Effects of Four-Day School Weeks on School Finance and Achievement: Evidence From Oklahoma

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Motivated by potential financial savings, 4-day school weeks have proliferated across the United States in recent years, reaching public schools in 24 states as of 2019. The consequences of the 4-day school week for students, schools, and communities are largely unknown. This article uses district-level panel data from Oklahoma and a difference-in-differences research design to examine the causal effect of the 4-day schedule on school district finance and academic achievement. Results indicate that 4-day weeks decrease districts' federal revenues and their noninstructional and support services expenditures. Decreases are concentrated specifically in operations, transportation, and food services expenditures and amount to approximately 2.03% of the average 4-day district's budget. There is no detectable effect of the 4-day week on academic achievement.

Keywords: achievement; educational policy; finance; learning environments; quasi-experimental analysis; rural education; secondary data

Four-day school weeks are widespread and growing across the United States, as 24 states had at least one school operating on a 4-day week schedule as of the 2018–2019 school year (Thompson et al., in press). Across these 24 states, approximately 665 districts and 1,607 schools were using the schedule, but information from state departments of education on district adoption exists only for six states and is otherwise sparse or unreliable (Heyward, 2018). Typically, these districts complete at least the same number of instructional hours required of all districts in a given state by lengthening the school day for the 4 days of the week they are in session. Most of the districts on the schedule are small and rural, but this is not always the case; for example, one urban Colorado district that serves approximately 18,000 students adopted the schedule at the start of the 2018–2019 school year. Based on responses from a sample of 342 districts nationwide, the most common reason cited as a main rationale for adoption (65.1% of districts) was financial savings; districts argue they are saving money by reducing costs such as transportation, heating, and support staff salaries (Thompson et al., in press). Districts acknowledge that reducing the school week by 1 day, or 20%, would not reduce spending by 20%, as teachers technically work the same number of hours, so their contracts, which comprise the greatest cost for the district, are not affected. Although less common, other rationales for adopting 4-day weeks were commonly cited (25%–35%) as well, such as attendance-related issues (e.g., chronic absenteeism, missing school for appointments or athletics) and rural-related issues (e.g., long bus rides, time to work on family farms and ranches, student/teacher retention).

Despite considerable anecdotal information and opinions on the advantages and disadvantages of the 4-day week, there remains a paucity of rigorous research examining its effects. The lack of empirical information raises concerns about the unknown potential impacts of the 4-day schedule related to a multitude of factors: school finance, student achievement, teacher hiring and retention, student attendance, students' health and well-being, delinquency, student motivation, and community and family resources (e.g., child care). The effect of this schedule change on a student may also depend on the student's age or other demographic characteristics. This article makes a key contribution to the small body of literature on 4-day school weeks by employing quasi-experimental research methods and district-level data from Oklahoma to examine the effect of the 4-day week on the factors

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of primary importance to policymakers and practitioners: school finance and academic achievement.

Effects of the 4-Day School Week on School Finance

Districts' spending, although connected to students' achievement outcomes, is an independently important topic for policymakers and government officials who are responsible for allocating resources across public needs, including education. If a public school district were able to make a change that reduced spending without any detriment to students, teachers, or communities, the district would certainly make that change. A large body of research has investigated the relationship between school finance and student achievement. Although the relationship was controversial historically (Greenwald et al., 1996; Hanushek, 1997), the more recent rigorous quasi-experimental research leveraging exogenous variation in school funding caused by school finance reforms (SFRs) consistently shows a positive relationship between educational resources and student achievement over time (Baker, 2016; Card & Payne, 2002; Gigliotti & Sorensen, 2018; Lafortune et al., 2018). The magnitude of the effect, however, is unclear: Whereas Gigliotti and Sorensen (2018) find that increasing per-pupil spending by $1,000 in New York public school districts was associated with a substantial 0.042 to 0.047 standard deviation increase on state test scores, Lafortune et al. (2018) find a smaller 0.011 to 0.024 yearly standard deviation increase on test scores per $1,000 increase in per-pupil revenues across a national sample. Nevertheless, scholars generally agree that increasing educational resources, especially among low-income districts, predicts increases in student achievement.

Highly relevant to the present study, Thompson's (2019a) working paper uses a differences-in-differences approach and a national dataset to show that 4-day school weeks on average reduce operating expenditures per pupil by 3.1% relative to all U.S. public 5-day districts over the same time period. He finds statistically significant reductions in district expenditures on transportation (7.0%), food services (6.8%), general administration (4.7%), student services (4.5%), and operations and maintenance (4.5%). To test the sensitivity of his analysis, he conducts several additional analyses restricting the sample in a variety of ways that provide alternative control groups and finds similar point estimates. Using the most restrictive control group, including only districts that ever adopted a 4-day school week, he produces very similar point estimates for each expenditure category and estimates that total district expenditures per pupil decrease by 2.1%.

