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THE EFFECTS OF HIGHER-STAKES TEACHER EVALUATION ON OFFICE DISCIPLINARY REFERRALS

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ABSTRACT

Despite frequent political and policy debates, the effects of imposing accountability pressures on public school teachers are empirically indeterminate. In this paper, we study the effects of accountability in the context of teacher responses to student behavioral infractions in the aftermath of teacher evaluation reforms. We leverage cross-state variation in the timing of state policy implementation to estimate whether teachers change the rate at which they remove students from their classrooms. We find that higher-stakes teacher evaluation had no causal effect on the rates of disciplinary referrals, and we find no evidence of heterogeneous effects for grades subject to greater accountability pressures or in schools facing differing levels of disciplinary infractions. Our results are precisely estimated and robust to a battery of specification checks. Our findings provide insights on the effects of accountability policy on the black-box of classroom practice and highlight the loose-coupling of education policy and teacher behaviors.

Keywords: teacher evaluation, accountability, school discipline, difference-in-differences

JEL Classifications: I21, I24, I28

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1. Introduction

External accountability pressures and incentives are central tools policy makers possess to improve teaching and learning conditions in schools. However, there is a complex relationship between incentive- and accountability-based policies and local actor behavior, particularly in the public sector (e.g., Dixit, 2002). A rich theoretical and empirical debate on the relative merits of accountability-driven policies in education exists.[1] In fact, much of the body of evidence on the causal effects of accountability policies on school outcomes finds either mixed or no effects.[2] Even well-designed accountability and incentive policies can generate unintended responses, including educational triage (Ladd & Lauen, 2010; Neal & Schanzenbach, 2010; Reback, 2008), curriculum narrowing (Hamilton, Berends, & Stecher, 2005), and gaming (Figlio, 2006; Vogell, 2011). Thus, system leaders have a critical interest in understanding whether and in what ways teachers and principals respond to external accountability pressures.

In this paper, we study the effects of accountability pressures in the context of teacher responses to student behavioral infractions in the aftermath of reforms that imposed higher stakes on teacher evaluation at the beginning of the 2010s. The overwhelming majority of U.S. states implemented higher-stakes teacher evaluation policies between 2011 and 2016 with the goal of improving teachers' performance through increased accountability and feedback. These policies encourage evaluators to use observations of instruction and students' performance on external academic assessments to appraise teachers. In addition to providing incentives to improve

[1] Compare, for instance, Dee & Wyckoff (2015), Chiang (2009), Hanushek (2009), Jackson, Rockoff & Staiger (2014) and Macartney, McMillan & Petronijevic (2018) with Figlio (2006), Ladd & Lauen (2010), Rothstein (2015) and Strunk, Barret & Lincove (2017).

[2] See, among others, Brehm, Imberman & Lovenheim (2017), Chakrabarti (2014), Cullen, Koedel & Parsons (2019), Deming, Cohodes, Jennings, Jencks (2016), Eren (2019), Kraft, Brunner, Dougherty & Schwegman (2019), Macartney (2016), Özek (2012), Pope (2019), Reback, Rockoff & Schwartz (2014), Steinberg & Sartain (2015), and Stecher et al. (2018). Deming and Figlio (2016) and Liebowitz (2019) summarize this nuanced literature.

instructional pedagogy, the introduction of high-stakes evaluation based on observations and test scores increased pressures to create calm, orderly classroom learning environments. Unruly classrooms are easily observable for teachers' evaluators; more so than, for example, alignment of instruction to grade-level standards. Further, to the extent that disruptive students create negative externalities on other students' learning (Carrell & Hoekstra, 2010), teachers can both increase their expected observation score and improve their average student's performance by reducing incidences of active disruption in their classrooms. While one implicit goal of higher-stakes teacher evaluation policy is, therefore, to encourage teachers to improve their classroom management practices, teachers might also accomplish the goal of reducing disruptive behavior by imposing a lower floor of tolerance for misbehavior before removing a student from class and sending her to the office.[3] Thus, by estimating the effect of these evaluation policies on the rates of Office Disciplinary Referrals (ODRs), we investigate whether accountability pressures improve teacher skill in managing classroom behavior or create distortionary incentives.

We leverage Kraft, Brunner, Dougherty and Schwegman's (2019) tally of the introduction of teacher evaluation policy reform in conjunction with disciplinary data from a large network of over 2,500 schools implementing a common behavior management framework to fit a two-way, fixed effect difference-in-differences model that estimates the impact of higher-stakes evaluation on ODRs. Our first difference is the change in the rate of ODRs that may have been influenced by the change in evaluation policy for schools in states that experienced the teacher evaluation policy reform. Our second difference is the change in the rate of these ODRs for schools in states that had not yet (or did not) experienced the change.

[3] The typical mechanism by which teachers respond to student behavior that they have determined cannot be addressed in the classroom is to send students to a school administrator (principal, assistant principal, dean of students) in the school's office. Other approaches include waiting to speak to an administrator in the hallway while accompanied by a staff member. For the purpose of this paper, we describe all such events as Office Disciplinary Referrals.

To preview our results, our main findings are that high-stakes teacher evaluation has no causal effect on the overall rate of classroom or subjective-classroom ODRs. We estimate these null effects with precise zeros and can rule out effects larger than a decrease of 0.07 to 0.08 standard deviations (*SD*) or an increase of 0.01 to 0.03 *SDs* for classroom and subjective-classroom referrals. We find no evidence of a moderating effect when schools improve their implementation of a widely-used behavioral improvement strategy known as Positive Behavioral Interventions and Supports (PBIS). We find no evidence that the effects vary based on schools' rates of referrals prior to the start of higher-stakes evaluation. We also find no evidence of heterogeneous effects for grades that are subject to more intense accountability pressures because students' test scores in these grades contribute to a school's accountability status.

We subject our identification strategy and its assumptions to a host of robustness checks and consistently find that high-stakes evaluation policies do not change disciplinary referral rates. We conduct standard difference-in-difference assumption tests involving parallel trends, functional form, balanced panels, differential timing and negative weighting. In an improvement over standard approaches that struggle to capture endogenous differences across states, we have at our disposal a set of more severe behavioral and non-classroom-based outcomes. Because these types of infractions occur within the same contexts and presumably are not (or are less) influenced by changes in teacher evaluation policy (i.e., students are no more/less likely to bring a knife to school under pre- or post-treatment conditions and teachers are less likely to feel accountability pressures for student behavior in the cafeteria), we argue that these outcomes capture any secular changes in disciplinary climate or other policy reforms and should be unaffected by changes in teacher evaluation policy. We directly test the effects of the implementation of teacher evaluation on these unaffected outcomes, and we employ triple-difference estimates in which our third difference is

the change in the rate of non-classroom and more severe behavioral referrals in an effort to purge our main estimates of any secular trends or contemporaneous shocks. We also explicitly test whether concurrent teacher accountability and discipline policy changes in the same time-span predict changes in rates of disciplinary referrals or alter the predicted impact of teacher evaluation reforms. We find no evidence that they do. In order to promote research transparency, we pre-registered our quasi-experimental design in the Registry of Efficacy and Effectiveness Studies (REES #1748) prior to receipt of our data.

Our findings contribute to new literatures estimating the causal effects of accountability pressures on the black box of within-classroom behaviors by teachers and the effects of school policies on disciplinary processes. While there is a growing consensus that teacher evaluation policies increase voluntary exits, particularly among teachers rated poorly in observation- and value-added-based evaluation schemes[4] and some promising evidence that teacher observation improves student outcomes,[5] we know less about how high-stakes teacher evaluation policies change what teachers do in the classroom. Our findings align with Phipps and Wiseman (2019) who find no evidence that teachers shift their focus to a particular instructional domain as the accountability pressure of an evaluator observation increases. Similarly, Garet et al. (2017) find no evidence of changes in instructional practices in the aftermath of receiving performance feedback.

We study phenomena closely related to those described in Holbein and Ladd (2017). They find that the frequency of serious student misbehaviors increased in schools that were barely labeled as failing to make Adequate Yearly Progress under the No Child Left Behind Act. We study accountability pressures that fall individually and exclusively on teachers. Additionally, we have

[4] See Dee & Wyckoff (2015); Loeb, Miller & Wyckoff (2015); Cullen, Koedel & Parsons (2019).
[5] See Taylor and Tyler (2012); Phipps (2018); and Burgess, Rawal and Taylor (2019). Note also Kraft, Blazar and Hogan's (2018) meta-analysis of causal estimates of coaching interventions, which operate through some of the same mechanisms as evaluation.

available multiple outcome measures that allow us to assess the likelihood that our results are driven by changes in either student or educator behavior.

We also introduce a novel outcome to the causal literature that is rarely present in administrative data but is a critical pre-cursor to exclusionary school discipline. Teachers' classroom management practices and disciplinary responses are key mechanisms for student engagement and are frequently students' first points of entry into school disciplinary systems. Growing evidence indicates that suspensions harm both near-term academic performance and the future school attendance of suspended students (Anderson, 2019; Lacoe & Steinberg, 2018). This is particularly salient given recent evidence on the causal effects of exclusionary discipline on students' future college enrollment and involvement in the criminal justice system (Bacher-Hicks, Billings and Deming, 2019). However, while evidence indicates that zero tolerance policies increase suspensions (Curran, 2016) and alternatives such as PBIS decrease them when implemented well (Horner et al., 2009), the general equilibrium effects, including on non-suspended students, of alternative policy approaches are indeterminate (Steinberg & Lacoe, 2018), which may be a function of heterogeneous effects and uneven policy implementation (Skiba, 2015; Welsh & Little, 2018). Our results suggest that teacher accountability policies, either on their own or coupled with well-implemented systems to promote positive behavior, are not sufficient to limit students' entry into the disciplinary pipeline.

As a whole, we interpret our results as evidence that the introduction of high-stakes accountability policies did not dramatically alter classroom disciplinary climates. Given the nature of our data we are unable to examine the possibility that our results might be a product of heterogeneous responses to increased accountability by teachers' characteristics or skill.

Nevertheless, we take our findings as suggestive of the loose coupling between education policy and teaching practice.

We begin in Section 2 by providing an overview of teacher evaluation and student discipline policies and processes. Then, in Section 3, we generate a simple theoretical model to describe the factors that contribute to the generation of an ODR in order to motivate our analysis. In Section 4, we describe our disciplinary and policy data. In Section 5, we present our empirical estimation framework. In Section 6, we share our main results as well as evidence on the presence of heterogeneous effects. In Section 7, we present a host of difference-in-differences assumption and robustness checks. Finally, we conclude with a discussion of how our findings provide insight into the complex relationship between accountability policy and teachers' behaviors.

2. Teacher Evaluation and Student Discipline Policy

2.a Teacher Evaluation

In response to financial incentives from the Obama administration's 2009 Race to the Top competition, 44 states implemented new teacher evaluation policies between 2011 and 2016. In fact, 40 states enacted reforms of teacher evaluation between 2009 and 2011 (NCTQ, 2011, 2017),[6] but the exact timing of when these reforms came into effect spanned the subsequent six years. Our identification strategy exploits this exogenous federal shock to state policy and the plausibly random cross-state differences in the timing of the implementation of teacher evaluation reforms. Generally, state legislation and regulation defined parameters for the evaluation process to which local districts' collective bargaining agreements were required to adhere. States frequently

[6] Alaska, Kentucky and North Dakota passed new teacher evaluation laws in 2012; Texas did so in 2013.

promulgated model evaluation policies that most districts adopted as written, although others adopted them with modifications (NCTQ, 2017; Steinberg & Donaldson, 2016).

While the particulars of each state's evaluation framework vary, as does district-level policy implementation, this variation is largely endogenous. Thus, we focus on the common accountability elements rather than on the intensity of accountability pressures across states and districts. In almost all cases, teacher evaluation reforms entailed adopting a common rubric for evaluating teachers' performance with multiple rating categories. All state reforms to teacher evaluation require that classroom observation of teaching practice be a part of a teacher's final rating, and in most cases these reforms establish a minimum frequency of classroom observations. In addition, many states require some or all teachers to be evaluated based on student-learning gains (either through formal measurements of students learning, through teachers' contributions to students' progress towards locally determined learning objectives, or both). Some states additionally include measures of whole-school performance or parent-, student-, and peer-surveys of teacher competency. Some require annual evaluations for all teachers, while in other states evaluation focuses primarily on new or probationary teachers. (Donaldson & Papay, 2015; Jacobs & Doherty, 2015; Steinberg & Donaldson, 2016; Winters & Cowen, 2013).

While summative teacher ratings have varying consequences, the policy reforms represent substantial increases in accountability pressures on teachers. Over three-fifths (61 percent) of states instituted rules that led to the dismissal of teachers who were not rated Proficient, and almost half (48 percent) of states use evaluation results for tenure decisions (Steinberg & Donaldson, 2016).

2.b Student Discipline

In the late 1980s and 1990s, states and districts increasingly adopted sets of policies collectively known as "zero-tolerance discipline." Many of these policies originated in response

to the federal Gun-Free School Act of 1994, but soon extended beyond firearm offenses (Curran, 2016). Under such policies, students committing one among a set of pre-specified disciplinary infractions were to be suspended or expelled from school, without discretion. In response to widespread concern about the long-term effects of exclusionary discipline and the disproportionate use of such disciplinary approaches for students of color, states and districts initiated reforms of many zero-tolerance laws in the 2000s and 2010s (Rafa, 2019; U.S. Department of Education & U.S. Department of Justice, 2014).

Some states have prohibited the use of suspension for less severe infractions (such as defiance or truancy) or the length of suspensions overall, while others prohibit the use of suspension in earlier grades except for extreme misbehavior (Steinberg & Lacoe, 2017; Anderson, 2019). Others require school districts to develop discipline plans that incorporate alternative discipline programs, such as Positive Behavioral Intervention and Supports (PBIS), Multi-Tiered Systems of Support (MTSS) and restorative justice practices (Rafa, 2019; Welsh & Little, 2018).

While discipline policy reforms have generally focused on suspension and expulsion, the process by which students are removed from class for lower-level infractions is less frequently a target of policy. To the student, Office Disciplinary Referrals (ODRs) are an entry point into the disciplinary system. ODRs cause students to miss instructional time, may lead to additional consequences ranging from lunch detention to suspension, and represent a signal to students about how teachers perceive their behavior. However, they may provide students with opportunity to reflect, develop relationships with school administrators, and improve their future behavior.

The consequences of ODRs extend beyond the referred student to classmates and educators. ODRs have spillover effects as they influence the classroom composition during the time the student is out of class. Further, the subjective, classroom-based ODR represents a teachers'

interpretation of student behavior, and thus yields important signals about teachers' tolerance for student misbehavior and classroom management skills. Finally, ODRs impose significant time burdens on educators who receive students removed from the classroom. While no precise estimates of the amount of time that students spend outside of the classroom during each referral exist, some states mandate that students not be sent back to class sooner than 30 minutes, within the same class period, or before the principal has undertaken one of a set of prescribed disciplinary measures (e.g., Louisiana Revised Statute §17.416 A.(1)(b)(iii)). For our purposes, the two essential takeaways are: (1) state policy generally grants broad discretion to teachers to remove students from the classroom; and (2) these removals have significant, though imperfectly understood, implications for both students and educators.

3. Theoretical Predictions

We begin with a simple theoretical model to derive a test for the effects of teacher accountability on instructional practice in the context of disciplinary referrals from the classroom to a school administrator. The probability of an Office Disciplinary Referral (ODR) is a function of the seriousness of the particular behavior (b), given a student's characteristics (i) as well as her past behavior in school (j), the school and system characteristics and policies (s), and teacher characteristics and beliefs (t), such that: $P(ODR_{bijst}) = f(b, i, j, s, t)$.

Behaviors can be categorized into events that, for the purposes of student safety, require students to leave the classroom (e.g., fights, weapons, smoking, etc.) and others in which the decision to send the student from the classroom is more subjective in nature. We define this rough dichotomization in our data more below, but generally $P(ODR^{Objective}) \approx 1$. $P(ODR^{Subjective})$ is, all else equal, strictly increasing asymptotically to 1 in the perceived severity of the infraction.

Emerging evidence indicates that students' race and family income, orthogonal to their behavior, predicts the severity of disciplinary responses (e.g., Anderson & Ritter, 2017, 2018; Barrett, McEachin, Mills, & Valant, 2019). Thus, we anticipate that $P(ODR_{Black,Poor}) \geq P(ODR_{White,Non-poor})$. We anticipate that *P(ODR)* is generally declining as *t* skill increases, though not necessarily monotonically, and that it depends on teacher beliefs about the value of ODRs.

For the purposes of our study, we seek to understand the values of the coefficients on the accountability components of *s*. Teachers who observe a particular student misbehavior that requires a subjective decision to determine whether to refer the student to the office make a choice such that $P(ODR^{Subjective} \mid b,i,j,t) = EVAL_{bijt}\beta_1 + S_{bijt}\beta_2 + \ldots S_{bijt}\beta_n + \mathbf{Y}\cdot\mathbf{Z}_{bijst} + \varepsilon_{bijst}$. Given a particular behavior, student and teacher interaction, the probability of an office referral depends on a vector of school characteristics, policies and practices (*S*), which include teacher evaluation policy (*EVAL*) alongside up to *n* other school characteristics. $\mathbf{Z}_{bijst}$ describes a vector of interactions between policy, practices and individuals. For example, schools that successfully implement a system of behavioral supports should have, on average, fewer ODRs, $P(ODR_{bijst}|PBIS = 0) \geq P(ODR_{bijst}|PBIS = 1)$, but the effects of these policies may depend on the beliefs and preferences of the teacher.

