four and the maximum Earned Income Tax Credit (EITC) for a family with two dependents.[14] We obtain these measures from the University of Kentucky Center for Poverty Research (UKCPR, 2022) and, to ease in the interpretation of the interaction terms, standardize them to each have a mean of 0 and a standard deviation of 1.

Table 6 presents our results adding interactions between layoffs and these additional social programs. We find that, if anything, the interaction terms with these additional social program benefit measures are *positive*, indicating that they exacerbate —rather than offset —our main layoff effect. Moreover, our main effects on layoffs and the UI-layoff interaction term become larger in magnitude and more precise as we add these additional interactions. Thus, we do not have reason to believe that our results are contaminated by the generosity of other social programs.

Finally, 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. We find very similar results when measuring layoffs at the school district level and qualitatively similar, though attenuated and less precise, results when using a broader, county-level measure. This finding suggests that very local layoffs matter most in affecting school discipline. In Panel B, we then define layoff treatment as experiencing an above-median layoff in a given year.[15] With this measure, we find larger and more precise effects at both the city and school district levels, while the county-level results remain attenuated

[14]Both measures vary across states and years. Our state EITC measures are a combination of the maximum federal EITC benefits and state-level benefits.

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

and insignificant.

Lastly, panel C uses a continuous measure of layoffs per capita, measured in standard deviation units. The results remain qualitatively similar for out-of-school suspensions: a one-standard deviation increase in layoff exposure increases out-of-school suspensions, but the impact is mitigated by more generous UI. However, we interpret these results with some caution because they conflate the average response from a larger exposure to a layoff and differences in treatment effects between schools that experience higher and lower layoff exposure (Callaway et al., 2021).

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 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), out-of-school suspensions increase by 4.5% from its mean following a local layoff event. However, when UI generosity increases to $480 to $600 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. The increases in disciplinary incidence that we document are driven primarily by male and Black students. At the lowest UI benefit level, Black students experience a percentage point increase in out-of-school suspensions that is 4 times larger than their White counterparts, 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 to suspensions for male and Black students (Bacher-Hicks et al., 2019), our findings suggest that the generosity of UI benefits following layoff events may play an important role in promoting educational attainment and reducing future incarceration for students from these groups.

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. Examination of these sorts of heterogeneous effects is essential to furthering our understanding of the complementary relationship between education and social policy, as well as how policies can reduce racial and gender disparities in student disciplinary outcomes.