Before Thompson's (2019a) work, Griffith's (2011) report provided the most rigorous descriptive estimates of the potential savings a district realizes from switching to a 4-day week. Using national finance data and financial estimates provided by individual districts across several states, he finds that districts on average saved between 0.4% and 2.5% of their budget and a maximum of 5.43% of their budget after making the switch. He further specifies that the cost savings, in order from greatest to least, appeared to be coming from the following categories of expenditures: operations and maintenance, school administration, student support, transportation, and food services. These categories, however, comprised only approximately 29% of districts' budgets, as most of their resources (~65%) were allocated for instructional costs (i.e., teachers' salaries and benefits), which were not significantly affected by the schedule change. For districts that operate their buildings on the 5th day or extend the hours of maintenance staff during the week, the savings would likely come only from student transportation and food service costs, which Griffith (2011) estimates would total to a maximum potential savings of 1.6% of a district's total budget. In Oklahoma, among the districts that ever adopt 4-day weeks, the average budget from 2009 to 2016 was approximately $3 million (in 2016 dollars), making a 1.6% savings equal to $48,000. Though 1.6% of the budget may seem inconsequential, such savings may be impactful for a small, rural district.

Effects of the 4-Day School Week on Academic Achievement

Two studies causally examine the effect of 4-day weeks and present conflicting results. Anderson and Walker (2015) use a school-level panel dataset from fourth and fifth graders in Colorado and find a 4% to 7% increase in students scoring above the proficient threshold in math and English language arts (ELA). However, Thompson (2019b), using a student-level panel dataset of students in Grades 3 through 8 in Oregon, finds a 5% to 7% decrease in students scoring above the proficient threshold in math and ELA. He attributes this finding, at least in part, to an average 3.5-hour reduction in instructional time in districts with 4-day weeks in Oregon. Furthermore, he finds that the 4-day school week is significantly more detrimental for low-income students. These contradictory study results indicate the need for additional quasi-experimental research examining the effect of 4-day weeks on achievement.

Other Effects of the 4-Day School Week

Further quasi-experimental research that considers the effects of the 4-day school week on other student and community outcomes is recent and sparse: The entirety consists of two working papers examining housing prices (Nowak et al., 2019) and parental labor supply (Ward, 2019) and one peer-reviewed publication examining delinquency (Fischer & Argyle, 2018). Based on Colorado housing transaction data from the first urban district to ever adopt a 4-day school week, Nowak et al. (2019) find a 2% to 5% decrease in house prices relative to surrounding school districts as an effect of 4-day school week adoption. Though this finding draws attention to an important potential consequence of adopting a 4-day school week in an urban district, it is unknown if this finding is generalizable to rural districts, which comprise the vast majority of 4-day week districts.

Ward (2019) uses data from all Public Use Microdata Areas (PUMA) across Colorado, Idaho, Oklahoma, and Oregon to estimate effects of 4-day school weeks on parental employment. He finds that increasing the portion of students in a PUMA enrolled in a district with a 4-day school week from 0% to 25% causes an 11% decrease in employment (7.6 percentage points) relative to baseline among married mothers whose children are all between the ages of 5 and 13. This negative effect is largely

borne by mothers who have a 4-year college degree or greater, suggesting that there may be great heterogeneity in the effect at the district level based on demographics. The 4-day school week had no detectable effect on single mothers' or married fathers' employment status.

Fischer and Argyle's (2018) quasi-experimental study exploits variation in the adoption of the 4-day school week across rural schools in Colorado to examine the relationship between school attendance and juvenile crime. They find that on Fridays, the day off for 4-day schools, the corresponding police and sheriff agencies where at least one 4-day high school was located experienced a 20% increase in overall crime and a 27% increase in property crime among juveniles compared to the agencies in areas composed of high schools with 5-day weeks. Perhaps shockingly high, these findings are supported by other research considering the school attendance–crime relationship and strongly suggest that the impact of the 4-day schedule extends beyond the school context and into communities (Jacob & Lefgren, 2003; Luallen, 2006). Of course, however, these potentially negative consequences may be offset by financial gains from switching to a 4-day week schedule.

