STAFFING, RETENTION, AND EXPENDITURES IN THE FOUR-DAY SCHOOL WEEK

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ABSTRACT

School districts across the United States are increasingly adopting the four-day school week, often in the hope of addressing teacher staffing challenges. We examine the effects of the four-day week on non-teacher staffing and recruitment, and schoolwide expenditures in Missouri, where three in ten districts currently use it. Using a difference-in-differences design to identify the causal impact of four-day school week adoption, we find limited evidence that the policy impacts non-teacher staffing, nor do we find practically meaningful effects on retention. While we do see savings in some budget areas (e.g., operations), we observe an increase in tuition payments. Thus, overall, our findings suggest that four-day school week adoption does not result in meaningfully significant positive impacts on non-teacher staffing or retention, nor on schoolwide expenditures.

Keywords: staffing, retention, event study

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1 INTRODUCTION

School districts adopt the four-day school week (4DSW) in pursuit of increasing teacher retention and reducing costs. These perceived benefits have proven persuasive. Nationally, over 2,100 schools in 850 districts operated using 4DSW calendars in 2023, an increase from 1,600 schools in 650 districts during the 2018-19 school year (Morton et al., 2024). Adoption is concentrated among small, rural districts (Anglum & Park, 2021) with recent waves of adoption motivated by budget pressures following the Great Recession (Thompson, 2021) and a desire to improve teacher retention/recruitment following the COVID-19 pandemic (Barnes & McKenzie, 2025; Camp, Anglum, et al., 2026).

Notions of cost savings implicate a portion of the school workforce that research has largely overlooked. Non-teaching staff (e.g., paraeducators, custodians, and administrative staff) account for roughly half of public-school employment (National Center for Education Statistics, 2022). 4DSW promises for operational savings are, to a significant extent, focused on labor through fewer bus routes, fewer meals served, and one fewer day of building operations. Simultaneously, if the shorter week is an amenity that helps districts retain teachers, there is no obvious reason its appeal should stop at the classroom door, meaning retention in non-teaching roles could improve even if wages declined. Yet nearly all credible causal evidence on the calendar's workforce effects focuses on teachers.

There are strong reasons to expect that the impacts of adopting a 4DSW calendar may differ between teaching and non-teaching staff as well as between categories of non-teaching staff. Outside of certain certified positions (e.g., administrators or instructional coordinators), most non-teaching staff are paid on an hourly basis. Adopting a 4DSW calendar may reduce compensation for these employees. Even if school districts lengthen the school day to meet instructional hour

requirements, the longer school day may not fully offset lost wages as bus routes and food service work do not scale with instructional time. For salaried staff, compensation tends to be unchanged while the total number of working hours may decrease meaningfully. We might, therefore, expect the 4DSW to have positive retention effects on certified staff with cost-savings arising from reduced hours among non-certified staff, potentially at the expense of their retention.

However, existing evidence offers limited insight into this hypothesis. A growing quasi-experimental literature examines the calendar's effects on teacher recruitment and retention (Ainsworth et al., 2026; Bowser, 2025; Camp, 2024; Camp, Anglum, et al., 2026; Khalid, 2025; Lawson, 2025; Morton & Dewil, 2024; Nowak et al., 2023; Thompson, 2025) and largely finds that the calendar does little to improve either outcome. To our knowledge, only two studies extend to other school employees and find little evidence of reduced turnover for non-certified staff (Ainsworth et al., 2024; Bowser, 2025).

We study the effects of the 4DSW on non-teacher retention, staffing levels, and expenditures in Missouri, a state that is among the most aggressive adopters of the calendar nationally. The first Missouri district switched to the four-day week in 2010-11. By 2023-24, 172 districts—31 percent of the state's total—used the calendar. We combine individual-level staffing data covering the universe of public-school employees with detailed financial records and estimate difference-in-differences models that exploit this staggered adoption to identify effects of the 4DSW on non-teacher retention, district staffing levels, and per-pupil expenditures. Specifically, we address the following research questions:

(RQ1) Do 4DSWs improve non-teaching staff retention?

(RQ2) Does 4DSW adoption change the staffing levels used in districts?

(RQ3) Does a 4DSW reduce expenditures? If so, where are those savings concentrated?

We find little evidence that the 4DSW calendar has effects on these outcomes when aggregated across groups and spending categories, and we show that this aggregation conceals meaningful differences. We find evidence of moderate decreases in turnover among central office administrators and a shift in the composition of paraeducator turnover with and increased probability of moving to another school district. These patterns are consistent with the calendar's uneven effects on compensation. We additionally find little evidence that 4DSW calendar adoption meaningfully changes staffing levels in adopting districts or expenditures on non-certified salaries and show that any potential savings in these areas are offset by payments made by districts to other districts, private schools, or educational service providers. Importantly, this suggests that districts debating 4DSW adoption with the hopes of cost savings should also be cognizant of the possibility that some families respond to the calendar by enrolling their children elsewhere, which may come at the district's expense.

Our results builds on and extends the evidence surrounding the effects of four-day school weeks in several ways. Most notably, we leverage detailed school finance data and uncover a mechanism that is invisible when using more aggregate data: 4DSWs cause an increase in payments to other educational institutions. This may occur, for example, when students whose IEP mandates that they receive services daily are provided these services by another district or private school with the associated costs being paid by a student's home district. This result does not stand in contrast to existing evidence but rather highlights that 4DSW adoption may create additional obligations that can offset potential savings. Our results are important for districts considering adopting the shorter school week because they indicate that districts should not expect the four-

day week to provide relief from budget constraints, substantially alter staffing levels, or improve retention among student-facing staff.

2 Background & Prior Research

The 4DSW reduces the traditional 5-day school week schedule by eliminating one school day. Although there is substantial heterogeneity both within and across states (Morton & Klinenberg, 2026), in most instances this structure consists of using Friday as an off-day while implementing earlier day start times and longer school days. Despite the addition of time to each of the four remaining school days, students in schools implementing the four-day week typically still spend less time in school overall, especially engaged in math and reading (Thompson et al., 2021). Thus, this structure substantially reduces both student and staff time spent in school buildings; while about half of 4DSW schools offer activities on the day off, such as remediation/enrichment activities or teacher professional development on the day off, the other half do not (Thompson et al., 2021).

4DSWs have become much more common in recent years as states have allowed districts to meet instructional time requirements in terms of hours (or minutes) instead of days. Approximately half of states now have at least one district using the calendar (Morton et al., 2024). Researchers have attempted to understand this recent growth using a combination of qualitative and quantitative approaches. Interviews of education leaders indicate that the largest factors motivating recent adoptions have been perceived benefits to retention and recruitment, particularly among teachers (Barnes & McKenzie, 2025; Camp, Anglum, et al., 2026), as well as anticipated cost savings associated with the calendar (Kilburn et al., 2021). Quantitative analyses of what predicts adoption reveal a pattern also supported in qualitative research: the largest predictor of

adopting a 4DSW is proximity to a district already operating on a 4DSW calendar (Anglum & Park, 2021).

Despite the marked increase in districts operating on a four-day school week calendar and enthusiasm among some adopters, much of the extant literature on the topic demonstrates null, negative, or substantively small impacts on student outcomes, employee retention, and school finances.

2.1 *Impacts on Student Outcomes*

Developing students' cognitive and non-cognitive abilities is widely viewed as the primary function of schools and thus is a particularly high-priority driver of education policies. Most studies on the causal impact of 4DSWs on students, however, indicate that transitioning to fewer school days results in null or negative impacts on student achievement. For example, in a multi-state analysis of the impact of four-day school weeks on student academic achievement and growth, Morton et al. (2024) use eleven years of longitudinal student-level data to estimate the impact of a four-day school week policy on districts in Colorado, Iowa, Kansas, Montana, North Dakota, and Wyoming. They find that the shift to a four-day school week resulted in an average loss of 0.05 SD in mathematics and 0.06 SD in reading scores. Other multi-state studies (Thompson & Ward, 2022) and a study in Oregon (Thompson, 2021b) also reveal negative effects of the 4DSW on student achievement. To our knowledge, only one study has found evidence suggesting that 4DSWs may have small positive benefits on student learning (Anderson & Walker, 2015).

Studies examining non-test outcomes have similarly found mixed results. In Oregon, Thompson et al. (2022) find small increases in the probability of a student being absent on any particular day which, when combined with the reduction in the total number of school days, results

in moderate increases in the probability of a student being classified as chronically absent (i.e., missing more than 10 percent of school days). Morton (2021) finds that the switch to the 4DSW reduces bullying and fighting incidents by 31 and 27 percent, respectively. Overall, this literature suggests that the effects of the 4DSW on most student outcomes are not significant at best and often negative, raising concerns about the consequences of the transition to four-day school weeks, despite what might be perceived as promising potential.

2.2 Impacts on Teacher Retention

Although leaders believe that 4DSW adoption will lead to increased teacher retention and some qualitative research suggests these benefits may be meaningful (e.g., Turner et al., 2017), research finds that adopting the calendar often has no, or even negative, effect on teacher retention. Even where positive effects have been found, the magnitude of effects is substantively small. For example, estimates in Camp (2024) correspond to retaining an additional one teacher every two years for the average adopting district.

For example, in a recent study on the impact of four-day school weeks on teacher mobility in Missouri and Oklahoma, Thompson (2025) finds an overall null effect on teacher retention and finds that a ten percentage point increase in the prevalence of four-day school weeks in the local area increases teacher turnover by approximately 1.5 percentage points. The study indicates that overall teacher turnover was driven in part by teacher movement from five-day districts to four-day districts and that negative impacts on teacher exit were driven by non-white teachers and advanced degree holders. Similarly, Nowak et al. (2023) find heterogenous effects of four-day school week adoption on teacher turnover. In a study of a district outside of Denver, Colorado, the authors find that switching to a four-day school week resulted in a 4.5 percentage point decrease in the probability that a teacher is retained in district. Further, the impact was most significant for

teachers with 5-12 years of experience, who were 5.8 percentage points less likely to return after the district transitioned to a four-day school week.

These heterogenous findings are particularly important to attend to in conversation with policy discussions surrounding increasing the diversity of the teacher workforce and retaining qualified teachers, including those with more years of experience. Morton & Dewil (2024) similarly find negative or statistically insignificant evidence of effects of adoption on retention in Colorado. As another example, in a study of four-day school weeks in Oregon, Ainsworth et al. (2026) find that adoption of a four-day school week increased teacher turnover. In Missouri specifically, Camp et al. (2026) find that the move to a four-day school week has null effects on teacher turnover. Thus, findings across states suggest that despite some qualitative evidence of positive perceptions of four-day school weeks, quantitative evidence reflects a null or negative impact of the policy on teacher retention.

Taken together, the literature thus far suggests that effects of 4DSW on teacher turnover are often not distinguishable from zero, are occasionally negative, and when positive are substantively smaller than enthusiasm for the calendar would suggest.

2.3 Impacts on Non-Teacher Retention

The 4DSW literature has focused almost exclusively on teachers, with little attention to effects on other staff. Although some recent studies have begun to explore the impact of 4DSW adoption on non-teaching staff (Ainsworth et al., 2024; Bowser, 2025), there is significant room to investigate the impacts of 4DSW adoption on retention of other staff such as paraprofessionals and school or district leaders given that 4DSW implementation may impact staff differently based on their roles and responsibilities. The flexibility of a 4DSW could be beneficial for school leaders, for example, who spend substantial time on administrative activities (amounting to 30% of the school day,

Horng et al., 2010), and for whom the time demands can often make the principalship feel unsustainable, leading to burnout (Rogers et al., 2025). The same could be true for other non-teaching staff such as school psychologists, for example, who devote substantial time to paperwork and compliance, and experience high levels of burnout (Boccio et al., 2016; Pike & Floyd, 2024; Schilling et al., 2018; Schilling & Randolph, 2021). On the other hand, 4DSWs may be less attractive for some staff because of the ways that they lengthen the overall school day, and for hourly staff could result in reduced compensation. For paraprofessionals, for example, for whom salary may be an important lever in ensuring retention (Kaler & Theobald, 2026), this shift could be particularly consequential if staff are paid reduced wages if their full-time equivalency is lowered given an adjusted school calendar.

Despite the important roles that non-teaching staff play in schools and the potential effects of their turnover on students and teachers, empirical evidence on the effects of 4DSW adoption on non-teaching staff is sparse. For example, Ainsworth et al. (2024) study the effect of 4DSW adoption in Oregon and find that while adoption resulted in increased turnover for teachers, the effects are null for non-teaching staff, including administrators, paraprofessionals, licensed staff like librarians and counselors, and non-licensed staff such as office personnel and cafeteria workers. Similarly, Bowser (2025) finds no statistically significant changes in turnover for teachers, principals, or paraeducators as a result of 4DSW calendar adoption. Yet, more is left to be explored about the impacts of 4DSWs on non-teaching staff.

2.4 Impacts on School Finance

Another reason why school leaders and policymakers report transitioning to four-day school weeks is that they hope to reduce their district-wide operating costs. Studies of effects on expenditures indicate that switching to a four-day school week may marginally reduce school budgets. For

example, Griffith (2011) uses national finance data supplemented with data from individual districts and finds that most districts who adopted a 4DSW realized savings between 0.4% and 2.5% of their budget.

Less is known about the causal effects of school spending on particular categories, although two studies suggest that the adoption of the 4DSW may result in small reductions in spending. First, Thompson (2021) uses a difference-in-differences approach to estimate the decrease in total district expenditures per pupil as a result of 4DSW adoption. He finds that the 4DSW reduces operating expenditures per pupil by about 2-3%, with the largest statistically significant decreases in transportation and food services. In a study of four-day school week adoption in Oklahoma, Morton et al. (2021) found that among districts that transitioned to a four-day school week, the average district reduced their operating costs by about two percent. The primary areas in which they reduced their costs were in operations, transportation, and food services. Similarly, Bowser (2025) finds reductions in expenditures related to compensation for food services and transportation workers.

Overall, prior literature suggests that the adoption of the 4DSW may result in small reductions in spending (Bowser, 2025; Griffith, 2011; Morton, 2021; Thompson, 2021). However, the grain size of the categories available to these researchers is large, making it difficult to understand how schools might reallocate among particular budget items when making the choice to adopt the 4DSW. Furthermore, using aggregate expenditures data from sources such as the F-33 limits researchers' ability to describe the specific ways that expenditures shift, where savings are concentrated, where districts may incur additional costs, and how pass-through payments to other districts are reported in the data used. Given the evidence that how funds are spent matters for student achievement (Biasi et al., 2025; Lafortune et al., 2018) and the tight budgets faced by

school districts, including those considering 4DSW adoption, these questions are particularly important to understanding fiscal effects.

3 DATA

To answer our research questions, we use longitudinal administrative data covering the universe of Missouri traditional public and charter school employees. For analyses examining retention and staffing, we leverage restricted-use, individual-level data from the Missouri Department of Elementary and Secondary Education (DESE). To construct our analytic dataset, we combine these individual-level job assignment data with publicly available data on district expenditures, enrollments, and four-day school week calendars. All data are merged and analyzed at the district level.

We identify four-day school week adoption using a list of adopting districts made publicly available by DESE. [1] We define a district's adoption year as the first school year in which it operates on a four-day calendar. Through the 2023-24 school year, 172 Missouri districts adopted a four-day school week, with adoptions staggered across 14 school years. Figure 1 summarizes this staggered adoption, plotting the percentage of Missouri districts, staff, and students in four-day districts by school year.

Our individual-level job assignment data span sixteen school years, from 2008-09 through 2023-24. Each individual-by-year observation categorizes individuals into one of nine position types: central office staff, building principals, supervisors, media personnel, guidance personnel, teachers, other pupil support services, paraeducators, and ancillary personnel. We define non-instructional staff as individuals in all position codes other than teachers. After removing teachers

[1] We verify this list against one maintained by Dr. Jon Turner at Missouri State University.

from our analytic sample and restricting our comparison group (see Section 4.2), we are left with 60,406 person-by-year observations for our retention analyses and 6,357 district-by-year observations for our staffing analysis.

We examine turnover using these longitudinal job assignments data. Because different types of turnover may be informative about who is affected by 4DSW calendar adoption, we examine any turnover, movements to other districts in Missouri, and exits from the state's educator workforce. We define turnover as having occurred if an individual was employed in a district in one year and is not employed by that district in the subsequent school year, meaning that we observe turnover entering the 2009-10 through 2023-24 school years. While some studies define switching schools or roles as types of turnover, we do not include that in our measures for two reasons. First, the treatment we examine is a district-wide adoption of a four-day school week calendar, meaning that within-district between-school movements are unlikely to be in response to 4DSW adoption. Second, 4DSW adoption does not provide additional incentives for role changes to occur. To the extent that an individual may change roles, we classify them as retained if they remain in the same district and as having moved districts if their new role occurs in a different district than the one they were employed by in the preceding year.

We combine our individual-level job assignment data with detailed district-level expenditure data reported by each district to DESE through the Annual Secretary of the Board Report (ASBR). These reports are required to be submitted by districts at the end of each fiscal year and are routinely reviewed by DESE for accuracy. Individual district-by-year reports are publicly available and collected using automated scraping to generate a combined dataset spanning

the 2000-01 through 2023-24 school years.[2] We restrict our expenditure analyses to the 2009-10 through 2023-24 school years, the period over which the expenditure categories we use are consistently defined, yielding 6,351 district-by-year observations in our final analytic sample.

Expenditure data from these reports is reported by both a function code (the *purpose* of the spending; e.g., transportation, food service, etc.), and an object code (*what* was purchased; e.g., certified salaries, supplies). Together, these codes produce detailed reporting categories, allowing us to discern, for example, between salaries paid for student dental services and supplies purchased for the same services. We leverage the granularity of these data, as well as accounting identities, to understand how district expenditures change after adopting a 4DSW calendar. Unless otherwise noted, we use per-pupil current expenditures reported in constant 2024 dollars[3] and aggregated across seven broad object-based categories: certified salaries, non-certified salaries, benefits, purchased services, supplies, capital outlay, and other expenditures.[4]

Finally, we merge several publicly available district-level measures. District enrollments, drawn from public records maintained by DESE, serve as the denominators for our per-pupil expenditure and FTEs-per-100-students staffing measures.[5] We additionally assign each district its NCES locale designation and geographic coordinates, which we use to construct the rural comparison groups and the distance-based spillover analyses described in sections 4.2 and 4.3.

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[2] To ensure that the data generated through our web scraping process is accurate, we compare figures from this dataset with publicly reported figures elsewhere available (e.g., total revenues and expenditures, assessed valuation). We exclude six district-year reports where web-scraped and reference data disagreed meaningfully and one district-year report where the underlying data appeared to be corrupted.

[3] We perform this inflation adjustment using core CPI (consumer price index less energy and food) averaged over the school fiscal year.

[4] In our results, we focus on current expenditures an exclude capital outlays in our results. Additionally, we exclude other expenditures from our tables as dollar amounts in these categories are almost entirely equal to zero.

[5] These records, alongside the previously discussed calendar data are available at https://dese.mo.gov/school-data.

Table 1 reports descriptive statistics for Missouri districts as of the 2023-24 school year, organized by district, teacher, and student characteristics. Panel A compares the 172 districts operating a four-day week in 2023-24 to the 380 districts operating a traditional five-day calendar. The two groups differ significantly on nearly every characteristic we observe: four-day districts enroll fewer students, operate fewer schools, employ fewer staff, and are overwhelmingly located in rural and remote locales.

Panel B repeats these comparisons, restricting the comparison group to the 224 rural five-day districts. Here, nearly all differences disappear. The only exceptions are the share of districts located in town locales, which differs by construction because the rural comparison group excludes town districts while a handful of four-day districts sit in town locales, and average teacher salary, with teachers in four-day districts earning $1,537 less per year on average. Taken together, Table 1 shows that four-day districts look little like the typical Missouri district but closely resemble other rural districts, a pattern we return to when constructing our comparison group in section 4.2. Because these characteristics are measured after most districts adopted a 4DSW calendar, some differences may partly reflect adoption itself; our estimation strategy does not rely on these levels, however, but on within-district changes over time.

