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EdWorkingPaper No. 25-1319

Shock Absorption: Did School Turnaround Shelter Schools from the Pandemic's Effects on Teacher Turnover?

Erica Harbatkin
Florida State University

Rebecca Hinze-Pifer
University of Illinois Urbana-Champaign

Katharine O. Strunk
University of Pennsylvania

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VERSION: October 2025

Suggested citation: Harbatkin, Erica, Rebecca Hinze-Pifer, and Katharine O. Strunk. (2025). Shock Absorption: Did School Turnaround Shelter Schools from the Pandemic's Effects on Teacher Turnover?. (EdWorkingPaper: 25-1319). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/3bcc-c477

Shock Absorption: Did School Turnaround Shelter Schools from the Pandemic's Effects on Teacher Turnover?

Erica Harbatkin,[1] Rebecca Hinze-Pifer,[2] & Katharine O. Strunk[3]

[1] Florida State University, [2] University of Illinois Urbana-Champaign,

[3]University of Pennsylvania

Abstract: Successful turnaround interventions should build school capacity to promote not just school improvement but also resilience to exogenous shocks that undermine schooling. While a large literature demonstrates that turnaround can improve school outcomes, little is known about whether it can help schools withstand negative shocks. We examine the impact of Michigan's turnaround policy on teacher turnover before and during the COVID-19 pandemic. Event study findings show limited immediate effects of the policy on teacher turnover, but significant and substantively large decreases in turnover after the pandemic's onset. Teacher survey data indicate that working conditions improvements beginning before the pandemic may have translated into increased retention after its onset, suggesting enhanced resilience in the teacher workforce stemming from the policy's impacts.

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School turnaround models seek to target the lowest performing schools and districts with intensive supports intended to rapidly improve student outcomes. These turnaround initiatives rely heavily on a stable, highly effective teacher workforce to impel and sustain improvement efforts (Harbatkin, 2022; Henry et al., 2020; Henry & Harbatkin, 2019; Malen & Rice, 2016; Pham et al., 2024; Schueler et al., 2017; Strunk, Marsh, Hashim, Bush-Mecenas, et al., 2016). Recognizing that workforce instability can undermine school turnaround processes, many turnaround models include systematic efforts to build school and district capacity to recruit and retain teachers (Camp et al., 2024; Hopkins & Woulfin, 2015; Papay et al., 2021; Peurach & Neumerski, 2015; Pham et al., 2024; Zimmer et al., 2017). Such efforts became especially salient during and after the onset of the COVID-19 pandemic.

Just as the pandemic wrought havoc on the overall workforce across the United States (Albanesi & Kim, 2021; Landivar et al., 2020), it also shifted K-12 education labor market patterns. Available evidence suggests that during the first year of the pandemic, teachers were *less* likely to leave their jobs than in previous years (Bacher-Hicks et al., 2023; Bastian & Fuller, 2023; Goldhaber & Theobald, 2023; Harbatkin et al., 2025; Hopkins et al., 2023). However, as the uncertainty and economic shock of the early pandemic resolved, teacher attrition underwent a correction, worsening for a year or two and then often stabilizing to pre-pandemic levels or slightly lower (Bastian & Fuller, 2023; Camp et al., 2024; Goldhaber & Theobald, 2023; Harbatkin, Nguyen, et al., 2025; Rogers et al., 2025).

These pandemic-induced shifts in the teacher labor market provide an opportunity to understand the extent to which turnaround efforts may help districts build internal capacity for teacher retention in a context of exogenous challenges. While such exogenous shocks are often

hard to observe in administrative data and their impacts can be challenging to measure, they are a frequent reality for public schools. For instance, natural disasters, recessions, and policy-induced funding cuts can disrupt operations, especially in low-performing schools. One result among many can be elevated rates of teacher turnover—already a significant challenge in low-performing schools, which have historically grappled with high rates of teacher turnover, particularly due to teachers switching to other schools (Boyd et al., 2013; Clotfelter et al., 2007; Fuchsman et al., 2022; Guin, 2004; Ingersoll, 2001). To the extent that the pandemic initially reduced vacancies in schools to which teachers might move, one would expect the pattern to be more extreme in low-performing schools: larger initial improvements in retention, followed by a worse correction. However, if turnaround successfully built organizational capacity to withstand negative shocks, one would expect an attenuated or absent correction in turnaround schools.

We examine this proposition through patterns of teacher turnover before and throughout the pandemic in schools served through the Partnership Model of School and District Turnaround, a statewide intervention targeting Michigan's lowest performing schools. Because Michigan began serving Partnership schools before the pandemic's onset, we are able to disentangle the intervention's effects from those of the pandemic and observe any mitigating effect of the supports for turnaround schools relative to other low-performing schools. Then, drawing on educator survey data collected both before and during the pandemic, we unpack the mechanisms that may have contributed to changes in teacher turnover. Specifically, we ask:

1) To what extent did the Partnership Model impact the probability that a teacher will leave the school, district, and Michigan public education entirely, and how did this change during the pandemic?
2) What mechanisms might explain observed turnover patterns in Partnership schools?

We find that most of the Partnership Model's positive impact on teacher retention occurred after the pandemic's onset. After the pandemic's onset, turnover from the school decreased immediately in Cohort 1, continued to fall in the second pandemic year, and the lower turnover persisted into the third. In Cohort 2, turnover from the school decreased in the second pandemic year and remained lower into the third. Our findings do not suggest that the pandemic-era improvements were driven by lower mobility rates statewide; instead, the turnaround processes that began before the pandemic appear to have shored up the educational infrastructure of Partnership schools in ways that helped them better withstand the shock of the pandemic—though only to a point. The second implementation cohort grappled with particularly salient challenges related to staffing, technology, and shifting instructional modalities that teachers felt heightened their workload and provoked them to leave. As these challenges attenuated in the second pandemic year, so too did teacher turnover. At the end of the intervention three years after the pandemic's onset, Partnership was reducing school turnover by about 10 percentage points in Cohort 1 and 6 percentage points in Cohort 2. These changes were driven by improved retention among more experienced teachers.

The remainder of this paper proceeds as follows. We begin with a conceptual framework and literature review that situates this study at the juncture of the school turnaround and teacher labor market literatures. Next, we review earlier findings regarding the implementation and effects of the Partnership Model prior to and during the COVID-19 pandemic. After a description of the data and methods, we present findings, including estimated impacts of Partnership on teacher turnover and suggestive evidence regarding mechanisms. We conclude with a brief summary of findings and discussion of policy implications.

Conceptual Framework and Literature Review

A variety of factors contribute to teachers' employment decisions, with local labor markets playing an especially important role. Of particular importance is the amount of churn in neighboring schools. This is because teachers who transfer from low-performing schools typically move to higher-performing, more well-resourced non-turnaround schools instead (Boyd et al., 2005, 2013; Horng, 2009; Loeb et al., 2005; Pham, 2023; Sun, 2018). Thus, to the extent that non-turnaround schools experienced lower turnover during the height of the pandemic (Harbatkin et al., 2025; Hopkins et al., 2023), teachers aiming to transfer away from turnaround schools would have fewer positions to select into.

Because teachers often transfer in pursuit of improved working conditions, turnover in challenging turnaround schools often depends on the existence of vacancies in better-resourced non-turnaround schools. Teachers also transfer *among* turnaround schools, meaning turnover in turnaround schools is dependent on anteceding turnover in both turnaround and non-turnaround schools. However, there was very little teacher mobility at the end of the 2019-20 school year as teachers avoided transferring during the height of the pandemic (Bacher-Hicks et al., 2023; Bastian & Fuller, 2023; Camp et al., 2024; Goldhaber & Theobald, 2023; Rogers et al., 2025). Thus, there were fewer positions for teachers in turnaround schools to transfer to. This decline in vacancies would reduce teacher turnover in turnaround schools, potentially misleadingly suggesting that the intervention yielded positive effects on teacher retention.

However, the reduction in turnover was short-lived. The pandemic led to declining morale, increased stress, and challenging working conditions, especially among less experienced teachers and teachers of color (Diliberti et al., 2021; Madigan & Kim, 2021; Pressley et al., 2021; Redding & Nguyen, 2024; Steiner & Woo, 2021), and turnover in general spiked in

subsequent years (Bastian & Fuller, 2023; Goldhaber & Theobald, 2023). Michigan in particular experienced increased attrition from the profession (Kilbride et al., 2023; Rogers et al., 2025), leaving even more vacancies that teachers in turnaround schools could select into. Thus, short-lived decreases in turnover in low-performing schools driven solely by fewer vacancies would be quickly replaced by amplified turnover as those teachers reshuffled into these positions.

Depending on the context, turnaround may have either exacerbated or attenuated the pandemic's deleterious effects on teacher morale. On one hand, pandemic teaching challenges may have been heightened in turnaround schools because they tend to be located in communities with high rates of poverty and minoritized populations, which experienced some of the pandemic's most harmful health and economic consequences (Abrams et al., 2022; Dawer & Woulfin, 2024; Harbatkin et al., 2023; Oberg et al., 2022). These challenges permeated the virtual walls of the school building as educators navigated a shift to online learning, increased student absenteeism, and changing district guidance and policies (Dawer & Woulfin, 2024; Harbatkin et al., 2023). Teachers in turnaround schools experienced the pandemic in the context of heightened accountability due to their school's low-performing designation, which is associated with greater teacher stress, reduced morale, and lower job satisfaction (Berryhill et al., 2009; Byrd-Blake et al., 2010; Finnigan & Gross, 2007; Harbatkin et al., 2024; Malen & Rice, 2004). Novice teachers missed out on the traditional first-year teacher experience, induction opportunities, and mentorship that may ease the transition into the classroom (Mecham et al., 2021), and may be especially important in turnaround contexts with an additional layer of accountability pressures.

On the other hand, a turnaround intervention could have absorbed some of the pandemic stressors if intervention activities helped to shore up the school's educational infrastructure before the pandemic's onset (Hill et al., 2023; Peurach & Neumerski, 2015; Redding & Nguyen,

2020). Specifically, school turnaround is intended as a whole-school reform model that involves transforming the systems underlying the school's low performance (Aladjem et al., 2010; Herman et al., 2008; Rhim & Redding, 2014). This transformation process involves a constellation of supports that are targeted to the school's unique needs (Bryk et al., 2010; Hill et al., 2023; Thompson et al., 2016; Zimmer et al., 2017). Depending on school needs, turnaround supports may have, for example, targeted and improved working conditions, built processes to foster more collaborative working relationships, created distributed leadership structures that spread responsibilities across staff, and improved school culture and climate. Prior research links each of these factors to greater teacher job satisfaction and retention (Erichsen & Reynolds, 2020; Harbatkin, Nguyen, et al., 2025; Ingersoll, 2001; Kraft et al., 2016), and indeed there is evidence that positive teacher working conditions may have had a protective effect on teacher morale during the pandemic (Kraft et al., 2021). Additionally, turnaround schools receive additional resources and funding—though this varies across interventions. Additional resources could have funded a variety of items that would improve teacher working conditions during the pandemic. For example, turnaround funding could have allowed districts to purchase new technology or internet access to facilitate online instruction; new data systems to streamline data-driven instruction efforts; or even to pay bonuses or higher salaries, though this latter category may not be sustainable after schools exit turnaround status (Hatch & Harbatkin, 2021; Kutash et al., 2010; Strunk et al., 2021; Vangronigen et al., 2022). Thus, to the extent that turnaround interventions successfully improved infrastructure and increased resources, these factors may have subsequently played a protective role during the pandemic.

While there is considerable evidence that school turnaround has the potential to improve school outcomes in conventional times (e.g., Redding & Nguyen, 2020; Schueler et al., 2021), it

is unclear whether those improvements could shelter turnaround schools from an external shock such as the pandemic. In particular, given the extant evidence that turnaround often exacerbates teacher turnover and staffing challenges (Harbatkin et al., 2024; Heissel & Ladd, 2018; Henry et al., 2020; Henry & Harbatkin, 2020; Malen & Rice, 2016; Strunk, Marsh, Hashim, & Bush-Mecenas, 2016), it may be the case that turnaround instead amplifies the effects of negative external shocks on teacher turnover. This paper provides the first evidence of which we are aware that disentangles the effects of a turnaround intervention from the pandemic, shedding light on the question of whether turnaround mitigates or exacerbates the effects of a negative external shock on the stability of the teacher workforce.

Michigan's Partnership Model

The aim of the Partnership Model is to improve student outcomes in the state's lowest performing schools by building district and school capacity. Michigan had identified three rounds of Partnership schools prior to the pandemic. The first round, identified in spring 2017, included schools that had been identified as low performing for three consecutive years under Michigan's No Child Left Behind (NCLB) waiver reform. This first cohort began implementing the Partnership Model the following school year (2017-18). The next two rounds, which together make up the second cohort of Partnership schools, were selected during the 2017-18 school year and began implementing Partnership in 2018-19. Schools in Round 2 were identified in fall 2017, after having been previously classified as low performing and displaying decreased student achievement into the 2016-17 school year. Round 3, designated in spring 2018, included the bottom 5% of schools statewide on Michigan's school performance index and represents the state's first Comprehensive Support and Improvement (CSI) schools under the Every Student Succeeds Act (ESSA, 2015). Though these two rounds of Cohort 2 Partnership schools were

identified at slightly different times and using different metrics, we consider them as a single cohort because they experienced the same intervention and both began implementing the Partnership Model at the beginning of the 2018-19 school year. In total, Michigan identified 123 Partnership schools from 36 districts across both cohorts. Partnership was intended to be a three-year intervention, but the state extended the intervention due to the pandemic. Thus, while Cohort 1 schools were eligible to exit Partnership status after the 2019-20 school year and Cohort 2 schools after the 2020-21 school year, they remained in Partnership through 2021-22.