Oklahoma Policy Context

Four-day school weeks have particularly proliferated in public schools in Oklahoma over the past 10 years. Oklahoma House Bill 1864, effective April 24, 2009, changed the state requirements on instructional time for traditional public school districts such that they no longer had to have both 180 days and 1,080 hours of classroom instruction in a school year, but only had to fulfill the 1,080 hours (H.B. 1864 [2009]). As a result, districts could have fewer school days per year if they lengthened their days. Schools first recorded the complete schedule change, meaning every full school week was planned to be 4 days long, in the fall of 2010, and increasing numbers of schools have made the switch since then (see Appendix Figure A1, available on the journal website, for the timing of 4-day school week adoption in Oklahoma). Public records from the Oklahoma State Department of Education indicate that 91 of Oklahoma's 513 public school districts (17.7%), representing approximately 41,000 of the 640,000 total K–12 public school students (6.4%), had at least one school on an entirely 4-day week schedule at the start of the 2017–2018 school year.

Examining the increase in 4-day schooling specifically in Oklahoma is critical because of Oklahoma's particular destitution in regard to education funding; over the past several years, the state has consistently ranked among the five states with the lowest spending per pupil, spending only $8,097 per pupil in the 2016 fiscal year, $3,665 short of the national average (U.S. Census Bureau, 2016). Oklahoma has undergone years of tax cuts and consequent decreases in sales tax revenue that have led the state to face one of the largest education budget crises in the history of the United States. The extremity of the situation has recently been illuminated by teacher walkouts in pursuit of better pay, as Oklahoma teachers were the second lowest paid in the United States at the start of the 2017 school year, earning an average annual salary of about $45,000 ($7,000 less than teachers in Texas, a neighboring state). Therefore, any savings a district realizes by switching to this 4-day schedule, even if small, could be seen as worthwhile. However, whether the 4-day week actually saves money is unclear and controversial.

Research Questions

The existing research on 4-day school weeks is scarce, inconclusive, and, in some cases, conflicting; nevertheless, the schedule continues to spread. Given the 4-day week's supposed financial motivation, its potential to significantly impact student achievement, and its continued growth in Oklahoma and across the country, this study notably provides the first analysis of both the financial and academic effects of the 4-day school week using data from Oklahoma. Estimating these effects using the same methods and sample uniquely allows for the examination of the schedule's effect on the major motivating reason for adopting the schedule, financial strain, in context of its effects on what many are most concerned by, student achievement. More specifically, the study seeks to answer the two following questions: (1) What is the effect of the 4-day school week on district revenues and expenditures per pupil? and (2) What is the effect of the 4-day school week on students' math and ELA achievement?

Methods

Data

This study employs 13 years (2004–2005 to 2016–2017) of demographic and financial data and 7 years (2008–2009 to 2015–2016) of achievement data from all K–12 public school districts in Oklahoma. The complete panels include district-year observations from 512 traditional public school districts in Oklahoma and were constructed using school-level calendar data from the Oklahoma State Department of Education (available through 2018–2019), district-level demographic data from the National Center for Education Statistics Common Core of Data (CCD), district-level achievement data from the Stanford Education Data Archive[1] (SEDA; Reardon et al., 2017), and district-level finance data from the CCD Local Education Agency (School District) Finance Survey (F-33).

The district-level calendar data are constructed from K–8 school-level calendar data. Typically, the 4-day week districts in the sample do not have multiple elementary or middle schools serving the same district; and even if they do, all K–8 grade levels have adopted the 4-day week at the same point in every case; thus, the treatment is effectively adopted at the district-level. The CCD demographic data include each district's yearly number of students enrolled, racial composition (available from only 2008–2009 to 2016–2017), percentage of students eligible for the National School Lunch Program (NSLP), percentage of English learners, percentage of special education students, and pupil-teacher ratio. The SEDA dataset includes yearly estimates of each district's math and ELA test scores relative to the statewide standardized test score distributions aggregated over Grades 3 through 8 at the district level. Financial data from the F-33 include yearly estimates of districts' spring 2009–2017 revenues and expenditures. The mutually exclusive and exhaustive categories of per-pupil revenues (i.e., federal, state, and local) and per-pupil expenditures (i.e., instructional, support

service, noninstructional, nonelementary/secondary, capital, and other local education agency [LEA]) are examined as well as several specific categories of per-pupil expenditures selected for their potential relevance to 4-day weeks (i.e., operations, food, transportation, administration, student support, and instruction). All financial estimates should be interpreted in 2010 dollar-value unless otherwise noted.