4 IDENTIFICATION STRATEGY

We identify the effects of adopting a 4DSW calendar on school district staffing, retention, and expenditures using a difference-in-differences design. As a simplified illustration of this approach, consider a scenario in which we are interested in identifying the effects of 4DSW adoption on outcome Y_{jt}, where j indexes district and t indexes school year. A naïve estimate of effects would be given by the two-way fixed effects (TWFE) specification shown in Equation (1).

$$Y_{jt} = 4\text{DSW}_{jt} \cdot \beta_1 + \lambda_j + \gamma_t + e_{jt} \quad (1)$$

Here, λ_j are district fixed effects that capture time-invariant differences between districts, γ_t are school year fixed effects that capture common shocks (e.g., a change in state laws affecting expenditures or broader labor market conditions that affect retention), 4DSW_{jt} takes a value of one for district-year observations operating on a 4DSW and zero otherwise, and the coefficient β_1 is the parameter of interest which is interpretable as the causal effect of 4DSW adoption on Y under several assumptions.

First, we must assume that absent adoption, the mean potential outcome for adopting districts would have evolved in parallel with comparison districts, conditional upon district and year fixed effects. This is the familiar parallel trends assumption common to all difference-in-differences identification strategies. Second, we must assume no anticipation; that adoption has no effect on outcomes before it occurs. Third, we must assume that adoption in district j does not affect the outcomes of district j's untreated neighbors. We discuss our approach to evaluating the credibility of these assumptions and present supporting evidence in Section 4.3 and Appendix D.

However, recent econometric work has cast doubt upon the use of naïve difference-in-difference estimators such as the one specified by Equation (1) in setting where treatment is staggered across several adopting cohorts. Our preferred estimator[6] uses a two-stage procedure proposed by Gardner et al. (2024), represented by Equation (2).

$$\begin{aligned} Y_{jt} &= \phi_j + \psi_t + \nu_{jt} \quad \text{for all} \quad \text{4DSW}_{jt} = 0 \\ Y_{jt} - \hat{\phi}_j - \hat{\psi}_t &= \text{4DSW}_{jt} \cdot \pi_1 + \epsilon_{jt} \end{aligned} \tag{2}$$

[6] In Appendix A, we show that we obtain qualitatively similar results using other heterogeneity-robust difference-in-differences estimators. However, we prefer the two-stage estimator here as it allows for a richer set of comparison districts, a decision we discuss in detail in section 4.2. Additionally, the two-stage procedure accounts for first-stage estimation error via a generalized method of moments formulation and are clustered at the district level; i.e., the level of treatment.

This approach first obtains estimates of district and year fixed effects that are uncontaminated by heterogeneous treatment effects by restricting the estimation sample to only untreated observations. Following this, Y_{jt} is residualized on estimates of district and year fixed effects and regressed onto the treatment indicator 4DSW_{jt}, leading to unbiased and efficient estimation of effects, averaged across all treated district-year observations, under the first three assumptions previously mentioned.

In all specifications, we cluster standard errors by district, matching the level of treatment assignment (Abadie et al., 2022). Because the second stage treats the first stage estimates of district and year fixed effects as known quantities, conventional standard errors would understate sampling uncertainty. We therefore compute standard errors using the generalized method of moments representation of the two-stage procedure, which accounts for estimation error in the first stage (Gardner et al., 2024).

Using the two-stage difference-in-differences estimator, the comparison group is not a separate design choice but the composition of the first stage estimation sample. Observations from eventual adopters prior to adoption contribute under any construction; the candidates for comparison groups in our analysis vary only in the never-treated districts they retain. In Appendix B, we consider three possible sets of the comparison group: including all never-treated districts, only not-yet-treated observations, and restricting not-yet and never-treated rural districts. Based upon results presented there, we select a comparison group comprised of rural never-treated and not-yet-treated observations, though we note that our results are not sensitive to this choice.

Having selected our choice of estimator and preferred comparison group, we now turn to transformations of our outcome variables. Our analysis examines two families of outcomes. The first family concerns individual-level retention and district-level staffing. Following Camp et al.

(2026), we define retention as individuals who worked in a district in year $t-1$ returning to that district in year t. Because different types of turnover can be informative about what margin, if any, the 4DSW effects operate on, we consider any turnover as well as turnover resulting in individual movements to another Missouri district/charter and turnover resulting in an exit from the Missouri public education system entirely. We similarly examine staffing as the number of full-time equivalent employees in a district-year-position per 100 students. We estimate Equation (2) for all non-teaching roles and four discrete positions: central office staff, principals, guidance personnel, and paraeducators.[7]

The second family of outcomes are district expenditures per pupil, expressed in constant 2024 dollars, disaggregated by function and object codes using Missouri's Annual Secretary of the Board reports. We estimate expenditure effects in dollar levels rather than logarithms because dollar-per-pupil effects are additive—the effect on total expenditures equals the sum of effects across mutually exclusive categories. This accounting identity permits decomposition of expenditure effects, thereby allowing us to better identify the ways in which the 4DSW calendar affects district expenditures.

Both our results examining staffing levels and expenditures share a denominator of concurrent student enrollment. This raises the concern that enrollment itself may be affected by 4DSW adoption, meaning that our results would carry both true effects on the outcomes of interest and effects on student enrollments. The enrollment event studies in Appendix B show no detectable effects, indicating that our results for staffing or expenditures are unlikely to be driven by changes

[7] In results available upon request, we similarly estimate effects on retention and staffing levels for supervisors, media personnel, other pupil services, and ancillary staff. However, we do not report these results here as the comparatively small number of individuals in these positions results in imprecise estimation and instead focus on the positions for which we have a sufficient sample size. Individuals in these positions are included in the results for all non-instructional positions.

in student enrollments in adopting districts. Still, there is a concern that minute enrollment effects could occur, leading to marginal contamination of our estimates. To further show that our results are not sensitive to this choice of denominator, in Appendix C we estimate effects fixing post-treatment enrollment figures at their pre-treatment levels, meaning that by construction our results could not suffer from enrollment effect contamination.

Each regression is unweighted, so that retention results reflect the retention decisions of the average person in that position and staffing and expenditure results reflect the effects for the average adopting district, a more policy relevant estimand than weighting by enrollment.

5 RESULTS

Identification of causal effects in our setting requires three assumptions: parallel trends in mean potential outcomes, no anticipatory effects, and no spillovers. For brevity, we discuss our approach to spillovers in Appendix D and here focus on the first two of these assumptions which are common in the difference-in-differences literature. Evidence in support of the first two of these assumptions comes from event study analogs of Equation (2).

In each event study, coefficients are normalized relative to the school year immediately preceding adoption (i.e., the last five-day calendar year). Anticipatory effects, if present, would then result in pre-period indicator coefficients consistently appearing above or below zero because the anticipatory effect would be subsumed by the reference period. Lack of a clear deviation in pre-period trends between $t-2$ and $t-1$ is evidence against meaningful anticipatory effects. Furthermore, it is worth noting that the decision to adopt a 4DSW calendar typically occurs during the school year preceding adoption. The outcomes we study—retention entering each year, district staffing levels, and district expenditures—are unlikely to be affected by anticipated adoption

because individual and district decisions typically occur before the move to a four-day school week is formally decided.

Figures 2 through 4 present event studies for each family of outcomes.[8] Across all three figures, pre-adoption coefficients are small in magnitude and statistically indistinguishable from zero, with no deviation between the final two pre-adoption years where anticipatory responses would be most likely to appear. This pattern supports both the assumption that adopting and comparison districts were evolving in parallel before 4DSW adoption and the assumption that individuals and districts did not adjust their behavior in anticipation of adoption.

5.1 Effects on Retention

Our overall estimates of 4DSW adoption on turnover, both for all non-instructional staff and by role, are reported in Table 2. In the first column, we observe a statistically significant but small retention effect on the number of all non-instructional staff leaving the workforce. All else equal, adopting a 4DSW calendar leads to a 1.5 percentage point lower probability of exiting the Missouri education workforce, an 11 percent reduction relative to the pre-treatment mean. Reading across the second through fifth columns of Table 2, we see that this effect is most pronounced among central office staff. When our analytic sample is restricted to central office staff, the point estimate (column 2, row 3) is nearly twice as large as when the sample includes all non-instructional staff, though we note that this effect is imprecisely estimated and only marginally significant at the 90% confidence level.

Our event studies for retention, Figures 2 and 5, are informative about the dynamics of these retention effects. In both figures, the reduction in any turnover appears in the first years after 4DSW adoption. Figure 5, which displays event studies for the any turnover outcome separately

[8] See Appendix F for additional event studies.

by role, shows that while the initial retention effects for central office staff are large, they appear to fade out after three years of adoption. Event studies by position for the moving district and workforce exits outcomes are available in Appendix F and support this interpretation.

Retention effects appear to differ by role; while estimates of the effects on the moving district outcomes are insignificant for most groups, we find that paraeducators were 1.8 percentage points more likely to move out of a 4DSW district to another district, a 41 percent increase over the pre-treatment mean. This increase in mobility across districts indicates that even if we take the negative point estimate of any turnover at face value, this does not necessarily translate into improved retention for adopting districts broadly speaking.

5.2 Effects on Staffing Levels

Next, we explore the impact of transitioning to a 4DSW on staffing levels for distinct non-instructional positions, measured as FTE per 100 students. We find predominantly null effects of a 4DSW on staffing levels, with a small but marginally significant increase in paraeducators per 100 students (see Table 3). Event study estimates in Figures 3 and 6 similarly reflect largely no discernible effects.

5.3 Effects on Expenditures

Given the absence of staffing reductions, any cost savings from 4DSW calendar adoption would need to operate through channels other than staff headcount (e.g., reduced fuel for transportation or food for student meals), reduced hours among classified staff who are paid by the hour/day (vs. salaried), or changes within staffing categories too small to estimate with precision. We investigate these potential expenditure effects in Table 4. Panel A reports estimated effects on per-pupil expenditures, with function codes (the purpose of the purchase) across the rows and object codes (what was purchased) across the columns. The first row aggregates across all functions while the

final “Total” column aggregates across all objects. The top-right cell therefore reports the effect of 4DSW adoption on total current expenditures; reading across columns and rows decomposes this total into progressively more granular categories.

Across the first row, we find no statistically significant change in expenditures on salaries for certified employees, non-certified employees, employee benefits, purchased services, or supplies. The estimated effect on total expenditures is likewise insignificant and, taken at face value, would represent an increase of less than half of one percent relative to the pre-adoption mean. The corresponding event study (Figure 4) shows no evidence of differential pre-adoption trends with an apparent drift upward through the fourth post-adoption year before turning negative in the fifth year, with confidence intervals including zero throughout.

The aggregate null conceals movement within district budgets, however. Because per-pupil dollar effects are additive, the cells of Table 4 decompose the total effect exactly, and the decomposition reveals a single statistically significant increase set against diffuse reductions elsewhere. Adopting districts increase spending on tuition and conduit payments by $111 per pupil, an increase of approximately 21 percent relative to the pre-adoption mean. This row accounts for essentially all of the district-wide movement in purchased services: net of tuition payments, the all-functions purchased service estimate of $104 falls to roughly -$7. Set against the tuition payment increases are reductions located precisely where operational savings would be expected: non-certified salaries within operations and maintenance (-$24 per-pupil; 5.6 percent), transportation supplies (-$12 per-pupil; 7.6 percent); and total food service spending (-$30 per-pupil; 3.7 percent). Each is marginally significant at the 90% confidence level and together they amount to roughly $66 per-pupil, or about 0.4 percent of total spending. No other cell in the table reaches even a marginal level of significance.

The increase in tuition and conduit payments is among the strongest and most consistent findings across all our analyses. We examine this finding in more detail in several figures. Figure 7 shows estimates that are flat and near zero throughout the pre-adoption period, with the effect emerging in the year of adoption and growing through the third post-adoption year; the estimate in Table 4 is therefore not an artifact of adopting districts already trending toward greater tuition spending. To identify the channels behind this growth, we leverage more detailed budgetary data available from the 2014-15 school year onward, which distinguish four components of the tuition and conduit category. Figure 8 reports event studies for the intensive margin (per-pupil dollars) for each component. Here, we see that the increase is almost entirely contained in tuition paid to other districts, private schools, and virtual programs with no notable movement in area career center fees, special education tuition, or contracted educational services.

The special education component of Figure 8 should be interpreted in light of how Missouri's financial accounting system defines these categories. These payments are reimbursements for special education and related services delivered by another provider rather than the base tuition of students with disabilities; base tuition payments for students with disabilities would be tracked by the tuition paid to other districts, private schools, and virtual programs category. Two limitations of Figure 8 are apparent due to this coding scheme: aggregate flows cannot distinguish full tuition payments for a small number of students from partial tuition payments to a larger set of students (e.g., individual virtual courses). Additionally, small reimbursement flows are nearly invisible at this resolution: five students each generating $1,000 in annual service reimbursement for special education students would amount to a $5 per pupil increase in special education payments for a district of 1,000 students.

Figure 9 complements Figure 8 by examining the extensive margin for these payments—i.e., the probability that a district makes any payment within each component. This is informative not about how much money moves but about whether new payment relationships form. Here the pattern inverts. The probability of making any district, private, or virtual tuition payment is unchanged, indicating that the dollar growth in Figure 8 occurs among districts already making such payments. The probability of making any special education reimbursement, by contrast, rises by approximately 8 percentage points following adoption (22% relative to a pre-adoption mean of 35.4%) and is the only component where we find extensive margin effects. Read jointly, Figures 8 and 9 identify distinct features of the same underlying flows: the dollars concentrate where base tuition is coded while the extensive margin reveals that adopting districts increasingly make at least some payments for special education services delivered by another provider.

Together, these results are consistent with a scenario in which adopting districts agree to pay tuition for a small number of students to enroll in another district, private school, or virtual program; following 4DSW adoption and in which a non-trivial share of these students receive relatively inexpensive special education services from their new providers. Because state reporting attributes tuition-out students to their resident district, these students remain in the adopting district's enrollment counts; the shift registers in the budget as outgoing tuition rather than as enrollment loss. Supplementary analyses (available upon request) lend further texture: district-level increases in out-of-district tuition arrive in rough multiples of $10,000, a lumpiness more consistent with full tuition payments for individual students than with many small per-course payments, though our data cannot rule out the latter. It is essential to note, however, that we cannot rule out other alternatives such as changes to contracts or accounting reclassification.

Whatever the precise mix, the fiscal implications of this finding are the same. The point estimates imply that the modest operational savings that adopting districts realize—in transportation, food service, and operations payroll—are more than fully consumed by rising tuition payments. On net, we find no evidence that districts save money by adopting a four-day school week; the budgetary consequence of adoption is compositional, shifting resources from districts' own operations toward payments that follow students to outside providers.

6 DISCUSSION

As four-day school weeks continue to expand, so does interest in understanding what the calendar does (and does not) change. We provide new evidence on a segment of the school workforce and a set of budget flows that prior research has been largely unable to observe. Our results show that 4DSW adoption has limited effects on aggregate non-teacher staffing and total district spending, but that these aggregates conceal meaningful shifts in who turns over and where the money goes.

First, we find little evidence that the 4DSW affects turnover for most groups of non-teaching staff, consistent with Ainsworth et al. (2024) and Bowser (2025). Tow exceptions emerge. Central office staff become less likely to exit the education workforce, though the effect fades by the third year after adoption. We interpret this result cautiously because central office staff may also be in the individuals championing and implementing the transition, so their elevated retention may reflect the commitments of leaders invested in a signature initiative rather than the calendar's value as a workplace amenity—a form of individual-level selection our district-design cannot rule out. The pattern is consistent with the generally positive perceptions superintendents report (Barnes & McKenzie, 2025; Camp, Anglum, et al., 2026) even where measurable staffing benefits fail to materialize. We also note that administrators are rarely the stated target of the policy, and far less is known about the consequences of central office turnover than of teacher or principal

turnover. The second exception is paraeducators, who become more likely to move to other districts with no change in their overall turnover. This compositional shift is consistent with the calendar's uneven incidence on compensation: hourly staff plausibly lose earnings under a shorter week and, unlike salaried employees, have reasons to recover them elsewhere.

Second, we find no evidence that this substantial restructuring of the school week leads districts to meaningfully reorganize their staffing. Adopting districts continue to operate with position mixes similar to their pre-adoption arrangements. Changes invisible to administrative positions codes (e.g., reallocated duties within existing roles or new off-day responsibilities) remain possible, a measurement point we return to below.

Third, adoption produces real but modest operational savings concentrated in transportation, food service, and operations payroll that are consistent in location and magnitude with prior estimates. Our contribution is to show where those savings go. Tuition and conduit payments to other districts, private schools, and virtual programs rise by more than operational spending falls, leaving total expenditures unchanged, and the extensive-margin growth in special education service reimbursements suggests that a nontrivial share of the students behind these flows receive special education services from their new providers. Because payments to other districts are largely obfuscated in the current-expenditure aggregates in the federal finance data prior studies relied upon, this offsetting channel was structurally invisible to them. Our findings therefore complement rather than contradict the small savings results in that literature: the savings are there, but so are new obligations that outpace them.

6.1 Implications

The strongest predictor of 4DSW adoption is proximity to an existing adopter (Anglum & Park, 2021), a diffusion pattern that appears to operate somewhat independently of evidence that the

calendar delivers measurable benefits for students, staff, or budgets. For leaders weighing adoption to address staffing shortages, our results counsel modest expectations: shortening the week does not, by itself, improve retention among student-facing staff, and for hourly employees it may encourage movement to neighboring districts. Policy tools that act directly on working conditions and compensation may be better matched to the staffing problems that motivate adoption.

The fiscal implications of our findings also temper expectations about potential benefits of four-day school weeks. Operational savings are real but small, and on average they are more than offset by new tuition obligations. Prospective adopters budgeting for savings should also budget for the possibility that some families respond to the calendar by enrolling children elsewhere at the district's expense. Even the clearest savings carry distributional questions: reduced food service spending means fewer meals served, a potential cost for food-insecure students that is not reflected in district budgets. In particular, the increase in out-of-district spending could provide further context for the literature suggesting that student achievement decreases when the 4DSW is adopted. Indeed, disenrollment may be an indicator of dissatisfaction with educational services received in a 4DSW, reflecting that families may be changing schools due to worsening outcomes or changing schools due to the *perception* that outcomes may decline.

Finally, that stability of staffing arrangements suggests that districts are not treating adoption as an occasion to rethink how adult time is deployed. To the extent the calendar creates slack through days off and longer school days, strategic staffing approaches (e.g., Laski et al., 2026) could, in principle, convert it into new configurations of staff time; we see no evidence in administrative records that this is occurring.

6.2 Limitations and Next Steps

Several limitations qualify these conclusions and suggest areas for future inquiry. Most importantly, our expenditure data record dollars and not students directly. We observe district-level tuition flows but not the students who generate them, the providers who receive them, or whether payments reflect full tuition for a few students or partial tuition (e.g., individual virtual courses). The interpretation we advance, in which a small number of students enroll outside their resident district is the reading most consistent with the joint intensive- and extensive- margin patterns and the lumpiness of payment changes, but it remains an inference from financial records. Student-level enrollment and transfer data could confirm it directly, identify receiving providers, and speak to consequences for the students involved—particularly students with disabilities for whom continuity of services is a significant educational concern.