Based on the belief that struggling schools do not operate in isolation, but rather reflect systemic concerns at the district level, the Partnership Model not only focused on these low-performing schools, but also the districts that house them. Districts with schools identified for Partnership were tasked with developing a Partnership Agreement, a three-year turnaround plan that describes district and school challenges, defined goals for turnaround, identified strategies to work toward meeting those goals, and laid out consequences to be implemented in the case of failure to meet goals. Through the development of Partnership Agreements and the intervention itself, districts could draw on a constellation of supports from the state Department of Education (MDE); the district's intermediate school district (ISD), a regional organization that supports Partnership districts through activities such as trainings, coaching, and professional development for district staff; and local stakeholders (see Strunk et al., 2019, 2020 for more in-depth descriptions of the Partnership Model). To fund their turnaround efforts, districts are eligible for state turnaround dollars, known as 21h funds. To receive these funds, districts submit a request to the state Office of Partnership Districts (OPD) that includes the activity/item they'd like to fund and the cost. In each of the years of Cohort 1 and 2 Partnership implementation, the state allocated $6 million in 21h funds to be allocated across requesting districts.

The contexts in which the two cohorts were selected for Partnership were different. Cohort 1 schools faced closure threats and were permitted to avoid closure by instead entering into a Partnership Agreement. To that end, Partnership identification for Cohort 1 reduced accountability pressure for educators, who understood that their school would not be closed as long as they had an active Partnership Agreement. Cohort 2 schools did not face the same accountability threats prior to Partnership and they entered into Partnership as the statewide context was evolving to become more supportive than punitive. Thus, while Partnership identification necessarily introduced new accountability for both cohorts, it did not create the same level of sweeping change for Cohort 2 as for Cohort 1. This may matter to the intervention's effects because there is evidence that more sweeping and disruptive interventions are more effective in many contexts (Carlson & Lavertu, 2018; Dee, 2012; Strunk, Marsh, Hashim, Bush-Mecenas, et al., 2016). Additionally, there is little evidence that the threat of school closure is an effective mechanism for school improvement (Bifulco & Schwegman, 2019; Chiu et al., 2016; Sunderman et al., 2017; Syeed, 2019); thus, the removal of that threat may have improved teacher retention by improving climate and morale. For both cohorts, however, educators worked in schools and districts that had been labeled as the lowest performing in the state, which brings with it inherent accountability pressures related to receipt of this signal (Figlio & Rouse, 2006; Saw et al., 2017; Winters & Cowen, 2012).

In addition, the two cohorts differed in the level of funding they received for Partnership. While the nine Cohort 1 districts with 37 schools split about $6 million in state 21h funding in their first intervention year, the state in subsequent years allocated the same total amount to be shared across 40 Cohort 1 and 2 districts with a total of about 120 schools. To receive 21h funding, districts requested funds from the state for specific purposes and the state then allocated

those funds directly to districts. As a share of total funding, this was small, as Partnership was not intended as a school finance reform but rather a targeted program aimed at improving the structures underlying low school performance. However, the intervention did result in higher revenues for Partnership than non-Partnership districts. Specifically, from 2017-18 to 2018-19 and 2019-20, revenue per pupil in real terms grew by 12% each year in Partnership districts compared with 6% and 7% in non-Partnership districts (Strunk et al., 2021). These increases came from both state and federal funding sources; in other words, Partnership districts received more than just 21h funding.

Although the Partnership Model allows for variation across districts as they work to craft an approach to turnaround that is aligned with their context, Partnership districts identified a similar set of challenges to overcome in designing their Partnership Agreements. The most prevalent was difficulty staffing their schools with high quality, and sometimes even simply qualified, teachers and leaders. Indeed, research on the Partnership Model points to human capital as the most pressing issue Partnership schools and districts were working to address in their turnaround efforts. Districts reported focusing on several strategies in their efforts to improve teacher retention, such as grow-your-own programs, targeted supports for school leaders, efforts to improve school climate, and professional development opportunities for teachers (Burns et al., 2023; Strunk et al., 2020).

During the pandemic, Partnership schools and districts continued to receive turnaround supports through MDE, in addition to pandemic emergency relief funds through the state and the federal government. However, the pandemic disrupted ongoing turnaround processes, including those focused on human capital. Partnership districts began the 2020-21 school year under fully remote instruction and remained remote longer than higher performing districts throughout the

state (Hatch & Harbatkin, 2021; B. Hopkins et al., 2021; Strunk et al., 2021). Interventions focused on recruiting and retaining teachers became less central as schools managed challenges around student attendance, engagement, and student mental health (Strunk et al., 2021, 2022). Teachers in Partnership schools and districts reported relatively high job satisfaction and support from their administrators during the 2020-21 school year even as teachers nationally began to report rising levels of burnout, but job satisfaction dipped back down in 2021-22 as they returned to in-person learning and grappled with new challenges around student behavior, quarantine and isolation procedures, and accelerating learning against a backdrop of continued COVID spread, economic precarity, growing student socioemotional needs, and a renewed focus on school accountability (Harbatkin et al., 2022; Strunk et al., 2022). Thus far, little is known about the extent to which these challenges were associated with increased turnover from Partnership schools, districts, or the teaching profession.

Data and Methods

Data, Sample, and Measures

To examine the extent to which the Partnership Model impacted teacher turnover before and through the pandemic, we draw from nine years of statewide administrative data from 2014-15 through 2022-23 provided by MDE and the Center for Educational Performance and Information (CEPI). Using these data, we can measure turnover that occurred up through the 2021-22 school year—the third year in which teacher turnover was affected by the pandemic and the final year of the Partnership intervention for the observed cohorts.

The analytic sample includes teachers in Partnership schools and a set of similarly low-performing near-selected schools that were not identified for Partnership in any of the three identification rounds. MDE identified schools for Partnership in Cohort 1 if they were in the bottom 5% of schools on Michigan's school performance index in all three of the 2013-14, 2014-

15, and 2015-16 school years. Cohort 2 schools were either in the bottom 5% of the school performance index in 2015-16 *and* had continued low performance in 2016-17 or were in the bottom 5% in 2016-17. We generate our comparison group of near-selected schools as those that were low performing during the Partnership selection timeframe but were not targeted for intervention. The near-selected comparison group includes schools that were: (1) in the bottom 5% on the state index system in 2015-16 but not selected for Partnership, and (2) in the 6th to 10th percentile on the state's ESSA index in 2016-17. In total, the analytic sample includes 120 Partnership schools across 36 districts and 203 comparison schools in 130 districts, constituting 45,175 teacher-year observations representing about 13,500 unique teachers in 323 unique schools. We run additional analyses in which we drop from the comparison group 57 non-Partnership schools in Partnership districts.

We examine three dichotomous nested measures of teacher turnover: (1) leaving the school regardless of pathway out (including switching to another school within the district), (2) leaving the district, and (3) leaving the Michigan public education system entirely. We construct each of these measures for school year t based on data from the fall of school year t+1. For example, we consider a teacher as leaving the school in 2021-22 if they are no longer observed in that school in fall 2022.

We include baseline covariates representing school-level characteristics of the student body, including the proportion of students by race/ethnicity, economic disadvantage, English learner status, special education status, and school enrollment. We also include covariates for individual teacher race/ethnicity (Black, Hispanic or Latino/a/x, other race, with White as the reference category) and gender (male, with female as the reference category). Finally, in a heterogeneity analysis, we compare teachers who are new to their district with returning

teachers. Our data do not include detailed measures of teaching experience, though our new-to-district measure should capture all new-to-the-profession teachers along with others who are coming from other districts, private schools, and out of state.

Table 1 provides baseline descriptive statistics, with teacher-level values in the first panel and school-level values in the second. In each panel, the first two columns represent Cohort 1 and 2, respectively, and the third provides the mean for the full treated group. Column 4 shows values for the comparison group, and Column 5 for the comparison group excluding non-Partnership schools housed Partnership districts, which we use in a robustness check described below. Finally, Column 6 shows values for the rest of the state (i.e., teachers and schools not in Partnership or comparison schools) in order to highlight differences between low-performing schools and their higher performing counterparts in Michigan. The first panel shows important demographic differences between Partnership schools, comparison schools, and the rest of the state, with Partnership school teachers disproportionately Black, teachers in the rest of the state disproportionately White, and comparison school teachers falling between the two groups of schools. The outcomes in the first panel, shown below the teacher characteristics, underscore that turnover from the school and district, respectively, were similar in Partnership and comparison schools—both of which were substantially higher than non-Partnership contexts. Turnover was especially high in Cohort 1, where school turnover was about two times higher and district turnover nearly three times higher than in the rest of the state. At baseline, about 1.7% of Partnership school teachers, 3.6% of comparison school teachers, and 2.3% of other teachers left the Michigan public education system.

Columns 4 and 5 show that the comparison group that includes Partnership district schools not targeted for treatment is more similar to the comparison group that excludes them.

For this reason, we focus on this analytic sample in our primary estimation strategy, though we also estimate models with Column 5 as a comparison group due to the possibility of spillover effects within the district (Burns et al., 2023).

TABLE 1

To examine our second research question on the mechanisms that might contribute to the effects of Partnership, we leverage data from teacher surveys administered in all schools in Partnership districts (both Partnership and non-Partnership schools) for two pre-pandemic years (fall 2018 and fall 2019) and two pandemic years (spring 2021 and spring 2022). The teacher response rate was similar across cohorts and ranged from a low of 32% in 2021-22 to a high of 57% in 2019-20. We provide full response rates by cohort and year in Appendix Table A1.

The survey was administered as part of a larger evaluation of the Partnership Model, and we draw on a subset of items relevant to teacher turnover and retention. First, to understand teacher perceptions of accountability threats, we draw on responses to the question, "If your improvement goals are not met, to what extent do you believe your school will face the following consequences?" This question was included in each of the pre-pandemic surveys (i.e., 2018 and 2019). Response options were (1) very unlikely, (2) somewhat unlikely, (3) somewhat likely, (4) very likely, and finally an uncertain option.[1]

Second, we examine teacher responses to a variety of items relevant to teacher mobility decisions. Although the survey does not include a baseline measure for Cohort 1, it does allow examination of the evolution of key mechanisms as the Partnership Model is implemented and through the pandemic years. To do so, we begin with exploratory factor analysis (EFA) drawing on items from survey questions asking teachers (a) to assign their school an A-F grade in a variety of areas, (b) the extent to which they agree with a series of statements about their school, and (c) to rate their principal's effectiveness along a variety of dimensions. We started with a

parallel analysis to identify the appropriate number of factors (Horn, 1965), and applied the varimax rotation to identify factors with meaningful interpretations. Ultimately, the EFA identified four factors present in the relevant questions: improvement goal buy-in, school culture and climate,[2] school leadership, and safety and student behavior. We validated our approach by running separate models by year and Partnership status and ensuring that the results were similar across groups. Cronbach's alphas for our full sample range from 0.78 for culture and climate to 0.94 for school leadership. Drawing on our EFA, we then ran confirmatory factor analyses (CFA) and extracted factor scores for each respondent for each of the four constructs. Survey questions and factor loadings are provided in Appendix Table A2.

Finally, we draw on responses to two questions about teachers' employment plans for the following year. Teachers first responded to a question asking about their plans for the next school year, with response options (a) continue teaching in this school, (b) serve in a different position in the same school, (c) continue teaching in district but in a different school, (d) leave the district to work in a different district or charter network, (e) leave to pursue a job outside of the education sector, or (f) retire. For the purposes of this analysis, we code a response of a or b as an intended stayer, and a response of c, d, e, or f as an intended leaver. Although reported intent is not a proxy for actual turnover behavior, other research shows it is a meaningful predictor generally and in this specific survey (Harbatkin, Nguyen, et al., 2025; Nguyen et al., 2024). Thus, these responses can help to unpack teacher motivations for staying in or leaving Partnership schools. Following the question about intent, a follow-up question asked about the extent to which a variety of factors contributed to teachers' reported intent. These items are leadership, culture and climate, workload, commute, pay, students, and school/district accountability designation, instructional modality, pandemic-related safety precautions in school,

the administration's treatment of teachers during the pandemic, and the impact of the pandemic on personal/family health. For each item, respondents could select from (1) not a factor, (2) minor factor, (3) moderate factor, (4) major factor, or (5) a primary factor. We code these on a five-point scale. These questions were posed in this format in 2020-21 and 2021-22 only.