Our analytic challenge is to estimate the parameter on accountability policies holding all else equal. *Ex ante*, it is not obvious what the effects of greater accountability pressures might be on the rate of ODRs. On one hand, it is possible that greater accountability pressures may lead to an increase in the rate at which teachers send students out of class. In particular, if classroom observations are key contributors to teacher evaluation scores under high stakes evaluation systems, teachers may be more likely to send students out of class for lesser infractions than under low-stakes evaluation conditions in the hopes that fewer disruptions occur during a supervisor's

unanticipated visit. Further, if teacher evaluation scores are tied to assessment performance, teachers may utilize the ODR more frequently to send students out of class if they perceive doing so will result in a more effective learning environment for the majority of students.

On the other hand, it is possible that greater accountability pressures might lead to a decrease in the rate of ODRs. A decrease in referrals may be evidence of improved teacher skills resulting from high-stakes teacher evaluation policies. An alternative explanation for how the introduction of high-stakes evaluation policies might decrease ODRs is that evaluators only weakly observe and monitor teachers' behavior management skills. Given principals' difficulty in finding time to observe teaching practice (Kraft & Gilmour, 2016), they may use the frequency of teachers' referral of students to the office as a proxy for their skill in classroom management. This would create an incentive for teachers to reduce the frequency of ODRs in the aftermath of high-stakes evaluation reform but not be desirable from the policy maker's standpoint. Thus, positively signed coefficients would provide relatively straightforward interpretations of the effects of increased accountability on teachers' practice, while negatively signed ones would be ambiguous.

4. Data

4.a Discipline data

School-Wide Information System (SWIS) data

The primary data source for our analytic strategy is the School-Wide Information System (SWIS) data system. This data system is used by schools that track behavioral data associated with the implementation of Positive Behavioral Implementation and Supports and maintained by the Education and Community Supports research and outreach unit at the University of Oregon. We

present in the main text of the paper a brief description of this data and our sample construction and share complete information in Appendix B.

Our analytic sample focuses on U.S. traditional public schools subject to state evaluation policies, for which we have outcome measures both before and after policy implementation. We restrict our sample to grade-year observations nested in schools which we observe at least four years before the adoption of high-stakes teacher evaluation and one year after the initial implementation year. We form our measures of Office Disciplinary Referrals from counts of referrals at the grade-school-year level. Thus, our main analytic sample includes 107,458 grade-school-year observations, nested in 20,135 school-year observations. These represent a total of 2,564 schools in 939 districts. These data also include enrollment information that combine the best available information from schools' self-reports and their October 1 administrative count from the NCES Common Core Data.

In Table 1, we present summary statistics for the full sample, for schools and students located in states that never implemented high-stakes evaluation, and for schools and students in states that did. Our sample is clearly not random. Schools that attempt to implement PBIS, have a standardized office referral process, use the SWIS data management system, and agree that their behavioral data can be used for research purposes are different from the full population of U.S. schools. Thus, our results should be interpreted as generalizable only to schools with these characteristics. However, our sample represents a vast number of schools across nearly all U.S. states. Further, as we highlight in Table 1, the demographic characteristics of our sample broadly match national racial and family income enrollment patterns; none differ by more than 4 percentage points. Our sample does over-represent urban and town-based schools by around 5 percentage points each and under-represent rural schools by around 9 percentage points.

The average school in our sample enrolls 510 students. For ease of interpretation, we scale our outcomes to be the rate of referrals per-500 students, per-day. In our full sample, the average per-day number of referrals from classroom settings is 2.03 per 500 students and the average per-day number of referrals from all other settings is 1.56 referrals per 500 students. This implies that the average number of referrals across a 180-day school year for an average-sized school is around 650. Assuming each student spends 30 minutes out of class for each referral, this would suggest a total of 325 hours of lost instructional time each year and a considerable administrative staffing burden.[7] There is considerable cross-school variability in the rate of referrals. The full-sample standard deviation for classroom-based referrals is 2.83 (1.77 for non-classroom referrals). In many schools, students miss substantial portions of the school year due to disciplinary events. The average per-day number of referrals from classroom settings that are for one of six behaviors defined by an expert panel (Greflund, McIntosh, Mercer, & May, 2014) as "subjective" in nature was 1.29 referrals per 500 students. The average per-day number of referrals from classrooms that fall into one of fourteen behaviors defined as "objective" was 0.48 per 500 students. 61.5 percent of our grade-school-year observations are located in schools that conducted assessments of their implementation of PBIS practices.[8] In 71 percent of our school-year observations, schools were assessed to be successfully implementing PBIS.

Civil Rights Data Collection (CRDC) suspension data

We supplement our main estimates with placebo tests using suspension data from the restricted-use Civil Rights Data Collection (CRDC). We draw on five waves of data, from the

[7] While this number is a rough approximation given the lack of precise estimates, we believe it is a conservative one. Students are typically required to complete a reflection form and conference with a school administrator before returning to class. Students unprepared to return to class remain with the administrator for longer periods. If one administrator were responsible for all referrals in a 500 student school (a reality in many contexts), this would mean that 22.5 percent of her 8-hour work days over 180 school days would be devoted to these referrals.

[8] McIntosh et al. (2013) and Mercer, McIntosh & Hoselton (2017) detail the validation of these instruments.

2005-2006 school year to the most recently-collected 2015-2016 school year that count the number of students suspended in a given year. In the sample of 284,460 school-year observations in the CRDC, the average school suspends 6 percent of its students per year (*SD* = 0.09).

4.b Teacher evaluation and discipline policy data

High-stakes evaluation reforms

Kraft et al. (2019) extend prior reviews by Steinberg and Donaldson (2016) and the National Council on Teacher Quality (2016) and codify the timing of evaluation reforms in 44 states. Following Kraft and co-authors, we code *Implement Evaluation* as one in the first fall in which a new statewide evaluation policy is implemented. In Figure 1, we map the differential timing, by state, of the implementation of higher-stakes evaluation policies as well as the six states that never enacted new policies.[9] The majority of schools in our sample experience high stakes evaluation, though the overrepresentation of California schools in our data means that while 44 of 50 states experienced high-stakes teacher evaluation policies, only 72 percent of schools in our sample end up operating under a higher-stakes evaluation framework.

In Figure 2, we plot the raw outcome data for schools in states that experienced high-stakes evaluation reform for the years before and after policy implementation. In Panel A, we present trends for ODRs that originate in the classroom, our first outcome of interest. We observe no evident discontinuity coinciding with the enactment of increased accountability measures for teachers or any change in the slope of these raw averages. We observe the same patterns in Panel B of Figure 2 which displays classroom-originating referrals for infractions that are subjective in nature, our second outcome of interest. Thus, the descriptive evidence suggests that there may be limited effects of the implementation of higher-stakes evaluation on teachers' disciplinary

[9] Appendix Table A1 provides state-by-state policy implementation details and counts of schools by state.

decisions; however, secular patterns may mask an underlying causal relationship. This motivates our identification strategy which we discuss in more detail below. One tool at our disposal are two alternate outcomes that we would not anticipate would be affected by policy implementation: non-classroom referrals and classroom-based referrals for objectively inappropriate behaviors. Figure 2 provides suggestive evidence that the trends in these placebo outcomes are unresponsive to policy implementation, and we test this formally below.

Discipline policy reforms

To address concerns that shifts in ODR activity reflects reform in school discipline policy, we collect data on two categories of disciplinary policy reforms and test whether our results are robust to these policies. We compile information from the Compendium of School Discipline Laws and Regulations (Bezinque, Garcia, Darling, & Stuart-Cassel, 2018) on whether any state-level policies related to *Teacher authority to remove students from the classroom* and *Limitations, conditions, or exclusions for use of suspension and expulsion* were enacted between 2006 and 2018. Eight states and the District of Columbia revised statutes or regulations related to teachers' authority to remove students from the classroom and 22 states and DC changed policies that limited suspensions or expulsions for particular offenses or groups of students.

5. Empirical Strategy

Our identification strategy relies on the differential timing across states in the implementation of teacher evaluation reforms. State legislatures enacted policy reforms in response to an exogenous federal shock, but the date of statewide adoption of the new evaluation systems varied as a function of the legislative bargaining process, the expiry of current collective bargaining agreements and other elements of the political process that we argue are near-random.

We begin by estimating a non-parametric event study. This approach allows us to flexibly estimate any pre-policy trends or time-varying treatment effects. We fit the following model:

$$ODR_{gjst} = \sum_{r=-6}^{3+} 1(t = t_s^* + r)\beta_r + \left(\mathbf{X_{jt}}\right)\theta + \Delta_g + \Gamma_j + \Pi_t + \varepsilon_s \quad (1)$$

In simplified form, this represents the per-500-student per-day rate of Office Disciplinary Referrals (ODR_{gjst}) for each grade-year observation in grade *g*, school *j*, state *s* and time *t*, regressed on a series of indicators that take the value of 1 when the grade-year observation is a given number of years pre- and post-policy reform, with t_s^* indicating the year in which state *s* implemented the higher-stakes teacher evaluation reform. This model includes grade- (delta), school- (gamma) and year- (pi) fixed effects and a vector (**X**) of school-level (*j*) background characteristics. We include a parsimonious set of plausibly exogenous school characteristic adjustments to capture school-specific characteristics and improve the precision of our estimates. As policy affects every school in a state, we do not anticipate that evaluation reforms would alter the demographic composition of a school.[10] We include the following demographic characteristics measured at the school level: percent of students receiving free/reduced lunch, percent of students of various racial/ethnic backgrounds, and school enrollment. We cluster standard errors at the state level, given this is the level of the policy intervention that we study, and our errors are correlated across time within state (Abadie, Athey, Imbens, & Wooldridge, 2017; Bertrand, Duflo, & Mullainathan, 2004).

The coefficients of interest are the seven β_r ($-5 \leq r \leq 1$) which represent the effect of evaluation reforms on rates of ODRs *r* years before and after the policy introduction.[11] We measure

[10] In fact, we regress the seven school demographic characteristics on our evaluation indicator and reject the null in only one instance. Evaluation implementation predicts a small decrease in the FRPL composition of a school (Beta: 1.25 p.p., SE: 0.52). Given the multiple hypotheses we test and the small magnitude of the coefficient, we take these results as consistent with our claim that school demographic characteristics are exogenous to policy implementation, though we present estimates without these adjustments in all cases to address this concern.

[11] In our event-study results, we estimate coefficients for all available data (including binned categories for years 6+ pre-, 2 years post, and 3+ years post evaluation) but only interpret years -5 through 1 to ensure that we only compare units that are observable for all treatment timing years.

all effects compared to the year prior to the reform (r= -1), and we assign all non-treated schools to the same pre-policy year (r= -1).

Next, we extend Equation 1 into the pre/post difference-in-differences framework where we pool estimates across years to increase precision and test the global effects of the policy:

$$ODR_{gjst} = \beta_1 EVAL_{st} + (\mathbf{X_{jt}})\theta + \Delta_g + \Gamma_j + \Pi_t + \mu_s \quad (2)$$

The indicator $EVAL_{st}$ takes the value of 1 if the school is in a state that is in a year with a high-stakes evaluation system. β_1 is the causal parameter of interest. All other terms are defined as above.

We also relax the assumption of the standard difference-in-differences model of time-invariant treatment effects in Equation (2) by adding a linear time trend:[12]

$$ODR_{gjst} = \beta_1 EVAL_{st} + \beta_2 EVALxYEAR_{st} + \beta_3 YEAR_{st} + (\mathbf{X_{jt}})\theta + \Delta_g + \Gamma_j + \Pi_t + \nu_s \quad (3)$$

where $YEAR_{st}$ is a linear time trend for state *s*, centered around the year the state implemented the high-stakes teacher evaluation policy. The interaction term $EVALxYEAR_{st}$ allows for the relative time trends among schools in treated states to differ post-reform. The coefficient on the main effect of treatment (β_1) identifies the immediate response of the introduction of high-stakes evaluation of ODRs and the coefficient on the interaction term (β_2) captures linear deviations from the average effect. The time-trend coefficient (β_3) tests for any differential trends in the pre-reform period among states that did and did not introduce high-stakes evaluation systems.[13]

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[12] Some analysts refer to Equation (3) as a Comparative Interrupted Time Series (C-ITS)

[13] We may be concerned that the estimates from Equations (2) and (3) will be biased as a result of unobserved state-level factors that, contemporaneous with the introduction of high-stakes teacher evaluation, also affect ODRs. Triple difference (DDD) estimates that leverage alternative, potentially unaffected, outcomes help us address these sources of bias. We model these as follows:
$ODR_{gjst} = \beta_1 EVALxAFFECT_{st} + \beta_2 EVAL_{st} + \beta_3 AFFECT_{st} + (AFFECT_{st} \cdot \Gamma_j)\phi + (AFFECT_{st} \cdot \Pi_t)\delta + (\mathbf{X_{jt}})\theta + \Delta_g + \Gamma_j + \Pi_t + \upsilon_s$. $AFFECT_{st}$ is an indicator variable that takes the value of one if the observation is one in which we would anticipate the introduction of high-stakes evaluation policies will affect the rate of ODRs or affect the rate more intensively. We contrast locations in which ODRs occur, specifically comparing classroom-originating ODRs, which we anticipate would be influenced by changes in the teacher evaluation policies, and non-classroom-originating ODRs, which we anticipate would not be affected by the policy changes. Alternatively, we contrast the type of infraction (subjective or objective) resulting in the ODR. β_1 represents the effect of the introduction of the high-stakes evaluation policy on anticipated affected outcomes, compared to unaffected outcomes in states that had not yet or never adopted

To better understand the effects of accountability pressures, we examine differences between grade levels under greater and lesser accountability and differences across schools with varying rates of referrals prior to the higher-stakes evaluation era. Specifically, we hypothesize that grades 3-11 will be subject to greater levels of accountability as these are years in which high-stakes testing occurs in schools. We theorize that non-classroom and objective ODRs, as well as ODRs from grades K-2 and 12, will be less sensitive to the introduction of high-stakes teacher evaluation. We note here the pre-registration (Registry of Efficacy and Effectiveness Studies in Education 1748.2) of our analytic plan in which we propose to explore the higher accountability applied to grades 3-11.[14] Additionally, we explore whether teachers respond differently to increased accountability when the starting behavioral climate in their school differs by interacting ODR-rates in the year prior to the implementation of evaluation policies with our policy indicator.

We also examine the extent to which the implementation of effective disciplinary support strategies serves to moderate the effects of greater accountability. Specifically, what are the effects on ODRs when schools develop better systems of behavioral supports in the context of higher-stakes evaluation? The fixed-effects structure of our analysis means that our estimates are of the effect of within-school improvements in the implementation of PBIS and the interaction of these improvements with the introduction of high-stakes evaluation. However, we note explicitly the exploratory nature of this analysis as the successful implementation of PBIS is clearly an endogenous characteristic of the school.

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the evaluation policy. We adjust for unexplained within-school and within-year heterogeneity in affected outcomes by interacting our *AFFECT* indicator with year- and school-indicators. As we show below, we find null effects for all of our double difference models, and so we do not feature our triple difference framework prominently. We do present these results in Tables A12 and A13 and, as expected, they also return precise zeros.

[14] While all schools in states in our sample require high-stakes assessments in grades 3-8, high-school assessment requirements vary. All states require students to test at some point in grades 9-11. Some states require testing only in one of these grades, other states require testing across multiple years, still others allow student discretion on the grade in which students take tests. Our estimates are even closer to zero when we restrict our definition of higher-accountability grades to 3-8 (class: -0.026 (0.067); subjective-class: -0.022 (0.067)).

For the coefficient β_1 to be an unbiased estimand, we make three assumptions about our estimates: (1) schools and grades in untreated states (and not-yet-treated states) provide a valid counterfactual for schools and grades in treated states; (2) there are no unobserved simultaneous shocks correlated with our outcomes and the introduction of higher-stakes teacher evaluation reforms; and (3) the estimands for each grade and year are appropriately pooled to create the full sample Average Treatment Effect (ATE). We test our first assumption by examining pre-trends in our non-parametric event study specifications and then formally test for the existence of linear trends in our DD estimates. We also restrict our sample to only those observations in states that enacted teacher evaluation, so that our estimates rely only on plausibly random timing variations.

We address secular trends in school discipline activity through year fixed effects and, in some specifications, time trends. However, if other policy reforms are contemporaneous with evaluation policy reforms, our results would be biased. Thus, we test our second assumption with a series of robustness checks in a set of placebo models in which we estimate the effects of teacher evaluation on disciplinary events from outside the classroom or that are in response to behavioral events so severe that we do not anticipate they would be affected by evaluation policy reform alone. Other placebo tests explore the effects of evaluation reform on grade levels (K-2 and 12) which we argue would be less subject to accountability pressures. In addition to using placebo outcomes, we also create fictional dates for evaluation policy implementation that precede the actual years of adoption and test whether these false evaluation years predict changes in referral rates. As a final test of the second assumption, we fit a series of models in which we include other teacher accountability and discipline policy reforms to assess whether they predict changes in ODR rates or moderate the main effect of teacher accountability pressures.[15]

[15] Kraft et al. (2019) seek to rule out threats to their identification strategy from contemporaneous teacher and accountability policy reforms, such as the implementation of Common Core Standards or licensure tests. These are

There has been a recent explosion in the econometric literature documenting frequent failures of the third assumption, particularly in the context of differential timing as an identification strategy.[16] We test our fixed effects for the presence of time-varying weights and negative weighting, and we replicate our standard two-way fixed effect models using de Chaisemartin and D'Haultfoeuille's (2019) time-corrected Wald (Wald-TC) estimand.