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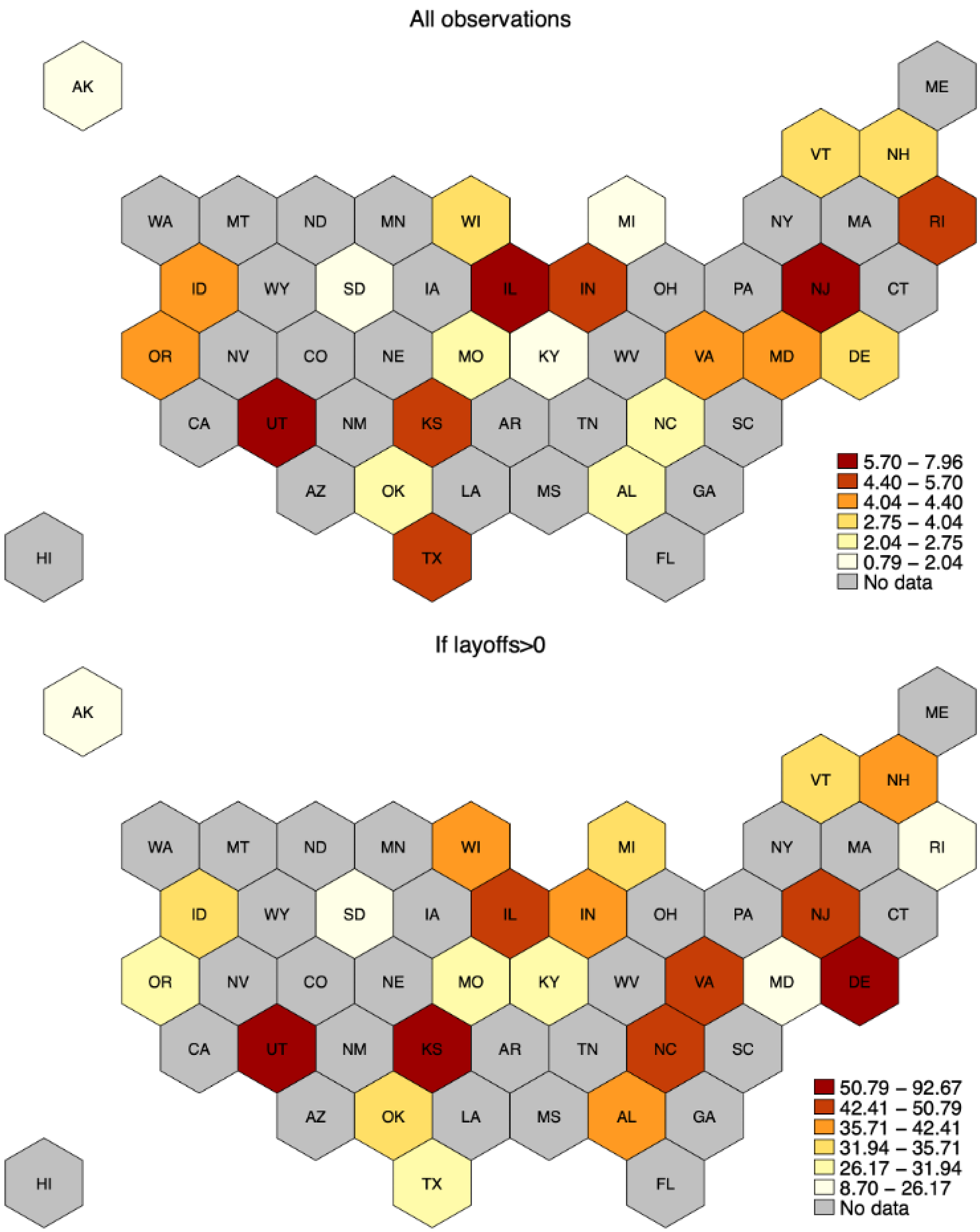
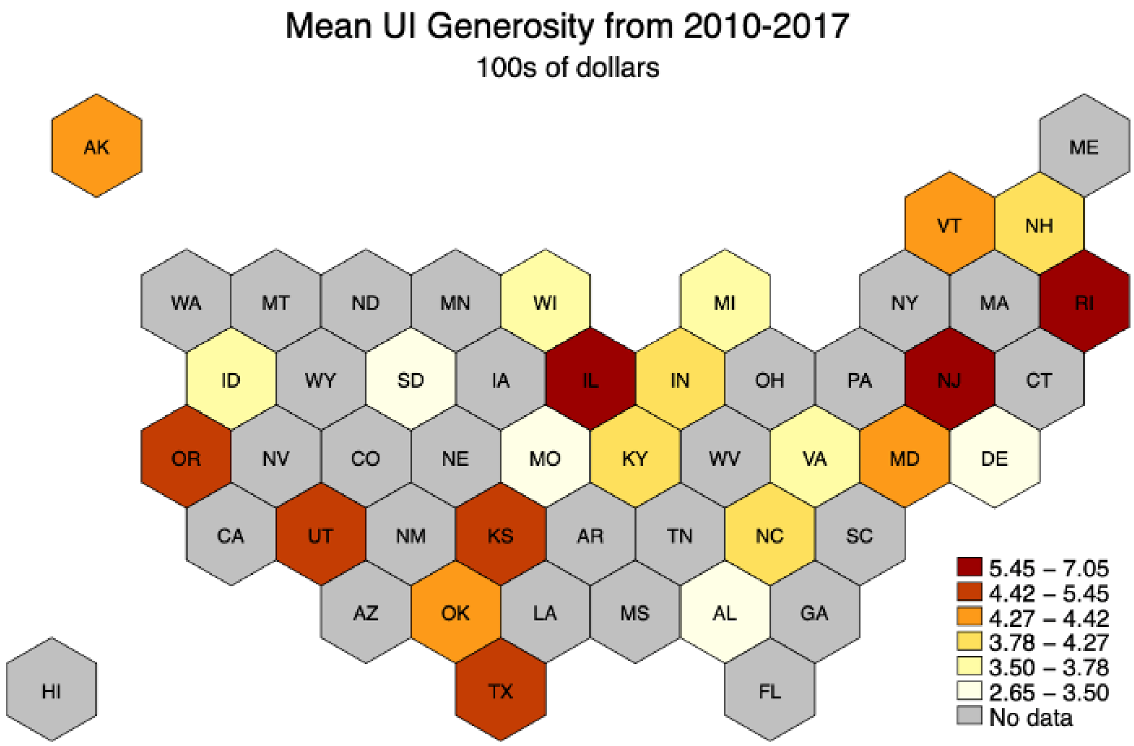
Figure 1: Mean Layoff Prevalence 2010-2017