Second, while pre-adoption trends are flat for our aggregate outcomes, the position-specific event studies are estimate over smaller cells and exhibit occasional nonzero pre-period estimates; we accordingly read the subgroup contrasts—including the central office result—as suggestive rather than definitive. Third, much of Missouri's adoption wave coincided with the COVID-19 pandemic and its aftermath. Year fixed effects absorb shocks common to adopting and comparison districts, but our estimates are largely driven by cohorts that adopted amid unusual labor market and enrollment conditions, and effects for adopters in calmer periods may differ.

Third, we note that some of the findings are only marginally significant and not always consistently significant at conventional levels across different models. Given this fact, we both encourage readers to interpret the findings with these caveats in mind and hope that future research will take on these questions with additional settings.

Finally, all of our evidence comes from one state, whose tuition arrangements, virtual schooling options, and rural district structure shape the mechanisms we document. Whether similar

offsetting obligations arise in other states, whether paraeducator movers disproportionately land in five-day districts—as the compensation account predicts—and the deeper cohort heterogeneity suggested by our saturation analysis, in which later adopters exhibit larger tuition responses (Appendix E), are open questions for future work.

6.3 Conclusion

The four-day school week is often presented as a low-cost lever for districts facing hiring challenges and tight budgets. For non-teaching staff and district finances in Missouri, we find it functions as neither. Adoption does not meaningfully improve retention among student-facing staff, does not change how districts organize their workforces, and does not reduce what districts spend. What it changes is composition: turnover shifts toward cross-district movement among paraeducators, and spending shifts from districts' own operations toward tuition payments that follow students to other providers. Districts considering the calendar should weigh anticipated benefits against what our evidence suggests it is—a change in how time and money are organized, not a source of budget relief or a staffing solution.

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FIGURES

Figure 1: Growth of the Four-Day School Week in Missouri

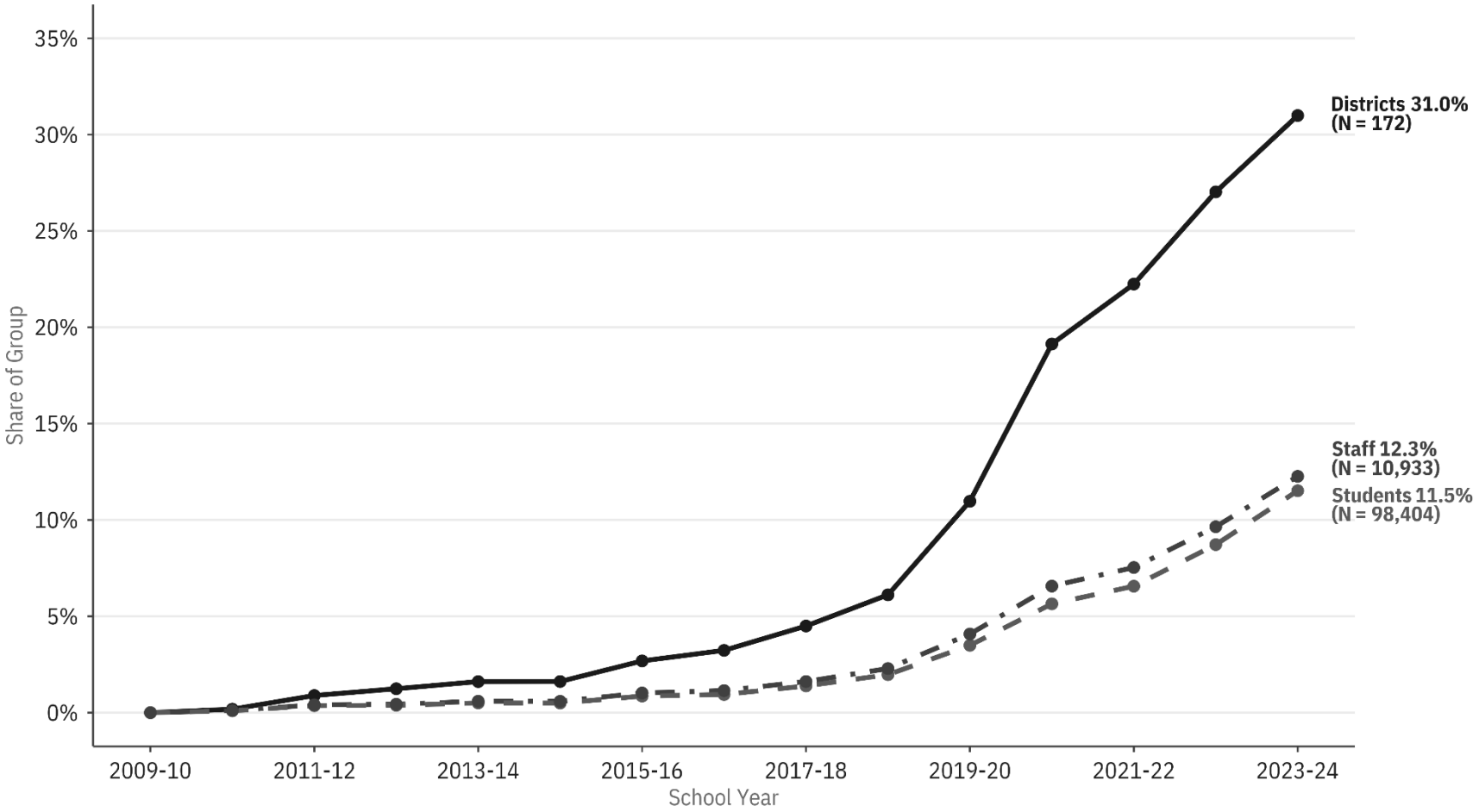


Figure 2: Event Study Estimates of Effects on Retention, All Non-Instructional Staff

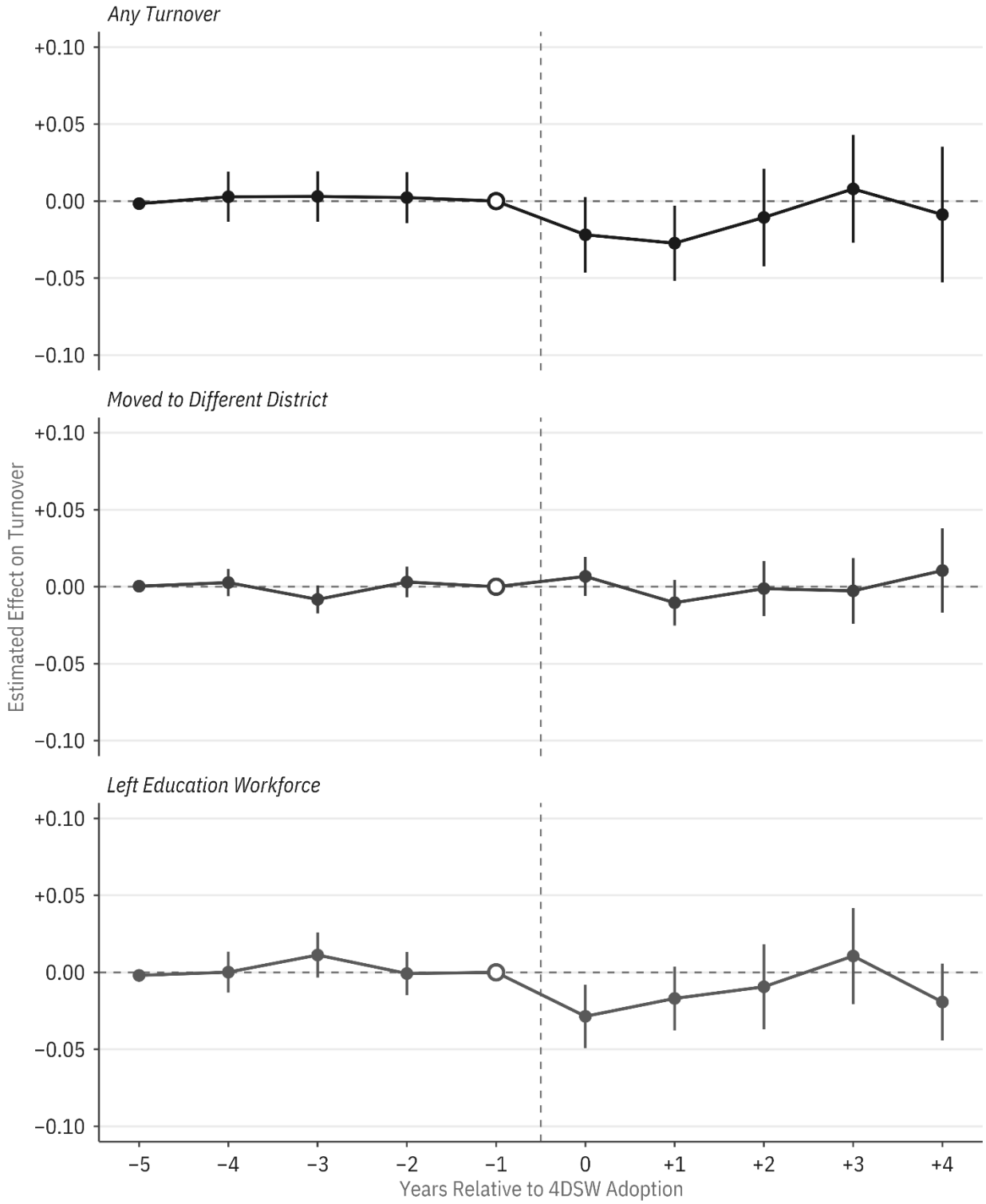


Figure 3: Event Study Estimates of Effects on Staffing Levels, All Non-Instructional Staff

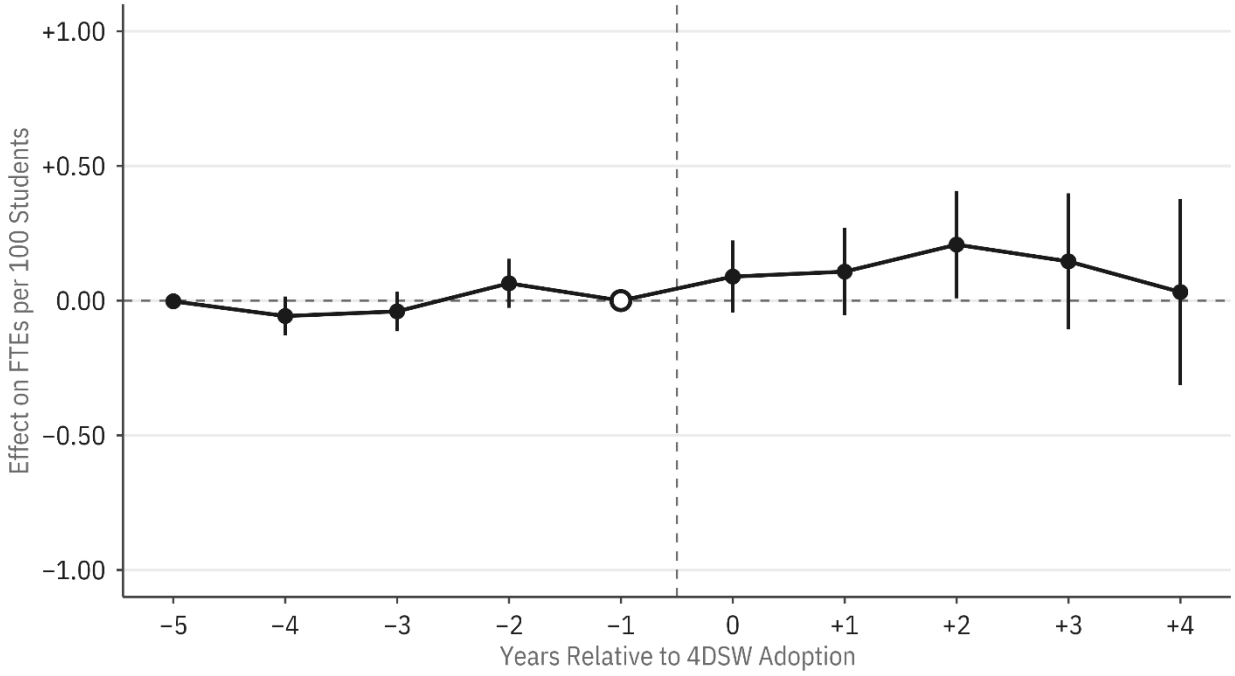


Figure 4: Event Study Estimates of Effects on Net Current Expenditures, Total Expenditures