We present information in Table 1 on the differences in schools and students which did and did not experience high-stakes evaluation. The most notable difference is the larger school size for schools that ever experienced high-stakes evaluation. While there are some baseline differences in the characteristics of schools and students in states that did and did not experience high-stakes evaluation, we account for this in our difference-in-differences estimation framework. As we compare the difference in values before and after the policy change with differences in values over the same time period in locales that did not or had not yet experienced the policy change, starting differences between treated and untreated locales does not threaten the validity of our design. Nevertheless, for external validity purposes, it is reassuring that the outcome values prior to the start of the era of evaluation reform are quite close in schools located in states that did and did not experience high-stakes teacher evaluation, all within 0.2 referrals per-500 students per day.

less relevant to our identification strategy as we are ultimately interested in whether and how increased accountability shifts teachers' classroom practices. Our results are robust to the inclusion of policy indicators for the reform of tenure laws and weakening of collective bargaining (see Appendix Tables A14 and A15). To the extent that our estimates of teacher evaluation reforms are influenced by other accountability-related policy reforms, this would imply that our results are evidence of overall accountability pressures on teacher practice, rather than specific to teacher evaluation.

[16] A non-exhaustive list includes: Athey & Imbens (2018); Borusyak & Jaravel (2017); de Chaisemartin & D'Haultfoueille (2019); Ferman & Pinto (2019); Gibbons, Serrato & Urbancic (2018); Imai & Kim (2019); and Goodman-Bacon (2018).

6. Results

6.a Event-study estimates

We find no evidence that rates of Office Disciplinary Referrals (ODRs) changed in the aftermath of the introduction of higher-stakes teacher evaluation policies. In Figure 3, we present results from Equation 1 for both referrals originating in the classroom (Panel A) and referrals originating in the classroom that are subjective in nature (Panel B). While there is some visual evidence that ODRs decline somewhat after the introduction of high-stakes evaluation policies, all estimates fall within the 95 percent confidence interval and are small in magnitude.

Independently, the results in Figure 3 provide little evidence that there were trends in the rate of classroom-based or subjective referrals prior to the implementation of teacher evaluation policies. This suggests the first assumption required of our DD estimates holds and we have no need to instrument with leads in our event study (Freyaldenhoven, Hansen, & Shapiro, 2019).

6.b Difference-in-differences estimates

Results from our main difference-in-differences estimates confirm that we find no causal effect of the implementation of high-stakes evaluation on rates of classroom- or classroom-subjective ODRs. In Table 2, we present the results of Equations (2) and (3). While our estimates are consistently signed and of nearly identical magnitude, in all cases we fail to reject the null. We estimate these effects with precise zeros. In our preferred estimates (Models II and V), we can confidently rule out ranges of effects greater than a decrease of 0.21 referrals or an increase of 0.04 referrals per-500 students, per day for classroom referrals and a decrease of 0.14 or an increase of 0.06 referrals per-500 students, per day for subjective-classroom referrals. These confidence intervals correspond to a 0.08 standard deviations (*SD*) decrease and a 0.01 *SDs* increase or a 0.07

SDs decrease and a 0.03 *SDs* increase for classroom and subjective referrals, respectively.[17] In addition to ruling out ODR rate shocks of all but the smallest substantive magnitudes, we find no evidence of differential post-evaluation policy implementation trends in Models III or VI.

6.c Heterogeneity of effects

Strong systems of behavioral supports

We find no evidence that improvements in schools' implementation of Positive Behavioral Interventions and Supports (PBIS) practices serves to moderate the effects of accountability. We present results in Table 3 of a series of estimates in the subset of grade-school-year observations for which we have measures of PBIS implementation. As such measures are available in only 61.5 percent of our grade-school-year observations, in Models I and V we first re-estimate the results from Table 2 and examine the main effect of evaluation implementation in this sub-sample of observations. For these schools, the effects are even closer to zero. We then introduce the time-varying effect of PBIS implementation and its interaction with teacher evaluation.[18] In our preferred specifications (Models III and VII), we can confidently rule out ranges of moderating effects greater than a decrease of 0.16 referrals or an increase of 0.23 referrals per-500 students, per day for classroom referrals and a decrease of 0.10 or an increase of 0.14 referrals per-500 students, per day for subjective-classroom referrals. These correspond to 95 percent confidence intervals of -0.06 SDs to +0.08 SDs for classroom referrals and -0.05 SDs to +0.07 SDs for subjective referrals.[19] We observe no post-evaluation implementation time trends.

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[17] We scale the precision of these null effects to the standard deviation of our outcomes across the full analytic sample. When we scale our outcome to the *within-school* standard deviation of our outcomes, our 95 percent confidence intervals are -0.13 to +0.02 *SD* and -0.12 to +0.05 *SD* units for the main effects of evaluation on classroom and subjective referrals, respectively.

[18] The sign of the main effect of PBIS implementation is negative, consistent with our theoretical predictions, though relatively small in magnitude and imprecisely estimated.

[19] When we scale our outcome to the within-school standard deviation, the 95 percent confidence interval on the moderating effects of PBIS are -0.09 to +0.14 and -0.08 to +0.11 *SD* units for classroom and subjective referrals.

Pre-policy referral rates

We do not find any evidence of heterogeneity of effects by the rates of disciplinary referrals in the year prior to evaluation policy implementation. In Table 4, we present results in Models I and IV in which we re-estimate our primary models on the sub-sample of grade-school-year observations in states that ever experienced evaluation and that are not observed in the year immediately prior to policy implementation (t=-1). We exclude this year so as not interact our policy predictor with a value that is on both the left- and right-hand sides of our equations. These results are consistent with our main estimates. In Models II-III and VI-VII, both the main effect of evaluation implementation and its interaction with the pre-policy rate of referral are indistinguishable from zero.[20]

Intensity of accountability pressures

We find no evidence of heterogeneous effects for grades that should be subject to more intensive accountability pressures. In Table 5, we present results in which we restrict our grade-school-year observations to those that represent grades 3 through 11. We again fail to reject the null hypothesis and can confidently rule out small effects, both using effect size metrics and substantive interpretations. We present the corresponding event study estimates for high-accountability grades in Appendix Figure A2 and Table A3. We present analogous results for the moderating effects of successful PBIS implementation in higher-accountability grades (3-11) in Appendix Table A4.

[20] We similarly find no effects on quadratic terms for pre-policy referral rates (class: -0.000 (0.004); subjective: -0.000 (0.006)) and in models where we average referral rates from the two years prior to policy implementation and then leave these two years out (class: -0.011 (0.044); subjective: 0.019 (0.038)).

7. Assumption Checks and Sensitivity Analyses

The three central assumptions of our identification strategy hold across multiple tests. Given the extensive robustness checks we conduct, for the purpose of parsimony we display the relevant results from these checks in Figures 4 and 5 in the main text of the paper and display the full set of coefficients and statistics in Appendix A.

7.a Pre-trends

Our formal tests of the parallel trends assumption reinforce the graphical evidence from Figures 2 and 3 that schools in states that did not implement higher-stakes evaluation (or had not yet) provide valid counterfactuals. We present results of our tests of parallel pre-trends in Panel A of Figures 4 and 5 (corresponding to Appendix Tables A5 and A6). If the assumption holds, these coefficients should be indistinguishable from zero, which in all cases is true. Note that the pre-trend coefficients without covariate adjustments are also indistinguishable from zero (Estimates A1 and A4). As the unconditional parallel trends assumption is met, our models are robust to concerns raised by Sant'Anna and Zhao (Sant'Anna & Zhao, 2019) about inaccurate treatment effect estimates in the presence of heterogeneous treatment effects and covariate-specific time trends. Estimates A3i and A6i present coefficients on quadratic time trends and are also indistinguishable from zero.

7.b Placebo tests

We find no evidence that the introduction of high-stakes evaluation affects outcomes that we do not anticipate these reforms would influence. Further, when we adjust the date of policy implementation to create falsification tests, we find no evidence that these artificial policy implementation dates influenced referral rates. We present these results in Panel B of Figures 4 and 5 (corresponding to Appendix Tables A7 – A10). Estimates of the effect of evaluation policy

on unaffected outcomes should be indistinguishable from zero. Similarly, estimates of the effect of placebo evaluation dates in years before the policy was actually implemented should also be zero (or at least substantially attenuated in pooled pre- and post- tests).

We present Estimates B1 – B6 in Figure 4 as evidence that evaluation policy implementation had no effect on ODRs from locations other than the classroom and on ODRs for behavioral infractions that were objectively reasons to send students to the office. In estimates B7 – B10 we demonstrate that using a date of evaluation implementation two or four years[21] before the actual implementation date is not predictive of changes in ODR rates.

In Estimates B1 – B6 in Figure 5, we present the corresponding placebo outcome tests in our analysis of potential heterogeneous effects in higher-accountability grades. Here, we use both our main outcomes (classroom and objective ODRs) in the theoretically "unaffected" grades (K-2, 12) as well as the secondary outcomes (other location and subjective ODRs) in the theoretically "affected" grades (3-11). In Estimates B7 – B10, we present analogous falsification tests where we use a date two or four years prior to the actual policy implementation and examine the effects on just the high-accountability grades. Again, all estimates are indistinguishable from zero.

In Estimates B11 – B13 of Figure 4, we present results in which we test the effect of evaluation policy implementation on rates of suspension from the Civil Rights Data Collection (CRDC) sample. In this national sample of schools, we note that the introduction of evaluation policies resulted in a one-percentage-point increase in the proportion of suspended students. This outcome is scaled differently than our main outcome, nevertheless it is small in absolute magnitude and not fully robust to alternate specifications (see Appendix Table A8). Thus, we interpret the

[21] The four-year placebo test is the largest for which we are able to observe all schools pre-treatment.

CRDC findings as indicative of some potential for endogenous shifts in state discipline policy but generally in line with our other falsification tests.

7.c Alternate sample, policy, specification and weighting approaches

In our last set of robustness checks, we present further evidence that the schools in untreated states provide valid counterfactuals, that our results are not driven by concurrent policies, and that our results are robust to the method of weighting individual fixed effects ATEs into a pooled estimate. In Panel C of Figures 4 and 5 (corresponding to Appendix Tables A11 – A17), we present these results. In addition to the zero line, we also include for reference the point estimates from our preferred models (Models II and V in Tables 2 and 5). Results of these robustness checks should overlap with the main results. Given that our main models find that teacher evaluation policy reform does not change the rate of ODRs, they should also overlap with zero.

We first test the robustness of our results to a slight expansion of our main analytic sample. Our primary sample is comprised of 107,458 grade-school-year observations that represent 20,135 school-year observations. However, our data includes outcomes reported only at the school level for an additional 384 school-year observations. We present results from re-estimating Equations (2) and (3) using data aggregated at the school level in Estimates C1 – C4 in Figure 4. The estimates are essentially identical to our grade-level models.

We estimate our primary models using Weighted Least Squares in which we weight each observation by the grade-level enrollment. OLS estimates will return a heteroskedastic error term because estimates of ODRs will be known with more precision in grades (and schools and states) with a larger enrollment. Weighting our observations allows us to interpret our estimates as the effect of teacher evaluation on the rate of ODRs in the average-sized grade. In Estimates C5 and

C6 in Figure 4 and C1 and C4 of Figure 5, we present results from unweighted OLS models and again fail to reject the null with respect to the main results or zero.

We find no evidence that our results are driven by endogenous differences between schools in states that do and do not adopt high-stakes evaluation. Miller, Shenhav and Grosz (2019) recently added to the literature finding that fixed-effects models which rely on selection into identification often return biased results due to endogenous differences in those units which select the treatment. Thus, in Estimates C7 and C9 of Figure 4 and C2 and C5 of Figure 5 we present results in which we restrict our sample to only those grade-year observations nested in states which ever implemented evaluation. These results, therefore, identify causal effects only off of differential timing of *when,* and not *whether*, states enacted evaluation policy. We again fail to reject the null.

We may also be concerned that our difference-in-difference results are driven by events substantially removed from policy enactment, particularly when we observe these time periods for only some units. Given the start and end periods of our data, the maximal years pre- and post-teacher evaluation reform that we can see for all observations is 5 years pre- and 1 year after the initial policy implementation. In Estimates C8 and C10 of Figure 4 and C3 and C6 of Figure 5, we restrict our sample to grade-year observations during this frame. In all of these estimates but one, we reject the null. When we estimate the effect of evaluation reform for high-accountability grades (3-11) on subjective ODRs in the balanced panel defined above, we find that it modestly reduced the rate of ODRs. We choose to interpret this estimate as consistent with our main findings which are modestly negative in magnitude but indistinguishable from zero.

As expected, our triple-difference estimates (Estimates C11 and C12 of Figure 4 and C7 and 8 of Figure 5) that difference out the change in non-classroom or objective-rationale referrals from the change in our primary outcomes return estimates even closer to zero.

We find no evidence that alternate teacher accountability or school discipline policy reforms either predict any changes in the rates of disciplinary referrals or that they moderate the effects of teacher evaluation. In Estimates C13 and C14 of Figure 4 and C9 and C10 of Figure 5, we present the results of adjusting the main effect of teacher evaluation implementation for the adoption of these other policy reforms. The results are indistinguishable from our main estimates and zero. Appendix Tables A14 and A15 further demonstrate that none of the policy reforms individually predicts changes in ODR rates. We also estimate the effects of bundles of accountability policies separately from the effects of discipline reforms. All results are consistent.

We find no evidence that our results are driven or biased by greater treatment weights imposed on units treated in the middle of the policy window due to greater conditional variance in treatment (Goodman-Bacon, 2018), that there are any negative weights on our individual unit-year observations, or that our results are sensitive to alternate mechanisms for weighting fixed effect ITEs. In Panel A of Appendix Figure A3, we present the distribution of weights on each school-year fixed effect by year that the unit was treated. We find no evidence of systematic variation in fixed effect weight by year of implementation of teacher evaluation reform. In Panel B of Figure A3, we plot the school-year-level ITE against its weight. Notably, there are no negative weights and relatively few outlying values; thus, we are relatively unconcerned with the recent concerns raised about fixed effects estimates in our sample.

We formally compare our results with de Chaisemartin and D'Haultfoeuille's (2019) Wald-TC estimator in Estimates C15 and C16 of Figure 4. Though the exact coefficients on these estimates are slightly different than our main models, they are nevertheless extremely small in magnitude and statistically indistinguishable from either zero or our main model coefficients. We present the graphical event study using the Wald-TC estimands in Appendix Figure A4. Note,

however, that the results using this estimand are less precise, and we are unable to rule out relatively large effects given this approach.[22]

Finally, we note that our finding that successfully implementing PBIS has no moderating effect on the implementation of high-stakes evaluation is robust to alternate sample construction. In Appendix Tables A18 and A19, we present results that restrict the sample to grade-year observations in states that ever implemented teacher evaluation as well as ones that restrict the sample to 5-years-pre- and 1-year post-evaluation implementation. Results across all models are equivalent to our main models.

8. Conclusion

Policy makers and school system leaders have a critical interest in understanding whether, and if so how, educators respond to external accountability pressures. Designers of accountability-based policies must carefully weight the purported benefits of the policy against its potential harms. In this paper we find that, in the context of pressures from higher-stakes teacher evaluation policies, teachers do not, on average, alter their responses to students' classroom misbehavior. Across a variety of specifications and robustness checks, we find no evidence that the rates of removing students from class changed in the aftermath of these policy reforms, though we cannot rule out the possibility of very small effects. Furthermore, we find no evidence that when schools improve at developing systems of behavioral supports that these serve to moderate any effects of evaluation implementation.

[22] Replications of the Wald-TC results will return slightly different values due to the Stata package **did_multipleGT**'s use of the bootstrapping method for obtaining standard errors (we use 50 replications). To reduce computing resource demands, we estimate these results using our school-year sample, though in practice this does not affect our standard errors as we cluster them at the state level.

We make considerable efforts to ensure that we have selected appropriate counterfactuals to ensure that our findings do not reflect countervailing forces, either secular trends or unobserved shocks, that mask the true effect of increased accountability. These threats are difficult to fully disprove. Furthermore, data limitations prevent us from fully modeling teachers' classroom-based disciplinary responses. To better understand the data generating process, we would benefit from records of each instance of student misbehavior, including those that do not result in an office referral. Ideally, we would like to observe heterogeneity in teachers' responses to accountability pressures by demographic characteristics, preparation pathways, professional experiences, assessed skill and more. Additionally, we are unable to distinguish whether our findings reflect low-level intensity in the implementation of teacher evaluation or limits in the ability of accountability pressures to influence teacher practice. We also do not observe the date of students' disciplinary infractions, and so are unable to test Figlio's (2006) finding that impending state accountability assessments increase the severity of disciplinary responses. These outstanding issues present promising opportunities for future analysis of the effects of accountability pressures on classroom choices by teachers, including those beyond pedagogy.

Nevertheless, our findings contribute to the limited understanding of the effects of accountability policy inside the black-box of classroom practice. For those hoping for dramatic improvements in teaching practice as well as for those concerned about serious unintended consequences of high-stakes evaluation policy, our findings present another reminder of the loose-coupling between education policy, teacher behavior and classroom practices.