Empirical Strategy

To examine the effects of Partnership on teacher mobility before and during the pandemic, we estimate event study models examining the extent to which the probability of turnover deviates from pre-identification trends for Partnership schools relative to near-selected comparison schools. To do so, we stack data from the two cohorts and the comparison group, create a series of year indicators centered at the identification year for each cohort, and then estimate event study models with school and year fixed effects. A growing research base shows that these two-way fixed effects models can lead to biased estimates when there are heterogeneous effects across implementation cohorts (Athey & Imbens, 2022; Baker et al., 2022; Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; L. Sun & Abraham, 2021; Wooldridge, 2021). Because there is evidence from prior research of heterogeneous effects on student outcomes by cohort (Burns et al., 2023; Harbatkin, Burns, et al., 2025), and because there is reason to expect differential effects before and during the COVID-19 pandemic (which initially affects the two cohorts in different implementation years), we estimate two-way Mundlak regressions (Wooldridge, 2021) allowing for flexible effects by implementation cohort in each of the pre- and post-COVID years. These models take the form

$$T_{ijct} = \sum_{k=-3}^{5} \sum_{c=1}^{2} \tau_k 1(t = t_s^* + k) \times Coh_c + \rho\left(\boldsymbol{X}'_{jt=2016} \times Yr_t\right) + \gamma \boldsymbol{Z}'_i + \alpha_j + \theta_t + \varepsilon_{ijct} \quad (1)$$

where T_{ijct} is a dichotomous variable representing one of the three turnover outcomes for teacher *t* in school *j* in implementation cohort *c* in school year *t*. The term $1(t = t_s^* + k)$ represents a set

of indicators for the years pre- and post-Partnership implementation, with t_s^* denoting the year in which school *s* adopted Partnership spanning from three years prior to Partnership identification through up to five years of implementation. Coh_c takes a value of one for schools that were included as part of each of the two implementation cohorts, respectively, and zero otherwise. $\boldsymbol{X}$ is a vector of school-level covariates measured in 2016 (Cohort 1's identification year) as described above, interacted with a linear time trend, denoted as Yr_t. $\boldsymbol{Z}$ is a vector of teacher covariates described above. Each model includes school fixed effects (α_j), year fixed effects (θ_t), and an idiosyncratic error term (ε) clustered at the school level.

The coefficients of interest are those represented by the $\tau_k s$, which provide the estimated effect of Partnership for Cohort *c* in the k^{th} year of implementation. In our main models, we measure the effects relative to the year of Partnership identification (k=0), so that τ_{-3} through τ_{-1} are the difference between Partnership and comparison schools in the years prior to Partnership and τ_1 through τ_5 are the estimated effects in the years of implementation.

The τ_k estimates are relative to remaining in the school for the school turnover model, remaining in the district for the district turnover model, and remaining in the Michigan public education system in any capacity for the model predicting leaving the profession. Because these are linear probability models, the estimates can be interpreted as the difference in the probability of turnover for teachers in Partnership schools in a given cohort relative to teachers in comparison schools in relative year *k*.

We present each cohort's τ_k estimates separately and do not present estimates with the two cohorts pooled together because the pandemic's onset came at different implementation years for the two cohorts. For Cohort 1 schools, which were identified for Partnership in 2016-17 and first implemented in 2017-18, we observe two pre-identification and five implementation

years, with years 3-5 impacted by COVID-19. For Cohort 2 schools, which were identified for Partnership in 2017-18 and first implemented in 2018-19, we observe three pre-identification years and four implementation years, with years 2-4 impacted by COVID-19.

One limitation of this approach is that we only observe two pre-identification years for Cohort 1. We trim the panel to 2014-15 and later because the Michigan legislature implemented a series of teacher reforms in 2011 and 2012 that reduced teacher tenure protections and collective bargaining rights. The last of these policies (making Michigan a "right-to-work" state) went into effect in March 2013 and differentially increased teacher turnover in the state's "hardest to staff" schools and districts, of which Partnership are a part (Anderson et al., 2022; Brunner et al., 2019).

Intuitively, our approach involves controlling for separate pretreatment trends for each implementation cohort, estimating the deviation from those trends in the treatment years, and then comparing the deviation in Partnership with that in comparison schools. There are two core identifying assumptions for this model to provide causal effects. First, the pre-identification trends for the treated (i.e., Cohorts 1 and 2 together) and comparison groups should be parallel, conditional on covariates (Olden & Møen, 2022; Wooldridge, 2021). Second, there should be no evidence of an anticipatory effect of Partnership in the year prior to identification, again conditional on covariates. We examine the parallel trends assumption visually and by testing whether the pre-identification coefficients (τ_{-3} through τ_{-1} for the two cohorts) are jointly significantly different from zero. We test the no anticipation assumption through an examination of the τ_{-1} coefficients.

We examine the robustness of our main estimates in two ways. First, we estimate a parallel set of models in which we move the reference year back from the identification year to the year prior to identification. To the extent that our main estimates are driven by mean

regression following an anticipatory effect in the identification year, estimates would be attenuated in these models. Second, we estimate models with non-Partnership schools in Partnership districts removed from the comparison group. This revised comparison group has the benefit of eliminating any spillover effects of Partnership, but is less similar to the treated group and therefore may provide a less valid counterfactual.

To answer the descriptive research question about factors contributing to our event study findings, we examine subgroup differences in event study findings as well as descriptive patterns of survey responses. We begin with an extension of our event study analyses that allow for flexible effects by teacher experience. To do so, we leverage administrative data on teacher experience to estimate event study models with an additional interaction term that allows for separate effect estimates for new vs. returning teachers. In our main specifications, we operationalize "new teacher" as a teacher who is new to the district, but also estimate models that operationalize "new teacher" as a teacher who is new to the school and find very similar results. This model takes the form:

$$\begin{aligned} T_{ijct} = \sum_{k=-3}^{5}\sum_{c=1}^{2} & \upsilon_k 1(t = t_s^* + k) \times Coh_c \times NewTeacher_{ijt} \\ & + \phi_k 1(t = t_s^* + k) \times Coh_c \times ReturningTeacher_{ijt} \\ & + \rho\left(\boldsymbol{X}'_{jt=2016} \times Yr_t\right) + \gamma \boldsymbol{Z}'_i + \alpha_j + \theta_t + \varepsilon_{ijct} \end{aligned} \tag{2}$$

Here, the $\tau_k s$ from Equation 1 are replaced by $\upsilon_k s$ for new teachers and $\phi_k s$ for returning teachers. Thus, we identify separate pre- and post-implementation estimates for new and returning teachers, respectively. Estimates are relative to comparison teachers of the same experience level. The remainder of the model is identical to Equation 1.

We next draw on survey data to describe differences between cohorts to unpack differences in estimated effects. Because our survey data are limited to Partnership districts,

these analyses simply compare levels and trends in Cohort 1 and 2 Partnership schools, respectively, with those in non-Partnership schools in Partnership districts. First, to examine our hypothesis that the two cohorts experienced different accountability threats, we use *t*-tests to compare mean Cohort 1 and 2 responses to the question about consequences of failure to meet improvement goals. We also show means for non-Partnership schools in Partnership districts.

To understand cohort differences during the first pandemic year in particular, we then examine responses to the question about factors contributing to teacher employment plans (e.g., pay, workload, school leadership, etc.). Specifically, we run two sets of comparisons, again using *t*-tests. First, we look *across* cohorts by comparing Cohort 1 with Cohort 2 responses, separately for intended leavers and intended stayers. This analysis helps to unpack whether specific factors are more salient to teachers in Cohort 1 relative to Cohort 2. Second, we compare intended stayers with intended leavers *within* cohort. This second analysis elucidates whether some factors were more relevant to intended stayers or intended leavers within a cohort.

Finally, we estimate models predicting each of the four survey constructs we developed with the CFA: improvement goal buy-in, culture and climate, school leadership, and safety. These descriptive models take the form:

$$y_{ijta} = \sum_{t=1}^{5} \eta_t 1\left(Yr_t \times Coh1_{ijt}\right) + \theta_t 1\left(Yr_t \times Coh2_{ijt}\right) + \pi_t + \mu_a + \varepsilon_{ijc} \quad (3)$$

predicting outcome *y* for teacher *i* in school *j* in relative year *t* and academic year *a* as a function of relative year indicators (Yr_t) from implementation years 1–5 interacted with each of the cohort indicators (*Coh1* and *Coh2*, respectively). The model also includes relative year fixed effects (π, representing non-Partnership schools in Partnership districts), academic year fixed effects (μ), and a design-based error term. The coefficients of primary interest are the $\eta_t s$ and the

$\theta_t s$, which represent the deviation of Cohort 1 and 2 respondents, respectively, from non-Partnership school respondents in Partnership districts in implementation year *t*.

We weight all survey analyses using sampling and nonresponse weights, where the sampling weight is the school-level coverage of our sampling frame and the nonresponse weight is the inverse probability of response within school, and estimate design-based standard errors that account for the clustering of respondents in schools. Nonresponse weights use demographic characteristics (race/ethnicity, gender) and certification type (i.e., elementary, secondary).

This sample is different from the event study sample in two important ways. First, we observe survey measures only in the implementation years, and therefore cannot use an event study here. Second, we only have survey measures from Partnership districts. That means our comparison group is composed of non-Partnership schools in Partnership districts rather than the near-selected schools across the state that comprise the event study comparison group. As a result, the comparison group for the descriptive survey analysis has likely received some "dose" of treatment since they are situated in Partnership districts, though they receive less than the targeted schools that make up the treatment group. In sum, the findings from these ancillary analyses should be interpreted as associational in nature. However, they shed light on the mechanisms that may be underlying the effects we identify in the event study models.

Findings

Impacts of Turnaround on Teacher Turnover

Our event study findings are presented in Table 2, with findings from the model predicting leaving the school (regardless of pathway out) in Column 1, leaving the district in Column 2, and leaving the Michigan public education system in Column 3. Beneath the *N*, we provide the *p*-value from the *F*-test on the joint significance of all pretreatment indicators for each model, providing evidence for the conditional parallel trends assumption. Here, evidence in

support of the assumption would be apparent in statistically insignificant *F*-statistics. All three models cleanly meet the assumption, with the statistically insignificant estimates on each of the individual pretreatment years providing additional evidence. The estimates on Cohort × *t*-1 indicators provide evidence in support of the conditional no anticipation assumption, though we do note that the *t*-1 estimates on leaving the school show a statistically insignificant descriptive dip in turnover the year before identification (or a climb in the identification year). The models that move the reference year back to *t*-1 should help to mitigate concerns of bias arising from this descriptive pattern.

We now turn to the estimates on leaving the school, the district, and the Michigan public education system, shown visually in Figure 1. Beginning with the pre-pandemic treatment period, teachers in Cohort 1 were descriptively less likely to leave their schools and significantly less likely to leave their districts and Michigan education in the first year of the intervention, which was prior to the pandemic. Specifically, for Cohort 1 teachers in Year 1, the probability of leaving the district was 5.9 percentage points lower and the probability of leaving Michigan education was 3.9 percentage points lower than teachers in similarly low-performing comparison schools. This suggests the intervention was initially successful at retaining teachers in Partnership districts in Cohort 1, though both estimates attenuate to zero in the second intervention year. We see no pre-pandemic effects in Cohort 2, which experienced only one pre-pandemic intervention year.

TABLE 2

Moving down to the pandemic treatment period, Column 1 shows a significant decrease in the probability of leaving the school for teachers in both cohorts, relative to comparison school teachers. Specifically, Cohort 1 teachers were 6.6 percentage points less likely to leave their school in the first year of the pandemic (third year of Partnership), 9.7 percentage points less

likely to leave their school in the second year of the pandemic (fourth year of Partnership), and 9.9 percentage points less likely to leave their school in the third year of the pandemic (fifth year of Partnership). *F*-tests confirm that Cohort 1's Year 4 and 5 decreases are significantly different ($p<.05$) from the Cohort 1 estimate in the last pre-pandemic treated year of 2018-19—pointing to a stronger Partnership effect during the pandemic. Starting in the second year of the pandemic (third year of Partnership), Cohort 2 teachers were significantly less likely than comparison school teachers to leave their schools—by 5.8 percentage points at the end of 2020-21 and 5.7 percentage points at the end of 2021-22. Each of these estimates were again significantly different from Cohort 2's pre-pandemic treated year ($p<.05$).

FIGURE 1

For leaving the district and leaving Michigan education (Columns 2 and 3, respectively), we find consistently signed though not consistently significant results for Cohort 1, and mixed results for Cohort 2. We observe negative coefficient estimates for both leaving the district and leaving Michigan public education in all three pandemic years for Cohort 1. These estimates are statistically significant in implementation Year 4: 6.8 percentage points less likely to leave the district and 3.7 percentage points less likely to leave Michigan public schools.

However, teachers in Cohort 2 were 3.5 percentage points *more* likely to leave the district and 2.7 percentage points *more* likely to leave Michigan education than comparison teachers at the end of 2019-20—the end of the second Partnership year and the end of the first pandemic-affected year. Cohort 2 also shows a marginally significant increase in leaving MI education at the end of Year 4 of Partnership.

We find similar patterns of estimates in our models that move the reference year back to *t*-1, though estimates on leaving the school are attenuated and models show higher turnover from the school and Michigan public schools in pretreatment years relative to the year before

identification (Appendix Table A-3). We also find similar patterns in the models that exclude non-Partnership schools in Partnership districts from the comparison group (Appendix Table A-4), though there is some evidence of differential pretreatment trends in the models predicting leaving the district and leaving Michigan public schools. We also find nearly identical point estimates and patterns of significance in models excluding teacher characteristics and/or school covariates (Appendix Table A-5).

Assessing Differential Pandemic Era Trends

One key concern for our identification approach is that Partnership schools might have experienced differential impacts of the pandemic. Nationwide, turnover dipped in the first pandemic-affected year, followed by a correction to near baseline levels or higher in the second pandemic year (Bacher-Hicks et al., 2023; Goldhaber & Theobald, 2023). The decrease was especially pronounced in school contexts with the largest shares of students in poverty and students of color (Rogers et al., 2025). Thus, Partnership schools (especially those in Cohort 1, which serve the largest shares of students in poverty and Black students, respectively, and had the highest rates of turnover pre-intervention) may have experienced lower turnover in the first pandemic year even in the absence of Partnership. However, this explanation would yield two predictions that we do not observe in our results. First, improvements would be limited to—or at least strongest during—the height of the pandemic (i.e., 2019-20 and 2020-21) and then worsen again in 2021-22 as schools returned to their typical operations. Instead, we find that turnover decreases by a similar magnitude in each of the last two years of the intervention. Second, Cohort 2 schools would not be expected to show similarly consequential changes during the pandemic relative to comparison schools. In fact, teachers in Cohort 2 schools were about 6 percentage points less likely than their comparison school counterparts to leave their school in

each of the last two years. In sum, the results do not suggest that the decreased turnover we observe are driven by differential responses to the pandemic.