The analytic sample is restricted to include only rural schools because all schools in Oklahoma on 4-day weeks during this period were located in rural areas; therefore, rural 5-day schools are likely to provide a better counterfactual than nonrural 5-day schools when examining change over time. Oklahoma's 399 rural districts in 2017 comprise approximately 78% of the state's total districts. The analytic sample also excludes districts that adopted a 4-day school week for the first time in 2017 (as well as those that adopted it for the first time in 2016 for analyses including the achievement data) in order to allow for sufficient posttreatment observations and to ensure the presence of each treated district in all pretreatment and posttreatment years in the selected analytical models. Descriptive statistics for 4-day districts (all rural), 5-day rural districts, and 5-day nonrural districts from spring 2005 to 2017 (Grades 3–8) are presented in Table 1. Notable differences include that, on average, rural districts have much smaller enrollments, higher percentages of NSLP-eligible students, higher percentages of White and Native American students, lower standardized test scores, higher funding per pupil, and higher expenditures per pupil than the 5-day nonrural (i.e., town, suburban, and urban) districts. These differences support the decision to exclude non-rural districts from the analytic sample.

Empirical Strategy

The present study uses panel data and a quasi-experimental difference-in-differences research design to estimate the impact of 4-day school weeks by comparing the changes over time in outcomes of districts with 4-day weeks to the contemporaneous changes in districts that never or did not yet have four-day weeks. Specifically, I estimate variations of the following difference-in-differences (DID) specification:

$$Y_{dt} = \lambda_d + \theta_t + \beta Fourday_{dt} + X'_{dt}\gamma + \epsilon_{dt}, \quad (1)$$

where Y_{dt} is the dependent variable of interest (e.g., transportation expenditures per pupil), λ_d are district fixed effects, θ_t are year fixed effects, β represents the effect of the 4-day week, $Fourday_{dt}$ is an indicator variable that takes on a value of one each year a district has a 4-day week schedule, and ϵ_{dt} is an error term that accommodates for clustering at the district level (Bertrand et al., 2004). X_{dt} is a vector of covariates that controls for potential shocks varying within districts over time that are historically linked to both financial and achievement outcomes. These covariates include, for each district-year observation, the percentage of students eligible to receive free or reduced-price lunch through the NSLP, the percentage of English learners, and the percentage of special education students.

Embedded in Equation (1) are several important assumptions that require scrutiny. First, the specification implies that the effect of 4-day school weeks (i.e., the "treatment") will be constant, or "static." However, one might expect that the effect of a 4-day week could vary depending on the length of time the district had the schedule. For example, Thompson (2019b) finds students may experience an initial decline in achievement when they switch schedules but then stabilize to preswitch achievement levels over time. Alternatively, students' achievement may be benefited or harmed by the schedule increasingly each year they are exposed to the treatment, resulting in a growing effect (positive or negative) of the 4-day week over time. These considerations are also important in relation to school finance outcomes, as savings may decrease or increase over time as districts adjust to and, ideally, learn how to optimize the schedule for savings. To account for potential time-varying treatment effects, I specify semidynamic fixed-effects DID models that allow the schedule to have distinct effects the first year the district adopts the 4-day week and after 2 or more years of having a 4-day week:

$$Y_{dt} = \lambda_d + \theta_t + \sum_{\tau=0}^{1+} \beta_{+\tau} Fourday_{d,t+\tau} + X'_{dt}\gamma + \epsilon_{dt}, \quad (2)$$

where τ is the number of years after a school has adopted the 4-day schedule (the first year of adoption, $\tau = 0$) and $\beta_{+\tau}$ represents the effect of 4-day weeks τ years after a district adopts the schedule. Joint *F*-tests are additionally employed to test the null hypothesis of a constant treatment effect, $H_0 : \beta_0 = \beta_{1+}$.

Another critical assumption embedded in the DID specification is the "parallel trends" assumption, which requires that changes in the outcomes over time in the "control" districts (i.e., the districts that never had a 4-day schedule or had not yet adopted the 4-day schedule) are comparable to the changes that would have occurred in districts that switched to the 4-day schedule had they never switched. In order to interpret estimates as causal effects, it is essential that the parallel trends assumption is not violated. For example, it would be problematic if districts that eventually switch to 4-day weeks had, preswitch, decreasing transportation expenditures in comparison to districts that never switch. In that case, it would not be possible to attribute any changes in transportation expenditures after adoption of the 4-day week to the schedule change as opposed to the different trends existing between the districts before the switch; the districts that never switch to a 4-day week would not be a valid counterfactual for the districts that do switch. To examine the empirical validity of this assumption in this study, I use the Granger causality test ("event study") as a falsification check (Angrist & Pischke, 2009) that estimates the effect of the schedule change on the outcome variables for the years before and after the change:

$$Y_{dt} = \lambda_d + \theta_t + \sum_{\tau=1}^{a} \beta_{-\tau} Fourday_{d,t-\tau} + \sum_{\tau=0}^{1+} \beta_{+\tau} Fourday_{d,t+\tau} + X'_{dt}\gamma + \epsilon_{dt}, \quad (3)$$

where *a* is the number of years in which all districts are observed preswitch, and $\beta_{-\tau}$ represents the "effect" of being τ years prior