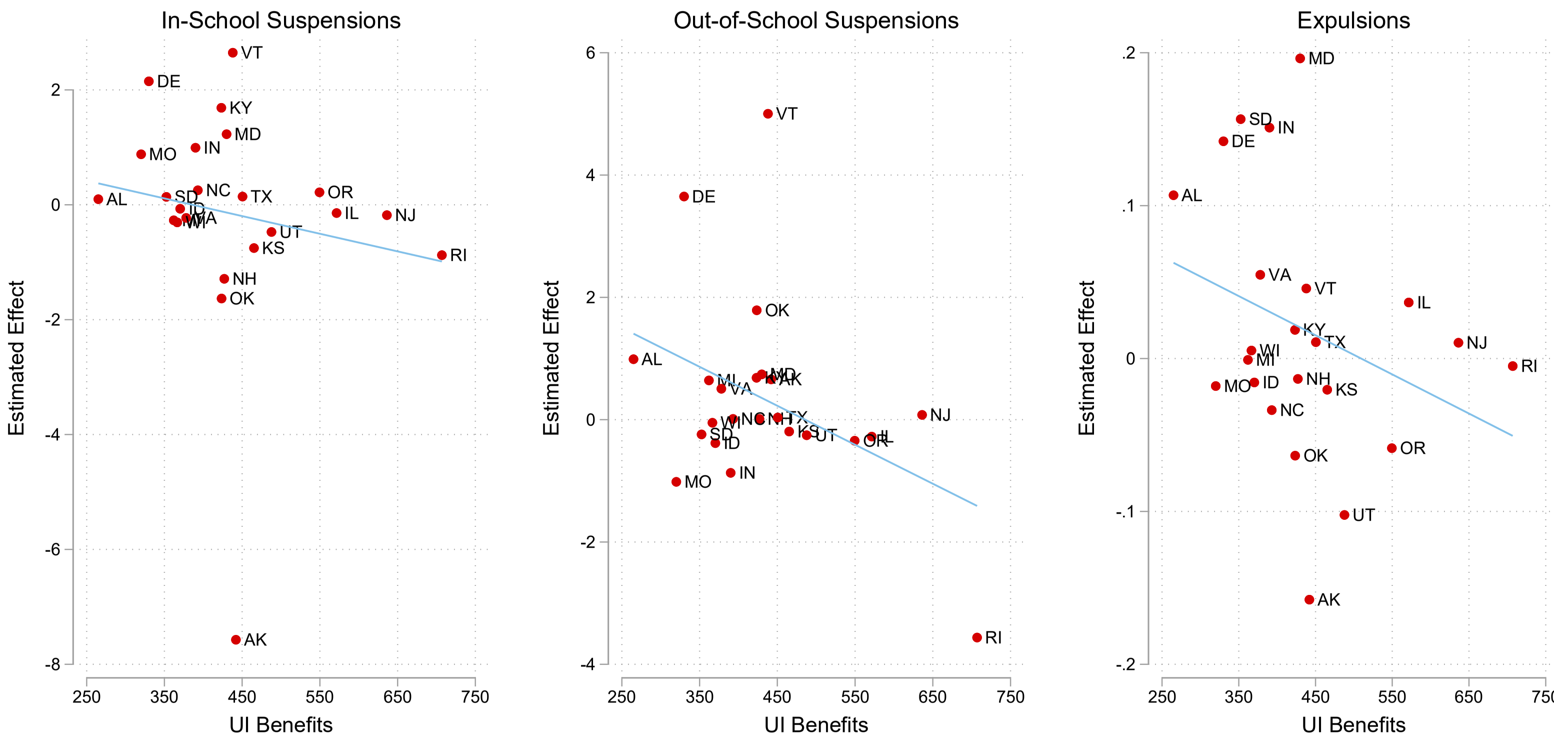
Notes: This figure depicts the average layoff size per 10,000 workers across all years for all states in our sample. Panel A includes observations in which there are no layoffs, while panel B includes only observations in which a layoff occurs.