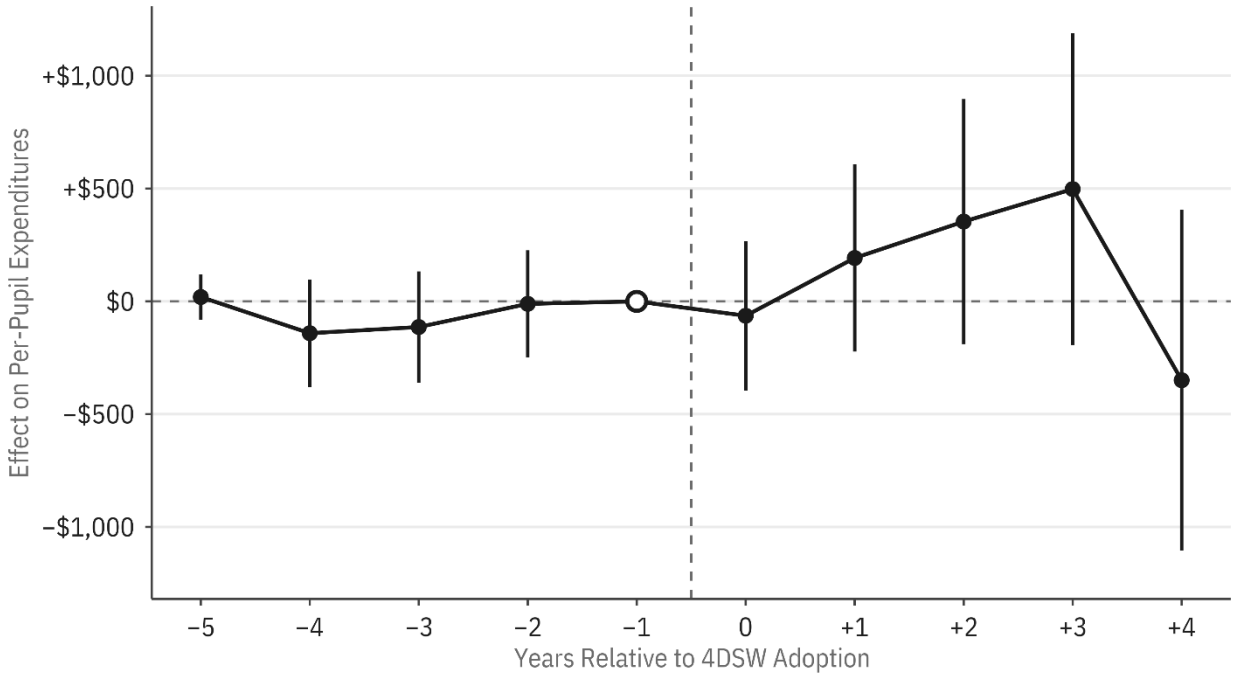


Figure 5: Event Study Estimates of Effects on Any Turnover, by Position

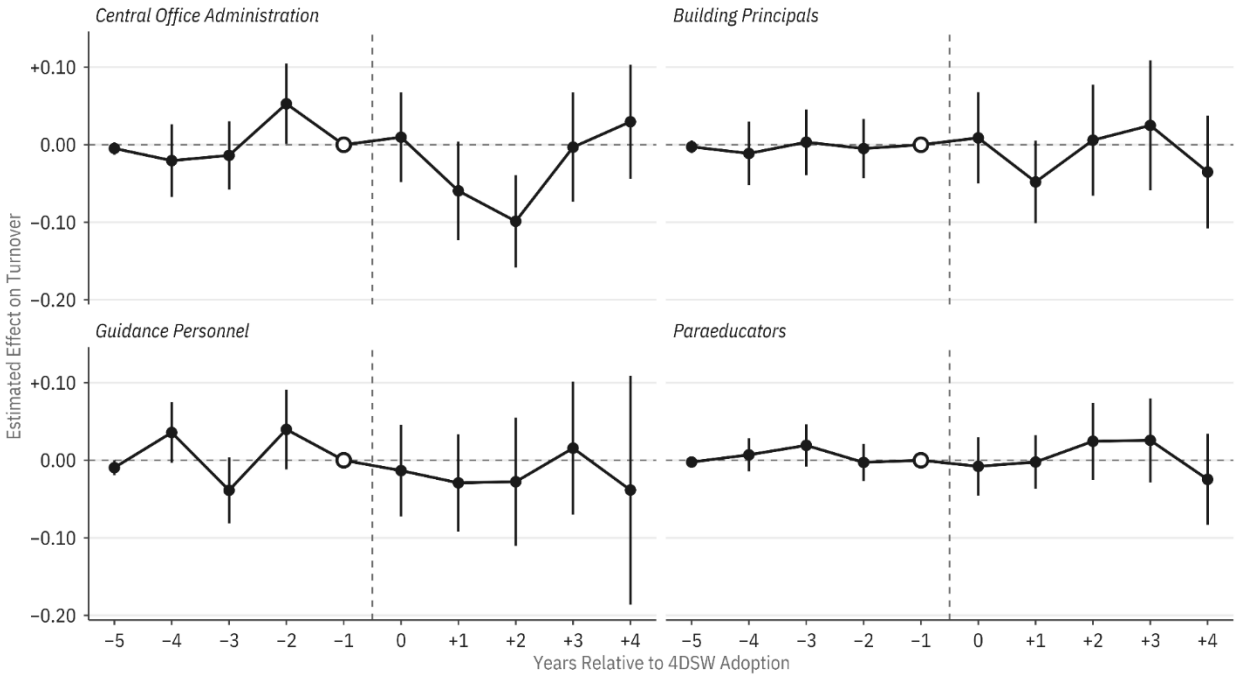


Figure 6: Event Study Estimates of Effects on Staffing Levels, by Position

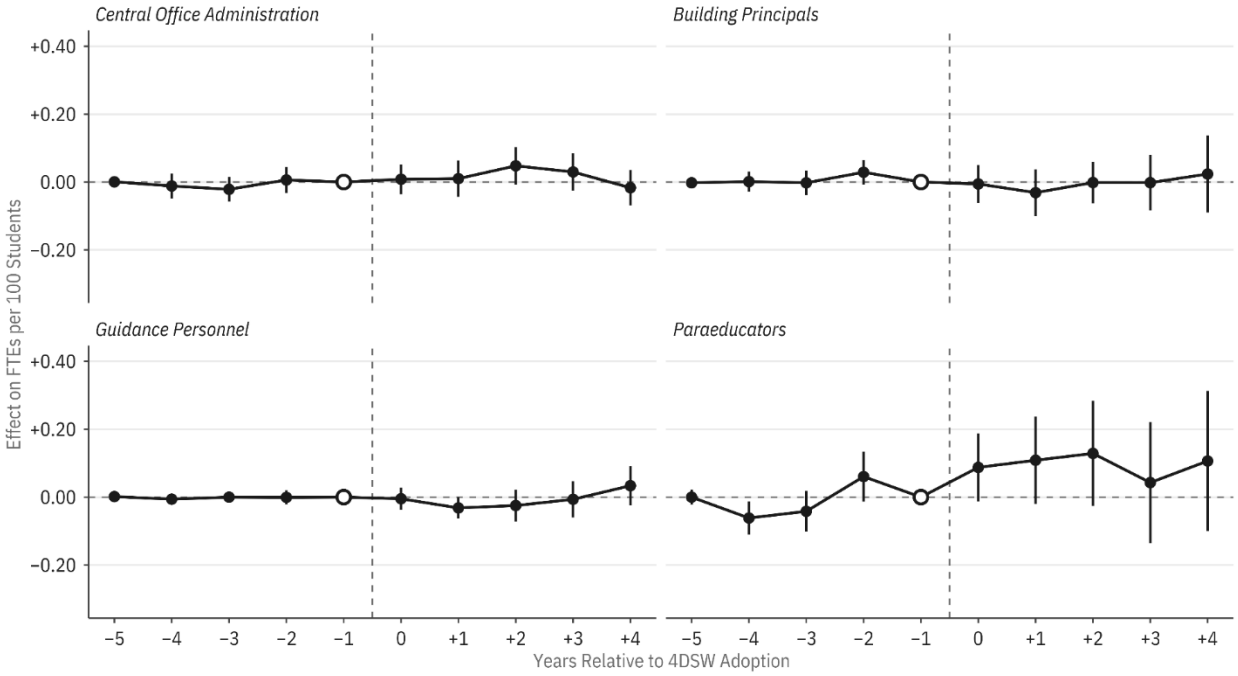


Figure 7: Event Study Estimates of Effects on Tuition Payments, Tuition Payments

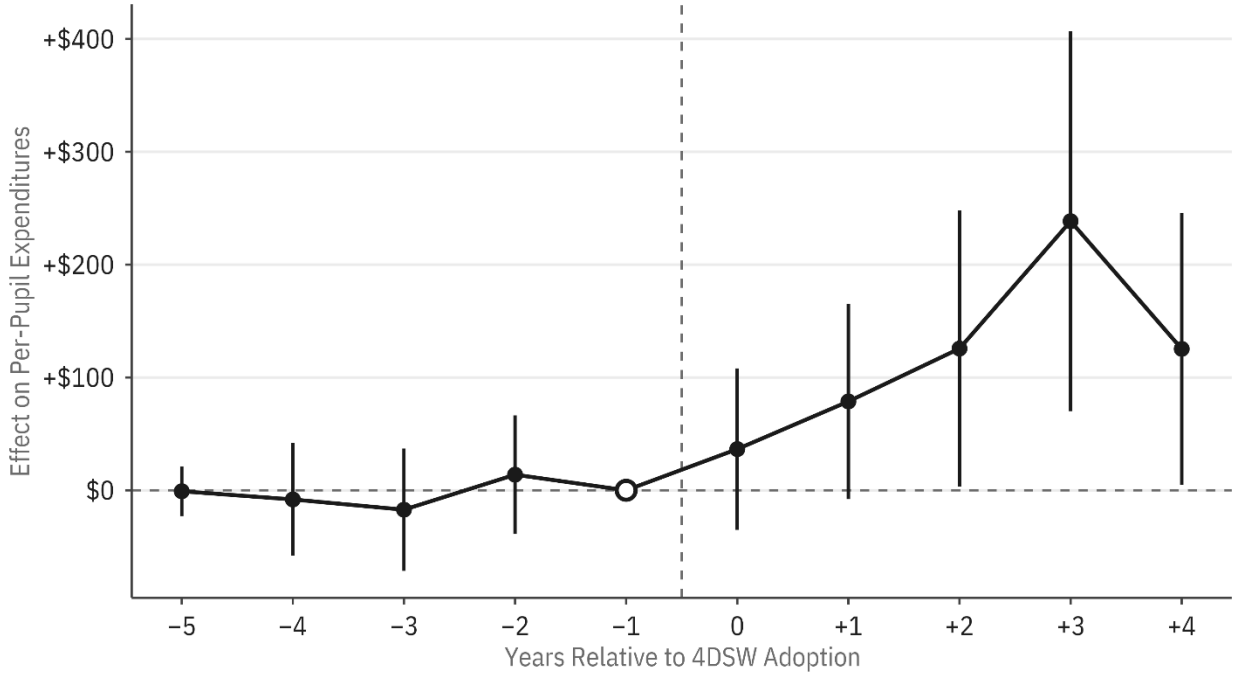


Figure 8: Event Study Estimates of Effects on Tuition Payment, Intensive Margin

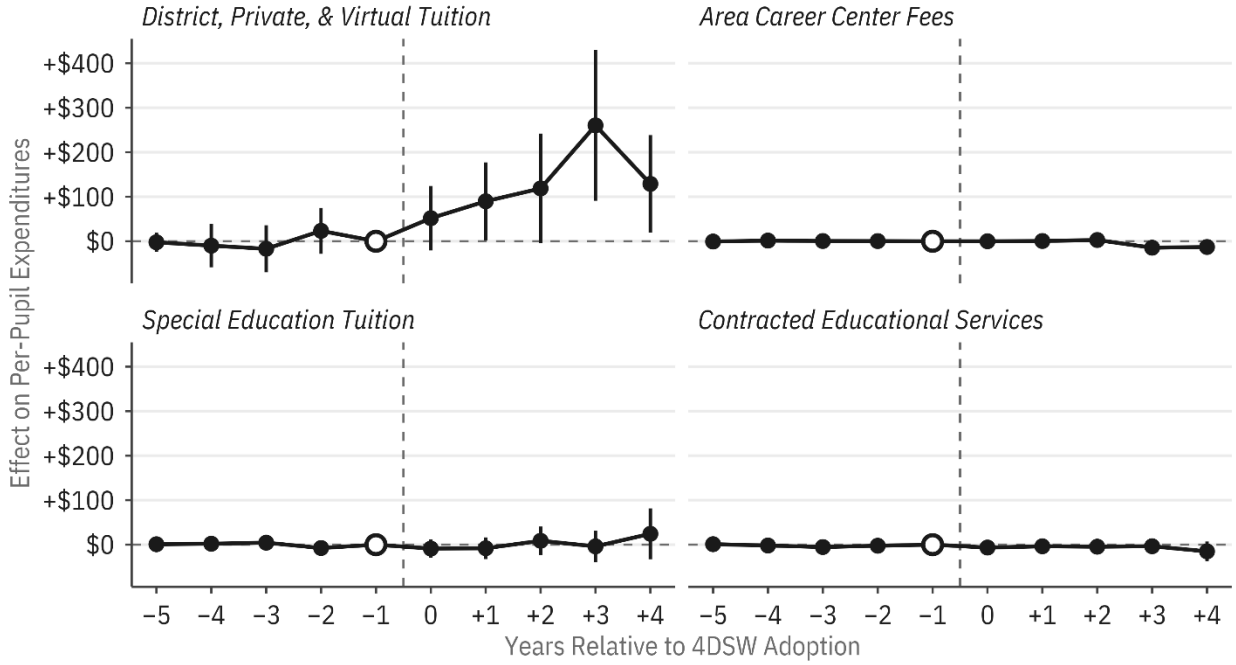
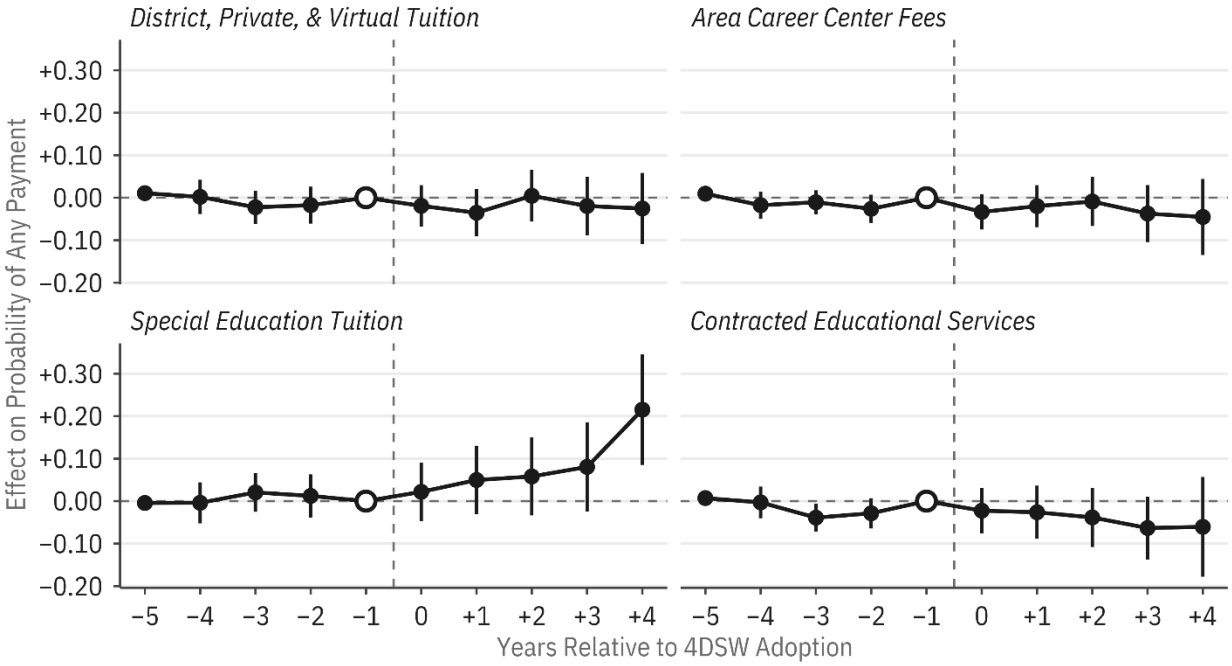


Figure 9: Event Study Estimates of Effects on Tuition Payments, Extensive Margin



TABLES

Table 1: District Characteristics by 4DSW Adoption Status, 2023-24

	Panel A: Adopters vs. All Non-Adopters			*Panel B: Adopters vs. Rural Non-Adopters*		
	4DSW (N = 172)	**Non-4DSW (N = 380)**	**Difference**	**4DSW (N = 172)**	**Rural Non-4DSW Districts (N = 224)**	**Difference**
District characteristics						
Enrollment	572	1,989	−1,417**	572	544	28
Number of schools	2.4	4.6	−2.2**	2.4	2.3	0.0
Total staff FTE	63.6	205.8	−142.3**	63.6	64.3	−0.7
Teacher FTE	51.9	165.3	−113.4**	51.9	52.0	−0.1
Administrator FTE	2.6	8.5	−5.9**	2.6	2.6	−0.1
% Urban	0.6	12.6	−12.1**	0.6	0.0	0.6
% Suburban	1.2	11.6	−10.4**	1.2	0.0	1.2
% Town	2.9	16.8	−13.9**	2.9	0.0	2.9*
% Rural	52.3	35.0	17.3**	52.3	59.4	−7.0
% Remote rural	43.0	23.9	19.1**	43.0	40.6	2.4
Teacher characteristics						
Average total salary	$43,376	$50,191	−$6,816**	$43,376	$44,913	−$1,537*
Average years of experience	12.4	12.3	0.1	12.4	12.7	−0.3
% with advanced degree	46.9	51.7	−4.8**	46.9	47.2	−0.3
Student characteristics						
% Black	2.1	11.4	−9.3**	2.1	1.7	0.4
% Hispanic	3.6	5.8	−2.2**	3.6	3.1	0.5
% Other race/ethnicity	3.3	5.2	−1.9**	3.3	3.3	0.0
% White	91.0	77.6	13.4**	91.0	91.9	−0.9
% Direct certification	25.3	23.9	1.4	25.3	21.9	3.4

*Notes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Each cell reports the unweighted district-level mean in 2023-24; differences are the 4DSW mean minus the comparison mean, with p-values from Welch two-sample t-tests. Adopters are districts operating a four-day week in 2023-24; districts adopting in later years count as non-adopters. Panel B restricts the comparison group to rural and remote-rural districts matching the comparison in our primary estimates. Administrator FTE is the sum of central office administration and building principal FTEs.*

Table 2: Effect of the 4DSW on Non-Instructional Staff Turnover

	All Non-Instructional	Central Admin	Principals	Guidance	Paraeducators
Panel A: Estimated effects					
Any turnover	−0.014	−0.020	−0.011	−0.021	−0.002
	(0.010)	(0.020)	(0.019)	(0.029)	(0.016)
Moved district	0.002	0.010	−0.011	0.004	0.018**
	(0.006)	(0.014)	(0.016)	(0.022)	(0.007)
Left workforce	−0.015*	−0.029+	0.000	−0.025	−0.020
	(0.007)	(0.015)	(0.013)	(0.019)	(0.014)
Panel B: Pre-treatment means					
Any turnover	0.188	0.163	0.161	0.174	0.218
Moved district	0.057	0.059	0.090	0.070	0.044
Left workforce	0.131	0.104	0.071	0.105	0.174
Observations (Person-Years)	60,406	7,718	9,774	7,089	25,822

*Notes: + p < 0.1, * p < 0.05, ** p < 0.01. Each cell is the average treatment effect on the treated from a separate regression; standard errors clustered at the district level in parentheses. All Non-Instructional includes all staff except teachers. Pre-treatment means are turnover rates for ever-4DSW staff in transitions before their own district's adoption.*

Table 3: Effect of the 4DSW on Non-Instructional Staffing Levels

	All Non-Instructional	Central Admin	Principals	Guidance	Paraeducators
Panel A: Estimated effects					
FTEs per 100 students	0.107	0.012	−0.002	−0.004	0.097+
	(0.081)	(0.017)	(0.028)	(0.017)	(0.058)
Panel B: Pre-treatment means					
FTEs per 100 students	1.696	0.318	0.325	0.164	0.655
Observations (District-Years)	6,357	6,357	6,357	6,357	6,357

*Notes: + p < 0.1, * p < 0.05, ** p < 0.01. Each cell is the average treatment effect on the treated from a separate regression; standard errors clustered at the district level in parentheses. All Non-Instructional includes all staff except teachers. Pre-treatment means are turnover rates for ever-4DSW staff in transitions before their own district's adoption. Regressions and pre-treatment means are unweighted. Pre-treatment means are FTEs per 100 students for ever-4DSW districts in years before their own adoption.*

Table 4: Effect of the 4DSW on Per-Pupil Expenditures

	Certificated Salaries	Non-Certificated Salaries	Benefits	Purchased Services	Supplies	Total
Panel A: Estimated effects						
All Functions	$47.59	−$61.23	−$16.23	$104.05	−$4.68	$69.50
	($105.67)	($47.36)	($39.36)	($66.29)	($34.39)	($215.96)
Instruction	$72.88	−$7.57	$0.56	$17.10	−$8.30	$74.67
	($88.08)	($24.71)	($27.17)	($18.83)	($20.53)	($122.22)
Tuition & Conduit Payments	-	-	-	$111.18**	-	$111.34**
				($42.37)		($42.38)
Student Support Services	−$6.02	$6.45	$3.23	−$0.96	$5.86	$8.57
	($17.55)	($7.43)	($5.31)	($14.83)	($7.95)	($29.40)
Operations & Maintenance	-	−$23.78+	−$6.08	$32.66	$9.99	$13.80
		($12.34)	($4.95)	($23.53)	($11.26)	($31.67)
Transportation	−$4.15	−$3.26	$4.67	−$14.69	−$12.40+	−$29.81
	($3.16)	($10.62)	($3.73)	($15.92)	($7.13)	($20.60)
Food Service	-	−$6.40	−$0.88	−$20.87	−$3.43	−$30.27+
		($8.95)	($3.93)	($16.23)	($13.53)	($15.74)
Administration	−$3.30	−$4.62	−$9.80	−$1.65	−$0.24	−$19.60
	($22.68)	($15.02)	($8.98)	($17.57)	($7.75)	($48.12)
Community Services	−$14.27	−$22.05	−$7.92	−$18.73	$3.79	−$59.19
	($12.89)	($15.91)	($6.14)	($17.95)	($7.21)	($42.12)
Panel B: Pre-treatment means						
All Functions	$6,815.03	$2,032.73	$2,492.38	$2,163.90	$2,033.67	$15,537.72
Instruction	$5,313.33	$383.72	$1,614.30	$332.01	$848.47	$8,491.83
Tuition & Conduit Payments	-	-	-	$539.07	-	$539.08
Student Support Services	$455.76	$151.93	$154.88	$170.61	$85.04	$1,018.23
Operations & Maintenance	-	$427.75	$139.76	$371.36	$489.57	$1,430.73
Transportation	$21.49	$303.52	$77.67	$270.15	$163.60	$836.43
Food Service	-	$243.31	$85.95	$135.53	$351.17	$818.37
Administration	$896.75	$421.37	$359.66	$277.42	$66.74	$2,021.95
Community Services	$122.99	$101.11	$60.17	$67.75	$29.07	$381.09
Observations (District-Years)	6,351	6,351	6,351	6,351	6,351	6,351

*Notes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Each cell is the average treatment effect on the treated from a separate regression; standard errors clustered at the district level in parentheses. Each row decomposes a function group across object categories; Total is current operating spending (net of capital outlay and debt objects), which the five object columns sum to. All Functions - Total is district-wide current spending. Regressions are unweighted; the unit of observation is the district-year. Pre-treatment means are per-pupil expenditures for ever-4DSW districts in years before their own adoption. Blank cells reflect structural zeros in the underlaying data.*

APPENDIX A – ALTERNATIVE ESTIMATORS

Table A1: Effect of the 4DSW on Non-Instructional Staff Turnover

Outcome	All Non-Instructional	Central Admin	Principals	Guidance	Paraeducators
Panel A: TWFE Estimator					
Any turnover	−0.013	−0.028	−0.007	−0.010	0.007
	(0.009)	(0.020)	(0.019)	(0.024)	(0.014)
Moved district	0.001	0.010	−0.010	0.009	0.015*
	(0.005)	(0.013)	(0.016)	(0.016)	(0.006)
Left workforce	−0.015*	−0.038**	0.004	−0.019	−0.008
	(0.007)	(0.014)	(0.014)	(0.017)	(0.012)
Panel B: Borusyak et al. Imputation Estimator					
Any turnover	−0.014	−0.020	−0.011	−0.021	−0.002
	(0.009)	(0.018)	(0.018)	(0.022)	(0.013)
Moved district	0.002	0.010	−0.011	0.004	0.018**
	(0.005)	(0.013)	(0.014)	(0.012)	(0.006)
Left workforce	−0.015*	−0.029*	0.000	−0.025	−0.020+
	(0.006)	(0.013)	(0.012)	(0.017)	(0.011)
Observations (Person-Years)	60,406	7,718	9,774	7,089	25,822

*Notes: + p < 0.1, * p < 0.05, ** p < 0.01. Each cell is the average treatment effect on the treated from the indicated estimator with district and school-year fixed effects; standard errors clustered at the district level in parentheses. All Non-Instructional includes all staff except teachers.*

Table A2: Effect of the 4DSW on Non-Instructional Staff Turnover

Outcome	All Non-Instructional	Central Admin	Principals	Guidance	Paraeducators
Panel A: TWFE Estimator					
FTEs per 100 students	0.129+	0.017	−0.006	−0.011	0.099+
	(0.071)	(0.018)	(0.026)	(0.016)	(0.055)
Panel B: Borusyak et al. Imputation Estimator					
FTEs per 100 students	0.107	0.012	−0.002	−0.004	0.097+
	(0.079)	(0.016)	(0.026)	(0.016)	(0.056)
Observations (District-Years)	6,357	6,357	6,357	6,357	6,357

*Notes: + p < 0.1, * p < 0.05, ** p < 0.01. Each cell is the average treatment effect on the treated from the indicated estimator with district and school-year fixed effects; standard errors clustered at the district level in parentheses. All Non-Instructional is the sum of all non-teaching FTEs.*

Table A3: Effect of the 4DSW on Per-Pupil Expenditures (TWFE Estimator)