Potential Mechanisms

Above, we show that Partnership was particularly helpful at reducing turnover for Cohort 1 schools, with more modest benefits for Cohort 2 during the pandemic and a worsening of turnover from the district and Michigan public schools in Partnership Year 2. Here, we examine several distinct mechanisms that might explain these patterns, both across and between cohorts. First, we examine the possibility that Partnership could have differential impacts by teacher experience before and after the pandemic's onset given the unique challenges new teachers faced during the pandemic. Next, we discuss the possibility that differences between cohorts emerged due to differences in the policy context at the beginning of Partnership related to accountability pressures and funding. Finally, we examine changes in teacher working conditions, which are of central importance to teacher retention.

Composition of the Teacher Labor Force

Teacher turnover follows a well-established U-shaped curve by experience, where the highest turnover is among teachers in their first few years of teaching and those eligible for retirement (Grissmer & Kirby, 1987). Early-career teachers are also more likely to switch schools than their more experienced peers (National Center for Education Statistics, 2024). Schools with higher turnover tend to have fewer experienced teachers (Boyd et al., 2005), so if interventions had differential impacts by teacher experience they would be expected to also have differential impacts for high turnover schools.

Teachers new to the district and returning teachers (those with at least 1 year of experience in the district) did seem to respond differently to Partnership. Table 3 shows estimates from Equation 2, which includes an interaction term allowing for flexible effects by

teacher experience. We find that the pre-pandemic Cohort 1 decreases in leaving the district and Michigan public education were similar for new-to-district and returning teachers in the first implementation year. Then in 2018-19 (the second implementation year for Cohort 1 and the first for Cohort 2), turnover from the school and district increased for new teachers but not for returning teachers. In other words, Partnership reduced turnover for both new and returning teachers in 2017-18 but *increased* turnover for new teachers in 2018-19.

After the pandemic's onset, the reduced overall turnover is driven by returning teachers. Specifically, Column 1 shows that Cohort 1 returning teachers were about 7 percentage points less likely to leave their schools in 2019-20, and 11 percentage points less likely in 2020-21 and 2021-22, respectively, relative to returning teachers in comparison schools. We find qualitatively similar but smaller in magnitude estimates for leaving the district. By contrast, we do not observe an effect on school or district turnover for new-to-district teachers. Among Cohort 2 returning teachers, we observe negative point estimates on leaving the school in each pandemic year that are statistically significant 2020-21 and 2021-22. Among new-to-district Cohort 2 teachers, we actually find a 9 percentage point *increase* in turnover from the school in the first pandemic year followed by no effect the subsequent two years. In other words, our findings suggest that Partnership decreased turnover from the school and district among returning teachers but not new teachers during the pandemic. The improvements for returning teachers persist through 2021-22 when all districts had returned to in-person schooling. Differences between new-to-district and returning teachers are less apparent when we turn to likelihood of leaving Michigan public education. In Cohort 1, we find evidence of reduced attrition among both new and returning teachers during the pandemic. In Cohort 2, point estimates for both new and returning teachers follow the same pattern as the overall estimates above.[3]

Taken together, this pattern of evidence suggests that within schools, the impacts of Partnership are likely functioning by changing the experiences of established teachers rather than those new to the school or district. No marked differences between patterns within cohorts emerged in this analysis, suggesting that differential impacts by teacher experience cannot explain the differences we observe between the two cohorts.

Differing Policy Contexts

There are two core ways that the intervention was different for the two cohorts: the policy context in which the Partnership designation occurred and the resources available to designated schools in their first intervention year. First, in terms of policy context, Cohort 1 schools faced a credible threat of closure, and Partnership provided a lifeline that removed the threat and offered resources to address schools' challenges. These schools may have experienced higher attrition during the pre-Partnership period in response to the threat, which could have then stabilized during the intervention. Although we do not observe teacher survey responses in Cohort 1's baseline year, we did ask teachers in fall 2018 and fall 2019 about their perceptions of the likelihood that they would face consequences if their school failed to meet improvement goals, potentially shedding light on differences in teacher perceptions of accountability pressures.

Figure 2 summarizes responses for four subgroups of teachers—Cohort 1, Cohort 2, comparison schools in Partnership districts, and non-comparison schools in Partnership districts. The first two bars of each panel and year show responses from the two Partnership cohorts. In fall 2018 (i.e., beginning of Year 2 for Cohort 1), descriptively lesser shares of Cohort 1 teachers believed that their schools were likely to face closure, staff and leader removal, loss of students, and even a low accountability score if they failed to meet improvement goals, though none of these differences between the two cohorts were statistically significant. There was no clear

pattern of differences between cohorts by fall 2019 (Cohort 1's third and Cohort 2's second implementation year). These responses suggest that both cohorts ultimately settled into what was perceived as a supportive rather than punitive intervention—though the clearly lower perceived likelihood of consequences in non-Partnership schools underscores that Partnership teachers were still feeling accountability pressures.

Panel E shows that Cohort 1 teachers were more inclined to believe that "nothing will happen" if they failed to meet improvement goals—though teachers from both cohorts believed it was somewhat unlikely that nothing would happen. The statistically significant difference ($p<.05$) between the two cohorts in Panel E was 0.26 and 0.21 in 2018 and 2019, respectively, on the four-point scale.[4] In standard deviation terms, this represents differences of 0.23 SD and 0.18 SD, respectively. In real terms, in 2018, 39% of Cohort 1 compared with 24% of Cohort 2 teachers believed it was somewhat or very likely that nothing would happen. This percentage remained stable in 2019 for Cohort 1 and increased to 30% in Cohort 2.

FIGURE 2

These responses suggest that teachers across Partnership schools perceived real accountability threats, but Cohort 1 teachers may have experienced the designation as somewhat less worrying. Literature on school turnaround shows that accountability reforms can be associated with increased teacher demoralization, stress, and ultimately turnover (Lincove et al., 2018; Weixler et al., 2018; Welsh et al., 2019; Welsh & Williams, 2018). Thus, it is plausible that the removal of that threat could have reduced stress and turnover from Cohort 1 schools.

Second, regarding resources, Cohort 1 schools received more funding in their first implementation year because the state's turnaround funding pool—which remained stable from year to year regardless of the number of Partnership schools—was divided among fewer schools at that point (i.e., Cohort 1 schools only instead of Cohort 1 and 2). This could be particularly

salient because most Partnership districts used at least some of their state turnaround funding for activities related to staffing (e.g., hiring infrastructure, grow-your-own programs, instructional support staff) and educator development (e.g., professional development, certification prep, coaching), respectively (Strunk et al., 2022). These additional resources for Cohort 1 schools could help to explain the immediate declines in turnover among Cohort 1 but not Cohort 2 schools. To the extent that early resources helped districts with Cohort 1 schools build systems that mitigated turnover in subsequent years as well, these differences might help to explain the descriptively larger improvements in Cohort 1 during the pandemic.

Teacher Buy-In and Working Conditions

Teacher working conditions and job commitment are consistently implicated as factors influencing teacher turnover, and the strategies adopted by many Partnership districts sought to improve these factors either directly or indirectly. Figure 3 plots coefficient estimates from the Y*ear* × *Cohort* indicators in Equation 3 for each of four relevant teacher survey constructs – teacher buy-in to Partnership improvement goals, school culture and climate, perceptions of school leadership, and perceptions of safety within the school. Estimates in the top row are relative to non-Partnership schools in Partnership districts, whereas the second row restricts the sample to the comparison schools from the event study that are in Partnership districts. Because the survey was first administered in fall 2018-19, the first data point for Cohort 2 provides a near-baseline measure (fall of the first implementation year); in contrast, the first observation for Cohort 1 is fall of the second implementation year. For both cohorts, the first two data points in the series are pre-pandemic (fall 2018 and 2019, denoted by hollow markers) and the last two are pandemic-impacted years (fall 2020-21 and fall 2021-22, denoted by filled markers).

Throughout the implementation period, all four measures show a general upward trend relative to non-Partnership that predates the pandemic and largely persists through the pandemic-affected years. The upward trajectory is clearest relative to the event study comparison schools we observe in Partnership districts (Panel 2), helping to explain Partnership's positive effects on retention. This pattern is consistent with the notion that Partnership may have improved important aspects of teachers' experiences working in schools, though they do not explain the differences between the two cohorts in the first pandemic year.

FIGURE 3

In fact, Cohort 2, which experienced more uneven effects on turnover, shows arguably more stable improvements on these measures. Of particular importance in interpreting these descriptive findings, we collected our 2019-20 survey data in fall 2019 before the pandemic's onset. Cohort 2's increased turnover from the district and profession occurred *between* year 3 and 4 on the x-axis of Figure 3. Thus, there may be differences that emerged in spring of the 2019-20 school year that help to explain why turnover decreased immediately in Cohort 1 but not Cohort 2. Although we do not observe survey data in spring 2019-20 to speak directly to teacher decisions in the first semester of pandemic schooling, we can draw on responses from the following spring as the most proximate data collection to the pandemic's onset. Figure 4 shows 2020-21 responses a question asking teachers about the extent to which a variety of items were a factor in the teacher's intention to stay in or leave their school. Panel A shows differences *between* the two cohorts for leavers (A1) and stayers (A2), whereas Panel B shows differences between stayers and leavers *within* Cohort 1 (B1) and Cohort 2 (B2). Bars are sorted consistently across all panels based on the magnitude of differences in Panel A.

Panel A1 shows that pay and workload were each more prominent factors for intended leavers in Cohort 2 than Cohort 1 by about 0.7 points on the 1–5 scale. To place those

differences in context, the mean response to the pay question was about 2.7 among Cohort 1 intended leavers and 3.4 among Cohort 2 intended leavers. One way to think about the difference, then, is that teachers reporting plans to leave Cohort 2 schools reported that pay was about 26% more important to their decision than their peers reporting plans to leave Cohort 1 schools. Intended leavers in Cohort 2 reported that workload was about 27% more important than their counterparts in Cohort 1. Panel A2, by contrast, shows no significant differences between intended stayers in the two cohorts.

Panel B provides additional evidence that pay and workload were especially salient for intended leavers in Cohort 2. Specifically, whereas intended stayers tended to report greater relevance of each of the factors than intended leavers in both cohorts, pay and workload among Cohort 2 leavers were the only two exceptions (though the differences were not statistically significant). Though not shown here, the observed cohort differences among intended leavers attenuate close to zero in 2021-22 when school turnover rates were similar across the two cohorts. Appendix Table A-7 provides means for each item and *t*-test results for both sets of comparisons in both 2020-21 and 2021-22.

FIGURE 4

We do not have administrative data on teacher pay to examine whether Cohort 2 teachers were actually receiving lower wages than Cohort 1 teachers; however, any differences in true pay would likely be minor because Partnership districts tended to have schools in both cohorts. Thus, the more likely interpretation of these differences is that Cohort 2 teachers who expressed plans to leave felt that their pay did not justify their workload during the pandemic.

We offer two possible explanations grounded in the survey data (Appendix Table A-8). One is that Cohort 2 schools struggled with more serious day-to-day staffing challenges in 2020-21. In response to a survey question to hindrances about improvement, Cohort 2 teachers

reported that an insufficient supply of certified teachers, lack of availability of substitute teachers, and low quality of substitute teachers were greater hindrances to improvement than did Cohort 1 teachers. A second possible explanation is that Cohort 2 teachers reported greater challenges with online classroom management and maintaining instructional continuity across shifting modalities (e.g., moving across face-to-face, hybrid, and online learning). Together, these findings suggest that working conditions for Cohort 2 teachers may have been more negative during the height of the pandemic than they were for Cohort 1 teachers before stabilizing in the intervention's final year and/or that Cohort 2 schools were less well equipped to immediately buffer teachers from the pandemic's worst effects.

Discussion and Conclusion

School turnaround models seek to build school capacity along multiple dimensions, including through activities expected to reduce the ad hoc teacher turnover that undermines improvement efforts. It is rarely straightforward to assess the extent to which interventions impact organizational capacity, particularly as it relates to resilience to shocks. This study provides a rare opportunity to examine the interaction between such an intervention and an external shock. We find that Michigan's Partnership Model did appear to improve capacity for turnaround schools to better withstand the pandemic's impact on the teacher labor market. Positive effects on teacher retention were immediately apparent in the first cohort of Partnership schools, where turnover from the district decreased in the first intervention year before stabilizing as the pandemic approached. Cohort 2 did not see any improvements in its single year of turnaround that came before the pandemic struck. We offer two possible reasons for these pre-pandemic differences across implementation cohorts. First, it may be the case that Partnership identification itself allayed Cohort 1 teacher concerns about school closures, prompting more

teachers to stay in their positions. By contrast, Cohort 2 teachers were never facing a serious threat of closure, so accountability pressures did not shift with the Partnership designation. Second, Cohort 1 schools received more turnaround resources in their first implementation year, and many used those resources for staffing and development. These investments may have helped to improve immediate teacher retention in addition to laying the groundwork for improvements in future years.

After the pandemic's onset, Cohort 1 experienced an immediate decrease in teacher turnover that grew in the second pandemic year and persisted into the third. Cohort 2 did not experience the same immediately positive effect during the pandemic; in fact, turnover from the district and profession increased in the first pandemic year. However, turnover then decreased among Cohort 2 teachers in the second and third pandemic years. Specifically, in 2020-21 and 2021-22, the probability of turnover from the school was about 10 percentage points lower among Cohort 1 teachers and 6 percentage points lower among Cohort 2 teachers.