Figure 2: UI Generosity of States in Sample: Maximum UI



Notes: This figure depicts the average maximum weekly unemployment insurance (UI) benefits in years 2010-2017 in 100s of dollars for all states in our sample.

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.

Table 1: Summary Statistics

	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)
Panel A. School Characteristics				
Enrollment	825.4	584.3	41.00	4885
% FRPL	0.418	0.211	0.000	1.004
% Non-White	0.338	0.249	0.002	1.000
City	0.088	0.283	0.000	1.000
Suburb	0.354	0.478	0.000	1.000
Town	0.202	0.401	0.000	1.000
Rural	0.356	0.479	0.000	1.000
Panel B. Discipline Outcomes				
In-school suspensions per 100 (all)	10.29	9.760	0.000	63.89
In-school suspensions per 100 (Black)	18.44	20.38	0.000	100.0
In-school suspensions per 100 (male)	13.61	12.27	0.000	68.57
Out-of-school suspensions per 100 (all)	6.731	5.715	0.000	43.81
Out-of-school suspensions per 100 (Black)	13.32	14.46	0.000	100.0
Out-of-school suspensions per 100 (male)	9.095	7.293	0.000	52.29
Expulsions per 100 (all)	0.213	0.541	0.000	7.110
Expulsions per 100 (Black)	0.287	1.296	0.000	21.62
Expulsions per 100 (male)	0.311	0.778	0.000	9.091
Panel C. Labor Market Characteristics				
Ever experience layoff (city)	0.420	0.494	0.000	1.000
Ever experience layoff (S.D.)	0.469	0.499	0.000	1.000
Experience layoff (city)	0.117	0.321	0.000	1.000
Experience layoff (S.D.)	0.201	0.401	0.000	1.000
Layoffs per 10,000 if layoffs >0 (city)	45.28	38.51	0.311	159.3
Layoffs per 10,000 if layoffs >0 (S.D.)	45.41	38.98	0.311	159.3
Maximum UI weekly benefits	439.9	98.75	265.0	707.0
Unique Schools		5,288		
School-Year Obs.		21,152		

Notes: Summary statistics are displayed for the full 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	-0.077	0.069	0.884*
	(0.106)	(0.101)	(0.476)
Exposed to layoff X UI			-0.178*
			(0.103)
Observations	21,152	21,152	21,152
Panel B. Out-of-School Suspensions			
Exposed to layoff	0.038	0.032	0.684**
	(0.063)	(0.065)	(0.289)
Exposed to layoff X UI			-0.143**
			(0.063)
Observations	21,152	21,152	21,152
Panel C. Expulsions			
Exposed to layoff	0.010	0.015	0.067**
	(0.011)	(0.011)	(0.029)
Exposed to layoff X UI			-0.011**
			(0.006)
Observations	21,152	21,152	21,152
School FEs	X	X	X
Year FEs	X		
State-Year FEs		X	X

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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	0.884*	1.020	0.550	1.443**	0.621
	(0.476)	(1.052)	(0.456)	(0.591)	(0.380)
Exposed to layoff X UI	-0.178*	-0.261	-0.065	-0.288**	-0.128
	(0.103)	(0.224)	(0.099)	(0.130)	(0.082)
Observations	21,152	21,152	21,152	21,152	21,152
Panel B. Out-of-School Suspensions					
Exposed to layoff	0.684**	2.345***	0.499*	1.017**	0.598***
	(0.289)	(0.763)	(0.291)	(0.414)	(0.230)
Exposed to layoff X UI	-0.143**	-0.516***	-0.095	-0.225**	-0.111**
	(0.063)	(0.161)	(0.065)	(0.091)	(0.047)
Observations	21,152	21,152	21,152	21,152	21,152
Panel C. Expulsions					
Exposed to layoff	0.067**	0.303***	0.014	0.064	0.075***
	(0.029)	(0.083)	(0.025)	(0.042)	(0.022)
Exposed to layoff X UI	-0.011**	-0.054***	-0.004	-0.010	-0.013***
	(0.006)	(0.016)	(0.005)	(0.008)	(0.005)
Observations	21,152	21,152	21,152	21,152	21,152