Outcome	Certificated Salaries	Non-Certificated Salaries	Benefits	Purchased Services	Supplies	Total
All Functions	$81.43	−$29.90	−$0.16	$109.79+	−$3.09	$158.07
	($94.15)	($46.49)	($36.89)	($63.91)	($33.31)	($201.30)
Instruction	$94.69	−$2.20	$6.01	$22.01	−$4.72	$115.78
	($73.50)	($23.11)	($24.96)	($17.44)	($20.36)	($113.07)
Tuition & Conduit Payments	-	-	-	$109.53**	-	$109.70**
				($41.49)		($41.50)
Student Support Services	$2.98	$6.62	$4.68	$1.62	$3.62	$19.52
	($15.18)	($7.43)	($5.37)	($14.34)	($7.23)	($26.60)
Operations & Maintenance	-	−$11.36	−$2.54	$29.17	$9.90	$25.49
		($11.02)	($4.35)	($21.93)	($11.74)	($31.31)
Transportation	−$3.27	−$2.80	$5.37	−$12.34	−$7.53	−$20.57
	($3.26)	($10.20)	($3.56)	($16.74)	($4.71)	($18.44)
Food Service	-	−$3.31	$1.18	−$16.53	−$4.83	−$22.40
		($8.30)	($3.73)	($14.10)	($12.61)	($15.91)
Administration	−$4.33	$1.71	−$9.17	−$2.14	$0.53	−$13.40
	($22.47)	($13.96)	($8.86)	($17.47)	($8.21)	($48.92)
Community Services	−$10.19	−$18.55	−$5.70	−$21.52	−$0.09	−$56.06
	($12.35)	($15.80)	($5.64)	($19.10)	($6.73)	($42.45)
Observations (District-Years)	6,351	6,351	6,351	6,351	6,351	6,351

*Notes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Each cell is the average treatment effect on the treated from a standard two-way fixed effects estimator, with district and school-year fixed effects; standard errors clustered at the district level in parentheses. Total is current operating spending (net of capital outlay and debt objects), which the five object columns sum to. All Functions is district-wide current spending. Regressions are unweighted; the unit of observation is the district-year.*

Table A4: Effect of the 4DSW on Per-Pupil Expenditures (Imputation Estimator)

Outcome	Certificated Salaries	Non-Certificated Salaries	Benefits	Purchased Services	Supplies	Total
All Functions	$47.59	−$61.23	−$16.23	$104.05	−$4.68	$69.50
	($101.26)	($45.57)	($38.82)	($64.06)	($32.70)	($210.49)
Instruction	$72.88	−$7.57	$0.56	$17.10	−$8.30	$74.67
	($84.90)	($24.22)	($26.90)	($17.17)	($19.87)	($120.36)
Tuition & Conduit Payments	-	-	-	$111.18**	-	$111.34**
				($41.37)		($41.38)
Student Support Services	−$6.02	$6.45	$3.23	−$0.96	$5.86	$8.57
	($14.83)	($7.25)	($5.14)	($14.37)	($7.22)	($26.89)
Operations & Maintenance	-	−$23.78*	−$6.08	$32.66	$9.99	$13.80
		($10.87)	($4.19)	($22.08)	($11.06)	($29.32)
Transportation	−$4.15	−$3.26	$4.67	−$14.69	−$12.40*	−$29.81
	($3.07)	($10.41)	($3.66)	($15.48)	($5.93)	($19.96)
Food Service	-	−$6.40	−$0.88	−$20.87	−$3.43	−$30.27*
		($7.96)	($3.69)	($14.16)	($12.51)	($15.20)
Administration	−$3.30	−$4.62	−$9.80	−$1.65	−$0.24	−$19.60
	($20.87)	($14.46)	($8.36)	($17.43)	($7.61)	($46.68)
Community Services	−$14.27	−$22.05	−$7.92	−$18.73	$3.79	−$59.19
	($11.69)	($15.48)	($5.71)	($17.74)	($6.64)	($39.98)
Observations (District-Years)	6,351	6,351	6,351	6,351	6,351	6,351

*Notes: + p < 0.1, * p < 0.05, ** p < 0.01. Each cell is the average treatment effect on the treated from the Borusyak et al. imputation-based estimator, with district and school-year fixed effects; standard errors clustered at the district level in parentheses. Total is current operating spending (net of capital outlay and debt objects), which the five object columns sum to. All Functions is district-wide current spending. Regressions are unweighted; the unit of observation is the district-year.*

Appendix B – Choice of Comparison Group

We consider three possible definitions of the comparison group. The broadest retains all never-treated districts, so that any untreated district-year observation contributes to the counterfactual. An intermediate option retains only rural never-treated districts, motivated by the reality that 4DSW adoption in Missouri is an almost entirely rural phenomenon with over 95% of adopting districts located in an NCES-designated rural locale. The most restrictive option retains no never-treated districts, drawing the counterfactual entirely from eventual adopters observed before adoption and enabled by the staggered adoption of the 4DSW calendar.

We first rule out the broadest construction which includes all never-treated districts. As discussed above, the credibility of parallel trends in levels degrades as comparison districts are drawn from more distant regions of the outcome distribution, and Missouri's non-rural never-treated districts—including its large urban systems and suburban districts—operate at per-pupil expenditure levels far from those of the almost exclusively rural adopting population. Table 1 documents this divergence descriptively in that four-day districts differ significantly from the average five-day district on nearly every characteristic we observe, while differing from rural five-day districts on almost none. This concern is supported empirically in Appendix B where we estimate event studies for six district characteristics under each candidate comparison group and find that including all never-treated districts induces differential pre-adoption trends in student enrollment and the share of students receiving free- or reduced-price lunch, both of which are likely linked to the staffing and expenditure outcomes we study. These districts are also exposed to policy variation that adopting districts are not, including a notable school finance reform in St. Louis and Kansas City that affected expenditures (Camp, Hitt, et al., 2026). Together, these results

indicate that non-rural never-treated districts are poor candidates for constructing the counterfactual, and we exclude them from all analyses.

The choice between the two remaining constructions is closer, and both appear in the 4DSW literature, with the combination of not-yet-treated and rural never-treated districts the more common approach in recent work. Appendix B shows that the choice carries little identification risk *ex ante* in that pre-adoption trends are flat for all six district characteristics we examine under both remaining constructions. We therefore treat credibility as a screen that both constructions pass and select between them based on anticipated precision of results.

Precision favors retaining rural never-treated districts due to specific features of our estimator. When the first stage is restricted to not-yet-treated districts only, year fixed effects in the later years of the panel are identified from the shrinking set of districts that have not yet adopted, and the resulting imprecision in these estimates propagates into the second stage. This cost is most consequential for outcomes measured over small cells as each district employs many teachers but few, for example, building principals and counselors. Accordingly, our main analyses use the comparison group of not-yet-treated and rural never-treated districts.

Figure B1: Event Study Estimates of Effects on Enrollment, by Comparison Group

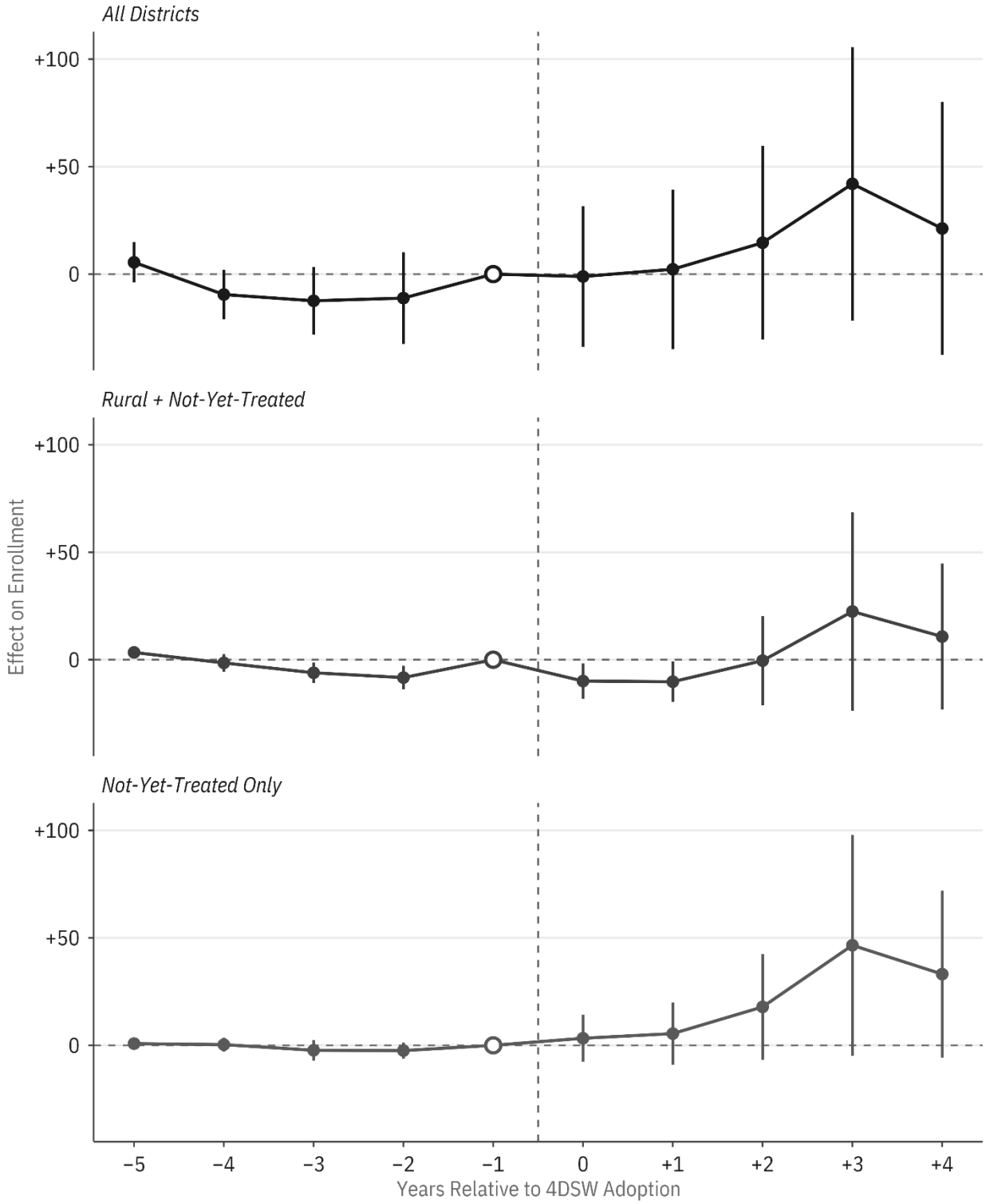


Figure B2: Event Study Estimates of Effects on Average Teacher Salary, by Comparison Group

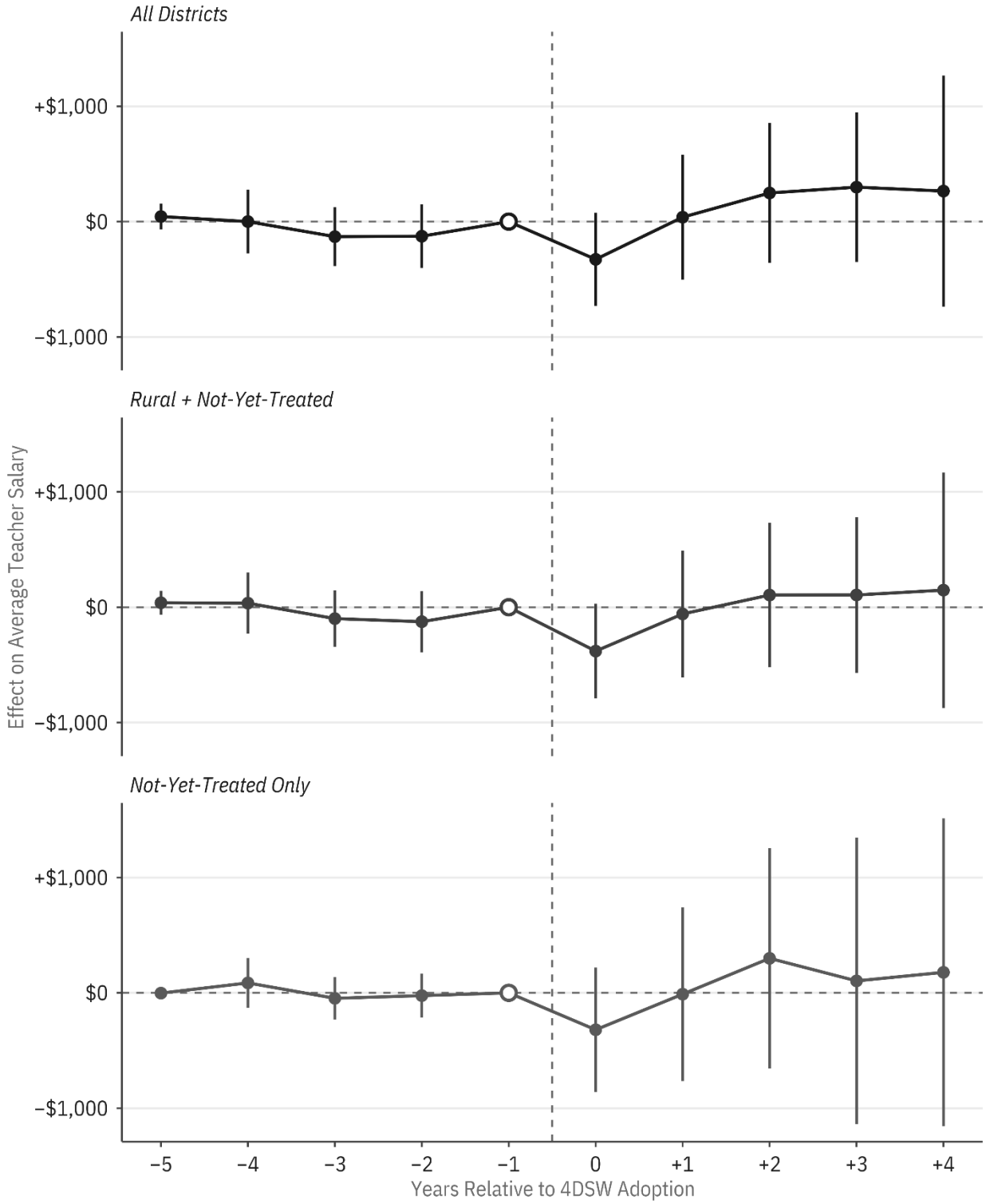


Figure B3: Event Study Estimates of Effects on Average Teacher Years of Experience, by Comparison Group

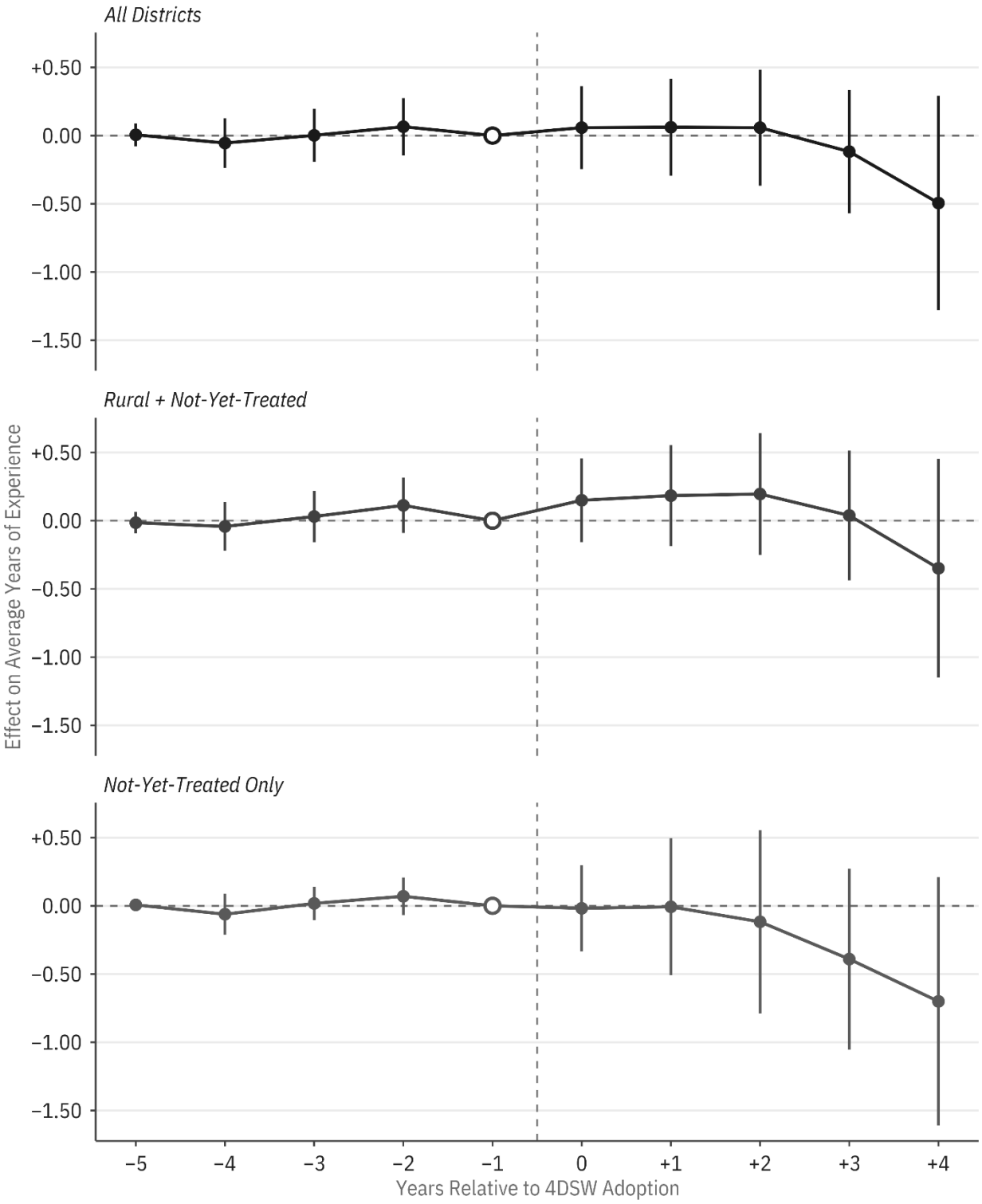


Figure B4: Event Study Estimates of Effects on Share of Teachers with Advanced Degrees, by Comparison Group

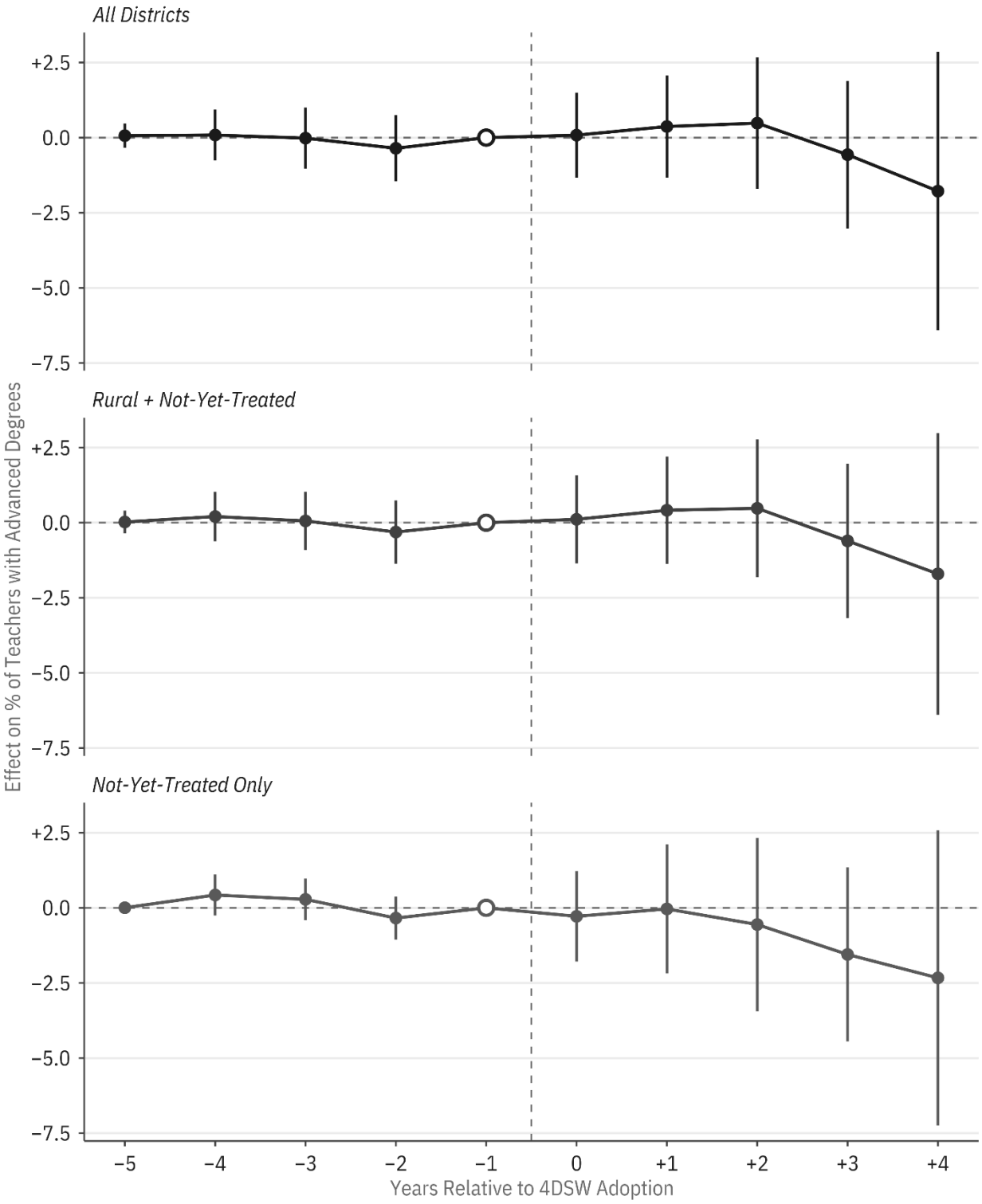


Figure B5: Event Study Estimates of Effects on Percent White Students, by Comparison Group