Although turnaround may seek to achieve "rapid and dramatic" improvement (Aladjem et al., 2010; Herman et al., 2008), the reality is often more slow and steady as the intervention builds up school capacity, gains buy-in from staff, fosters a positive school climate, and expands the capacity of the school leadership team. These organizational improvements may then take time to translate into greater teacher retention and improved student achievement. In fact, comprehensive school reform, which was the 1990s-era predecessor to school turnaround, yielded meaningful positive effects only after 5–7 years of intervention (Borman et al., 2003; Good et al., 2005). Our survey data suggest that improvements were beginning to take shape in Partnership schools early in the intervention. After the pandemic's onset, expanded school capacity and more positive climate may have translated into greater teacher retention—

immediately in Cohort 1 and later in Cohort 2. The delayed effect in Cohort 2 may stem from particularly salient staffing challenges in those schools that exacerbated already taxing working conditions during the first pandemic year.

To that end, our findings suggest that Partnership did indeed improve organizational capacity to build schools' resilience to shocks, but only to a point. Without an adequate pool of qualified teachers, the intervention was not sufficient to allay challenging working conditions and improve teacher retention early in the pandemic. Our finding that positive effects during the pandemic were concentrated among returning teachers further underscores the importance of building teacher capacity to help schools withstand negative external shocks. It may be the case that Partnership's efforts were sufficient to retain returning teachers even in the face of serious challenges, but not to retain new teachers needing even more support.

There are some limitations to our study. First, we cannot distinguish in our data between voluntary and involuntary teacher turnover. Some teacher turnover can be beneficial if, for example, it serves to enhance the fit of teachers with schools or replaces lower effectiveness teachers with higher effectiveness ones (Adnot et al., 2016; Boyd et al., 2008; Dhaliwal et al., 2023). By contrast, the ad hoc turnover that occurs at especially high rates in low-performing turnaround schools is detrimental to student learning and school improvement (Atteberry et al., 2016; Hanushek et al., 2016; Henry & Redding, 2020). We cannot say with certainty whether the turnover we observe is the "good" turnover or the "bad" kind. However, interviews with school and district leaders as part of a larger evaluation of the Partnership Model do not suggest that schools were intentionally dismissing or reassigning teachers as part of the intervention (Strunk et al., 2019, 2020). Second, the Partnership Model conceives of the district as the foundation for school improvement and therefore provides resources and support to the district with the

expectation that the district's expanded capacity trickles down to the schools. Thus, non-Partnership schools in Partnership districts receive some "dose" of treatment, and any resulting improvements will attenuate the effect estimates in our main models. We address this limitation by including a robustness check where we exclude non-Partnership schools in Partnership districts from our comparison group and find substantively similar results, though the resulting comparison group is less similar to Partnership schools than our preferred comparison. Another limitation to our supplemental event study where we allow flexible effects by teacher experience is that we have only a coarse measure of teacher experience. New-to-district teachers include first-year teachers, but also include experienced teachers who moved districts. Thus, our estimates for new-to-district teachers include both new and experienced teachers. Next, with respect to our descriptive mechanisms analyses (i.e., RQ2), we do not observe baseline measures for Cohort 1 in our survey data, so we cannot plot Cohort 1 trajectories over the course of the intervention. Thus, while we observe generally increasing trends in working conditions in both cohorts, we cannot say with certainty that Cohort 1 also improved over its first intervention year. Additionally, as with all survey data, we are capturing *perceptions* and observe only teachers who responded to the survey. Our nonresponse weights mitigate differences based on observed teacher characteristics, but would not account for systematic differences between respondents and nonrespondents along unobserved dimensions.

Despite these limitations, we find clear evidence that helped Partnership schools retain teachers—particularly returning teachers—through the pandemic. There are several policy implications. First, school turnaround does appear to have the ability to build school resilience to withstand negative external shocks. While we examine this question in the context of the COVID-19 pandemic, this resilience may extend to other external shocks, such as natural

disasters, sudden shifts in the economy or federal policy, or other events outside the locus of control of school and district leaders. Policies and interventions that build school capacity may serve to reduce harm to school operations when these shocks occur. Investment in school turnaround, then, may help to cushion the nation's already struggling schools from unforeseen external shocks. Second, echoing a large research base showing that a stable and effective teacher workforce is central to successful turnaround (Harbatkin, Burns, et al., 2025; Henry et al., 2020; Hill et al., 2023; Pham, 2023b), when an external shock creates challenging working conditions, education leaders should make any possible efforts to mitigate its negative impact on teacher workload. Adverse effects on working conditions may be unavoidable during an external shock like the pandemic, but education leaders and policymakers should endeavor to minimize negative consequences by bolstering the educational infrastructure in which educators are operating. Investing in systems and training (e.g., up-to-date technology, data systems and training to support data driven instruction, teacher pay incentives) in non-emergency times can also serve to prop teachers up in advance when launching and learning new systems is easier.

This study motivates two possible strands of future research. First, much of more recent evidence on school turnaround comes from the era of School Improvement Grants (SIG) (see, e.g., Redding & Nguyen, 2020), which defined turnaround interventions that had explicit requirements for dismissing and replacing teachers. Although states can choose to implement turnaround interventions that involve teacher replacement, recent federal policy does not require it. Researchers with turnover data that reliably delineate between voluntary and involuntary turnover, or with exit surveys, could isolate the effect of turnaround on voluntary turnover in particular so that any estimate would not be confounded with intentional turnover that might be beneficial to turnaround. Second, there is evidence that positive effects of turnaround on student

outcomes often are not sustained after the intervention (Pham et al., 2020, 2025; Stuit, 2010). Follow-up research might investigate what happens to teacher retention after the turnaround supports end. Finally, the third strand is related to the potential of educational policies to help schools withstand negative external shocks. The pandemic provided one lens through which to ask this question, but schools experience negative shocks on a more micro level every year and more macro levels not infrequently (e.g., recessionary or policy-induced decreases in school funding; natural disasters). Future research might investigate whether school or district interventions—including but not limited to turnaround—that are already in place appear to mitigate the negative effects of one of these shocks.

In conclusion, our study shows that the Partnership Model improved teacher retention in Michigan's first cohort of turnaround schools before the pandemic, and those improvements compounded during and after the pandemic's onset. Effects were more uneven in the second cohort, which experienced a lagged decrease in teacher turnover in the second two pandemic-impacted years. Together, our findings provide encouraging evidence that school turnaround policies—which states are implementing throughout the nation—may have the potential to dampen the negative effects of external shocks on education, at least with respect to teacher stability. However, the capacity for turnaround to play a protective role depends on the capacity the intervention built before the external shock occurred. Hiring and developing a sufficiently deep bench of experienced teachers at the beginning of the intervention may be centrally important to the school's ultimate resilience to external shocks.

[1] We drop the uncertain responses in our main analyses but note that our findings are robust to including these responses as a midpoint response.
[2] We note that this is an abbreviated version of culture and climate because we draw items that were asked in all four survey years, and we did not include items specific to in-person schooling during the 2020-21 school year.
[3] We also examined differential impacts for Black and White teachers (our sample did not include enough teachers in other groups for separate analysis). We did not find evidence that teachers responded differently by race to the

intervention in any year or cohort (Appendix Table A-6). Furthermore, there were not sizeable differences between the cohorts in the racial composition of their teaching force, as shown in Table 1.

[4] While we report statistical differences from *t*-tests, the difference in distributions is also significantly different ($p<0.05$) in both years based on the design-based *F*-statistic.

Tables

Table 1. Baseline Sample Descriptive Statistics

Panel A. Teacher level characteristics

	(1) Cohort 1	(2) Cohort 2	(3) All Partnership	(4) Comparison	(5) Non-PD Comparison	(6) All other
Black or African American	0.498	0.470	0.479	0.217	0.196	0.029
	(0.500)	(0.499)	(0.500)	(0.412)	(0.397)	(0.167)
Hispanic or Latino/a/x	0.017	0.016	0.016	0.027	0.013	0.012
	(0.130)	(0.125)	(0.126)	(0.162)	(0.114)	(0.108)
American Indian, Asian, Pacific Islander, 2+ races	0.034	0.044	0.041	0.029	0.025	0.018
	(0.183)	(0.206)	(0.198)	(0.167)	(0.157)	(0.132)
White	0.451	0.470	0.464	0.727	0.765	0.942
	(0.498)	(0.499)	(0.499)	(0.445)	(0.424)	(0.235)
Male	0.227	0.195	0.206	0.218	0.220	0.237
	(0.419)	(0.396)	(0.404)	(0.413)	(0.415)	(0.425)
New to district	0.084	0.096	0.092	0.139	0.166	0.048
	(0.278)	(0.294)	(0.289)	(0.346)	(0.373)	(0.215)
New to school	0.194	0.174	0.181	0.200	0.205	0.087
	(0.396)	(0.379)	(0.385)	(0.400)	(0.403)	(0.282)
Outcomes						
Left school	0.247	0.189	0.208	0.200	0.213	0.122
	(0.432)	(0.391)	(0.406)	(0.400)	(0.410)	(0.327)
Left district	0.193	0.136	0.155	0.149	0.178	0.073
	(0.395)	(0.343)	(0.362)	(0.356)	(0.382)	(0.260)
Left MI public education	0.015	0.018	0.017	0.036	0.044	0.023
	(0.120)	(0.132)	(0.128)	(0.187)	(0.204)	(0.150)
Observations	886	1,743	2,629	3,868	2,505	71,948

Panel B. School level student characteristics

	(1) Cohort 1	(2) Cohort 2	(3) All Partnership	(4) Comparison	(5) Non-PD Comparison	(6) All other
Black or African American	0.901	0.795	0.827	0.553	0.532	0.134
	(0.146)	(0.242)	(0.222)	(0.357)	(0.377)	(0.225)
Hispanic or Latino/a/x	0.040	0.070	0.061	0.099	0.069	0.078
	(0.084)	(0.144)	(0.129)	(0.174)	(0.138)	(0.109)
American Indian, Asian, Pacific Islander, 2+ races	0.028	0.050	0.043	0.059	0.057	0.079
	(0.037)	(0.073)	(0.065)	(0.057)	(0.056)	(0.086)
White	0.031	0.085	0.069	0.290	0.343	0.709
	(0.051)	(0.137)	(0.119)	(0.309)	(0.337)	(0.262)
Economically disadvantaged	0.858	0.876	0.870	0.813	0.802	0.510
	(0.067)	(0.090)	(0.084)	(0.159)	(0.178)	(0.245)
English learner	0.024	0.060	0.049	0.101	0.088	0.061
	(0.062)	(0.137)	(0.120)	(0.207)	(0.207)	(0.124)
Special education	0.198	0.178	0.184	0.145	0.135	0.180
	(0.086)	(0.069)	(0.075)	(0.077)	(0.078)	(0.198)
Enrollment	477.514	425.217	441.342	404.473	379.671	444.230
	(201.472)	(253.118)	(238.780)	(315.653)	(313.977)	(341.354)
Observations	37	83	120	203	146	3,074

Note: Descriptive statistics from 2016-17, the year before the first cohort implemented. Standard deviations provided in parentheses. Comparison group schools were low performing in the same timeframe as Partnership but did not receive the intervention (see Data, Sample, and Measures for details). The Non-PD Comparison drops non-Partnership schools in Partnership districts from the comparison group.

Table 2. Estimated Effects on Teacher Turnover Outcomes

		(1) Leave school	(2) Leave district	(3) Leave MI ed
Pre-treatment years	Cohort 2 t-3	-0.009	0.015	0.016
		(0.028)	(0.018)	(0.014)
	Cohort 1 t-2	0.015	0.015	0.027
		(0.042)	(0.022)	(0.018)
	Cohort 2 t-2	-0.011	0.019	0.013
		(0.027)	(0.017)	(0.013)
	Cohort 1 t-1	-0.048	-0.012	-0.002
		(0.036)	(0.023)	(0.016)
	Cohort 2 t-1	-0.034	0.008	0.002
		(0.025)	(0.015)	(0.011)
Treatment, Pre-pandemic	Cohort 1 Year 1 (2017-18)	-0.041	-0.059*	-0.039*
		(0.046)	(0.027)	(0.018)
	Cohort 2 Year 1 (2018-19)	0.005	0.018	0.013
		(0.028)	(0.018)	(0.013)
	Cohort 1 Year 2 (2018-19)	-0.023	-0.010	0.005
		(0.038)	(0.023)	(0.017)
Treatment, Pandemic-era	Cohort 2 Year 2 (2019-20, COVID Y1)	-0.003	0.035*	0.027*
		(0.026)	(0.014)	(0.012)
	Cohort 1 Year 3 (2019-20, COVID Y1)	-0.066*	-0.025	-0.022
		(0.033)	(0.024)	(0.016)
	Cohort 2 Year 3 (2020-21, COVID Y2)	-0.058*	-0.019	-0.010
		(0.026)	(0.016)	(0.011)
	Cohort 1 Year 4 (2020-21, COVID Y2)	-0.097**	-0.068*	-0.037*
		(0.036)	(0.029)	(0.018)
	Cohort 2 Year 4 (2021-22, COVID Y3)	-0.057*	0.003	0.021+
		(0.026)	(0.017)	(0.012)
	Cohort 1 Year 5 (2021-22, COVID Y3)	-0.099*	-0.048	-0.019
		(0.041)	(0.032)	(0.020)
	N	45,175	45,175	45,175
	F(1, 322) on Cohort 1 pretreatment coefficients	*F*=0.22 (*p*=0.639)	*F*=0.00 (*p*=0.945)	*F*=0.73 (*p*=0.394)
	F(1, 322) on Cohort 2 pretreatment coefficients	*F*=0.57 (*p*=0.452)	*F*=1.09 (*p*=0.298)	*F*=1.11 (*p*=0.294)
	F(1, 322) on all pretreatment coefficients	*F*=0.72 (*p*=0.395)	*F*=0.61 (*p*=0.435)	*F*=1.73 (*p*=0.190)
	Adjusted R^2	0.042	0.050	0.019
	Within R^2	0.007	0.006	0.004