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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	0.684**	-0.319	0.786	0.525*	0.630	1.199	0.155	1.700**	0.444	0.697**	0.387
	(0.289)	(0.412)	(0.524)	(0.316)	(0.698)	(0.916)	(0.539)	(0.748)	(0.441)	(0.311)	(0.554)
Exposed to layoff X UI	-0.143**	0.090	-0.181	-0.127*	-0.110	-0.223	-0.061	-0.371**	-0.082	-0.171**	-0.076
	(0.063)	(0.085)	(0.113)	(0.070)	(0.147)	(0.185)	(0.116)	(0.179)	(0.091)	(0.067)	(0.117)
Observations	21,152	10,574	10,576	10,520	10,632	8,718	12,076	11,394	9,155	10,572	10,560
Panel B. Black Students											
Exposed to layoff	2.345***	0.420	2.896**	3.059***	2.032	3.079	1.118	2.420	1.844*	4.800***	1.294
	(0.763)	(1.564)	(1.178)	(1.087)	(1.499)	(2.499)	(1.493)	(2.181)	(1.067)	(1.434)	(1.043)
Exposed to layoff X UI	-0.516***	-0.091	-0.634***	-0.691***	-0.430	-0.703	-0.271	-0.564	-0.399*	-1.136***	-0.234
	(0.161)	(0.323)	(0.244)	(0.230)	(0.304)	(0.506)	(0.311)	(0.498)	(0.210)	(0.307)	(0.214)
Observations	21,152	10,574	10,576	10,520	10,632	8,718	12,076	11,394	9,155	10,572	10,560
Panel C. Male Students											
Exposed to layoff	1.017**	-0.432	1.330*	0.780*	1.119	1.664	0.396	2.521**	0.604	0.696	0.884
	(0.414)	(0.547)	(0.683)	(0.443)	(0.826)	(1.263)	(0.718)	(1.042)	(0.540)	(0.464)	(0.719)
Exposed to layoff X UI	-0.225**	0.122	-0.323**	-0.204**	-0.218	-0.330	-0.130	-0.567**	-0.119	-0.191*	-0.190
	(0.091)	(0.117)	(0.149)	(0.097)	(0.179)	(0.257)	(0.155)	(0.248)	(0.113)	(0.100)	(0.153)
Observations	21,152	10,574	10,576	10,520	10,632	8,718	12,076	11,394	9,155	10,572	10,560

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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. "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	-0.001
	(0.002)	(0.002)	(0.009)
Exposed to layoff X UI			0.000
			(0.002)
Observations	21,152	21,152	21,152
Panel B. % FRPL			
Exposed to layoff	-0.004***	-0.002	0.002
	(0.001)	(0.002)	(0.006)
Exposed to layoff X UI			-0.001
			(0.001)
Observations	21,072	21,072	21,072
Panel C. % Non-White			
Exposed to layoff	0.001	0.000	0.002
	(0.001)	(0.001)	(0.003)
Exposed to layoff X UI			-0.000
			(0.001)
Observations	21,152	21,152	21,152
Panel D. Student-Teacher Ratio			
Exposed to layoff	-0.114***	-0.056**	-0.086
	(0.024)	(0.025)	(0.112)
Exposed to layoff X UI			0.006
			(0.025)
Observations	20,654	20,654	20,654
School FEs	X	X	X
Year FEs	X		
State-Year FEs		X	X

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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: Out-of-School Suspensions

	(1)	(2)	(3)	(4)
Exposed to layoff	0.684**	0.780***	1.221***	1.221***
	(0.289)	(0.297)	(0.328)	(0.319)
Exposed to layoff X UI	-0.143**	-0.163**	-0.264***	-0.264***
	(0.063)	(0.064)	(0.071)	(0.070)
Exposed to layoff X TANF (std.)		0.115*		-0.034
		(0.063)		(0.070)
Exposed to layoff X EITC (std.)			0.289***	0.304***
			(0.068)	(0.076)
Observations	21,152	21,152	21,152	21,152