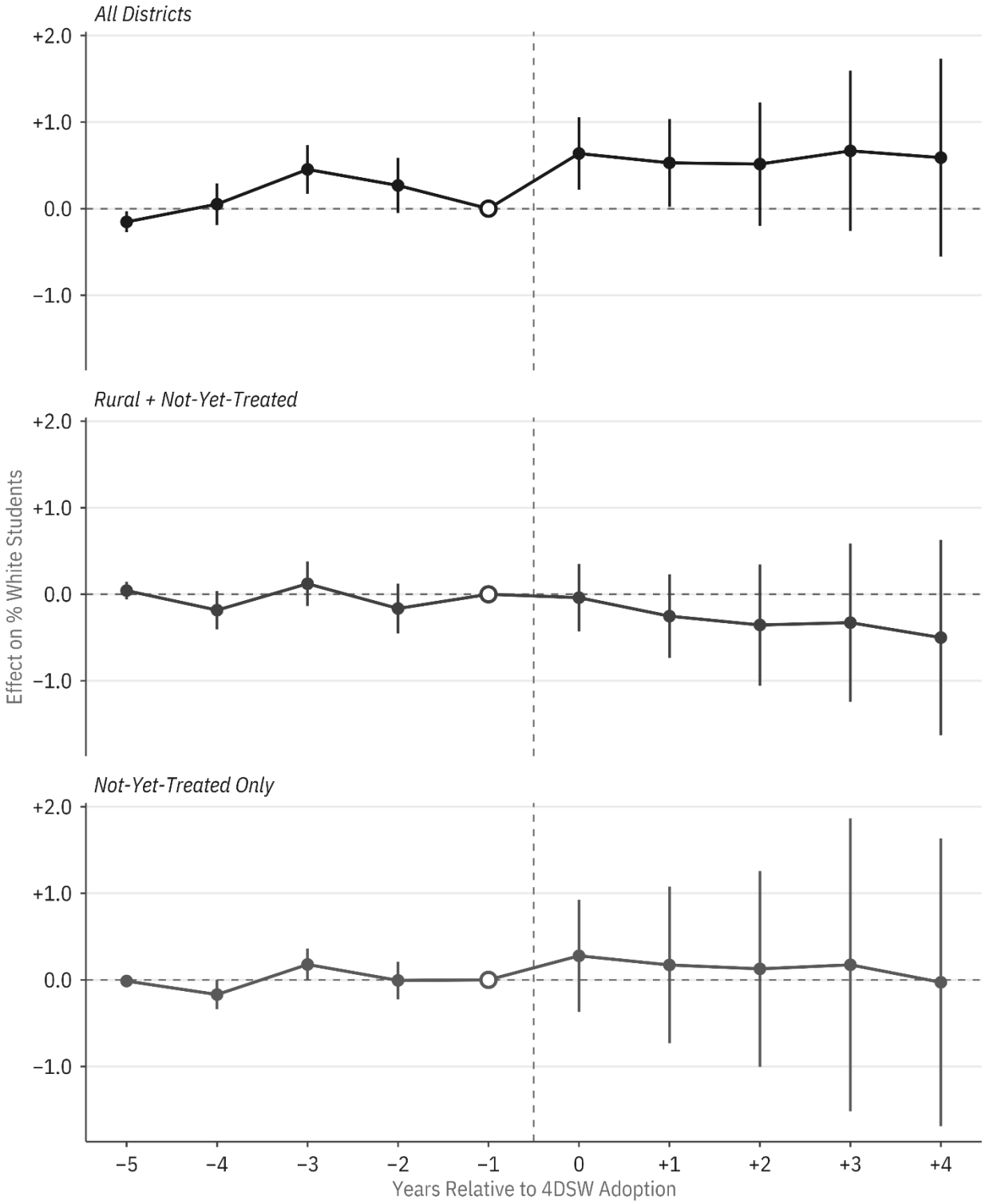


Figure B6: Event Study Estimates of Effects on Percent Free- and Reduced-Priced Lunch, by Comparison Group

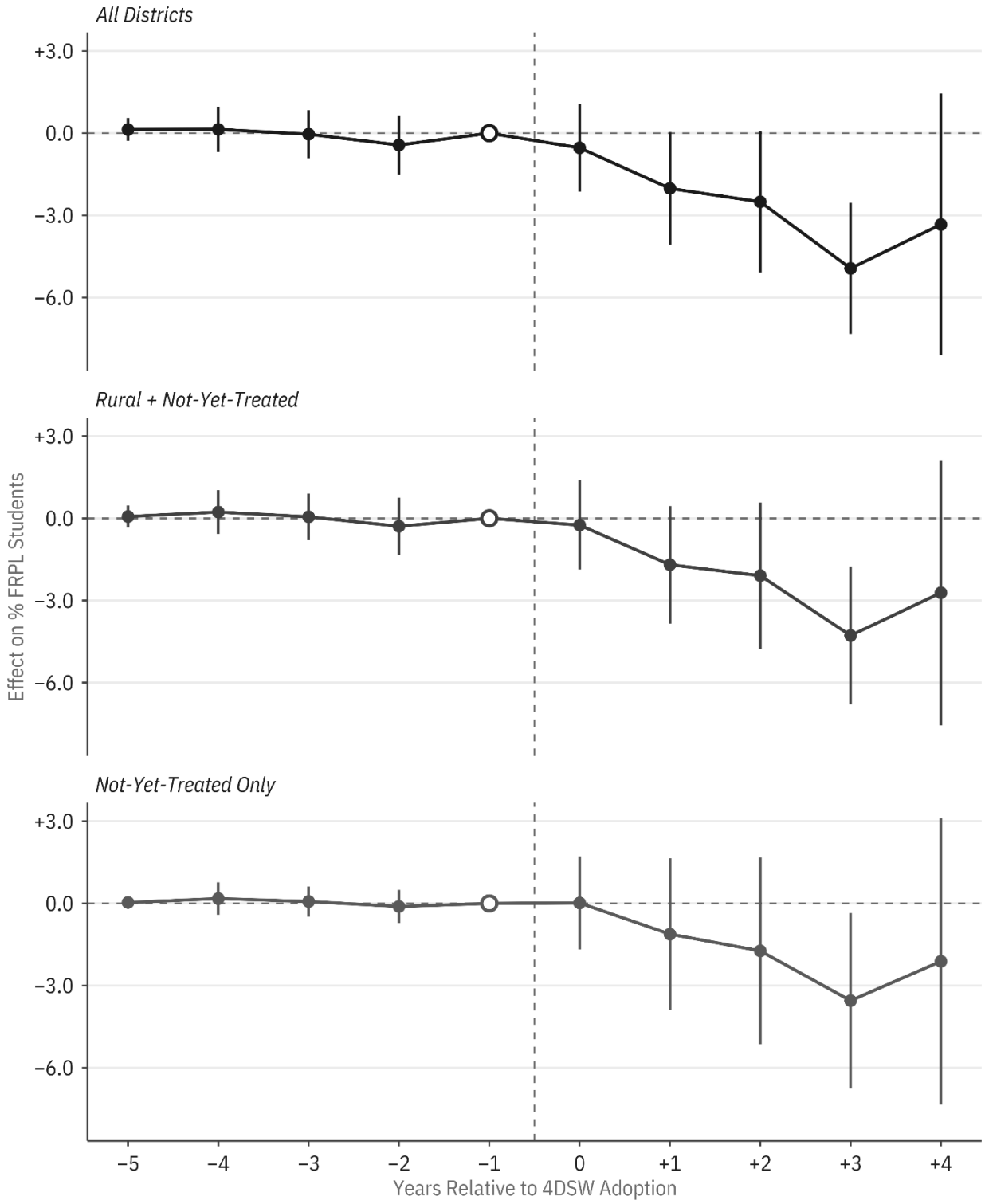


Figure B7: Event Study Estimates of Effects on Percent of Students with an Individualized Education Plan (IEP), by Comparison Group

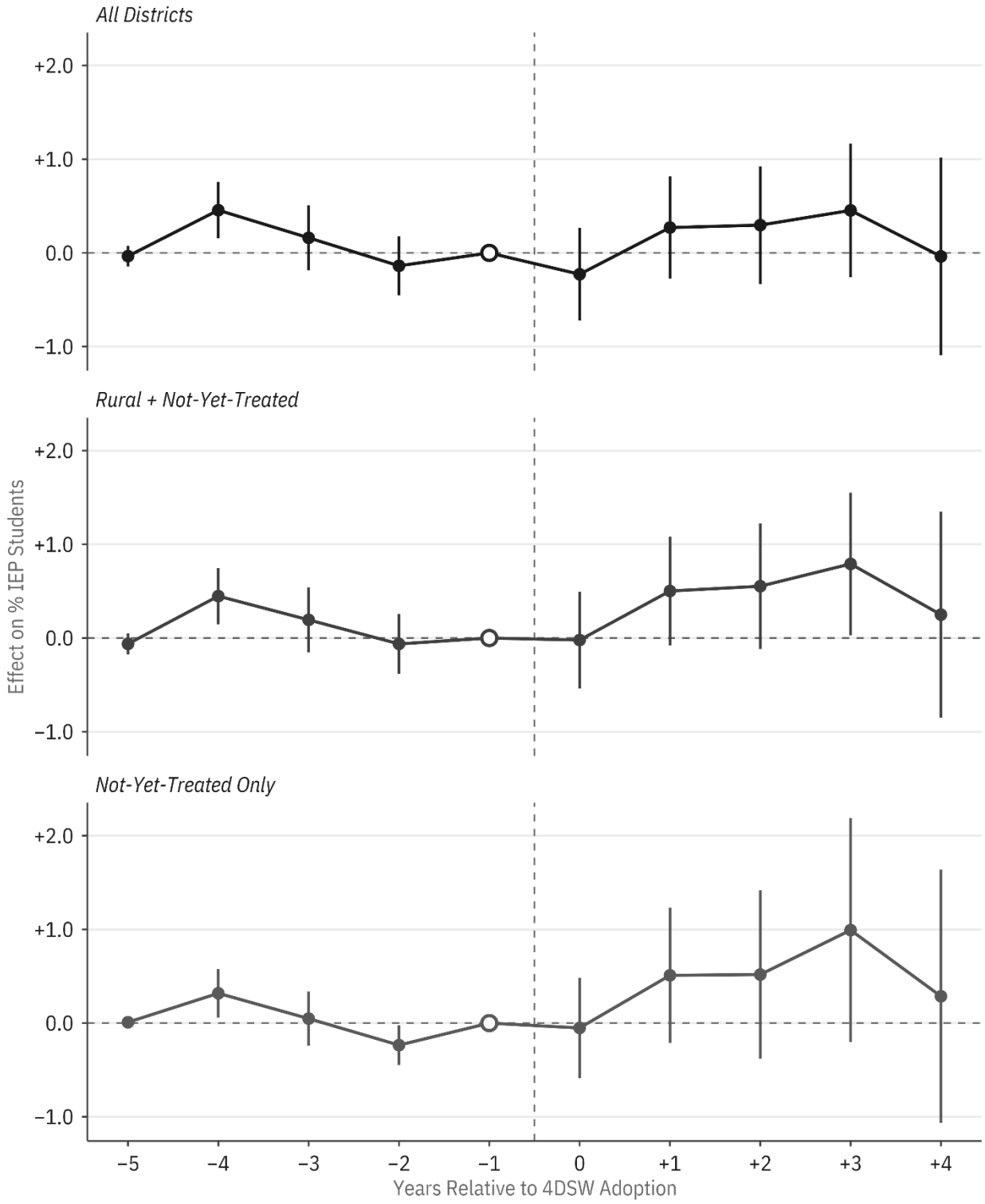
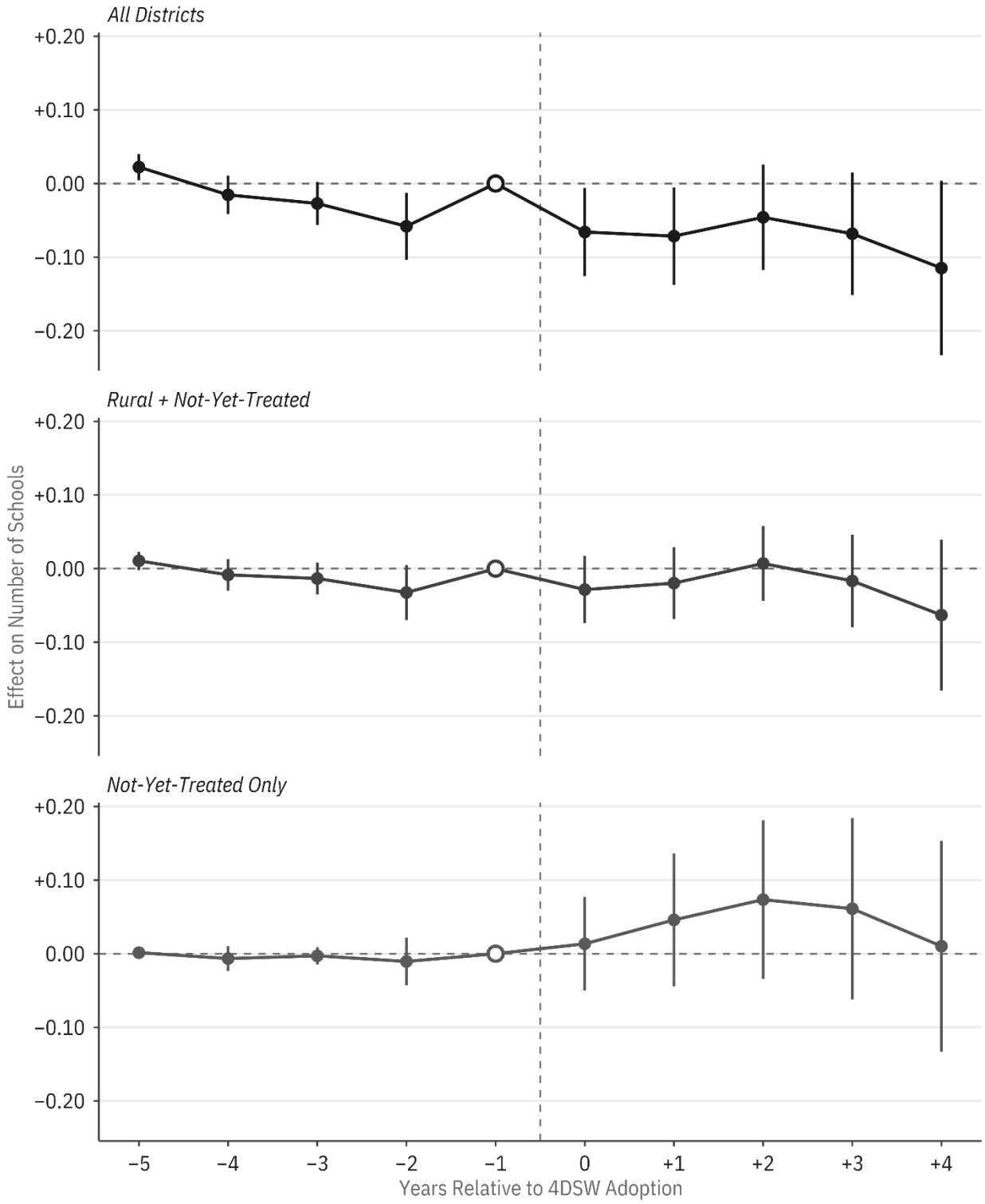


Figure B8: Event Study Estimates of Effects on Number of Schools in District, by Comparison Group



APPENDIX C – ROBUSTNESS TO CHANGES IN ENROLLMENTS

Table C2: Effect of the 4DSW on Non-Instructional Staff Turnover, Fixed Denominator

Outcome	All Non-Instructional	Central Admin	Principals	Guidance	Paraeducators
FTEs per 100 students	0.061	0.009	−0.010	−0.006	0.067
	(0.068)	(0.017)	(0.026)	(0.016)	(0.052)
Observations (District-Years)	6,357	6,357	6,357	6,357	6,357

*Notes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Each cell is the average treatment effect on the treated from the two-stage difference-in-differences estimator of Gardner et al. (2024), with district and school-year fixed effects; standard errors clustered at the district level in parentheses. All Non-Instructional is the sum of all non-teaching FTEs.*

Table C2: Effect of the 4DSW on Per-Pupil Expenditures, Fixed Denominator

Outcome	Certificated Salaries	Non-Certificated Salaries	Benefits	Purchased Services	Supplies	Total
All Functions	−$102.51	−$101.30*	−$73.37*	$119.63	−$47.19	−$204.74
	($73.28)	($49.81)	($31.33)	($102.53)	($33.47)	($182.28)
Instruction	−$41.38	−$12.05	−$34.17	$16.43	−$28.66	−$99.84
	($61.24)	($28.89)	($22.18)	($23.07)	($19.11)	($95.81)
Tuition & Conduit Payments	-	-	-	$168.64+	-	$168.79+
				($88.02)		($88.02)
Student Support Services	−$13.19	$4.21	$0.90	−$4.31	$4.82	−$7.55
	($15.21)	($7.28)	($5.01)	($14.50)	($8.19)	($26.43)
Operations & Maintenance	-	−$34.56**	−$10.16*	$25.47	−$1.10	−$19.04
		($11.54)	($4.47)	($21.77)	($11.15)	($27.01)
Transportation	−$4.59	−$5.98	$4.20	−$30.92*	−$15.00*	−$52.28**
	($3.11)	($9.65)	($3.37)	($14.45)	($6.25)	($16.52)
Food Service	-	−$11.86	−$3.40	−$23.99	−$7.47	−$45.53**
		($8.23)	($3.57)	($16.71)	($14.40)	($15.45)
Administration	−$29.12	−$16.57	−$20.93*	−$11.90	−$2.53	−$81.05+
	($22.53)	($14.28)	($8.27)	($16.79)	($7.86)	($45.70)
Community Services	−$16.86	−$24.50	−$9.80	−$19.79	$2.72	−$68.24
	($12.91)	($16.31)	($6.34)	($17.94)	($6.94)	($42.80)
Observations (District-Years)	6,351	6,351	6,351	6,351	6,351	6,351

*Notes: + p < 0.1, * p < 0.05, ** p < 0.01. Each cell is the average treatment effect on the treated from the two-stage difference-in-differences estimator of Gardner et al. (2024), with district and school-year fixed effects; standard errors clustered at the district level in parentheses. Total is current operating spending (net of capital outlay and debt objects), which the five object columns sum to. All Functions is district-wide current spending. Regressions are unweighted; the unit of observation is the district-year.*

APPENDIX D – ROBUSTNESS TO SPILLOVER EFFECTS

As discussed in Section 4.3, potential spillovers may bias estimates of our primary effects. Here, we discuss the nature of potential spillovers and show that our primary results are robust to these potential spillovers. We begin by first noting two distinct form of spatial spillovers.