Note: Estimates from two-way Mundlak models. All models include year fixed effects, baseline school covariates, and teacher demographics. School covariates, measured in 2016-17 and interacted with a linear time trend, include proportion of students by race/ethnicity, economic disadvantage, English learner status, special education status, and school enrollment. Teacher characteristics include race/ethnicity (Black, Hispanic or Latino/a/x, other race, with White as the reference category) and gender (male, with female as the reference category). Standard errors clustered at the school level. *F*-test on pretreatment coefficients tests whether the pretreatment coefficient estimates are jointly significantly different from zero. An insignificant estimate provides evidence for the conditional parallel trends assumption. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table 3. Estimated Effects on Teacher Mobility Outcomes by New to District Status

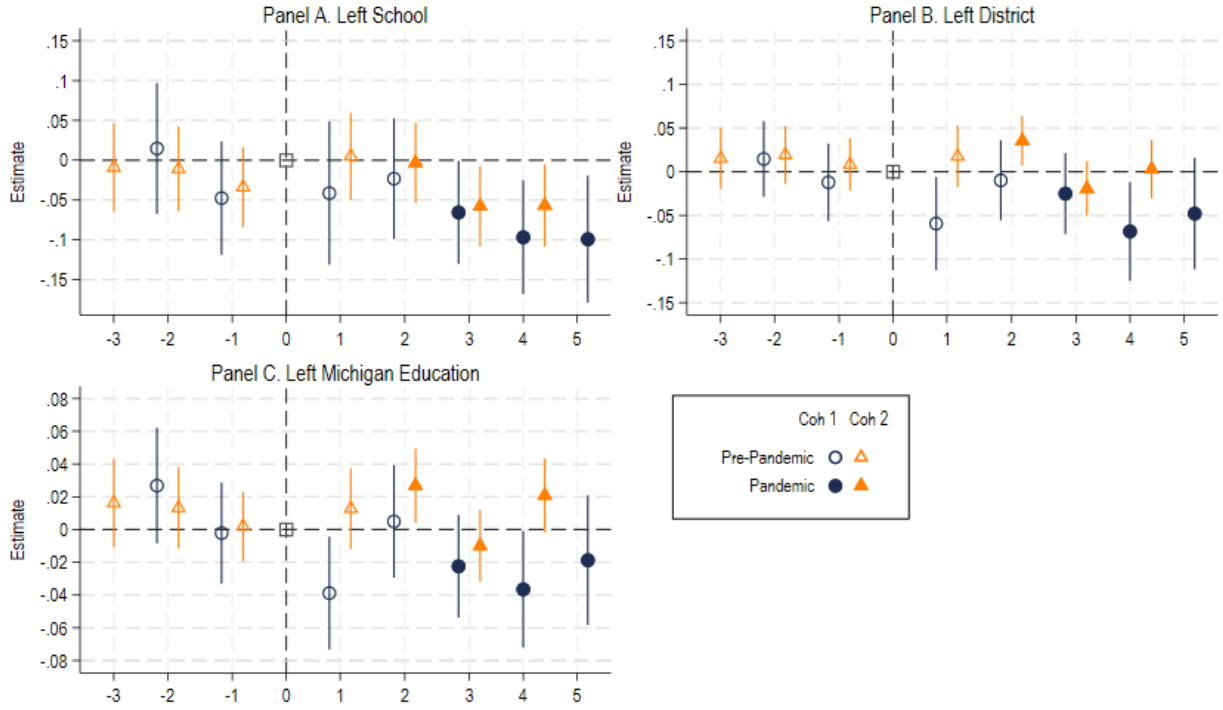
		(1) Leave school	(2) Leave district	(3) Leave MI ed
Pre-treatment years	Cohort 2 t-3	-0.009	0.015	0.016
		(0.028)	(0.018)	(0.014)
	Cohort 1 t-2	0.016	0.015	0.027
		(0.042)	(0.022)	(0.018)
	Cohort 2 t-2	-0.011	0.019	0.013
		(0.027)	(0.017)	(0.013)
	Cohort 1 t-1	-0.047	-0.012	-0.002
		(0.036)	(0.023)	(0.016)
	Cohort 2 t-1	-0.033	0.008	0.002
		(0.025)	(0.015)	(0.011)
Treatment, Pre-pandemic	Cohort 1 Y1, returning teacher (2017-18)	-0.063	-0.057*	-0.034*
		(0.047)	(0.027)	(0.017)
	Cohort 1 Y1, new to district teacher (2017-18)	0.043	-0.067+	-0.059*
		(0.058)	(0.038)	(0.026)
	Cohort 2 Y1, returning teacher (2018-19)	-0.013	0.004	0.008
		(0.029)	(0.018)	(0.012)
	Cohort 2 Y1, new to district teacher (2018-19)	0.093*	0.085**	0.035
		(0.041)	(0.031)	(0.023)
	Cohort 1 Y2, returning teacher (2018-19)	-0.042	-0.024	0.002
		(0.038)	(0.023)	(0.019)
	Cohort 1 Y2, new to district teacher (2018-19)	0.100+	0.082*	0.022
		(0.057)	(0.041)	(0.028)
Treatment, Pandemic-era	Cohort 2 Y2, returning teacher (2019-20, COVID Y1)	-0.022	0.029*	0.024*
		(0.026)	(0.015)	(0.012)
	Cohort 2 Y2, new to district teacher (2019-20, COVID Y1)	0.092*	0.069**	0.042+
		(0.036)	(0.025)	(0.022)
	Cohort 1 Y3, returning teacher (2019-20, COVID Y1)	-0.071*	-0.023	-0.019
		(0.034)	(0.024)	(0.017)
	Cohort 1 Y3, new to district teacher (2019-20, COVID Y1)	-0.027	-0.038	-0.044+
		(0.041)	(0.031)	(0.025)
	Cohort 2 Y3, returning teacher (2020-21, COVID Y2)	-0.057*	-0.016	-0.004
		(0.026)	(0.016)	(0.011)
	Cohort 2 Y3, new to district teacher (2020-21, COVID Y2)	-0.063	-0.044	-0.057**
		(0.038)	(0.027)	(0.018)
	Cohort 1 Y4, returning teacher (2020-21, COVID Y2)	-0.113**	-0.073*	-0.039*
		(0.035)	(0.029)	(0.019)
	Cohort 1 Y4, new to district teacher (2020-21, COVID Y2)	0.040	-0.031	-0.021
		(0.059)	(0.044)	(0.026)
	Cohort 2 Y4, returning teacher (2021-22, COVID Y3)	-0.068**	-0.003	0.019
		(0.026)	(0.017)	(0.012)
	Cohort 2 Y4, new to district teacher (2021-22, COVID Y3)	0.036	0.053	0.038
		(0.044)	(0.038)	(0.027)
	Cohort 1 Y5, returning teacher (2021-22, COVID Y3)	-0.108**	-0.046	-0.013
		(0.041)	(0.032)	(0.021)
	Cohort 1 Y5, new to district teacher (2021-22, COVID Y3)	-0.020	-0.061	-0.065*
		(0.064)	(0.053)	(0.030)
	N	45,175	45,175	45,175
	Adjusted R^2	0.044	0.051	0.020
	Within R^2	0.009	0.007	0.004

Note: Estimates from two-way Mundlak models. All models include year fixed effects, baseline school covariates, and teacher demographics. School covariates, measured in 2016-17 and interacted with a linear time trend, include proportion of students by race/ethnicity, economic disadvantage, English learner status, special education status, and school enrollment. Teacher characteristics include race/ethnicity and gender. Standard errors clustered at the school level.

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Figures

Figure 1. Event Study Plots, Leaving the School, District, and Michigan Public Schools

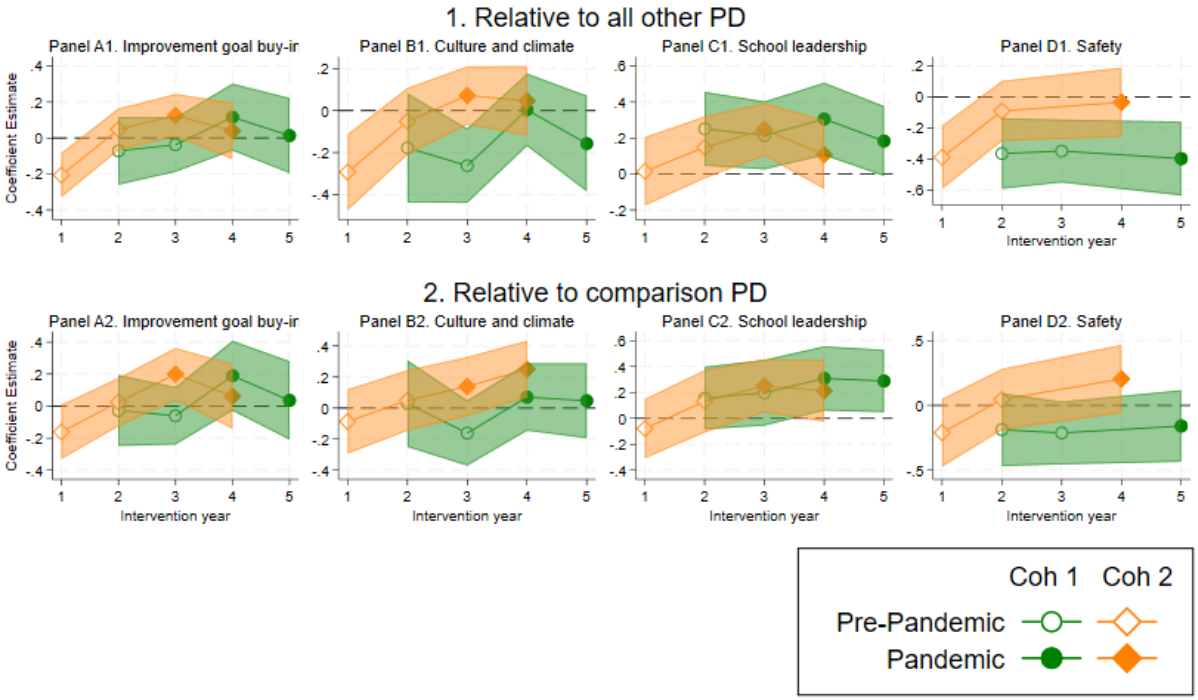


Note: Plots provide coefficient estimates from two-way Mundlak event study models predicting leaving the school (Panel A), leaving the district (Panel B), and leaving Michigan education (Panel C). Markers represent coefficient estimates on Cohort × Relative Year indicators (τ_k) and spikes denote 95% confidence intervals. Turnover is measured as of the end of the school year: e.g., if teacher *i* is in school *s* in year 1 but not in year 2, they would count as a turnover in year 1.

Figure 2. Teacher Perceptions of Likelihood of Consequences of Failure to Meet Improvement Goals

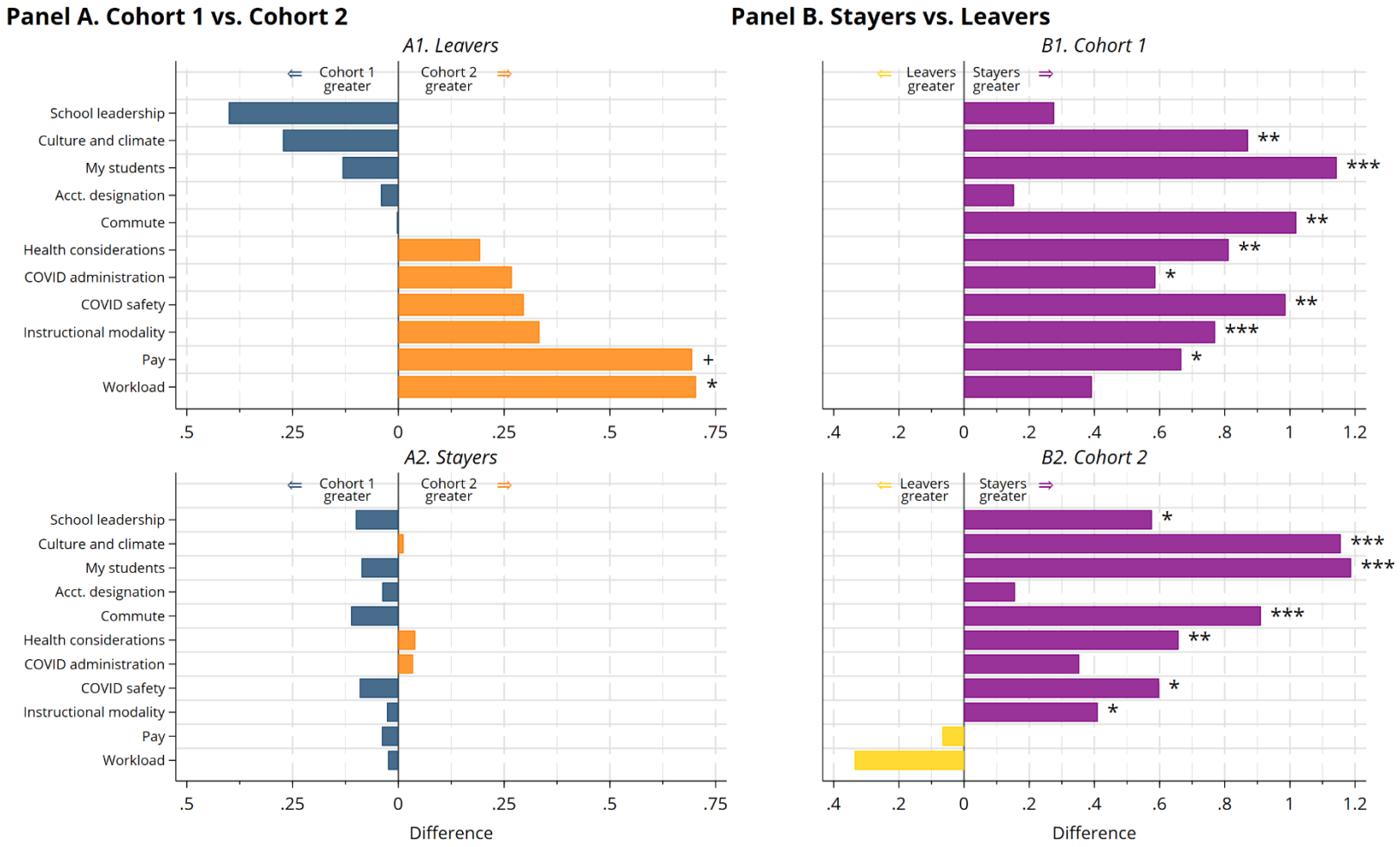
Note: Bar heights show weighted means of teacher responses to the question, "If your improvement goals are not met, to what extent do you believe your school will face the following consequences?" Stars denote significant differences (p<.05) based on t-tests comparing the two cohorts.