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Tables

Table 1. Descriptive statistics on School-Wide Information System (SWIS) data, 2006-2017

	Full Sample	NCES Public (2006-2016)	Never Evaluation	Ever Evaluation
Total Schools	2,564	98,556	750	1,814
School-Year Observations	20,135		4,332	15,803
Grade-Year Observations	107,458		24,489	82,969
Total Districts	939	13,647	313	626
IQR Schools per District	2-10		1-8	2-12
School Level				
Elem (K-6)	1,683		484	1,199
Middle (6-9)	405		97	308
High (9-12)	159		40	119
Multi-level Grade Span	317		128	189
School Locale				
Rural	516	0.29	195	321
Town	502	0.14	187	315
Suburban	834	0.30	104	730
Urban	712	0.22	263	449
			Pre-2011 Characteristics	
School Characteristics				
Avg. School Enrollment	510.2 (306.7)	521.2	437.6	509.2***
% Low Income	0.52 (0.25)	0.49	0.49	0.52**
% American Indian/Native Alask.	0.01 (0.05)	0.01	0.02	0.01***
% Asian/Pacific-Islander	0.05 (0.08)	0.05	0.05	0.03***
% Black	0.13 (0.19)	0.16	0.05	0.16***
% Hispanic	0.20 (0.23)	0.24	0.20	0.12***
% White Non-Hispanic	0.54 (0.31)	0.52	0.63	0.58***
% Schools by Year Implementing PBIS	0.71		0.60	0.62
Grade-Level Outcomes				
Daily Referrals per 500 students - Classroom	2.03 (2.83) [1.69]		2.30	2.17**
Daily Referrals per 500 students - Other Location	1.56 (1.77) [1.22]		1.97	1.84***
Daily Referrals per 500 students - Classroom Subject	1.29 (1.95) [1.23]		1.61	1.43***
Daily Referrals	0.48		0.41	0.45***

per 500 students - Classroom Object	(0.90)
	[0.40]
% Schools under High-Stakes Evaluation	
2010-11	0.00
2011-12	0.13
2012-13	0.16
2013-14	0.36
2014-15	0.52
2015-16	0.55
2016-17	0.72
2017-18	0.72

Notes: * $p<.05$, ** $p<.01$, *** $p<.001$. Sample characteristics and outcomes weighted by school enrollment. Standard deviations, where applicable, in parentheses. Within-school standard deviations in brackets. 85 school-year observations have race/ethnicity data imputed to the same-school median. 40 school-year observations have race/ethnicity data imputed to the district-school year median. 490 school-year observations have low-income data imputed to the same-school median. 59 school-year observations have low-income data imputed to the district-school year value. 67 school-year observations have low income data imputed to the district median. Low-income and race enrollment capped at 100 percent of school enrollment. Outcomes above 99th percentile re-coded to value of 99th percentile. Schools implementing PBIS defined in text. National public school averages are from NCES Digest of Education Statistics between fall 2006-2016 (latest available year). NCES counts are averages of counts from 2006-16, while NCES means are averages of yearly averages.

Table 2. The effect of teacher evaluation reforms on Office Disciplinary Referrals, by location and subjectivity

	A. Class			B. Subjective		
	I	II	III	IV	V	VI
Implement evaluation	-0.084	-0.089	-0.083	-0.041	-0.042	-0.054
	(0.063)	(0.064)	(0.073)	(0.050)	(0.051)	(0.044)
Implement evaluation * Trend			0.046			0.007
			(0.043)			(0.024)
Time trend			-0.017			0.004
			(0.033)			(0.023)
School composition controls		X	X		X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135	20,135
R-squared	0.559	0.559	0.559	0.55	0.55	0.55

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table 3. The moderating effect of Positive Behavioral Interventions and Supports (PBIS) on the effect of teacher evaluation reforms on Office Disciplinary Referrals, by location and subjectivity

	A. Classroom				B. Subjective			
	I	II	III	IV	V	VI	VII	VIII
Implement evaluation	-0.045	-0.083	-0.089	-0.244	-0.054	-0.075	-0.078	-0.193
	(0.072)	(0.110)	(0.111)	(0.200)	(0.068)	(0.085)	(0.086)	(0.110)
Implement PBIS well		-0.116	-0.116	-0.105		-0.086	-0.086	-0.081
		(0.064)	(0.064)	(0.064)		(0.046)	(0.046)	(0.046)
Implement evaluation * PBIS		0.036	0.037	0.171		0.018	0.019	0.116
		(0.099)	(0.099)	(0.191)		(0.060)	(0.061)	(0.102)
Implement evaluation * Trend				0.139				0.064
				(0.091)				(0.050)
Implement evaluation * Trend * PBIS				-0.087				-0.064
				(0.085)				(0.045)
Time trend				0.002				0.016
				(0.036)				(0.034)
School composition controls			X	X			X	X
Grade-year observations (N)	66,076	66,076	66,076	66,076	66,076	66,076	66,076	66,076
School-year observations	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309
R-squared	0.602	0.602	0.602	0.602	0.584	0.584	0.584	0.584

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment. Models I and V replicate results from main DD estimate on PBIS implementation sub-sample. Fewer observations reflect subset of grade-school-year observations (61.5 percent) reporting PBIS implementation information.

Table 4. The effect of teacher evaluation reforms on Office Disciplinary Referrals, by pre-evaluation implementation referral rates

	A. Classroom				B. Subjective			
	I	II	III	IV	V	VI	VII	VIII
Implement evaluation	-0.100	-0.120	-0.123	-0.024	-0.064	-0.101	-0.102	-0.057
	(0.083)	(0.089)	(0.089)	(0.078)	(0.051)	(0.054)	(0.054)	(0.049)
Implement evaluation *		0.011	0.012	-0.025		0.031	0.032	-0.005
$ODR_{r=-1}$		(0.042)	(0.042)	(0.044)		(0.033)	(0.033)	(0.032)
School composition controls			X	X			X	X
Grade-year observations (N)	74,452	74,452	74,452	82,969	74,452	74,452	74,452	82,969
School-year observations	14,175	14,175	14,175	15,803	14,175	14,175	14,175	15,803
R-squared	0.546	0.546	0.547	0.548	0.523	0.523	0.523	0.525

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects, are weighted by grade enrollment, and include only states ever-under higher-stakes evaluation law. Models I and V re-estimate main effects of evaluation from Table 2 on sample with all observations in year prior to evaluation implementation omitted. Models II-III and VI-VII estimate effects of evaluation interacted with rate in year prior to evaluation implementation. Models IV and VIII include all observations (including those in r=-1), though these are not our preferred specifications as they include the same ODR values on left- and right-hand side of equation.

Table 5. The effect of teacher evaluation reforms on Office Disciplinary Referrals, by grade-level accountability pressures, location and subjectivity

	A. Class (3-11 only)			B. Subjective (3-11 only)		
	I	II	III	IV	V	VI
Implement evaluation	-0.092	-0.098	-0.096	-0.052	-0.054	-0.075
	(0.067)	(0.068)	(0.078)	(0.057)	(0.059)	(0.046)
Implement evaluation * Trend			0.054			0.008
			(0.048)			(0.028)
Time trend			-0.018			0.008
			(0.034)			(0.025)
School composition controls		X	X		X	X
Grade-year observations (N)	64,431	64,431	64,431	64,431	64,431	64,431
School-year observations	19,630	19,630	19,630	19,630	19,630	19,630
R-squared	0.586	0.586	0.586	0.573	0.573	0.573

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Figures

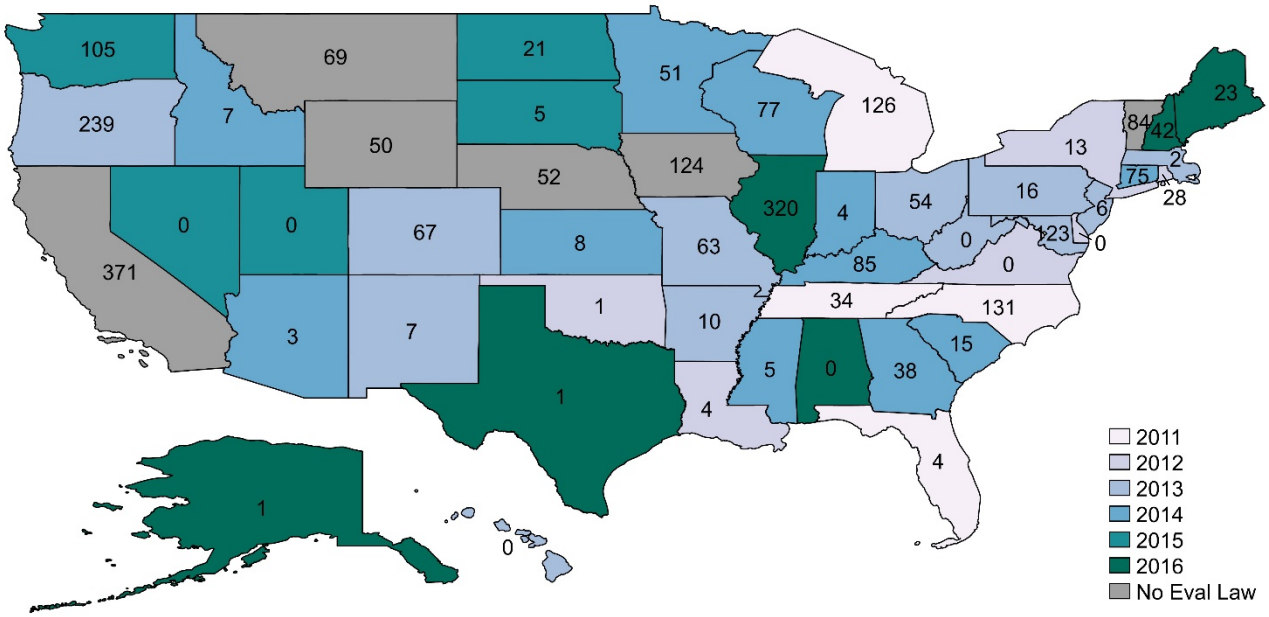
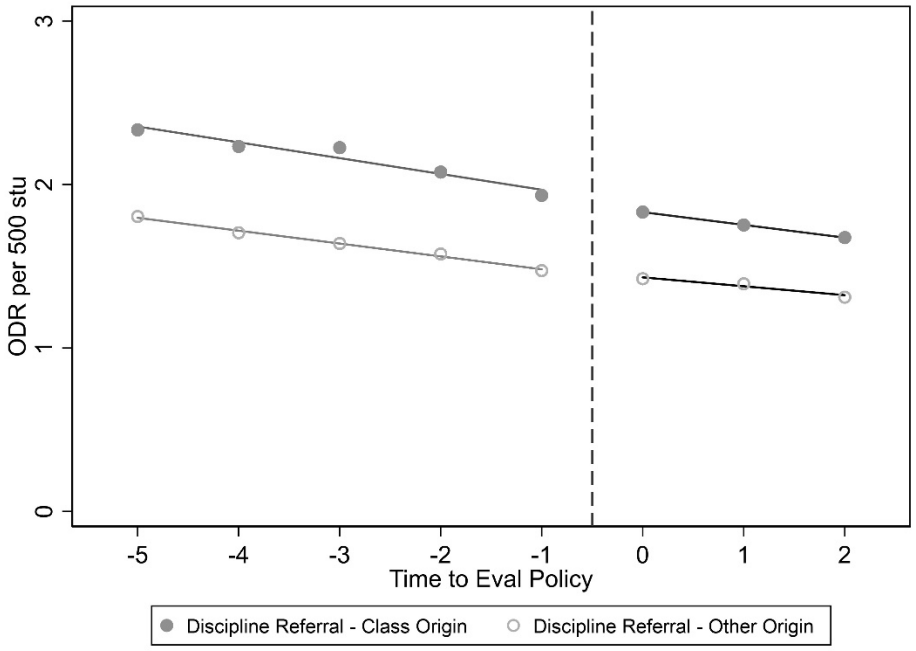
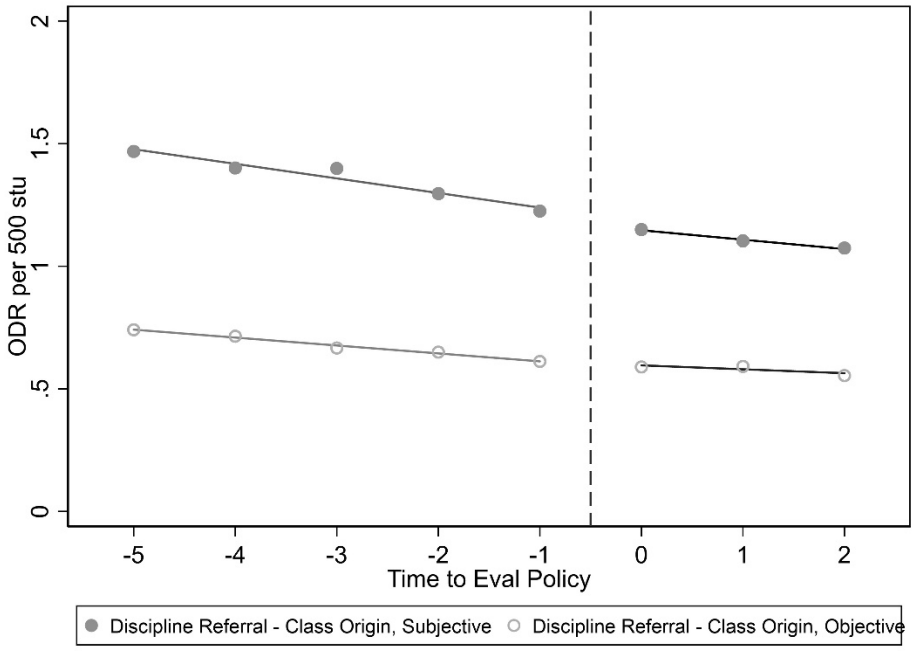


Figure 1. The timing of statewide teacher evaluation reforms and number of schools by state in analytic sample

Notes: Years represent the fall of the academic year in which new evaluation systems were fully implemented statewide. Numbers inside each state represent total schools in analytic sample (n=2,564). Full list of states with schools in sample and timing of evaluation in Appendix Table A1.



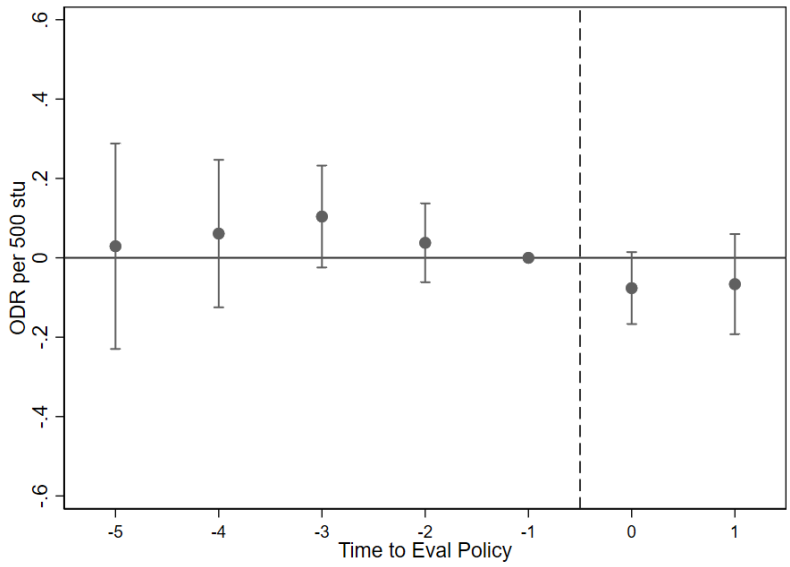
Panel A. Location of ODR



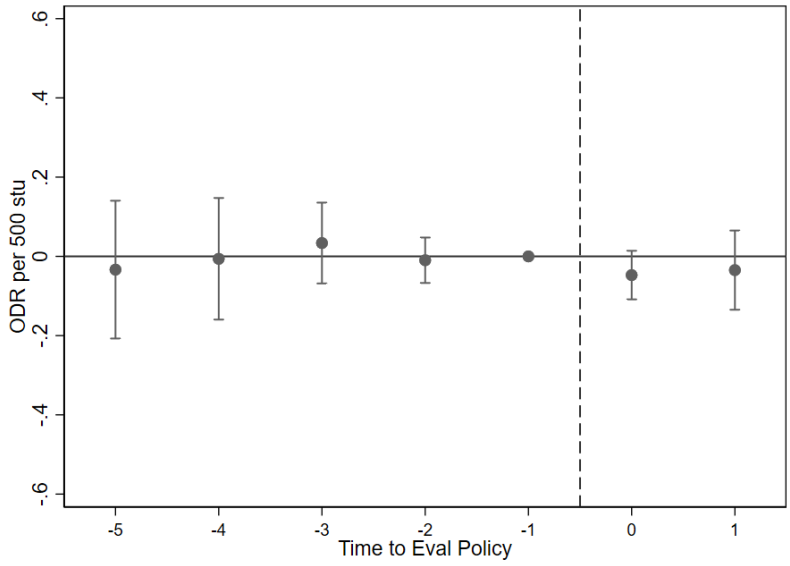
Panel B. Objectivity of ODR

Figure 2. Average Office Disciplinary Referral rates before and after the introduction of teacher evaluation policies, by location of referral (Panel A) and objectivity of infraction (Panel B)

Notes: Points represent weighted average of Office Disciplinary Referrals for schools in states that implemented high-stakes evaluation. Line is best fit for those averages.



Panel A. Classroom ODRs



Panel B. Subjective Classroom ODRs

Figure 3. Non-parametric event study displaying effect of high-stakes teacher evaluation reforms on rate of per-500-student, per-day Office Disciplinary Referrals (ODRs), by location and subjectivity

Notes: Point estimates for years pre- and post-evaluation reforms and corresponding 95 percent confidence intervals derived from event study model describe in Equation 1 that is weighted by grade enrollment, includes grade, school and year fixed effects and time-varying school characteristics, with standard errors clustered at state level. Full coefficients reported in Columns IIa and IIc of Appendix Table A2.