In the first, members of the treated unit (e.g., individuals in adopting districts) may be affected by the nearby availability of untreated units. In our contexts, this may occur if, for example, a paraeducator in an adopting district prefers the 5DSW calendar and has ample opportunities to realize that preference by moving to one of several nearby 5DSW districts. This first form of spillovers, an effect on treated units, is part of the policy-relevant treatment effect of 4DSW calendar adoption. The second form of spatial spillover in our context would occur when untreated districts, or individuals working within those districts, are affected by the adoption of a 4DSW calendar in a nearby district. An easy-to-understand example of this occurring would be if a paraeducator in a 5DSW district views the prospect of working in a 4DSW district as desirable and leaves their district to work in the 4DSW district.

Our approach to addressing these spillovers relies upon the plausible assumption that spatial spillovers would attenuate towards zero with distance. In other words, individuals in districts sufficiently far away from an adopting district are not affected by that district's adoption of the 4DSW. In developing this robustness check, we momentarily assume that there is some distance D^* at which the outcomes of non-adopting districts are unaffected by 4DSW adoption and estimate Equation (D1).

$$\begin{gathered} Y_{jt} = \alpha_j + \rho_t + \zeta_{jt} \quad \text{for all} \quad \text{4DSW}_{jt} = 0 \ \text{ and } \ d_{jt} \geq D^* \\ Y_{jt} - \hat{\alpha}_j - \hat{\rho}_t = \text{4DSW}_{jt} \cdot \tau_1 + \mathbf{1}\left[d_{jt} \leq D^*\right] \cdot (1 - \text{4DSW}_{jt}) \cdot \tau_2 + \iota_{jt} \end{gathered} \tag{D1}$$

As with our main results, Equation (D1) is estimated using the two-stage difference-in-differences estimator. Here, however, we restrict the first stage estimation sample to district-year observations that both operate on a five-day school week calendar and are located more than D^* kilometers from any 4DSW districts. In the second stage, we include an indicator variable which takes a value of one if the district-year observation d_{jt} is located less than D^* kilometers from the nearest 4DSW district. Structurally, this restricts the comparison group so that the parameter of interest, τ_1, is estimated off the pool of district-year observations that are more than D^* kilometers from a 4DSW district and therefore are unaffected by spillovers.

The remaining challenge in this approach is selecting some distance D^* where districts may not be subject to spillover effects. Selection of D^* constitutes a bias-variance tradeoff in that low values would preserve a large comparison group to identify treatment effects but may be biased by the previously described spillovers while higher values of D^* would be purged of bias but offer increasingly small comparison groups.

Instead of arbitrarily selecting D^*, we estimate effects across one-kilometer increments of D^* from 1 to 150 kilometers, or approximately the first through the 90th percentile of between-district movements that we observe individuals making prior to 4DSW adoption. If spatial spillovers were biasing our main results, these estimates would shift systematically as the distance threshold increases and potentially contaminated districts are removed from the comparison group. Stability of estimates across the full range of thresholds, at magnitudes similar to our main results, would instead indicate that spillovers into the comparison group do not meaningfully bias our estimates. We present these results in below where we find little evidence that the results we present in Section 5 are biased by spillovers.

Figure D1: Test for Bias in our Primary Retention Estimates from Spatial Spillovers

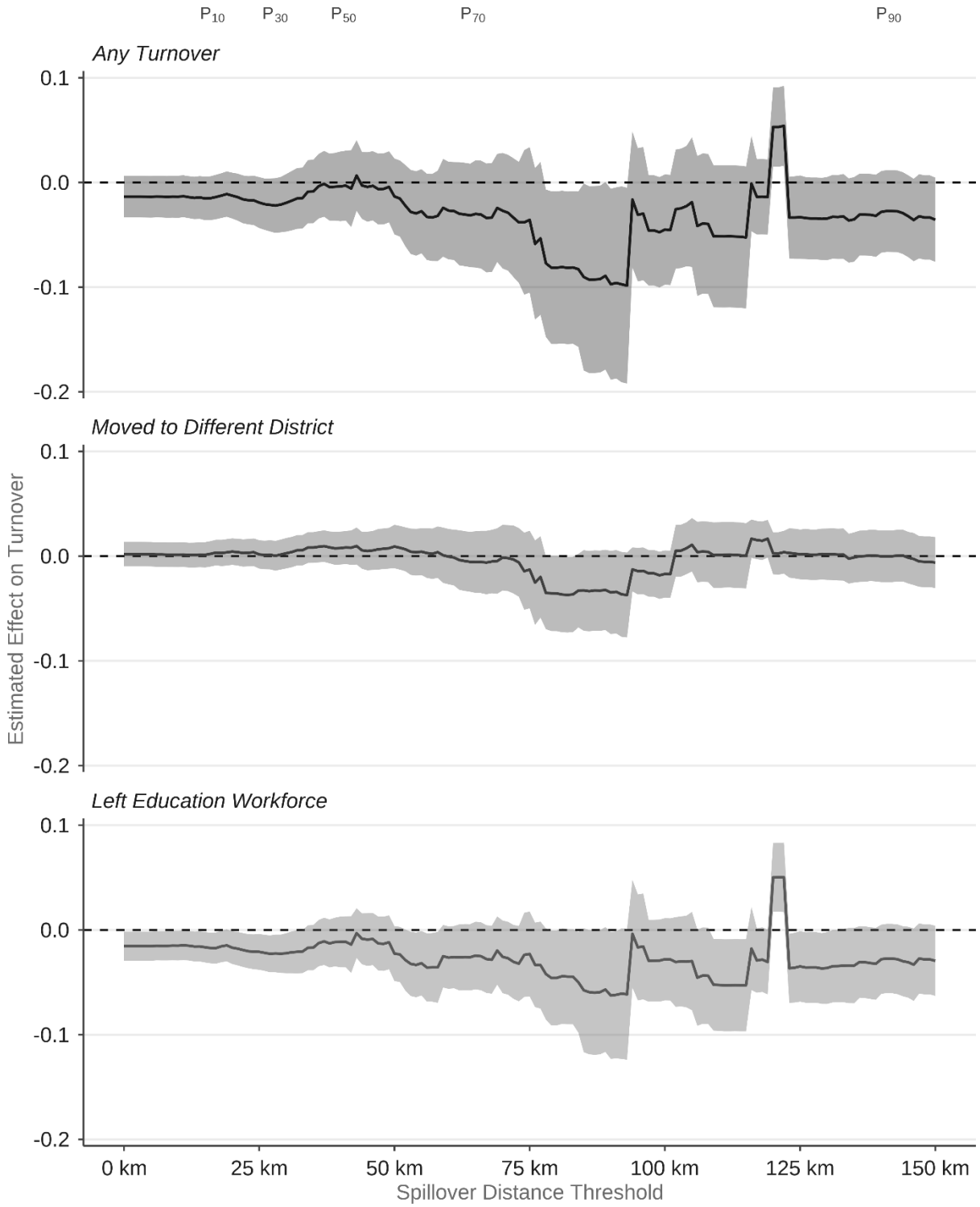


Figure D2: Test for Bias in our Primary Staffing Estimates from Spatial Spillovers

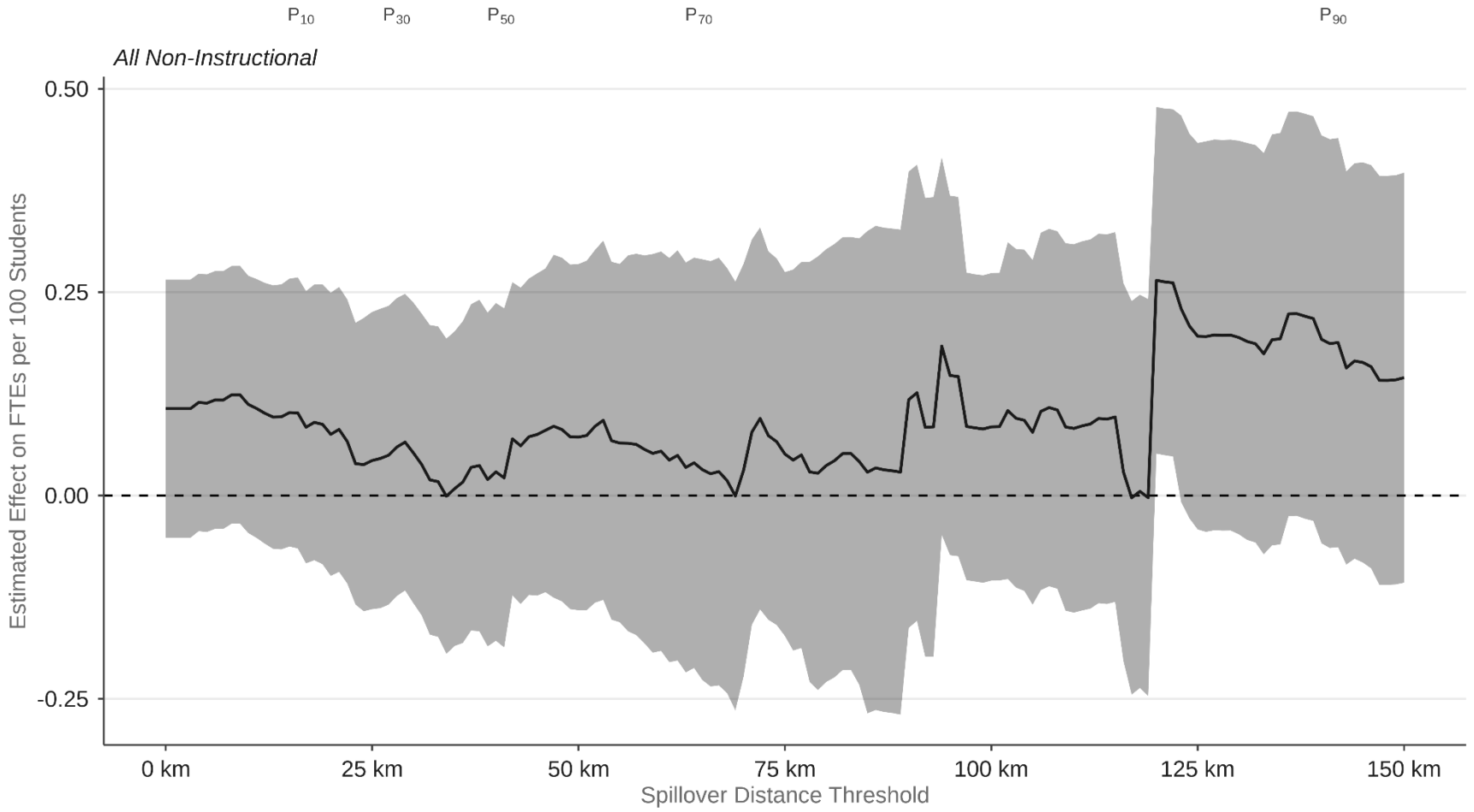
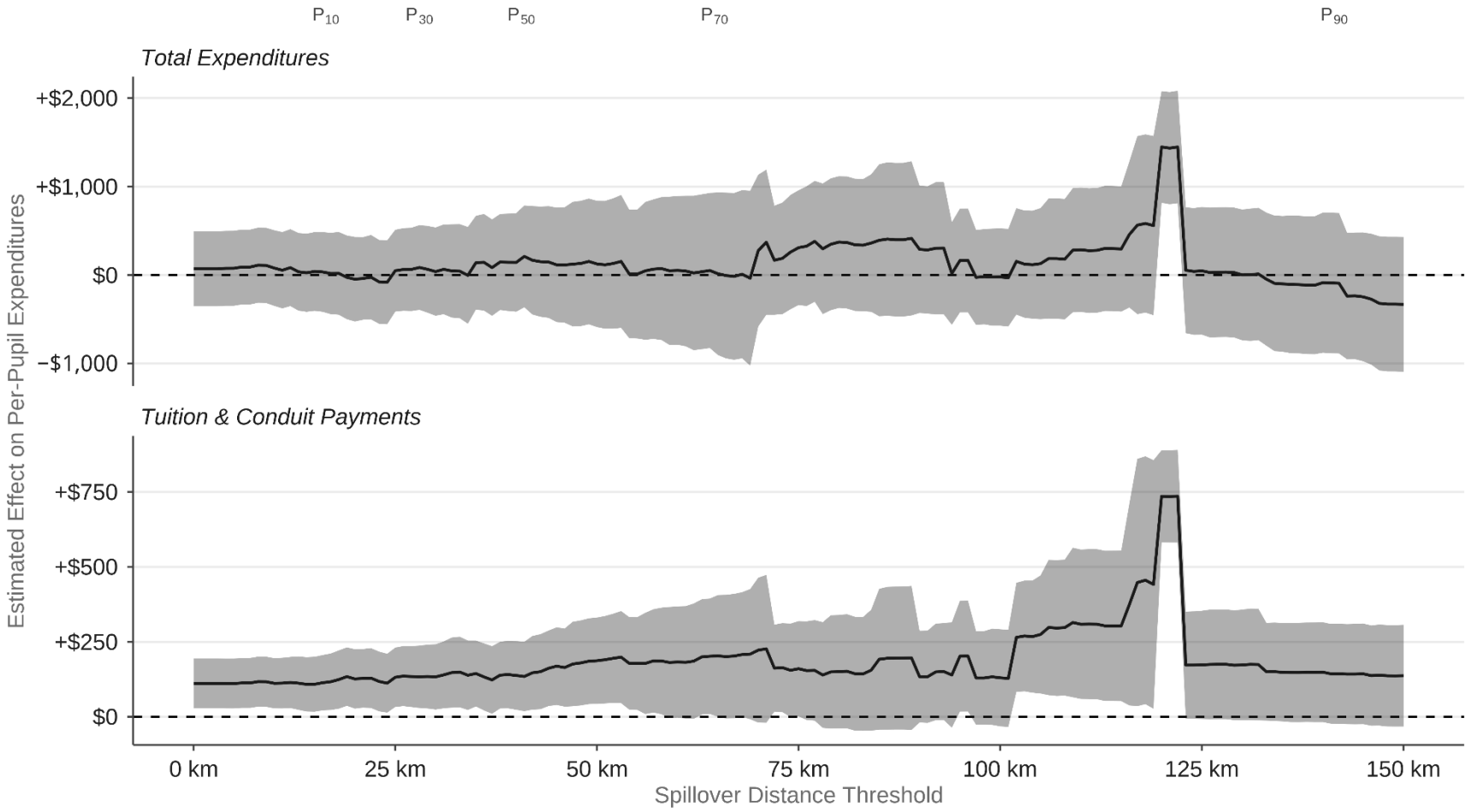


Figure D3: Test for Bias in our Primary Expenditure Estimates from Spatial Spillovers



APPENDIX E – ROBUSTNESS TO SATURATION EFFECTS

To determine saturation effects, we use the prevalence of 4DSWs in the commuting zone of a district at time of adoption to see if effects vary by the first adopter in an area versus later adopters. This allows us to explore if there are real short-term effects that fade out as 4DSWs become more prevalent. We find no significant effects for retention and staffing, and we find that our expenditure analysis works in the other direction; effects become stronger over time. Later adopters are spending comparably more on tuition payments, which is the most significant budgetary change that we identify in our analysis. Thus, overall, we do not find particularly notable heterogeneity by adopting time.

Figure E1: Event Study Estimates of Retention Effects, Net of Saturation & Saturation Effects

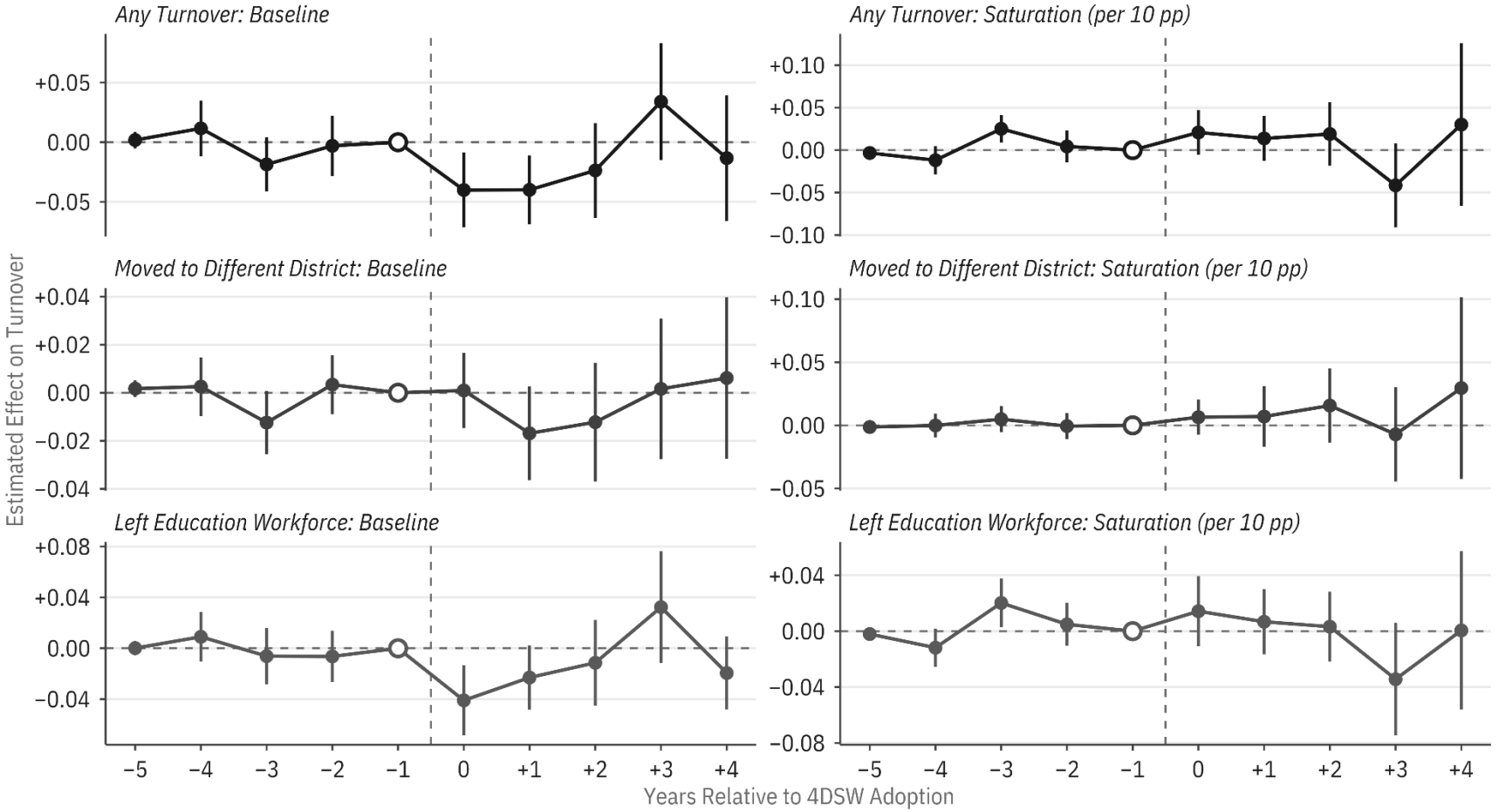


Figure E2: *Event Study Estimates of Staffing Effects, Net of Saturation & Saturation Effects*