Figure 3. Patterns of Teacher Perceptions of Working Conditions over Time



Note: The survey was first given in fall of the 2018-19 school year, which was Year 2 of Partnership for Cohort 1 (green circles) and Year 1 of Partnership for Cohort 2 (orange diamonds). The first observation for Cohort 2 (Year 1 fall) assesses baseline perceptions at the start of the intervention; there is not a baseline measure for Cohort 1. These four constructs emerged from factor analysis of survey items (see Appendix Table A1). The items on school safety were not asked in 2020-21 due to the pandemic. Hollow markers denote pre-pandemic surveys and solid markers denote pandemic-era surveys (starting in Year 4 for Cohort 1 and Year 3 for Cohort 2).

Figure 4. Differences in Reported Reasons for Teacher Plans to Stay or Leave School, 2020-21



Note: Figure shows differences between cohorts (Panel A) and intended stayers/leavers (Panel B) in responses to question asking about the extent to which each item factors into a teacher's reported plans to stay in or leave their school the following year. Response options on five-point scale ranging from 1 (not a factor) to 5 (a primary factor). Stars denote significant differences in *t*-tests comparing Cohort 1 with Cohort 2 (Panel A) and intended stayers with intended leavers (Panel B). ***p<.001, **p<.01, *p<.05, +p<.10

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Supplemental Online Appendix

Table A1. Survey Response Rates by Year and Cohort

	Teachers			Principals		
	Cohort 1	Cohort 2	All	Cohort 1	Cohort 2	All
2018-19	42.6%	42.1%	42.3%	16.7%	33.3%	28.3%
	(361)	(755)	(1,116)	(5)	(23)	(28)
2019-20	59.3%	56.0%	57.1%	48.0%	51.9%	50.7%
	(471)	(854)	(1,325)	(12)	(27)	(39)
2020-21	46.4%	41.8%	43.3%	65.5%	66.2%	66.0%
	(387)	(683)	(1,070)	(19)	(47)	(66)
2021-22	32.5%	31.3%	31.7%	35.5%	41.8%	39.8%
	(296)	(539)	(835)	(11)	(28)	(39)

Note: Figures in cells provide the response rate (top) and the total number of responses (bottom, in parentheses). Percentages exclude individuals who responded that they were not eligible (i.e., not classroom teachers or principals) or who opted out. Percentages represent the share of those who responded at least partially. Partial responses include those that answered at least one question beyond the introductory questions.

Table A2. Factor Loadings

Panel A. Improvement Goal Buy-In

	Loadings	ψ
Goals are feasible	0.807	0.349
Goals focus on most important issues facing school	0.876	0.232
Goals help meet needs of students	0.882	0.223
Clear and concrete steps to improve student outcomes.	0.837	0.300
Efforts align with goals	0.826	0.318
N	9,525	
α	0.900	

Panel B. School Climate and Culture

	Loadings	ψ
Meet socioemotional needs	0.703	0.505
Meet academic needs	0.805	0.352
Teachers have strong rapport with students	0.736	0.458
Teachers have high expectations for students	0.750	0.437
Students enthusiastic to come to school/learn	0.679	0.539
N	8,716	
α	0.780	

Panel C. School Safety and Student Behavior

	Loadings	ψ
Safe and orderly environment	0.851	0.276
Fights are frequent (reverse-coded)	0.682	0.535
Teachers consistently enforce behavioral standards	0.662	0.561
Students listen to staff	0.774	0.401
Teachers manage behavior	0.825	0.320
N	6,476	
α	0.811	

Panel D. School Leadership

	Loadings	ψ
Work with staff to meet curriculum standards	0.898	0.193
Communicate central mission of the school	0.894	0.201
Use evidence to make data-driven decisions	0.895	0.200
Work with community partners	0.866	0.250
Facilitate and encourage PD	0.884	0.218
Encourage parental engagement	0.866	0.251
N	7,853	
α	0.944	

Note: Loadings in Column 1 from factor analysis (principal components factors) on teacher responses to each item over all four study years. Psi in Column 2 provides variation remaining in item not captured in factor.

Table A-3. Estimated Effects on Teacher Turnover Outcomes, reference year *t*-1

		(1) Leave school	(2) Leave district	(3) Leave MI ed
Pre-treatment years	Cohort 2 *t*-3	0.025	0.007	0.014
		(0.023)	(0.016)	(0.013)
	Cohort 1 *t*-2	0.062+	0.027	0.029+
		(0.035)	(0.026)	(0.017)
	Cohort 2 *t*-2	0.023	0.011	0.011
		(0.019)	(0.016)	(0.012)
	Cohort 1 Year 0 (identification year)	0.048	0.012	0.002
		(0.036)	(0.023)	(0.016)
	Cohort 2 Year 0 (identification year)	0.034	-0.008	-0.002
		(0.025)	(0.015)	(0.011)
Pre-pandemic treatment	Cohort 1 Year 1 (2017-18, pre-COVID)	0.006	-0.047*	-0.037*
		(0.036)	(0.022)	(0.017)
	Cohort 2 Year 1 (2018-19, pre-COVID)	0.039	0.009	0.011
		(0.028)	(0.019)	(0.012)
	Cohort 1 Year 2 (2018-19, pre-COVID)	0.024	0.002	0.007
		(0.032)	(0.023)	(0.017)
Pandemic-era treatment	Cohort 2 Year 2 (2019-20, COVID Y1)	0.030	0.027+	0.025*
		(0.023)	(0.016)	(0.012)
	Cohort 1 Year 3 (2019-20, COVID Y1)	-0.018	-0.013	-0.020
		(0.027)	(0.022)	(0.018)
	Cohort 2 Year 3 (2020-21, COVID Y2)	-0.024	-0.028	-0.012
		(0.025)	(0.018)	(0.012)
	Cohort 1 Year 4 (2020-21, COVID Y2)	-0.049+	-0.056*	-0.034*
		(0.028)	(0.023)	(0.017)
	Cohort 2 Year 4 (2021-22, COVID Y3)	-0.023	-0.005	0.019
		(0.024)	(0.020)	(0.012)
	Cohort 1 Year 5 (2021-22, COVID Y3)	-0.052	-0.036	-0.017
		(0.036)	(0.030)	(0.022)
	N	45,175	45,175	45,175
	F(1, 323) on Cohort 1 pretreatment coefficients	*F*=3.66 (*p*=0.057)	*F*=0.81 (*p*=0.369)	*F*=1.24 (*p*=0.267)
	F(1, 323) on Cohort 2 pretreatment coefficients	*F*=2.58 (*p*=0.109)	*F*=0.07 (*p*=0.789)	*F*=0.77 (*p*=0.379)
	F(1, 323) on all pretreatment coefficients	*F*=6.61 (*p*=0.011)	*F*=0.84 (*p*=0.360)	*F*=2.33 (*p*=0.128)
	Adjusted R^2	0.042	0.050	0.019
	Within R^2	0.007	0.006	0.004

Note: Estimates from Mundlak ETWFE models with reference year moved back to *t*-1. All models include year fixed effects, baseline school covariates, and teacher characteristics. School covariates include ED, Black, Hispanic, other nonwhite, EL, special education, and logged enrollment measured at baseline and interacted with linear year trend. Teacher covariates include gender and race. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A-4. Estimated Effects on Teacher Mobility Outcomes, No Partnership District Schools in Comparison Group

		(1) Leave school	(2) Leave district	(3) Leave MI ed
Pre-treatment years	Cohort 2 t-3	0.000	0.032	0.026+
		(0.030)	(0.021)	(0.015)
	Cohort 1 t-2	0.029	0.037	0.044*
		(0.044)	(0.024)	(0.019)
	Cohort 2 t-2	0.008	0.033	0.016
		(0.029)	(0.020)	(0.014)
	Cohort 1 t-1	-0.024	0.007	0.009
		(0.037)	(0.024)	(0.017)
	Cohort 2 t-1	-0.039	0.002	-0.006
		(0.027)	(0.018)	(0.012)
Pre-pandemic treatment	Cohort 1 Year 1 (2017-18, pre-COVID)	-0.036	-0.053+	-0.031+
		(0.047)	(0.029)	(0.019)
	Cohort 2 Year 1 (2018-19, pre-COVID)	0.036	0.026	0.019
		(0.028)	(0.021)	(0.014)
	Cohort 1 Year 2 (2018-19, pre-COVID)	0.013	0.005	0.019
		(0.039)	(0.026)	(0.019)
Pandemic-era treatment	Cohort 2 Year 2 (2019-20, COVID Y1)	0.009	0.044**	0.023+
		(0.026)	(0.017)	(0.013)
	Cohort 1 Year 3 (2019-20, COVID Y1)	-0.048	-0.011	-0.019
		(0.034)	(0.025)	(0.017)
	Cohort 2 Year 3 (2020-21, COVID Y2)	-0.060*	-0.024	-0.022+
		(0.029)	(0.021)	(0.013)
	Cohort 1 Year 4 (2020-21, COVID Y2)	-0.095*	-0.067*	-0.042*
		(0.040)	(0.033)	(0.020)
	Cohort 2 Year 4 (2021-22, COVID Y3)	-0.039	0.009	0.021
		(0.028)	(0.020)	(0.013)
	Cohort 1 Year 5 (2021-22, COVID Y3)	-0.076+	-0.036	-0.012
		(0.043)	(0.035)	(0.021)
	N	35,493	35,493	35,493
	F(1, 265) on pretreatment coefficients	*F*=0.26 (*p*=0.608)	*F*=4.52 (*p*=.034)	*F*=7.16 (*p*=0.008)
	Adjusted R^2	0.042	0.055	0.021
	Within R^2	0.007	0.006	0.004

Note: Estimates from Mundlak ETWFE models with no Partnership district schools in the comparison group. All models include year fixed effects, baseline school covariates, and teacher characteristics. School covariates include ED, Black, Hispanic, other nonwhite, EL, special education, and logged enrollment measured at baseline and interacted with linear year trend. Teacher covariates include gender and race.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A-5. Estimated Effects on Teacher Turnover Outcomes, with and without Covariates

	Leave school			Leave district			Leave MI education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-treatment									
Cohort 2 t-3	-0.011	-0.009	-0.009	0.015	0.015	0.015	0.019	0.016	0.016
	(0.028)	(0.028)	(0.028)	(0.017)	(0.018)	(0.018)	(0.013)	(0.014)	(0.014)
Cohort 1 t-2	0.013	0.015	0.015	0.016	0.015	0.015	0.031+	0.027	0.027
	(0.042)	(0.042)	(0.042)	(0.022)	(0.022)	(0.022)	(0.018)	(0.018)	(0.018)
Cohort 2 t-2	-0.012	-0.011	-0.011	0.020	0.020	0.019	0.015	0.013	0.013
	(0.027)	(0.027)	(0.027)	(0.016)	(0.017)	(0.017)	(0.013)	(0.013)	(0.013)
Cohort 1 t-1	-0.048	-0.047	-0.048	-0.011	-0.012	-0.012	-0.000	-0.002	-0.002
	(0.036)	(0.036)	(0.036)	(0.022)	(0.022)	(0.023)	(0.016)	(0.016)	(0.016)
Cohort 2 t-1	-0.034	-0.033	-0.034	0.008	0.009	0.008	0.003	0.002	0.002
	(0.025)	(0.025)	(0.025)	(0.015)	(0.015)	(0.015)	(0.011)	(0.011)	(0.011)
Pre-pandemic treatment									
Cohort 1 Year 1	-0.039	-0.041	-0.041	-0.060*	-0.060*	-0.059*	-0.040*	-0.039*	-0.039*
(2017-18)	(0.046)	(0.046)	(0.046)	(0.027)	(0.027)	(0.027)	(0.018)	(0.018)	(0.018)
Cohort 2 Year 1	0.006	0.005	0.005	0.019	0.018	0.018	0.012	0.013	0.013
(2018-19)	(0.028)	(0.028)	(0.028)	(0.018)	(0.018)	(0.018)	(0.012)	(0.013)	(0.013)
Cohort 1 Year 2	-0.020	-0.023	-0.023	-0.010	-0.010	-0.010	0.002	0.005	0.005
(2018-19)	(0.039)	(0.039)	(0.038)	(0.023)	(0.023)	(0.023)	(0.018)	(0.018)	(0.017)
Pandemic-era treatment									
Cohort 2 Year 2	-0.001	-0.003	-0.003	0.037*	0.036*	0.035*	0.025*	0.027*	0.027*
(2019-20,	(0.026)	(0.026)	(0.026)	(0.015)	(0.014)	(0.014)	(0.011)	(0.012)	(0.012)
COVID Y1)									
Cohort 1 Year 3	-0.061+	-0.066*	-0.066*	-0.026	-0.025	-0.025	-0.027+	-0.022	-0.022
(2019-20,	(0.033)	(0.033)	(0.033)	(0.024)	(0.024)	(0.024)	(0.016)	(0.016)	(0.016)
COVID Y1)									
Cohort 2 Year 3	-0.054*	-0.057*	-0.058*	-0.018	-0.018	-0.019	-0.012	-0.009	-0.010
(2020-21,	(0.025)	(0.026)	(0.026)	(0.016)	(0.016)	(0.016)	(0.011)	(0.011)	(0.011)
COVID Y2)									
Cohort 1 Year 4	-0.090*	-0.097**	-0.097**	-0.069*	-0.068*	-0.068*	-0.042*	-0.036*	-0.037*
(2020-21,	(0.036)	(0.036)	(0.036)	(0.029)	(0.029)	(0.029)	(0.018)	(0.018)	(0.018)
COVID Y2)									