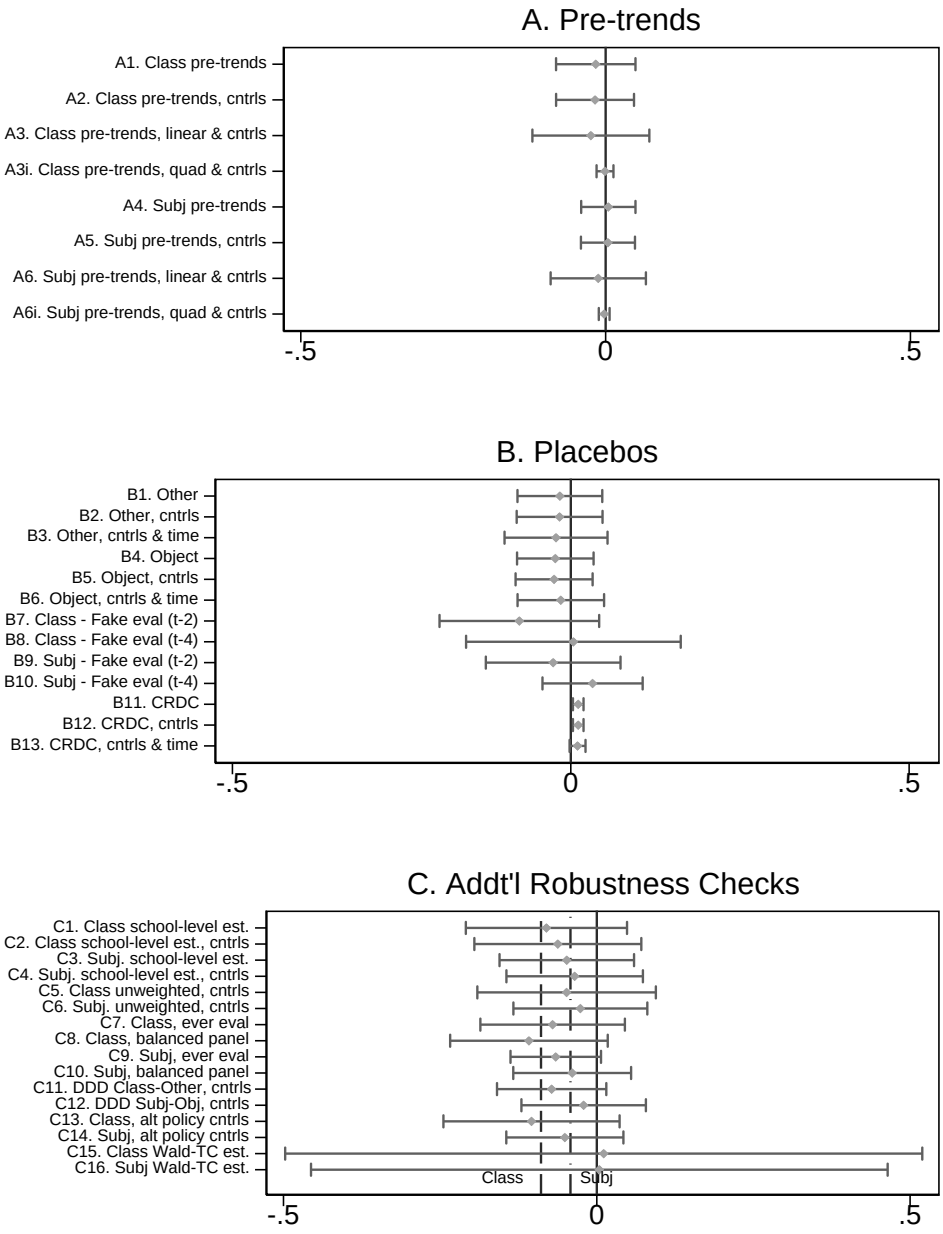


Figure 4. Point estimates and 95 percent confidence intervals of tests of assumptions on main difference-in-differences analysis (grades K-12).

Notes: To meet difference-in-differences assumptions, all 95 confidence intervals should overlap with zero in Panels A and B. Estimates in Panel C should overlap with preferred DD model point estimates for classroom (dash: -0.089) and classroom, subjective ODRs (dash-point: -0.042). As these are indistinguishable from zero in the population, all estimates in Panel C should also overlap with zero. Data for estimates B11-B13 from Civil Rights Data Collection. Full set of point estimates available in Appendix Tables A5, A7, A8, A11, A12, A14 and A16.

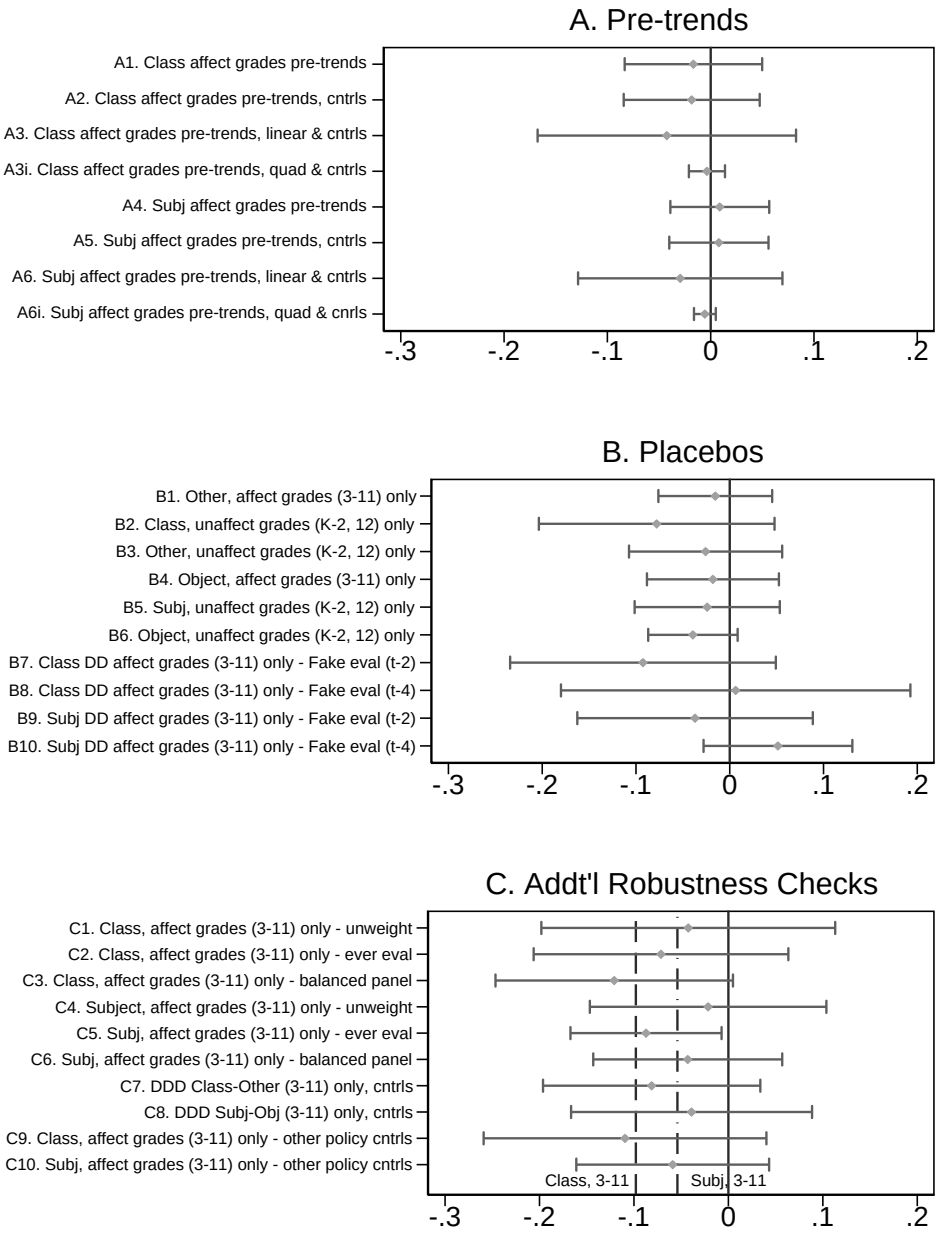


Figure 5. Point estimates and 95 percent confidence intervals of tests of assumptions on high-accountability grade difference-in-differences analysis (grades 3-11).

Notes: To meet difference-in-differences assumptions, all 95 confidence intervals should overlap with zero in Panels A and B. Estimates in Panel C should overlap with preferred DD model point estimates for classroom (dash: -0.098) and classroom, subjective ODRs (dash-point: -0.054). As these are indistinguishable from zero in the population, all estimates in Panel C should also overlap with zero. Full set of point estimates available in Appendix Tables A6, A9, A10, A13, A15, and A17.

Appendix A. Additional Tables and Figures

Table A1. Education policy reforms by state, 2006-2018

	Schools in sample	Implement evaluation	Eliminate tenure	Weaken collective bargaining	Change teach. authority to remove stud. from class	Limit suspension/ exclusion
Alabama	0	2016				
Alaska	1	2016				
Arizona	3	2014				
Arkansas	10	2013				
California	371	None				2014
Colorado	67	2013				2012
Connecticut	75	2014			2018	2018
Delaware	0	2012				2018
Distr. of Columbia	0	2009			**2009**	**2009**; 2018
Florida	4	2011	**2011**			2009; 2018
Georgia	38	2014				**2014**
Hawaii	0	2013				2009
Idaho	7	2014	2011	2011		
Illinois	320	2016				**2016**
Indiana	4	2014			2009	
Iowa	124	None				
Kansas	8	2014	**2014**			
Kentucky	85	2014				
Louisiana	4	2012	**2012**		2009	2007; 2008; 2009; **2012**; 2015
Maine	23	2016				
Maryland	123	2013			2009	2014; 2017
Massachusetts	2	2013				
Michigan	126	2011				2017
Minnesota	51	2014			2016	
Mississippi	5	2014				
Missouri	63	2013				
Montana	69	None				
Nebraska	52	None				
Nevada	0	2015				**2015**
New Hampshire	42	2016				
New Jersey	6	2013			2012	2016
New Mexico	7	2013			2009	
New York	13	2012				
North Carolina	131	2011	2013			**2011**
North Dakota	21	2015				

Ohio	54	2013			2017; 2018
Oklahoma	1	2012			
Oregon	239	2013		2014	2014
Pennsylvania	16	2013			
Rhode Island	28	2012			2007; 2009; **2012**
South Carolina	15	2014			
South Dakota	5	2015			2014
Tennessee	34	2011	**2011**		2007; 2008; 2013; 2015; 2018
Texas	1	2016		2015	2011; 2017
Utah	0	2015			
Vermont	84	None			2011
Virginia	0	2012			2009; 2018
Washington	105	2015			2016
West Virginia	0	2013			2014
Wisconsin	77	2014	2011		
Wyoming	50	None			

Notes: Evaluation and teacher accountability policies drawn from Steinberg and Donaldson (2016), NCTQ (2016), and Kraft et al.(2019). Discipline policy changes draw from and Bezinque et al. (2018) from the National Center on Safe Supportive Learning Environments School Discipline Laws and Regulations Compendium database fields: "Teacher authority to remove students from classrooms" and "Limitations, conditions, or exclusions for use of suspension and expulsion." Covers 2006-2018. Time-varying implementation measures account for states in which policy was passed but never implemented. Years in bold are policies occurring in same year as teacher evaluation reform. Nine states reformed suspension/expulsion laws multiple times during this window. Main robustness checks use first year of reform, alternate tests use year closest to evaluation law.

Table A2. Event study estimates of the effect of high-stakes teacher evaluation on ODRs, by location and subjectivity

	A. Class		B. Other		C. Subjective		D. Objective	
	Ia	IIa	Ib	IIb	Ic	IIc	Id	IId
-6 or more yrs pre	0.125	0.136	0.127	0.129	-0.013	-0.007	0.08	0.082
	(0.172)	(0.169)	(0.136)	(0.134)	(0.113)	(0.113)	(0.077)	(0.077)
5 yrs pre	0.028	0.029	0.109	0.109	-0.034	-0.033	0.028	0.029
	(0.135)	(0.132)	(0.094)	(0.093)	(0.089)	(0.089)	(0.064)	(0.063)
4 yrs pre	0.059	0.061	0.094	0.091	-0.006	-0.006	0.031	0.031
	(0.095)	(0.095)	(0.073)	(0.073)	(0.078)	(0.078)	(0.042)	(0.042)
3 yrs pre	0.1	0.104	0.06	0.058	0.033	0.034	0.028	0.029
	(0.066)	(0.066)	(0.040)	(0.040)	(0.051)	(0.052)	(0.032)	(0.033)
2 yrs pre	0.034	0.038	0.029	0.028	-0.01	-0.01	0.027	0.029
	(0.050)	(0.051)	(0.030)	(0.030)	(0.029)	(0.029)	(0.022)	(0.024)
1 yr pre	0	0	0	0	0	0	0	0
Evaluation introduced	-0.072	-0.076	-0.023	-0.025	-0.045	-0.047	-0.015	-0.016
	(0.045)	(0.046)	(0.020)	(0.020)	(0.031)	(0.031)	(0.022)	(0.022)
1 yr post	-0.063	-0.066	0.005	0.004	-0.033	-0.035	-0.018	-0.019
	(0.064)	(0.064)	(0.033)	(0.033)	(0.050)	(0.051)	(0.031)	(0.031)
2 yrs post	-0.027	-0.034	0.066	0.065	-0.016	-0.019	-0.011	-0.012
	(0.103)	(0.101)	(0.070)	(0.071)	(0.076)	(0.077)	(0.050)	(0.050)
3+ yrs post	0.007	0.002	0.186	0.186	-0.015	-0.018	0.007	0.006
	(0.181)	(0.179)	(0.116)	(0.117)	(0.124)	(0.125)	(0.079)	(0.078)
School composition controls		X		X		X		X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458	107,458	107,458	107,458

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment. Coefficients on 6 or more years pre, 2 yrs post and 3+ yrs post reported in table, but do not apply to all observations in sample due to differential timing; thus not reported in Figure 3.

Table A3. Event study estimates of the effect of high-stakes teacher evaluation on ODRs, by grade-level accountability pressures, location and subjectivity

Panel A. Classroom and Other Locations

	Class				Other			
	3-11		K-2, 12		3-11		K-2, 12	
-6 or more yrs pre	0.109	0.123	0.099	0.107	0.122	0.124	0.113	0.113
	(0.172)	(0.170)	(0.193)	(0.189)	(0.161)	(0.158)	(0.137)	(0.137)
5 yrs pre	0.042	0.045	-0.01	-0.011	0.125	0.125	0.074	0.072
	(0.147)	(0.146)	(0.133)	(0.130)	(0.095)	(0.093)	(0.105)	(0.104)
4 yrs pre	0.07	0.073	0.022	0.024	0.122	0.12	0.028	0.024
	(0.110)	(0.111)	(0.093)	(0.092)	(0.084)	(0.084)	(0.072)	(0.073)
3 yrs pre	0.106	0.11	0.083	0.085	0.064	0.062	0.052	0.05
	(0.071)	(0.071)	(0.076)	(0.075)	(0.040)	(0.041)	(0.060)	(0.060)
2 yrs pre	0.031	0.034	0.037	0.04	0.035	0.034	0.019	0.02
	(0.060)	(0.061)	(0.044)	(0.043)	(0.034)	(0.034)	(0.032)	(0.032)
1 yr pre	0	0	0	0	0	0	0	0
Evaluation introduced	-0.087	-0.093	-0.042	-0.045	-0.026	-0.028	-0.022	-0.023
	(0.054)	(0.055)	(0.045)	(0.045)	(0.021)	(0.021)	(0.033)	(0.033)
1 yr post	-0.061	-0.065	-0.075	-0.077	0.015	0.015	-0.019	-0.02
	(0.065)	(0.065)	(0.074)	(0.072)	(0.032)	(0.033)	(0.048)	(0.048)
2 yrs post	-0.021	-0.03	-0.049	-0.053	0.091	0.09	0.01	0.009
	(0.103)	(0.101)	(0.136)	(0.132)	(0.065)	(0.064)	(0.115)	(0.115)
3+ yrs post	0.019	0.011	-0.034	-0.036	0.213*	0.213*	0.119	0.121
	(0.179)	(0.177)	(0.203)	(0.198)	(0.102)	(0.103)	(0.181)	(0.181)
School composition controls		X		X		X		X
Grade-year observations (N)	64,431	64,431	43,027	43,027	64,431	64,431	43,027	43,027

Panel B. Subjective and Objective Reasons

	Subjective				Objective			
	3-11		K-2, 12		3-11		K-2, 12	
-6 or more yrs pre	-0.059	-0.052	0.043	0.047	0.098	0.101	0.028	0.03
	(0.112)	(0.113)	(0.118)	(0.116)	(0.090)	(0.090)	(0.064)	(0.063)
5 yrs pre	-0.04	-0.038	-0.031	-0.031	0.038	0.04	0.005	0.004
	(0.100)	(0.100)	(0.081)	(0.080)	(0.074)	(0.073)	(0.053)	(0.052)
4 yrs pre	-0.004	-0.004	-0.022	-0.022	0.032	0.033	0.025	0.025
	(0.095)	(0.096)	(0.058)	(0.057)	(0.046)	(0.045)	(0.043)	(0.042)
3 yrs pre	0.03	0.031	0.035	0.034	0.027	0.028	0.028	0.03
	(0.062)	(0.063)	(0.041)	(0.041)	(0.034)	(0.035)	(0.034)	(0.035)
2 yrs pre	-0.021	-0.021	0.008	0.008	0.034	0.036	0.012	0.014
	(0.036)	(0.036)	(0.024)	(0.023)	(0.026)	(0.028)	(0.020)	(0.021)

1 yr pre	0	0	0	0	0	0	0	0
Evaluation introduced	-0.06	-0.062	-0.017	-0.018	-0.008	-0.01	-0.027	-0.028
	(0.037)	(0.038)	(0.030)	(0.030)	(0.026)	(0.026)	(0.020)	(0.020)
1 yr post	-0.04	-0.041	-0.027	-0.028	-0.009	-0.01	-0.035	-0.036
	(0.054)	(0.056)	(0.047)	(0.047)	(0.035)	(0.034)	(0.029)	(0.029)
2 yrs post	-0.018	-0.022	-0.021	-0.024	-0.007	-0.009	-0.018	-0.019
	(0.080)	(0.082)	(0.086)	(0.084)	(0.057)	(0.057)	(0.049)	(0.049)
3+ yrs post	-0.011	-0.014	-0.039	-0.041	0.005	0.004	0.009	0.01
	(0.132)	(0.134)	(0.114)	(0.113)	(0.084)	(0.082)	(0.085)	(0.083)
School composition controls		X		X		X		X
Grade-year observations (N)	64,431	64,431	43,027	43,027	64,431	64,431	43,027	43,027

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment. Coefficients on 6 or more years pre, 2 yrs post and 3+ yrs post reported in table, but do not apply to all observations in sample due to differential timing; thus not reported in Figure A2.