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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 and EITC benefits are standardized to have a mean of 0 and standard deviation of 1. 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	0.684**	0.722**	0.455
	(0.289)	(0.320)	(0.440)
Exposed to layoff X UI	-0.143**	-0.133*	-0.107
	(0.063)	(0.071)	(0.108)
Observations	21,152	18,960	12,220
Panel B. Dummy Variable for Above-Median Layoff			
Exposed to layoff	1.242***	1.522***	0.910
	(0.347)	(0.400)	(0.625)
Exposed to layoff X UI	-0.270***	-0.317***	-0.206
	(0.073)	(0.089)	(0.154)
Observations	20,283	18,116	11,149
Panel C. Continuous Measure of Layoffs (standard deviations)			
Exposed to layoff	0.245***	0.287***	0.483
	(0.085)	(0.081)	(0.311)
Exposed to layoff X UI	-0.051***	-0.056***	-0.100
	(0.018)	(0.017)	(0.078)
Observations	21,152	18,960	12,220

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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

Table A.1: States in Sample

State	Number of Layoffs	Number of Schools
Alabama	37	201
Alaska	5	29
Delaware	15	38
Idaho	59	66
Illinois	291	470
Indiana	101	191
Kansas	72	173
Kentucky	58	217
Maryland	88	190
Michigan	127	363
Missouri	125	306
New Hampshire	25	61
New Jersey	202	387
North Carolina	260	452
Oklahoma	68	198
Oregon	81	136
Rhode Island	21	47
South Dakota	14	47
Texas	521	1053
Utah	54	134
Vermont	8	27
Virginia	113	248
Wisconsin	128	254
Total	2473	5288

Notes: This table presents the number of layoffs and the number of schools in each state in our sample across all available years.

Table A.2: 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	0.684**	0.166	0.518***	0.067**	0.055**	0.012
	(0.289)	(0.200)	(0.173)	(0.029)	(0.024)	(0.014)
Exposed to layoff X UI	-0.143**	-0.043	-0.100***	-0.011**	-0.010**	-0.001
	(0.063)	(0.043)	(0.038)	(0.006)	(0.005)	(0.003)
Observations	21,152	21,152	21,152	21,152	21,152	21,152

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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.3: Effects of Layoffs & UI on Racial Disproportionality in Out-of-School Suspensions

	ARD **(1)**	**RRR** **(2)**
Panel A. All Schools		
Exposed to layoff	1.846**	0.229
	(0.722)	(0.392)
Exposed to layoff X UI	-0.421***	-0.111
	(0.153)	(0.094)
Observations	21,152	19,287
Panel B. Low Non-White Schools		
Exposed to layoff	4.195***	1.531**
	(1.279)	(0.735)
Exposed to layoff X UI	-0.992***	-0.458**
	(0.278)	(0.190)
Observations	10,572	9,837
Panel C. High Non-White Schools		
Exposed to layoff	1.012	-0.369
	(0.851)	(0.380)
Exposed to layoff X UI	-0.187	0.056
	(0.180)	(0.086)
Observations	10,560	9,431

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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. Column (1) presents results for Black-white disproportionality in out-of-school suspension using the adjusted risk difference (ARD). Column (2) presents results for Black-white disproportionality using the relative risk ratio (RRR). Sample sizes differ as the RRR requires a school to have both white and Black students, as well as at least one student suspended from each racial group. "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.4: Robustness to Inclusion of Additional Control Variables

	(1)	**(2)**	**(3)**	**(4)**
Exposed to layoff	0.684**	0.570*	0.778**	0.643**
	(0.289)	(0.303)	(0.351)	(0.325)
Exposed to layoff X UI	-0.143**	-0.122*	-0.151**	-0.125*
	(0.063)	(0.064)	(0.073)	(0.069)
Observations	21,152	21,072	20,905	20,826
State-Year FEs	X	X		
CZ-Year FEs			X	X
Dem. Controls		X		X

Notes: Estimates use Gardner (2022)'s two-stage estimator. 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.