Table 1
Descriptive Statistics

	Rural Districts (Analytic Sample)				Nonrural Districts	
	4-Day (n = 49)[a]		5-Day (n = 352)		5-Day (n = 113)	
District-Level Variables	**Mean**	***SD***	**Mean**	***SD***	**Mean**	***SD***
District characteristics						
% NSLP eligible	74.93	14.41	64.95	16.43	60.59	16.48
% ELL	0.83	2.14	3.08	8.08	5.04	8.29
% special ed.	21.38	8.38	16.68	7.68	13.92	5.05
% White	64.81	19.07	60.66	21.52	57.82	17.42
% Native American	27.26	19.76	28.43	23.29	22.41	17.11
% Asian	0.18	0.87	0.52	1.67	1.24	1.65
% Hispanic	5.24	7.13	8.02	11.72	12.52	12.94
% Black	2.51	4.96	2.37	6.79	6.02	7.64
Enrollment count	278.69	210.63	502.99	513.76	4,091.45	6,778.03
Pupil-teacher ratio	13.21	2.84	13.89	3.86	15.77	1.49
Standardized test scores						
Math	–0.23	0.35	–0.10	0.31	0.01	0.26
ELA	–0.20	0.28	–0.08	0.26	0.01	0.22
District finances						
Revenues per pupil						
Total	10,064.83	3,472.08	9,500.02	2,855.16	7,974.75	903.80
Federal	1,295.36	520.21	1,398.50	898.80	1,096.89	538.43
State	5,068.63	1,476.40	4,863.99	1,303.19	4,354.34	743.39
Local	3,700.84	3,248.34	3,237.52	2,679.92	2,523.52	1,074.67
Per-pupil expenditures						
Total	10,031.38	3,546.62	9,423.66	2,694.01	7,953.87	986.72
Instructional	4,959.40	1,287.56	4,793.39	1,066.10	4,148.50	488.61
Support services	3,591.93	1,334.42	3,217.16	1,146.43	2,605.24	458.57
Student support	497.70	242.00	479.13	219.06	475.58	124.99
Administration	494.08	285.35	401.30	212.27	403.18	98.03
Operations	1,067.54	588.41	1,032.11	542.62	807.19	246.12
Transportation	322.86	211.03	297.77	157.01	217.93	78.64
Other	1,209.76	537.52	1,006.84	508.18	701.36	231.57
Noninstructional	763.78	294.93	701.63	221.41	504.78	113.54
Food services	589.61	209.19	559.98	192.51	425.22	105.79
Other	174.17	149.45	141.65	96.46	79.56	48.12
Non-EL/Sec	23.44	122.44	12.67	44.25	25.74	56.66
Capital outlays	602.68	1,681.14	613.89	1,126.29	596.35	578.21
Other LEA	90.15	126.16	84.92	183.79	73.25	100.39

Note. The data used in this table are drawn from a panel of the 512 traditional public school districts (Grades 3–8) in Oklahoma observed annually for 13 years from school year (SY) 2004–2005 to SY 2016–2017 (N = 6,656). Rural districts that adopted a 4-day week schedule for the first time in 2017 (n = 47) are excluded from the analytic sample; rural districts that adopted a 4-day week schedule for the first time in 2016 (n = 14) are additionally excluded from the analytic sample for standardized test scores. Schedule data are from the Oklahoma State Department of Education, spring 2005–2019. Achievement data are from the Stanford Education Data Archive (SEDA; Reardon et al., 2017), spring 2009–2016. Racial demographic data are from National Center for Education Statistics Common Core of Data (CCD), spring 2009–2017. All other demographic and school finance data are also from the CCD, including the Local Education Agency (School District) Finance Survey (F-33), spring 2005–2017. ELL = English language learner; LEA = local education agency; Non-EL/Sec = non-elementary/secondary; NSLP = National School Lunch Program.
[a]All 4-day districts are also represented in the 5-day districts group when they have not yet switched to a 4-day schedule.

to adopting a 4-day week schedule relative to never switching to the 4-day schedule or being a+1 or more years preswitch. In order to support the parallel trends assumption, the "effect" of eventual 4-day week adoption on treated districts relative to control districts should be constant in the years preceding a district's switch to the 4-day week. Joint F-tests are employed to test the null hypothesis of a constant pretreatment "effect" equal to zero, $H_0 : \beta_{-a} = \beta_{-a+1} = ... = \beta_{-1} = 0$ and a constant posttreatment "effect," $H_0 : \beta_0 = \beta_{1+}$.