Table A4. The moderating effect of Positive Behavioral Interventions and Supports (PBIS) on the effect of teacher evaluation reforms on Office Disciplinary Referrals, by grade-level accountability pressures, location and subjectivity

	A. Class			B. Subjective		
	I	II	III	IV	V	VI
	3-11	3-11	K-2, 12	3-11	3-11	K-2, 12
Implement evaluation	-0.017	-0.084	-0.053	-0.044	-0.089	-0.029
	(0.087)	(0.133)	(0.104)	(0.086)	(0.106)	(0.076)
Implement PBIS well		-0.148	-0.01		-0.115	-0.003
		(0.082)	(0.043)		(0.058)	(0.029)
Implement evaluation * PBIS		0.070	-0.083		0.044	-0.063
		(0.130)	(0.088)		(0.082)	(0.065)
School composition controls	X	X	X	X	X	X
Grade-year observations (N)	39,648	39,648	26,428	39,648	39,648	26,428
School-year observations	12,020	12,020	9,521	12,020	12,020	9,521
R-squared	0.631	0.631	0.59	0.61	0.61	0.584

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment. Models I-II and IV-V restricted to high-accountability grades (3-11). Models I and IV replicate results in the high accountability grades of the main effect of high-stakes evaluation in subset of schools reporting PBIS implementation information. Models II and V show moderating effect of PBIS implementation in grades 3-11. Models III and VI restricted to lower-accountability grades (K-2, 12). Fewer observations reflect subset of grade-year observations (61.5 percent) reporting PBIS implementation information.

Table A5. Parallel trends assumption checks, by location and subjectivity

	Class			Other		
	I	II	III	IV	V	VI
Panel A. Class and Other Locations						
Linear pre-trend	-0.016	-0.017	-0.024	-0.027	-0.027	-0.02
	(0.033)	(0.033)	(0.049)	(0.026)	(0.025)	(0.023)
Quadratic pre-trend			-0.001			0.001
			(0.007)			(0.004)
School composition controls		X	X		X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135	20,135
R-squared	0.559	0.559	0.559	0.534	0.535	0.535
	Subjective			Objective		
Panel B. Subjective and Objective Reasons						
Linear pre-trend	0.004	0.004	-0.012	-0.011	-0.011	0.003
	(0.023)	(0.023)	(0.040)	(0.015)	(0.015)	(0.018)
Quadratic pre-trend			-0.002			0.002
			(0.005)			(0.002)
School composition controls		X	X		X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135	20,135
R-squared	0.55	0.55	0.55	0.555	0.555	0.555

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table A6. Parallel trend assumption checks, by grade-level accountability pressures, location and subjectivity

	Class						Other					
	3-11			K-2, 12			3-11			K-2, 12		
Linear pre-trend	-0.017	-0.018	-0.042	-0.007	-0.008	0.001	-0.029	-0.03	-0.043	-0.019	-0.019	0.018
	(0.034)	(0.034)	(0.064)	(0.035)	(0.034)	(0.056)	(0.028)	(0.028)	(0.024)	(0.026)	(0.026)	(0.046)
Quadratic pre-trend			-0.004			0.001			-0.002			0.006
			(0.009)			(0.009)			(0.005)			(0.007)
School composition controls		X	X		X	X		X	X		X	X
Grade-year observations (N)	64,431	64,431	64,431	43,027	43,027	43,027	64,431	64,431	64,431	43,027	43,027	43,027
School-year observations	19,630	19,630	19,630	15,608	15,608	15,608	19,630	19,630	19,630	15,608	15,608	15,608
R-squared	0.586	0.586	0.586	0.546	0.546	0.546	0.56	0.561	0.561	0.553	0.553	0.554

	Subjective						Objective					
	3-11			K-2, 12			3-11			K-2, 12		
Linear pre-trend	0.009	0.008	-0.029	0.001	0.000	0.017	-0.013	-0.014	0.006	-0.004	-0.004	-0.009
	(0.024)	(0.025)	(0.050)	(0.021)	(0.021)	(0.036)	(0.018)	(0.017)	(0.020)	(0.013)	(0.012)	(0.023)
Quadratic pre-trend			-0.006			0.002			0.003			-0.001
			(0.005)			(0.006)			(0.003)			(0.003)
School composition controls		X	X		X	X		X	X		X	X
Grade-year observations (N)	64,431	64,431	64,431	43,027	43,027	43,027	64,431	64,431	64,431	43,027	43,027	43,027
School-year observations	19,630	19,630	19,630	15,608	15,608	15,608	19,630	19,630	19,630	15,608	15,608	15,608
R-squared	0.573	0.573	0.573	0.549	0.549	0.549	0.587	0.587	0.587	0.532	0.533	0.533

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table A7. Alternate outcome and evaluation implementation year placebo tests, by location and subjectivity

	I	II	III	IV	V
Panel A. Class and Other Locations		Other		Class (fake)	
Implement evaluation	-0.016	-0.017	-0.022		
	(0.032)	(0.032)	(0.039)		
Implement evaluation * Trend			0.096*		
			(0.043)		
False eval implementation (t-2)				-0.076	
				(0.060)	
False eval implementation (t-4)					0.004
					(0.081)
School composition controls		X	X	X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135
R-squared	0.534	0.534	0.535	0.559	0.559
Panel B. Subjective and Objective Reasons					
		Objective		Subjective (fake)	
Implement evaluation	-0.023	-0.025	-0.015		
	(0.029)	(0.029)	(0.033)		
Implement evaluation * Trend			0.019		
			(0.027)		
False eval implementation (t-2)				-0.026	
				(0.051)	
False eval implementation (t-4)					0.032
					(0.038)
School composition controls		X	X	X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135
R-squared	0.555	0.555	0.555	0.55	0.55

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table A8. CRDC difference-in-difference estimates of high-stakes teacher evaluation policies on out of school suspensions, 2006-2015

	A. Class	B. Subj	C. CRDC			
	I	II	III	IV	V	VI
Implement evaluation	-0.038	-0.026	0.011**	0.011**	0.010	0.009
	(0.062)	(0.047)	(0.004)	(0.004)	(0.006)	(0.005)
Implement evaluation * Trend						-0.004
						(0.003)
Time trend						0.002*
						(0.001)
School composition controls	X	X		X	X	X
Grade-school-year observant.	88,401	88,401	NA	NA	NA	NA
School-year observations	16,562	16,562	284,460	284,460	234,295	284,460
R-squared	0.585	0.571	0.715	0.717	0.718	0.717

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, Title I Status, and race/ethnicity. Models I and II re-estimate main results excluding years 2016-17 and 2017-18 to demonstrate comparability with time-frame of CRDC data. Models III-VI estimate rates of suspension in CRDC data. Model V restricted to schools in states that ever implemented high stakes evaluation reform. All CRDC models include school and year fixed-effects and are weighted by school enrollment. Data obtained from Civil Rights Data Collection. All CRDC sample sizes (school-year observations) rounded to nearest 5 per IES requirements.

Table A9. Alternate outcome placebo tests, by high-accountability grade, location and subjectivity

	A. By Location & Grade			B. By Type & Grade		
	I Other 3-11	II Class K-2, 12	III Other K-2, 12	IV Obj 3-11	V Subj K-2, 12	VI Obj K-2, 12
Implement evaluation	-0.015	-0.078	-0.026	-0.018	-0.024	-0.039
	(0.031)	(0.064)	(0.042)	(0.036)	(0.039)	(0.024)
School composition controls	X	X	X	X	X	X
Grade-year observations (N)	64,431	43,027	43,027	64,431	43,027	43,027
School-year observations	19,630	15,608	15,608	19,630	15,608	15,608
R-squared	0.56	0.546	0.553	0.587	0.549	0.533

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table A10. Alternate evaluation year implementation placebo tests, by location and subjectivity (grades 3-11 only)

	I	II	III	IV	V	VI	VII	VIII
	Class	Other	Class	Other	Subj	Obj	Subj	Obj
False eval implementation (t-2)	-0.093	-0.098			-0.037	-0.012		
	(0.072)	(0.064)			(0.064)	(0.033)		
False eval implementation (t-4)			0.006	-0.064			0.051	-0.032
			(0.095)	(0.073)			(0.040)	(0.053)
School composition controls	X	X	X	X	X	X	X	X
Grade-year observations (N)	64,431	64,431	64,431	64,431	64,431	64,431	64,431	64,431
School-year observations	19,630	19,630	19,630	19,630	19,630	19,630	19,630	19,630
R-squared	0.586	0.56	0.586	0.56	0.573	0.587	0.573	0.587

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table A11. Alternate specifications testing robustness to alternate samples and weighting of average treatment effects

	I	II	III	IV	V	VI	VII	VIII
	School estimates			Wald-TC	Ever eval		Balanced panel	
Panel A. Class and Other Locations								
	Class				Class	Other	Class	Other
Implement evaluation	-0.080	-0.062	-0.067	0.011	-0.070	-0.020	-0.108	-0.040
	(0.066)	(0.068)	(0.076)	(0.259)	(0.059)	(0.027)	(0.064)	(0.030)
Implement evaluation * Time trend			0.043					
			(0.046)					
Time trend			-0.011					
			(0.034)					
School composition controls		X	X	X	X	X	X	X
Grade-year observations (N)	N/A	N/A	N/A	N/A	82,969	82,969	83,455	83,455
School-year observations	20,519	20,519	20,519	20,519	15,803	15,803	15,599	15,599
R-squared	0.706	0.709	0.709	N/A	0.548	0.517	0.594	0.570
Panel B. Subjective and Objective Reasons								
	Subjective				Subject	Object	Subject	Object
Implement evaluation	-0.048	-0.035	-0.053	0.004	-0.066	0.004	-0.039	-0.036
	(0.055)	(0.056)	(0.046)	(0.235)	(0.037)	(0.028)	(0.048)	(0.028)
Implement evaluation * Time trend			-0.001					
			(0.029)					
Time trend			0.010					
			(0.026)					
School composition controls		X	X	X	X	X	X	X
Grade-year observations (N)	N/A	N/A	N/A	N/A	82,969	82,969	83,455	83,455
School-year observations	20,519	20,519	20,519	20,519	15,803	15,803	15,599	15,599
R-squared	0.724	0.727	0.727	N/A	0.526	0.577	0.582	0.583

* p < .05, ** p < .01, *** p < .001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. Models I-III estimated at school-level which includes 384 more school-year observations than main analytic sample. Models I-III include school and year fixed effects and are weighted by school enrollment. Models IV-VIII also include grade fixed effects and are weighted by grade enrollment. Models V and VI restricted to schools that experienced high-stakes evaluation. Models VII and VIII are a balanced panel restricted to school-year observations 5 years before through 1 year after introduction of high-stakes evaluation or never experienced it.

Table A12. Triple-difference estimates of the effects of teacher evaluation reforms on Office Disciplinary Referrals

	I	II	III
Panel A. Class and Other Locations			
Implement evaluation * classroom	-0.070	-0.072	-0.044
	(0.045)	(0.044)	(0.042)
Implement evaluation	-0.015	-0.017	-0.030
	(0.032)	(0.033)	(0.047)
Implement evaluation * classroom * Trend			-0.044
			(0.024)
Implement evaluation * Trend			0.093*
			(0.040)
Time trend			-0.022
			(0.026)
School composition controls		X	X
Grade-year observations (N)	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135
R-squared	0.554	0.554	0.554
	IV	V	VI
Panel B. Subjective and Objective Reasons			
Implement evaluation * Subjective	-0.020	-0.021	-0.019
	(0.051)	(0.051)	(0.039)
Implement evaluation	-0.022	-0.023	-0.025
	(0.028)	(0.028)	(0.034)
Implement evaluation * Subjective * Trend			-0.003
			(0.029)
Implement evaluation * Trend			0.015
			(0.023)
Time trend			-0.003
			(0.015)
School composition controls		X	X
Grade-year observations (N)	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135
R-squared	0.574	0.574	0.574

* $p < .05$, ** $p < .01$, *** $p < .001$. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school, year, classroom/subjective, classroom/subjective-by-school and classroom/subjective-by-year fixed effects and are weighted by grade enrollment. Double-difference models available in Table 2 (class and subjective) and Table A7 (other and objective).

Table A13. Triple-difference estimates of the effects of teacher evaluation reforms on Office Disciplinary Referrals, grades 3-11 only

	I	II	III
Panel A. Class and Other Locations			
Implement evaluation * classroom	-0.079	-0.081	-0.052
	(0.059)	(0.059)	(0.055)
Implement evaluation	-0.014	-0.016	-0.034
	(0.031)	(0.031)	(0.048)
Implement evaluation * classroom * Trend			-0.046
			(0.028)
Implement evaluation * Trend			0.104*
			(0.045)
Time trend			-0.024
			(0.025)
School composition controls		X	X
Grade-year observations (N)	64,431	64,431	64,431
School-year observations	19,630	19,630	19,630
R-squared	0.585	0.586	0.586
	IV	V	VI
Panel B. Subjective and Objective Reasons			
Implement evaluation * Subjective	-0.038	-0.039	-0.041
	(0.065)	(0.065)	(0.050)
Implement evaluation	-0.015	-0.016	-0.019
	(0.035)	(0.035)	(0.039)
Implement evaluation * Subjective * Trend			0.003
			(0.038)
Implement evaluation * Trend			0.012
			(0.028)
Time trend			-0.003
			(0.015)
School composition controls		X	X
Grade-year observations (N)	64,431	64,431	64,431
School-year observations	20,135	20,135	20,135
R-squared	0.609	0.609	0.609

* p < .05, ** p < .01, *** p < .001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school, year, classroom/subjective, classroom/subjective-by-school and classroom/subjective-by-year fixed effects and are weighted by grade enrollment. Double-difference models available in Tables 3 (class and subjective, grades 3-11) and Table A9 (other and objective, grades 3-11)

Table A14. The effects of teacher evaluation, other accountability, and discipline policy reforms on ODRs, by location and subjectivity

	Classroom ODRs				Subjective ODRs			
	Separate Models	Joint Account.	Joint discipline	Full joint model	Separate Models	Joint Account.	Joint discipline	Full joint model
	I	II	III	IV	V	VI	VII	VIII
Implement evaluation	-0.089	-0.091	-0.101	-0.104	-0.042	-0.047	-0.045	-0.051
	(0.064)	(0.067)	(0.067)	(0.072)	(0.051)	(0.053)	(0.045)	(0.048)
Eliminate tenure	-0.315	-0.316		-0.306	-0.121	-0.124		-0.105
	(0.205)	(0.204)		(0.205)	(0.153)	(0.154)		(0.156)
Weaken collective bargaining	0.105	0.134		0.156	0.193	0.207		0.212
	(0.385)	(0.373)		(0.370)	(0.248)	(0.241)		(0.235)
Alter teacher authority to remove student from class	0.110		0.119	0.109	0.070		0.094	0.094
	(0.195)		(0.199)	(0.195)	(0.144)		(0.153)	(0.151)
Alter limits to suspension/expulsion	0.032		0.018	0.03	-0.029		-0.044	-0.037
	(0.095)		(0.077)	(0.078)	(0.065)		(0.049)	(0.052)
School composition controls	X	X	X	X	X	X	X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135	20,135	20,135	20,135

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment. Coefficients on Suspension Limit models that use the year of discipline reform closest to evaluation reform are 0.004 (0.094) and -0.041 (0.063) for class and subjective ODRs, respectively.

Table A15. The effects of teacher evaluation, other accountability and discipline policy on ODRs, by location and subjectivity (grades 3-11 only)

	Classroom ODRs (3-11 only)				Subjective ODRs (3-11 only)			
	Separate Models	Joint Account.	Joint discipline	Full joint model	Separate Models	Joint Account.	Joint discipline	Full joint model
	I	II	III	IV	V	VI	VII	VIII
Implement evaluation	-0.098	-0.100	-0.107	-0.109	-0.054	-0.058	-0.054	-0.059
	(0.068)	(0.072)	(0.072)	(0.076)	(0.059)	(0.061)	(0.049)	(0.052)
Eliminate tenure	-0.342	-0.341		-0.332	-0.13	-0.133		-0.108
	(0.251)	(0.251)		(0.255)	(0.189)	(0.191)		(0.196)
Weaken collective bargaining	0.077	0.109		0.126	0.189	0.206		0.203
	(0.446)	(0.431)		(0.429)	(0.284)	(0.275)		(0.269)
Alter teacher authority to remove student from class	0.085		0.100	0.089	0.061		0.099	0.099
	(0.194)		(0.205)	(0.201)	(0.154)		(0.171)	(0.169)
Alter limits to suspension/exclusion	0.014		0.005	0.017	-0.064		-0.079	-0.072
	(0.104)		(0.097)	(0.097)	(0.071)		(0.060)	(0.062)
School composition controls	X	X	X	X	X	X	X	X
Grade-year observations (N)	64,431	64,431	64,431	64,431	64,431	64,431	64,431	64,431
School-year observations	19,630	19,630	19,630	19,630	19,630	19,630	19,630	19,630

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment. Coefficients on Suspension Limit models that use the year of discipline reform closest to evaluation reform are -0.025 (0.105) and -0.082 (0.069) for class and subjective ODRs, respectively.