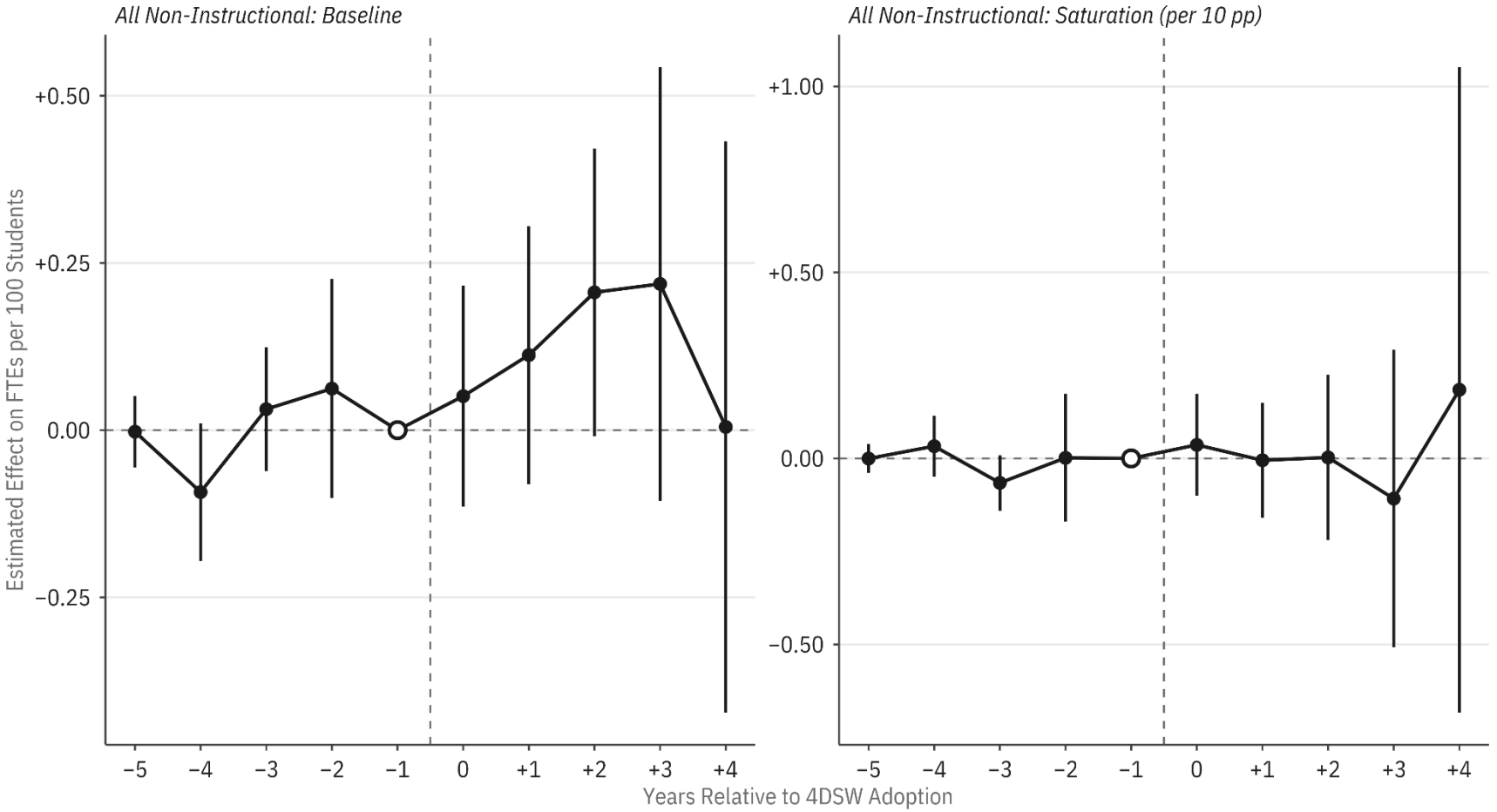
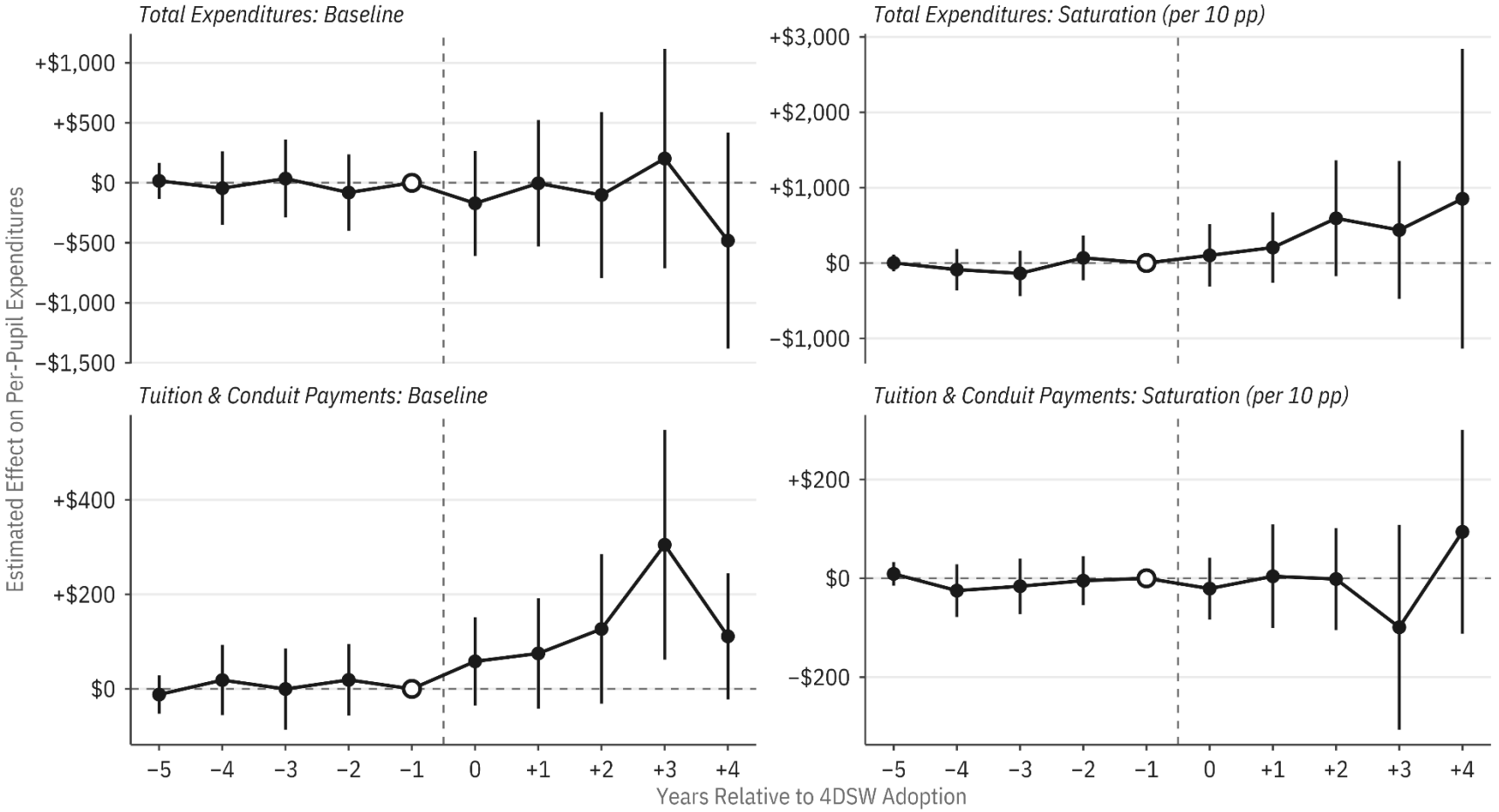


Figure E3: Event Study Estimates of Expenditure Effects, Net of Saturation & Saturation Effects



APPENDIX F – ADDITIONAL EVENT STUDIES

Figure F1: Event Study Estimates of Effects on Moving Districts, by Position

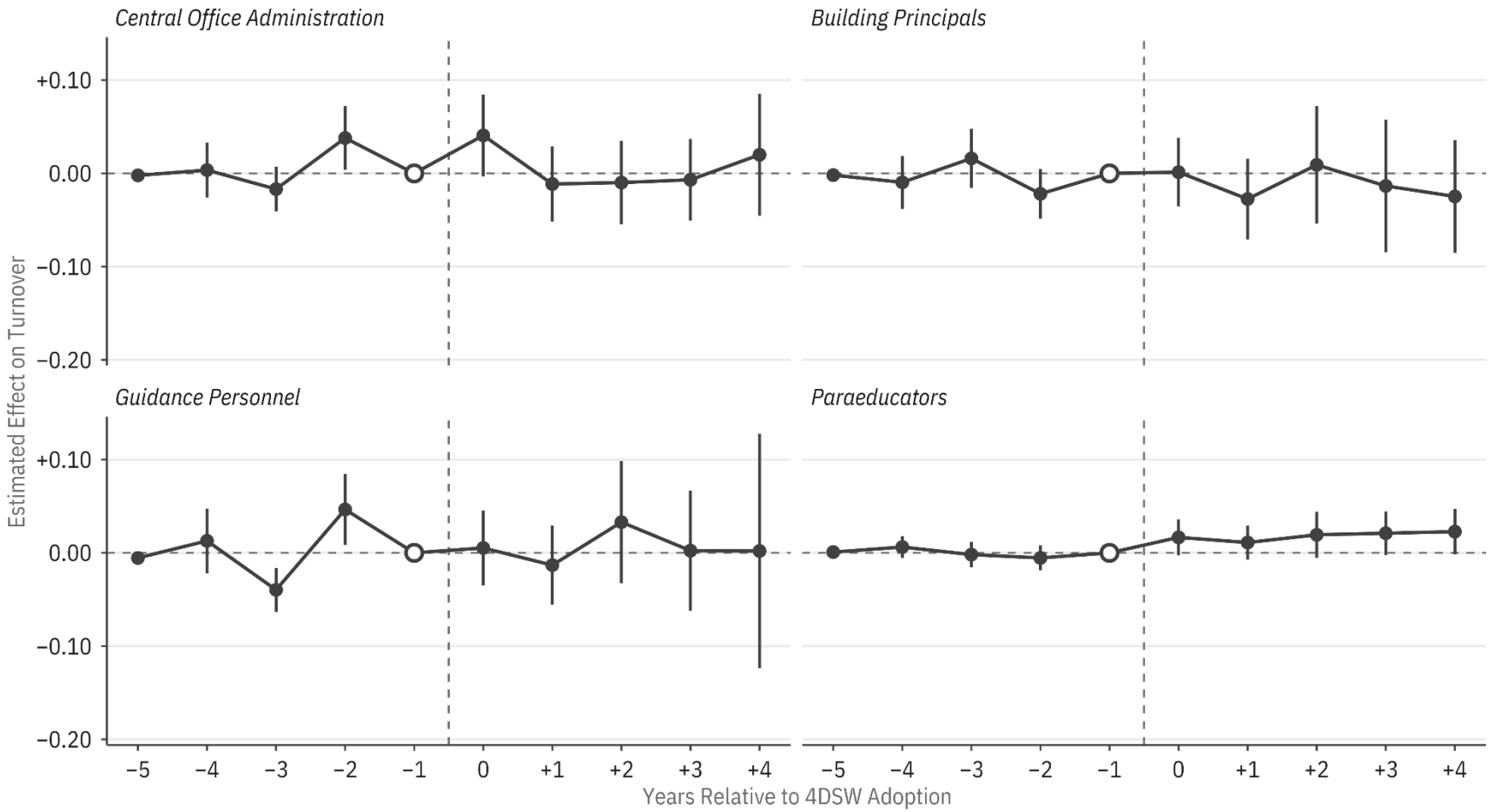


Figure F2: Event Study Estimates of Effects on Exiting Educator Workforce, by Position

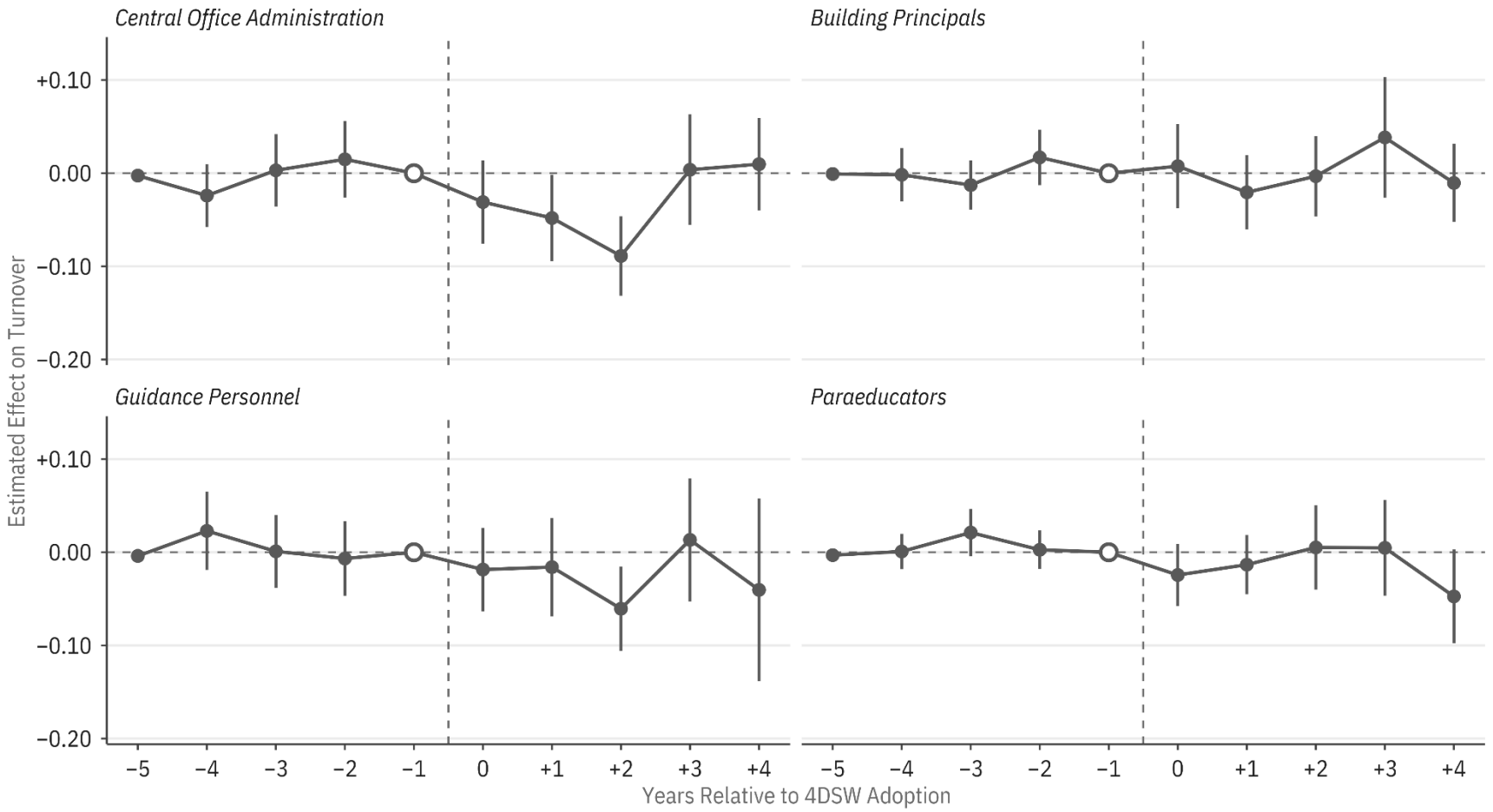


Figure F3: Event Study Estimates of Effects on Net Current Expenditures, by Function

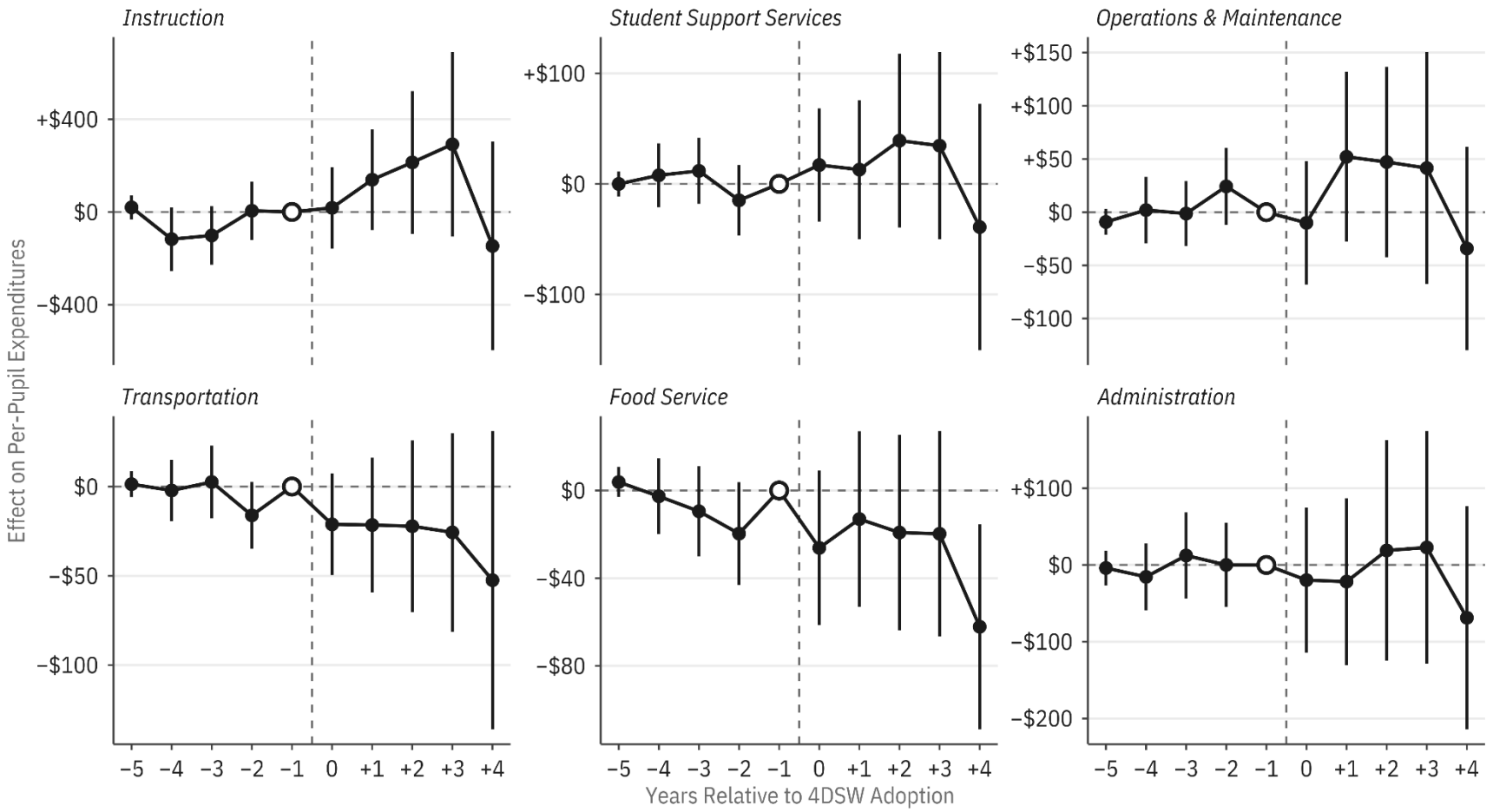


Figure F4: Event Study Estimates of Effects on Net Current Expenditures, by Object