The causal interpretation of Equation (1) also requires that there is not selection on observables into or out of treatment. Concern regarding selection *into* treatment would be warranted

if changes over time in treatment districts during the pretreatment period that differ from the concurrent changes in control districts are driving the treatment districts' switch to a 4-day schedule. For example, evidence that enrollments decrease more in districts that eventually switch to a 4-day week than in control districts during the pretreatment period would be suggestive of selection on observables into treatment. Concern regarding selection *out of* treatment would be warranted if districts' characteristics were changing over time in the posttreatment period differently in the treatment and control districts. For example, evidence that the percent of NSLP-eligible students in treatment districts increases more than it does in control districts during the posttreatment period would be suggestive of selection on observables out of treatment. I employ both of the following specifications to examine this assumption:

$$X_{\mathrm{dt}} = \lambda_{\mathrm{d}} + \theta_{\mathrm{t}} + \beta Fourday_{dt} + \epsilon_{\mathrm{dt}}, \tag{4}$$

$$X_{\mathrm{dt}} = \lambda_{\mathrm{d}} + \theta_{\mathrm{t}} + \sum_{\tau=1}^{a} \beta_{-\tau} Fourday_{d,t-\tau} + \sum_{\tau=0}^{1+} \beta_{+\tau} Fourday_{d,t+\tau} + \epsilon_{\mathrm{dt}}, \tag{5}$$

where X_{dt} represents time-variant district characteristics. The event study in Equation (5) provides a further interrogation of the DID estimates from Equation (4).

As an additional robustness check regarding selection into treatment based on observables, I conduct the Equation (1) DID analyses with two additional, more restrictive control groups. The first alternative control group is created by predicting each district's likelihood to receive treatment based on their observable characteristics from 2009 using the following specification:

$$EverFourday_d = X'_d \gamma + \epsilon_{\mathrm{d}}, \tag{6}$$

where $EverFourday_d$ is an indicator variable that takes on a value of one if the district adopted a 4-day school week by 2016 and X_d is a vector of covariates from 2009 plausibly linked to a district's adoption of the 4-day school week by 2016. Estimates of $\hat{\gamma}$ are used to generate a matched comparison group of the 5-day districts ($n = 49$) who were most likely to receive treatment by 2016 based on their observable characteristics. The second alternative control group includes the districts that adopted a 4-day school week in years following the sample data, 2018 or 2019 ($n = 10$). This control group is a valuable comparison group because districts that have adopted a 4-day school week in the past are likely similar in unobservable ways to those that adopt them in the future. The reduced sample sizes of both of these control groups in comparison to the primary control group of all rural districts in Oklahoma reduces the model's power to detect a treatment effect; therefore, the statistical significance of the resulting DID point estimates using the alternative control groups can be interpreted as conservative.

One limitation of this empirical strategy using the present data regards an assumption embedded in DID specifications that have variation in treatment timing. When there is variation in treatment timing and there are heterogeneous treatment effects over time, the fixed effects DID estimator is a weighted average of all two-group/two-period DID estimators (Goodman-Bacon, 2018). The two groups being compared in the DID specification in Equation (1) are the treatment group, which consists of districts that switch to the 4-day week in a particular year, and the control group, which consists of the districts not treated in the same year. The weights on each 2 × 2 comparison are proportional to the number of districts in the treatment versus control groups and the variance of treatment status within each pair. Whereas the proportion of districts in treatment versus control will be highest in comparisons made using districts that switch to a 4-day week later during the study period, the variance of treatment status within a comparison group will be largest in comparisons made using districts that switch earlier during the study period. Therefore, districts that switch to the 4-day schedule in the middle of the study period will have the highest weights and could be overrepresented in the fixed-effects DID estimator, which would be problematic if treatment effects were to vary over time. Gibbons et al. (2018) also demonstrate that fixed effects estimators provide weighted averages of treatment, further arguing that these weighted averages can poorly represent the average treatment effect (ATE) and are particularly likely to if there are heterogeneous treatment effects; they developed an estimator of the ATE that reweights the observations to produce consistent and unbiased point estimates. Implementing this regression-weighted estimator (RWE), I find that, although there are qualitative differences in some of the estimates due to the noisiness of particular estimates, the Gibbons et al. (2018) Wald tests indicate that none of the differences between the RWE estimates (i.e., the ATE) and the ordinary least squared (OLS) fixed-effects estimates are statistically significant.