Table A16. Unweighted OLS estimates of the effect of teacher evaluation reforms on Office Disciplinary Referrals, by location and subjectivity

	A. Class			B. Subjective		
	I	II	III	IV	V	VI
Implement evaluation	-0.045	-0.048	-0.032	-0.025	-0.026	-0.02
	(0.072)	(0.073)	(0.088)	(0.054)	(0.055)	(0.060)
Implement evaluation * Trend			0.038			0.006
			(0.039)			(0.026)
Time trend			-0.019			-0.005
			(0.032)			(0.024)
School composition controls		X	X		X	X
Grade-year observations (N)	107,458	107,458	107,458	107,458	107,458	107,458
School-year observations	20,135	20,135	20,135	20,135	20,135	20,135
R-squared	0.497	0.497	0.497	0.49	0.49	0.49

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects.

Table A17. Alternate specifications testing robustness to alternate samples, by location and subjectivity (grades 3-11 only)

	Class, 3-11		Subject, 3-11	
	I	II	III	IV
	Ever eval	Balanced panel	Ever eval	Balanced panel
Implement evaluation	-0.071	-0.121	-0.087*	-0.043
	(0.069)	(0.064)	(0.041)	(0.051)
School composition controls	X	X	X	X
Grade-year observations (N)	49,582	50,091	49,582	50,091
School-year observations	15,392	15,207	15,392	15,207
R-squared	0.583	0.621	0.558	0.603

Notes: *p<.05, **p<.01, ***p<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity. All models include grade, school and year fixed-effects and are weighted by grade enrollment.

Table A18. Alternate specification tests of the moderating effect of Positive Behavioral Interventions and Supports (PBIS) on the effect of teacher evaluation reforms on Office Disciplinary Referrals, by location and subjectivity

	Class		Subjective	
	Ever eval	Balanced Panel	Ever eval	Balanced Panel
Implement evaluation	-0.129	-0.076	-0.135*	-0.061
	(0.107)	(0.173)	(0.060)	(0.109)
Implement PBIS well	0.044	0.018	0.033	0.012
	(0.101)	(0.179)	(0.063)	(0.097)
Implement evaluation * PBIS	-0.122	-0.114	-0.099	-0.081
	(0.083)	(0.059)	(0.062)	(0.041)
School composition controls	X	X	X	X
Grade-year observations (N)	49,709	52,536	49,709	52,536
School-year observations	9,418	9,762	9,418	9,762
R-squared	0.594	0.623	0.558	0.608

* p < .05, ** p < .01, *** p < .001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity and are weighted by grade enrollment. Ever eval are restricted to schools that experienced high-stakes evaluation. Balanced panel are restricted to school-year observations 5 years before through 1 year after introduction of high-stakes evaluation or never experienced it.

Table A19. Alternate specification tests of the moderating effect of Positive Behavioral Interventions and Supports (PBIS) on the effect of teacher evaluation reforms on Office Disciplinary Referrals, by location and subjectivity (grades 3-11 only)

	Class, 3-11		Subj, 3-11	
	I	II	III	IV
	Ever eval	Balanced panel	Ever eval	Balanced panel
Implement evaluation	-0.110	-0.077	-0.159*	-0.075
	(0.136)	(0.185)	(0.077)	(0.116)
Implement PBIS well	0.054	0.048	0.047	0.047
	(0.136)	(0.201)	(0.088)	(0.110)
Implement evaluation * PBIS	-0.129	-0.142	-0.115	-0.108
	(0.108)	(0.079)	(0.080)	(0.054)
School composition controls	X	X	X	X
Grade-year observations (N)	29,678	31,560	29,678	31,560
School-year observations	9,195	9,535	9,195	9,535
R-squared	0.63	0.653	0.592	0.634

* p < .05, ** p < .01, *** p < .001. Cells report estimates and associated standard errors clustered at the state level in parentheses. School controls include time-varying enrollment, proportion low-income and race/ethnicity and are weighted by grade enrollment. Ever eval are restricted to schools that experienced high-stakes evaluation. Balanced panel are restricted to school-year observations 5 years before through 1 year after introduction of high-stakes evaluation or never experienced it.

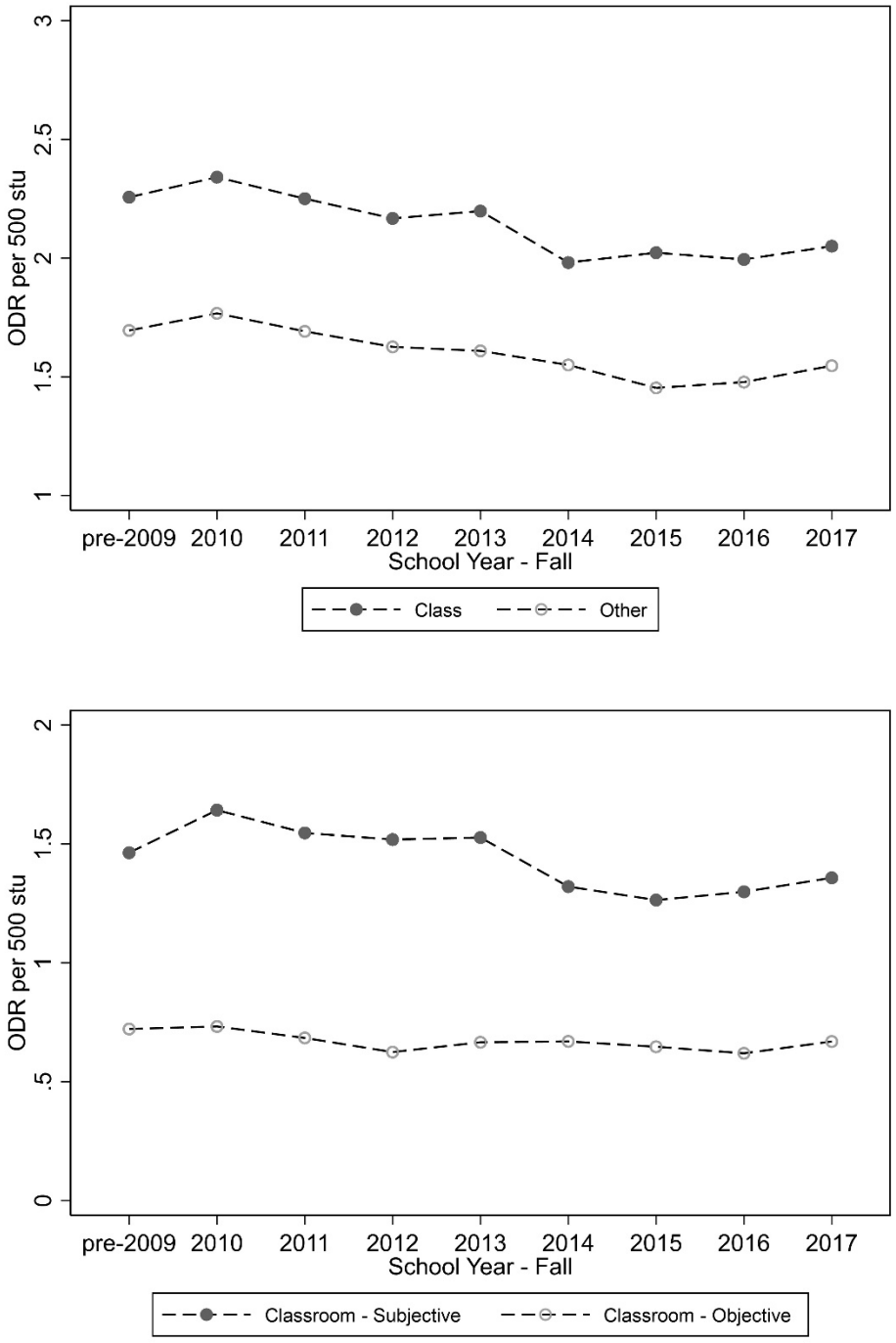
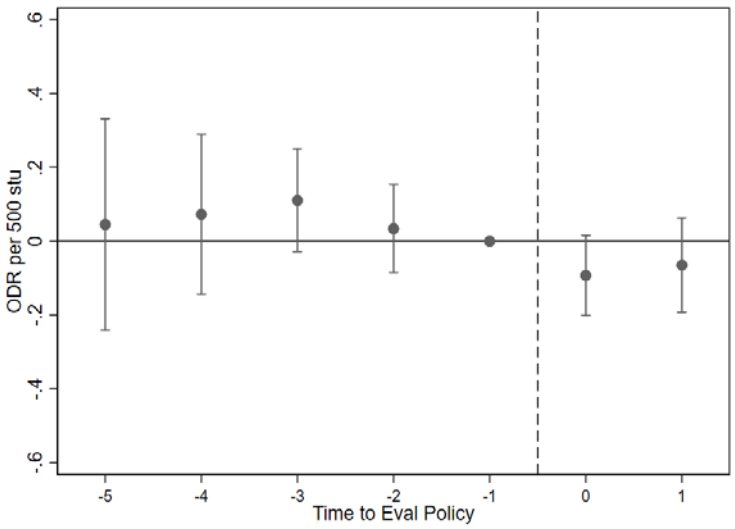
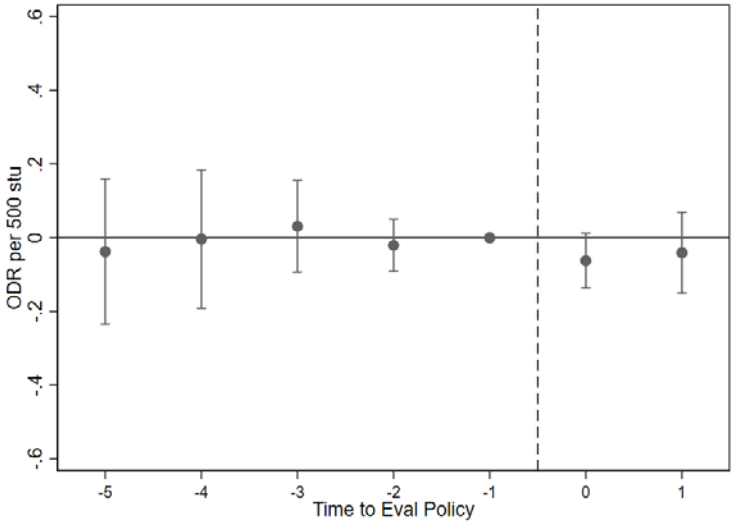


Figure A1. Office Disciplinary Referral (ODR) trends for states that never experienced evaluation reform, by location and type of referral



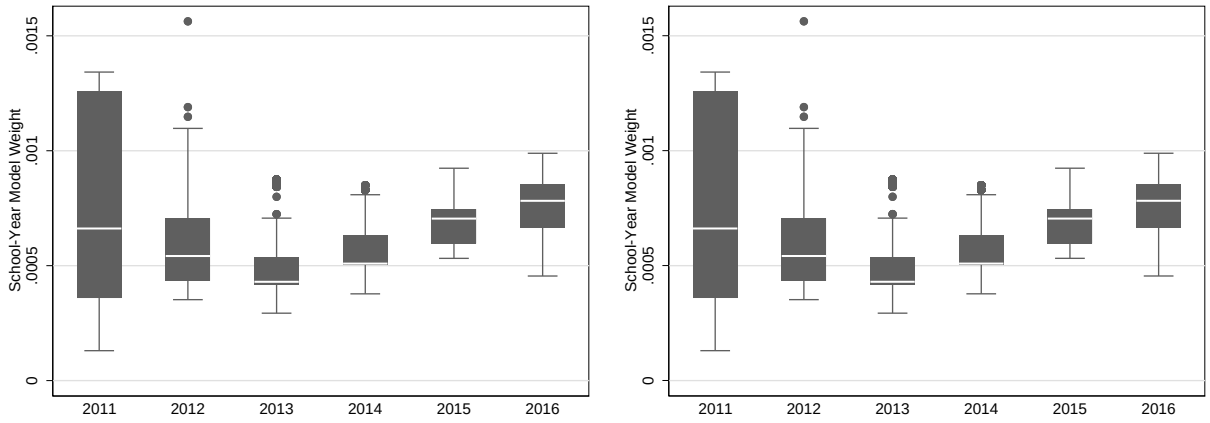
Panel A. Classroom ODRs



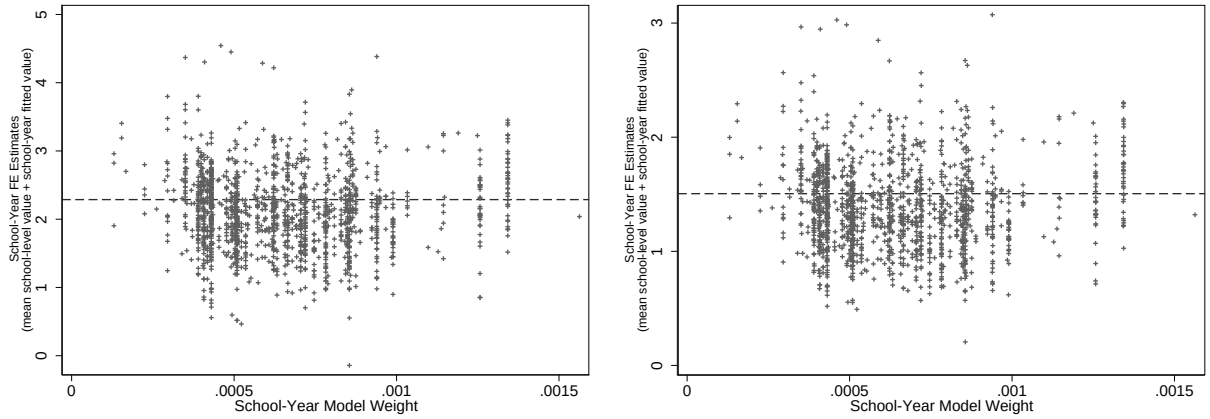
Panel B. Subjective Classroom ODRs

Figure A2. Non-parametric event study displaying effect of high-stakes teacher evaluation reforms on rate per-500-student, per-day Office Disciplinary Referrals (ODRs) in grades 3-11, by location and subjectivity

Notes: point estimates for years pre- and post-evaluation reforms and corresponding 95 percent confidence intervals derived from event study model describe in Equation 1 that is weighted by school size, includes grade, school and year fixed effects and time-varying school characteristics, with standard errors clustered at state level. Full coefficients reported in Models IIa and IIc of Appendix Table A3.



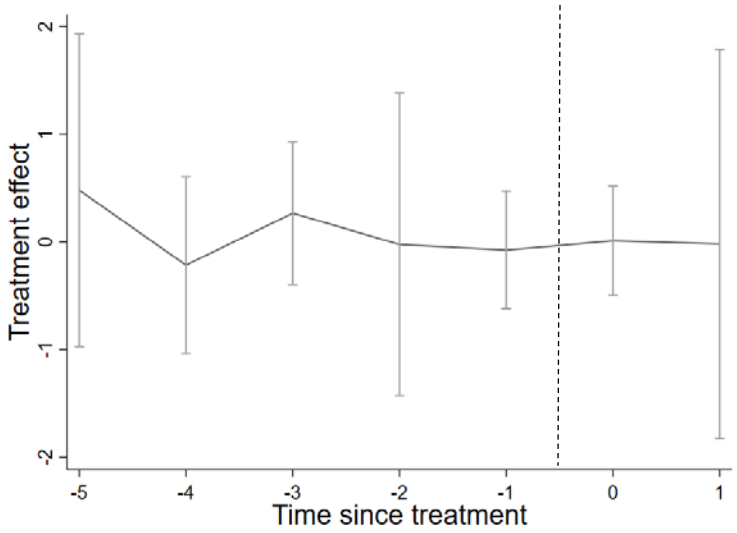
Panel A. Fixed effect weights by year of teacher evaluation implementation



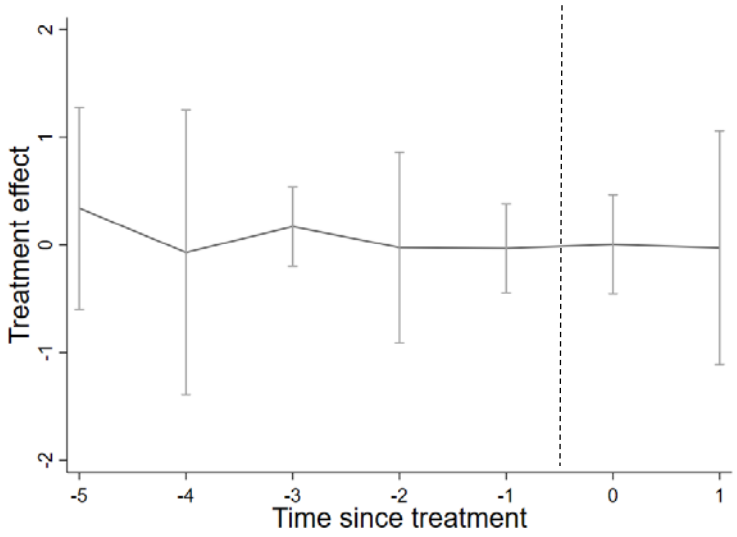
Panel B. School year fixed effect fitted values by fixed effect weight

Figure A3. Tests of difference-in-difference fixed effect weights variability by year and size of treatment effect

Notes: Dotted lines in Panel B are the unweighted, school-level sample fitted value of the effect of evaluation implementation, combining the mean ODR rate and the average treatment effect (2.29 for class and 1.51 for subjective)



Panel A. Class



Panel B. Subjective

Figure A4. Difference-in-difference estimates with Wald-TC estimand with prior-year placebos and post-implementation trend

Appendix B. Data Description

B.1 SWIS PBIS Sample and Data

The School-wide Information System (SWIS) data on Positive Behavioral Interventions and Supports come from the Education and Community Supports research unit of the University of Oregon (Bragg, 2019). A key strategy to improve behavioral supports in schools implementing PBIS is to track behavioral data. As such, each behavioral incident that prompts a student to be referred to an administrator responsible for addressing misbehavior is recorded. Approximately 25,000 schools in the 2016-17 school year were attempting to implement PBIS in some form. About 11,000 of these schools used the SWIS data management system and 5,745 of these schools agreed to have their data used for research purposes (Hoselton, 2018).