	Leave school			Leave district			Leave MI education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cohort 2 Year 4 (2021-22, COVID Y3)	-0.051*	-0.056*	-0.057*	0.005	0.004	0.003	0.017	0.022+	0.021+
	(0.026)	(0.026)	(0.026)	(0.016)	(0.017)	(0.017)	(0.011)	(0.012)	(0.012)
Cohort 1 Year 5 (2021-22, COVID Y3)	-0.091*	-0.099*	-0.099*	-0.048	-0.047	-0.048	-0.026	-0.018	-0.019
	(0.040)	(0.041)	(0.041)	(0.032)	(0.033)	(0.032)	(0.020)	(0.020)	(0.020)
School covariates		X	X		X	X		X	X
Teacher characteristics			X			X			X
N	45,175	45,175	45,175	45,175	45,175	45,175	45,175	45,175	45,175
Adjusted R^2	0.041	0.041	0.042	0.048	0.048	0.050	0.018	0.018	0.019
Within R^2	0.005	0.006	0.007	0.004	0.004	0.006	0.002	0.003	0.004

Note: Estimates from Mundlak ETWFE models with no Partnership district schools in the comparison group. School covariates include ED, Black, Hispanic, other nonwhite, EL, special education, and logged enrollment measured at baseline and interacted with linear year trend. Teacher covariates include gender and race.

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A-6. Estimated Effects on Teacher Turnover Outcomes by Teacher Race

		(1) Leave school	(2) Leave district	(3) Leave MI ed
Pre-treatment years	Cohort 2 t-3	-0.009	0.013	0.013
		(0.029)	(0.018)	(0.014)
	Cohort 1 t-2	0.008	0.011	0.021
		(0.043)	(0.022)	(0.019)
	Cohort 2 t-2	-0.013	0.017	0.010
		(0.028)	(0.018)	(0.013)
	Cohort 1 t-1	-0.053	-0.016	-0.008
		(0.038)	(0.024)	(0.016)
	Cohort 2 t-1	-0.039	0.004	-0.001
		(0.026)	(0.016)	(0.012)
Treatment, Pre-pandemic	Cohort 1 Y1 (2017-18) White teacher	-0.047	-0.042	-0.022
		(0.053)	(0.029)	(0.019)
	Cohort 1 Y1 (2017-18) Black teacher	-0.037	-0.077*	-0.050*
		(0.048)	(0.032)	(0.022)
	Cohort 2 Y1 (2018-19) White teacher	-0.009	0.023	0.014
		(0.032)	(0.023)	(0.017)
	Cohort 2 Y1 (2018-19) Black teacher	0.026	0.013	0.007
		(0.029)	(0.019)	(0.015)
	Cohort 1 Y2 (2018-19) White teacher	-0.003	0.021	0.026
		(0.047)	(0.033)	(0.027)
	Cohort 1 Y2 (2018-19) Black teacher	-0.035	-0.045+	-0.020
		(0.039)	(0.024)	(0.017)
Treatment, Pandemic-era	Cohort 2 Y2 (2019-20, COVID Y1) White teacher	-0.002	0.028	0.016
		(0.030)	(0.019)	(0.016)
	Cohort 2 Y2 (2019-20, COVID Y1) Black teacher	-0.009	0.032*	0.025*
		(0.026)	(0.015)	(0.012)
	Cohort 1 Y3 (2019-20, COVID Y1) White teacher	-0.073*	-0.016	-0.023
		(0.036)	(0.025)	(0.019)
	Cohort 1 Y3 (2019-20, COVID Y1) Black teacher	-0.066+	-0.039	-0.032+
		(0.036)	(0.028)	(0.018)
	Cohort 2 Y3 (2020-21, COVID Y2) White teacher	-0.075*	-0.028	-0.022
		(0.030)	(0.018)	(0.013)
	Cohort 2 Y3 (2020-21, COVID Y2) Black teacher	-0.048+	-0.018	-0.008
		(0.029)	(0.017)	(0.013)
	Cohort 1 Y4 (2020-21, COVID Y2) White teacher	-0.139***	-0.082**	-0.045*
		(0.038)	(0.028)	(0.019)
	Cohort 1 Y4 (2020-21, COVID Y2) Black teacher	-0.080+	-0.067*	-0.037+
		(0.041)	(0.034)	(0.021)
	Cohort 2 Y4 (2021-22, COVID Y3) White teacher	-0.051+	0.017	0.023
		(0.028)	(0.020)	(0.016)
	Cohort 2 Y4 (2021-22, COVID Y3) Black teacher	-0.052+	-0.000	0.021
		(0.030)	(0.019)	(0.014)

	(1) Leave school	(2) Leave district	(3) Leave MI ed
Cohort 1 Y5 (2021-22, COVID Y3) White teacher	-0.090*	-0.026	-0.013
	(0.045)	(0.038)	(0.026)
Cohort 1 Y5 (2021-22, COVID Y3) Black teacher	-0.112**	-0.062+	-0.025
	(0.041)	(0.034)	(0.023)
N	42,599	42,599	42,599
Adjusted R^2	0.041	0.050	0.019
Within R^2	0.007	0.006	0.004

Note: Estimates from Mundlak ETWFE models on sample that is restricted to Black and White teachers. School covariates include ED, Black, Hispanic, other nonwhite, EL, special education, and logged enrollment measured at baseline and interacted with linear year trend. Teacher covariates include gender and race.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A-7. Teacher Survey Responses About Factors Contributing to Their Decisions to Stay in or Leave Their School

Panel A. 2020-21

	Cohort 1 vs. Cohort 2 within Intended Pathway								Stayer vs. Leaver within Cohort			
	Intended Stayers				Intended Leavers				Cohort 1		Cohort 2	
	(1) Coh. 1	(2) Coh. 2	(3) Diff. (1) – (2)	(4) *p*-value	(5) Coh. 1	(6) Coh. 2	(7) Diff. (1) – (2)	(8) *p*-value	(9) Diff. (1) – (5)	(10) *p*-value	(11) Diff. (2) – (6)	(12) *p*-value
School leadership	3.542	3.441	0.101	0.514	3.265	2.864	0.401	0.261	**0.277**	0.453	0.577	**0.016**
Culture and climate	3.612	3.624	-0.012	0.921	2.741	2.468	0.273	0.389	**0.871**	0.007	1.157	**0.000**
Workload	3.102	3.077	0.025	0.841	2.709	3.413	**-0.704**	0.024	0.392	0.199	-0.336	0.122
Commute	2.855	2.743	0.112	0.322	1.835	1.832	0.004	0.990	**1.019**	0.001	0.911	**0.000**
Pay	3.222	3.182	0.039	0.691	2.555	3.249	**-0.694**	0.052	0.667	0.043	-0.067	0.757
My students	3.168	3.080	0.087	0.422	2.024	1.892	0.132	0.640	**1.144**	0.000	1.188	**0.000**
School or district accountability designation	2.723	2.685	0.038	0.732	2.570	2.529	0.041	0.891	0.153	0.552	0.156	0.470
Pandemic-specific items, 2020-21 only												
Instructional modality	2.454	2.427	0.027	0.832	1.683	2.016	-0.333	0.157	**0.771**	0.001	0.410	**0.028**
COVID-19 safety precautions	3.256	3.165	0.092	0.506	2.270	2.566	-0.296	0.372	**0.987**	0.002	0.599	**0.010**
How administration treats teachers during pandemic	3.219	3.253	-0.035	0.788	2.632	2.900	-0.268	0.359	0.587	0.032	0.354	0.121
COVID-19 health considerations	3.197	3.237	-0.040	0.789	2.384	2.578	-0.193	0.539	**0.812**	0.005	0.659	**0.002**

Panel B. 2021-22

	Cohort 1 vs. Cohort 2 within Intended Pathway								Stayer vs. Leaver within Cohort			
	Intended Stayers				Intended Leavers				Cohort 1		Cohort 2	
	(1) Coh. 1	(2) Coh. 2	(3) Diff. (1) – (2)	(4) *p*-value	(5) Coh. 1	(6) Coh. 2	(7) Diff. (1) – (2)	(8) *p*-value	(9) Diff. (1) – (5)	(10) *p*-value	(11) Diff. (2) – (6)	(12) *p*-value
School leadership	3.537	3.206	**0.331**	0.030	3.086	3.222	-0.136	0.693	0.451	0.101	-0.017	0.952
Culture and climate	3.587	3.575	0.012	0.932	2.639	3.019	-0.379	0.145	**0.948**	0.000	**0.556**	0.007
Workload	3.099	3.020	0.079	0.543	2.835	3.153	-0.317	0.216	0.263	0.283	-0.133	0.502
Commute	2.874	2.825	0.049	0.748	1.905	1.922	-0.017	0.932	**0.969**	0.000	**0.903**	0.000
Pay	3.204	3.199	0.005	0.967	2.697	3.173	-0.476	0.149	**0.507**	0.048	0.027	0.918
My students	3.124	3.197	-0.073	0.577	2.548	2.236	0.311	0.202	**0.577**	0.014	**0.961**	0.000
School or district accountability designation	2.525	2.472	0.053	0.645	2.834	2.758	0.075	0.728	-0.308	0.064	-0.286	0.111

Note: Tables show weighted means and mean differences in teacher responses to question asking about the extent to which each item was a factor in their plans to stay (Columns 1 and 2) or leave (Columns 5 and 6) their school the following year. Differences denoted by column number and p-values are from t-tests with design-based standard errors that account for clustering of respondents in schools. Responses on five-point scale in which 1 is not a factor, 2 minimal factor, 3 moderate factor, 4 major factor, and 5 a primary factor. Bolded differences denote $p<0.10$.

Table A-8. Teacher Perceptions of Hindrances to Improvement and Pandemic-Related Challenges, by Cohort, 2020-21

Panel A. Hindrances to Improvement

	(1) Cohort 1	(2) Cohort 2	(3) Diff	(4) *p*-val
Insufficient access to technology	3.411	3.272	-0.139	0.141
Low student enrollment	3.084	2.962	-0.123	0.250
Low teacher attendance	1.506	1.506	0.000	0.994
Low teacher retention	1.720	1.804	0.083	0.281
Lack of availability of substitutes	2.104	2.352	**0.248**	0.011
Low quality of substitute teachers	1.995	2.190	**0.195**	0.040
Lack of student motivation to learn	3.848	3.854	0.006	0.938
Teacher demoralization/mental health during COVID	2.970	3.031	0.060	0.505
Lack of reliable transportation for students	2.182	2.126	-0.056	0.541
Parents engagement/support	3.741	3.835	0.095	0.229
Insufficient supply of certified teachers	2.050	2.216	**0.166**	0.084

Panel B. Pandemic-Related Challenges

	(1) Cohort 1	(2) Cohort 2	(3) Diff	(4) *p*-val
Emotional connections with students	2.690	2.634	-0.056	0.541
Building trust with students	2.384	2.312	-0.072	0.407
In-person classroom management	1.710	1.823	0.113	0.204
Online classroom management	2.197	2.374	**0.177**	0.046
Maintaining instructional continuity across modalities	2.542	2.808	**0.266**	0.005
Communicating with families	2.912	2.857	-0.055	0.526
Access to supplementary materials	2.747	2.783	0.036	0.709
Educating students with disabilities	2.944	2.971	0.027	0.780
Educating students who transfer from out of district	2.131	2.187	0.056	0.496
Educating students who transfer within district or modality	2.135	2.158	0.023	0.776
Educating students who do not consistently attend	4.328	4.344	0.016	0.822

Note: Tables show weighted means and mean differences between cohorts in teacher survey responses on five-point scale. Panel A provides responses to question asking about the extent to which each item is a hindrance to improvement goals, with response options of 1 not a hindrance, 2 slight hindrance, 3 moderate hindrance, 4 great hindrance, 5 greatest hindrance. Panel A provides responses to question asking about extent to which each item was a challenge in the classroom, with response options of 1 not a challenge, 2 minimal challenge, 3 moderate challenge, 4 major challenge, 5 greatest challenge. Bolded differences denote $p<0.10$.