Results

Difference-in-Differences Results

DID and semidynamic DID analyses were conducted examining the effect of the 4-day school week on the following outcomes: the mutually exclusive and exhaustive categories of district revenues (i.e., federal, state, local), the mutually exclusive and exhaustive categories of district expenditures (i.e., instructional, social support, noninstructional, nonelementary/secondary, capital, other), a set of relevant granular categories of district expenditures (i.e., operations, food services, student transportation, student support, administration, and instruction), and students' math and ELA test scores.

District revenues. The DID and semidynamic DID point estimates of the effect of the 4-day week on the exhaustive categories of district revenues are presented in Table 2. Though the 4-day week schedule did not have a significant detectable effect on total per-pupil revenue, it significantly decreased districts' federal revenue per pupil by an average of $99.48, which is approximately 5.62% of a 4-day district's average federal revenue per pupil before switching schedules. The corresponding joint *F*-test ($p < .05$) indicates that the decrease in districts' federal revenue

Table 2
Effects of the 4-Day School Week on Per-Pupil Revenues (Exhaustive Categories)

	Per-Pupil Revenues							
	Total		Federal		State		Local	
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4-day week	–270.74		–99.48**		–177.34		6.08	
	(276.23)		(46.12)		(140.13)		(265.09)	
Adoption year		–308.25		–176.89***		–70.80		–60.56
		(286.41)		(57.28)		(131.13)		(252.94)
1+ year lag		–257.22		–71.58		–215.73		30.09
		(285.85)		(46.81)		(153.76)		(282.74)
Adj. R^2	.747	.746	.751	.751	.740	.740	.789	.789
p-value: ($H_0: \beta_0 = \beta_{1+}$)	—	.763	—	.020	—	.170	—	.590

Note. Standard errors, clustered at the district level, are in parentheses. All models include district fixed effects (FE), year FE, and the following district-level covariates: the percentage of students eligible to receive free or reduced-price lunch, the percentage of English learners, and the percentage of special education students (coefficients suppressed). The panel data in this table include the 352 rural districts (Grades 3–8) that adopted a 4-day school week between 2010 and 2016 or never had a 4-day week schedule. The districts are observed annually from 2005 to 2017 ($N = 4{,}576$). Schedule data are from the Oklahoma State Department of Education, demographic data are from the National Center for Education Statistics Common Core of Data (CCD), and school finance data are from the CCD Local Education Agency (School District) Finance Survey (F-33).
**$p < .05$. ***$p < .01$.

per pupil was significantly larger the first year of treatment (–$176.89) than in the following years (–$71.58). An additional DID regression examining the many subcategories of federal revenue revealed that, of the $99.48 average decrease in federal revenue per pupil experienced by 4-day week districts, the largest portion of the decrease that could be attributed to any one category of federal revenue was designated specifically for nutrition (β = –$42.44, $SE = 6.99$, $p < .01$). The federal government provides this revenue to districts to reimburse them for expenditures on free and reduced-price NSLP school meals. There was no significant detectable effect of 4-day weeks on state or local revenues, and the joint *F*-tests conducted for each semidynamic DID specification failed to reject a constant treatment effect over time for total, state, and local per-pupil revenues.

District expenditures. The DID and semidynamic DID point estimates of the effect of 4-day weeks on the exhaustive categories of district expenditures are presented in Table 3. The 4-day week did not have a significant detectable effect on total per-pupil expenditures; and it had small and insignificant effects on instructional, nonelementary/secondary, capital, and other district per-pupil expenditures. However, the results suggest that 4-day weeks decreased expenditures per pupil in the noninstructional category, which primarily includes spending on food services; and in the support services category, which includes spending on student transportation and operations, with marginal significance ($p < .10$). Noninstructional expenditures per pupil decreased by an average of $54.10 ($p < .10$), which is approximately 6.90% of what 4-day districts spent on this category per pupil before switching schedules. Support services expenditures per pupil decreased by an average of $170.15 ($p < .10$), which is approximately 4.53% of what 4-day districts spent on support services per pupil before switching schedules. The joint *F*-tests conducted for each semidynamic DID specification failed to reject a constant treatment effect over time for all expenditure categories.

The DID and semidynamic DID point estimates of the effect of 4-day weeks on the relevant granular categories of district expenditures are displayed in Table 4. The average decrease in operations expenditures per pupil is estimated to be $86.70 ($p < .01$), which is approximately 7.21% of what 4-day districts spent on operations before switching schedules and 0.84% of their average total expenditures per pupil. The average decrease in transportation expenditures per pupil is estimated to be $40.10 ($p < .01$), which is approximately 10.76% of what 4-day districts typically spent on transportation services before switching schedules and 0.39% of their average total expenditures per pupil. The average decrease in food services expenditures per pupil is estimated to be $77.25 ($p < .01$), which is approximately 12.02% of what 4-day districts spent on food services before switching schedules and 0.75% of their average total expenditures per pupil. The corresponding joint *F*-test ($p < .01$) indicates that the decrease in districts' food services expenditures per pupil was significantly larger in the years following the first year of treatment (–$87.18) than in the first year of treatment (–$49.70). There was no detectable effect of the 4-day week schedule on per-pupil expenditures for administration, student support, or instruction. The joint *F*-tests conducted for each semidynamic DID specification failed to reject a constant treatment effect over time for all other expenditure categories besides food services.