We begin by restricting our sample to observations with valid data that were subject to the policies of interest. Due to data inconsistencies in which schools sharing the same ID appear in different states, we drop 27 school-year observations. We then restrict our sample to school-years appearing in and after the 2006-07 school year. At this point, we have 75,066 school-year observations. We exclude all Alternative and Juvenile Justice schools as they have different governing regulations, resulting in dropping 5,105 school-year observations. We also drop 103 school-year observations that are exclusively pre-schools as are may not subject to the same evaluation policy. For similar reasons, we exclude Bureau of Indian Affairs (134 school-year observations), schools in Guam and the Virgin Islands (172 school-year observations), and charter schools (924 school-year observations).

We then require that we observe schools in states that experience evaluation reform for four years prior to the adoption of high-stakes teacher evaluation and one year following the initial policy implementation. For school in states that never experience evaluation, we require that we observe them four times between 2006 and 2017. This substantially reduces our sample to 23,538 school year observations.

The SWIS data include enrollment data from the school's NCES Common Core Data (CCD) record in most cases, but some schools do not report this October 1 count data and instead report a local tally of students. In the case of schools with highly transient populations, the October 1 count data in the CCD may represent enrollment of differences of 20 percent or more than a February 1 count. We attempt to capture the most complete enrollment data. We use the CCD enrollment data or the SWIS-reported enrollment when only one of the two is available. Otherwise, we use the average of the two. 1,512 school-year observations are either missing all enrollment data or have enrollment below 20 students and we exclude these.

We describe our imputations of race/ethnicity and low-income measures in the notes to Table 1. Even after these imputations, we still have 40 school-year observations without race/ethnicity data and 227 school-year observations without free- or reduced-price lunch (FRPL) data. We exclude these observations.

We then limit our sample to school-years which have recorded Office Disciplinary Referrals (ODRs) originating in the classroom or other locations, and are subjective or objective in nature. We restrict our sample to schools that have measured outcomes in all four of these areas. Excluding these school-years missing one or more outcomes results in a further reduction of our sample of 1,059 school-years. This results in a school-level sample that includes 20,521 school-year observations. This is our sample for our school-level estimates in Appendix Table A10.

From this school-level sample, we further identify our main grade-school-year analytic sample. We restrict from our sample any school-year observation that does not have an outcome recorded for all of the four primary outcomes recorded at the grade level. This results in the exclusion of an additional 384 school-year observations and results in a main sample of 20,135 school-years. Embedded in these school-year observations are 107,458 grade-school-year observations. We re-shape our data to use these grade-school-year observations as our primary analytic units.

We categorize subjective and objective behaviors data following Greflund et al. (2014):

> "Subjective behaviors were defined as behaviors that require not simply observing a discrete, objective event (e.g., a student smoking), but a significant value judgment regarding whether the intensity or quality of the behavior warrants an ODR (e.g., a student using inappropriate language). The average inter-rater agreement among the expert panel for all 24 problem behaviors was 90%. The following behaviors were categorized as subjective: abusive language/inappropriate language/profanity, defiance/disrespect/insubordination/ non-compliance, harassment/bullying, disruption, dress code violation, and inappropriate display of affection. The following behaviors were categorized as less subjective: physical aggression/fighting, tardy, skipping, truancy, property damage/vandalism, forgery/theft, inappropriate location/out of bounds, use/possession of tobacco, alcohol, drugs, combustibles, weapons, bomb threat/false alarm, and arson. Three problem behaviors did not meet the inter-rater reliability criterion and were also not classified as subjective: lying/cheating, technology violation, and gang affiliation display." (2014, pp. 220–221)

Our outcome measures are the grade-school-year count of a particular category of ODR, divided by the total grade enrollment, divided by the number of school days in the year for that school. Then, we scale the outcome to the approximate average school size in our sample by multiplying this ratio by 500. As we discuss in the notes to Table 1, due to a small number of outlying values, we cap all outcome measures above the 99th percentile to the value of the 99th percentile.

We construct school-level measures of racial composition and the proportion of low-income students (measured by their receipt of free- or reduced-price lunch). These measures are constructed by dividing enrollment by race by total enrollment. All models adjusting for racial composition include the following racial/ethnic group percentages from the CCD and school reports: American Indian/Native Alaskan, Asian/Pacific Islander, Black, Hispanic and White, Non-Hispanic. As we discuss in the notes to Table 1, we observe a small number of school-year observations in which a particular demographic group represents more than the total school

enrollment. These instances generally arise when total and sub-group enrollment figures differentially reflect in- and out-flows of students over the school year. We cap these values at 1. This affects 563 school-year observations.

We assign grade-school-year observations a value of 1 for PBIS implementation in years in which they meet self- and externally assessed criteria for successful implementation. To be classified as successfully implementing PBIS, schools had to meet one of the following thresholds: School-wide Evaluation Tool (SET): greater than or equal to 80 percent of expectations taught and overall implementation; Tiered Fidelity Inventory (TFI): Tier 1 ratio greater than or equal to 70 percent; Benchmark of Quality (BOQ: Total Ratio greater than or equal to 70 percent; Self-Assessment Survey (SAS): Implementation Average greater than or equal to 80; and Team Implementation Checklist (TIC): Implementation Average greater than or equal to 80 percent. We do not use the continuous implementation scores as they represent substantially different scales and are not linked across instruments (Greflund et al., 2014; Mercer, McIntosh, & Hoselton, 2017).

B.2 CRDC Data

As a placebo test of our main results, we estimate models using restricted-use Civil Rights Data Collection (CRDC) data set. CRDC data are collected biennially (for the most part) by the U.S. Department of Education, dating back to 1968. These data are primarily focused on civil rights issues such as discipline, bullying, and access for students with disabilities (U.S. Department of Education, 2018). In this study, we draw on five waves of data, from the 2005-2006 school year to the most recently-collected 2015-2016 school year. Within these waves of data, the U.S. Department of Education altered who was included in the sample. For the 2005-2006 and 2009-2010 waves, data were collected from a large, nationally representative sample of public schools and districts, while for the following three waves of data (2011-2012, 2013-2014, 2015-2016), data were collected from every public school and district in the country. These data do not include the last year of evaluation implementation covering schools in six states that implemented evaluation reforms in 2016-17. Thus, we re-estimate our results in our main sample restricting observations to only those appearing prior to 2016-17. As these results are consistent, we feel confident concluding that the CRDC sample provides an informative check for our main estimates.

To prepare the data, we first merge the school-level out-of-school suspension counts, enrollment, demographic, and Title I status data from all five waves of data to create a longitudinal dataset. There are differences in how the CRDC collected demographic data across the different waves, moving from five distinct racial/ethnic categories to seven. In order to create stable categories across the waves, we create five categories, Asian-American/Hawaiian students, African-American students, Hispanic students, White non-Hispanic students, and an Other category that includes American Indian and Multi-racial students. We impute missing enrollment values using the average enrollment values from the prior and proceeding wave of data, or in the case of the initial wave the two waves after, and the final wave the two waves prior. We use the same imputation approach for demographic enrollment values.

We then calculate the suspension rate by dividing the total number of students suspended by the schools' total enrollment. Importantly, the out-of-school suspension rate measures the total number

of students who were suspended throughout the year, not the total number of suspensions throughout the year. Therefore, the measure does not account for the additional number of suspensions that occurred if a student was suspended more than once in the school year. We do not include in-school suspensions nor expulsions in these analyses. We cap all suspension rates at 1, as there were rare cases in which schools reported a total number of students suspended that was larger than the school enrollment, which accounted for 1,435 observations. This has been previously documented in CRDC data (Losen & Gillespie, 2012), and may reflect that enrollment values are measured in Fall, while the count of students suspended is measured at the end of the school year, creating the opportunity to have more students suspended over the year's course than the initial Fall enrollment count.

The CRDC data collection does not have a measure of school or district percent of students who qualify for Free or Reduced Price Lunch, so as a measure of socioeconomic status we use whether or not a school qualified as a Title I school. Values are not available for the 2005-2006 school year, so we impute Title I status based on the following wave, or in the case of schools that did not have a 2009-2010 value, the 2011-2012 value. We also impute missing values for following waves, using the maximum (0/1) from the prior and proceeding waves.

Aligned with how we prepare the referral data, we exclude schools according to a number of criteria. For schools in states that did implement high-stakes teacher evaluation policies, we retain only schools for which we have an observation prior to, and after, the evaluation implementation. For schools in states that did not implement high-stakes teacher evaluation policies, we retain only those with two or more observations. This results in an exclusion of 104,770 observations, a product of the fact that states which implemented evaluation in 2016 do not have observations after evaluation implementation, as well as other factors, such as schools closing or providing incomplete data across years. We further exclude Alternative, Juvenile Justice, and Charter schools, for a total of 19,135 observations excluded. We also exclude schools that offered only pre-K (another 3,460 observations), as well as schools with enrollment below 20 students (another 3,640 observations). We also, after imputation, exclude three observations for which we are unable to impute total enrolment, another 6,335 observations for which we ae unable to impute Title I status, and 380 that have missing suspension rates. Note that the preceding description of the number of observations excluded will not align precisely with the total CRDC observations and those in our sample due to the fact that we round all reported values to the nearest 5 per Institute for Education Sciences requirements.

Table B1. CRDC descriptive statistics

	Full Sample	Never Under Evaluation	Ever Under Evaluation
Total Schools	65,315	11,730	53,580
School-Year Observations	284,460	50,165	234,295
Total Districts	9,460	1,880	7,580
Average School Enrollment	591.08	623.91	584.05
(SD)	(443.48)	(537.75)	(420.23)
Racial/Ethnic Composition			
% Asian/Hawaiian	0.06	0.11	0.04
(SD)	(0.10)	(0.15)	(0.08)
% African American	0.16	0.06	0.18
(SD)	(0.22)	(0.09)	(0.24)
% Hispanic	0.21	0.46	0.16
(SD)	(0.25)	(0.31)	(0.20)
% White	0.54	0.33	0.59
(SD)	(0.32	(0.30)	(0.30)
% Other	0.03	0.03	0.03
(SD)	(0.06)	(0.06)	(0.07)
Average Suspension Rate	0.06	0.05	0.06
(SD)	(0.09)	(0.07)	(0.09)

Notes: Standard deviations, where applicable, in parentheses. 25 school-year observations have total enrollment data imputed, 175 values were imputed for race/ethnicity, and 1,435 school-year observations have suspension rates capped at 1. Data obtained from Civil Rights Data Collection. All sample sizes (schools, school-year and district observations) rounded to nearest 5.

B.3 Teacher Evaluation and Accountability Policy Reform Data

We draw all data on teacher evaluation and accountability policies from Kraft et al. (2019) and refer our readers to their paper for details on this data collection process

B.4 Concurrent Discipline Policy Change Data

We compile data on concurrent discipline policy changes from the Compendium of School Discipline Laws and Regulations (Bezinque et al., 2018) on whether any state-level policies related to *Teacher authority to remove students from the classroom* and *Limitations, conditions, or exclusions for use of suspension and expulsion* were enacted between 2006 and 2018. We identify all relevant statute and regulation for each of these two categories in the online compendium:

https://safesupportivelearning.ed.gov/school-discipline-compendium

We then review each associated section of the state statute/regulation to identify any dates of revisions during the 2006-2018 window. Our default approach is to code any change in policy even minor ones; however, there are some instances when the language of the statute was revised to reflect the renaming of an agency or other minor shift. We exclude these from our reform tallies. We also exclude changes that focused exclusively on discipline policy for students with disabilities. In Table B2, we list the substance of these reforms with direct links to the statute. We code all of these policy changes based on the first fall of the school year under which the policy was implemented. There are six states that include schools in our sample that implemented multiple changes to limit suspension or expulsion (LA, MD, OH, RI, TN and TX). In our main robustness checks, we use the first observed policy change. We also test using the year closest to the implementation of evaluation reforms.

Table B2. Content of discipline policy reforms

	Tchr auth to remove	Limit suspension	Description (teacher authority)	Description (limit suspension/expulsion)
Alabama				
Alaska				
Arizona				
Arkansas				
California		2014		Limits on cause of suspension and age
Colorado		2012		Numerous changes to suspension reasons, disruptions/removal, and requiring that LEAs craft conduct and discipline codes
Connecticut	2018	2018	"Act Concerning Classroom Safety and Disruptive Behavior"	"Act Concerning Classroom Safety and Disruptive Behavior" focuses on reducing punitive/exclusionary discipline, which changed policy around both classroom removal and suspension/exclusion
Delaware		2018		Require LEA discipline reports and improvement plans (based on restorative justice) for schools under certain thresholds
Distr. of Columbia	**2009**	**2009**; 2018	Outline tiers of behavior for classroom discipline action and the accompanying discipline responses	2009: tiers include certain behaviors that cannot result in a suspension, such as absence ; 2018: limit length of suspension and who can be suspended

Florida		2009; 2018		2009: Revise zero tolerance policy to define out low-level offenses; 2018: provide alternatives to suspension and referral to law enforcement
Georgia		**2014**		Zero tolerance policy is only applicable to firearms, also clarifies the ability of local education boards to modify discipline policy for those who violate zero tolerance policy
Hawaii		2009		Interventions as alternatives to suspension required; limit on suspension due to truancy
Idaho				
Illinois		**2016**		Requires school officials to limit the number and duration of expulsions/suspensions, disallows zero-tolerance policies, and other requirements around how OSS may be used
Indiana	2009		Teacher Protection Act of 2009 had protections for teachers' disciplinary actions	
Iowa				
Kansas				Update to law was to reflect change in name from secretary of social and rehabilitation services to children and families
Kentucky				Changes to the suspension and expulsion policy defines what constitutes a threat, does not change grounds for suspension

Louisiana	2009	2007; 2008; 2009; **2012**; 2015	2009 permits principal to counsel alternatives to class removal; provide makeup work	Changes in suspension/exclusion policy from 2007-08 increase penalty for behavioral infractions. 2009: requires makeup work during suspension; 2012: requires alt education during suspension, adds provisions for bullying; 2015: prohibits suspension in K-5 for uniform violation;
Maine				
Maryland	2009	2014; 2017	Amend teachers' use of exclusion	2014: Require revisions of local student discipline policies to reflect a number of elements, including positive behavioral supports,; 2017: prohibit Prek-2nd grade suspensions/expulsions (w/specific exceptions)
Massachusetts				
Michigan		2017		"Rethink Discipline" law limits expulsion, requires consideration of alternatives to suspension, sets presumption that suspension longer than 10 days NOT justified
Minnesota	2016		Add in language that teachers may "may remove students from class under section 121A.61, subdivision 2, for violent or disruptive conduct."	
Mississippi				
Missouri				
Montana				Defines term of expulsion as 20+ days in 2009; requires annual review of policies in 2013, no substantive changes
Nebraska				
Nevada		**2015**		Outlines the circumstances under which students can be suspended/expelled for different firearm/weapon incidents
New Hampshire				Makes assignments available to students during period suspended

New Jersey	2012	2016	Harassment, intimidation, bullying grounds for removal from classroom	Limited suspension for K-2 students
New Mexico	2009		Revise procedure teachers go through for detention, suspension and expulsions.	
New York				
North Carolina		**2011**		Changes in who has authority to assign long-term suspensions and what services/opportunities are provided to those suspended
North Dakota				
Ohio		2017; 2018		2017: cannot be suspended for truancy; 2018: Limiting the use of out-of-school suspensions and expulsions for pre-K-third graders, money to support alternative discipline approaches
Oklahoma				
Oregon	2014	2014	Revise code to require LEAs to plan for reducing exclusionary discipline use	Major changes to discipline policy, focused on reducing suspensions and expulsions
Pennsylvania				
Rhode Island		2007; 2009; **2012**		2007: Changes to weapons/alcohol policy; 2009: adopt the "1.3 Safe, Healthy, and Supportive Learning Environment" policy ; 2012: Can no longer suspend students for truancy,
South Carolina				
South Dakota		2014		Adds phrase "No local school board may impose a lesser consequence than those established in § 13-32-9, but a local school district may adopt a policy (…) with more strict consequences to meet the needs of the district"

Tennessee		2007; 2008; 2013; 2015; 2018		2007 closed suspension hearing, fight=suspension, defines threat; 2008 discipline data reporting required; 2013 allows for self-defense, adjusts language around assault of staff leading to suspension; 2015 allows consequences for off-school ground behavior; 2018 specifies zero tolerance
Texas	2015	2011; 2017	Lists the campus behavior coordinator as a person to whom teachers can send students after student removal from classroom	2017: Limit grade of suspension for certain infractions, allowance for positive behavioral programs; 2011: outline what "serious misbehaviors" warrant expulsion
Utah				
Vermont		2011		Allows suspension for off-school events
Virginia		2009; 2018		2009: no suspension for truancy, 2018: limit suspensions for students grade 3 and below and outline time length limits on long-term suspensions
Washington		2016		Limits on length of suspension and use of suspension outside explicit circumstances
West Virginia		2014		Add section on weapon/substance possession procedures
Wisconsin				
Wyoming				