Academic achievement. The DID and semidynamic DID point estimates of the effect of 4-day weeks on students' standardized math and ELA test scores are presented in Table 5. Although the majority of the point estimates are negative, all point estimates in both the DID and semidynamic DID models are small and statistically insignificant from zero, indicating there is no detectable effect of the 4-day week on academic achievement.

Table 3
Effects of the 4-Day School Week on Per-Pupil Expenditures (Exhaustive Categories)

	Per-Pupil Expenditures													
	Total		Instructional		Support Services		Noninstructional		Non-EL/Sec		Capital Outlays		Other LEA	
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
4-day week	–273.59		–94.79		–170.15*		–54.10*		–2.12		17.77		–0.13	
	(241.39)		(91.14)		(89.52)		(27.72)		(14.02)		(166.84)		(17.41)	
Adoption year		–150.06		1.21		–130.02		–38.09**		–5.57		28.46		–13.10
		(199.75)		(109.07)		(80.48)		(17.27)		(5.25)		(131.24)		(14.82)
1+ year lag		–318.10		–129.37		–184.61*		–59.87*		–0.87		13.91		4.54
		(277.23)		(98.34)		(100.82)		(33.29)		(17.42)		(200.59)		(19.93)
Adj. R^2	.706	.706	.737	.737	.817	.817	.789	.789	.501	.501	.166	.166	.267	.266
p-value: $(H_0 : \beta_0 = \beta_{1+})$	—	.424	—	.181	—	.476	—	.390	—	.721	—	.936	—	.261

Note. Standard errors, clustered at the district level, are in parentheses. All models include district fixed effects (FE), year FE, and the following district-level covariates: the percentage of students eligible to receive free or reduced-price lunch, the percentage of English learners, and the percentage of special education students (coefficients suppressed). The panel data in this table include the 352 rural districts (Grades 3–8) that adopted a 4-day school week between 2010 and 2016 or never had a 4-day school week schedule. The districts are observed annually from 2005 to 2017 (N = 4,576). Schedule data are from the Oklahoma State Department of Education, demographic data are from the National Center for Education Statistics Common Core of Data (CCD), and school finance data are from the CCD Local Education Agency (School District) Finance Survey (F-33). LEA = local education agency; Non-EL/Sec = non-elementary/secondary.
*$p < .1$. **$p < .05$.

Table 4
Effects of the 4-Day School Week on Per-Pupil Expenditures

	Per-Pupil Expenditures									
	Operations		Food Services		Transportation		Admin		Student Support	
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
4-day week	–86.70***		–77.25***		–40.10***		42.98		–26.18	
	(31.57)		(15.99)		(15.23)		(29.92)		(22.14)	
Adoption year		–56.45**		–49.70***		–33.19***		27.15		–1.85
		(26.56)		(15.83)		(11.88)		(32.83)		(22.95)
1+ year lag		–97.61**		–87.18***		–42.59**		48.68		–34.95
		(39.81)		(17.48)		(17.31)		(32.33)		(24.31)
Adj. R^2	.809	.809	.827	.827	.752	.752	.647	.647	.660	.660
p-value: $(H_0 : \beta_0 = \beta_{1+})$.	—	.334	—	.007	—	.413	—	.453	—	.108

Note. Standard errors, clustered at the district level, are in parentheses. All models include district fixed effects (FE), year FE, and the following district-level covariates: the percentage of students eligible to receive free or reduced-price lunch, the percentage of English learners, and the percentage of special education students (coefficients suppressed). The panel data in this table include the 352 rural districts (Grades 3–8) that adopted a 4-day school week between 2010 and 2016 or never had a 4-day week schedule. The districts are observed annually from 2005 to 2017 (N = 4,576). Schedule data are from the Oklahoma State Department of Education, demographic data are from the National Center for Education Statistics Common Core of Data (CCD), and school finance data are from the CCD Local Education Agency (School District) Finance Survey (F-33).
**$p < .05$. ***$p < .01$.

Robustness Checks

The results of the event study specifications used to examine the robustness of the parallel trends assumptions embedded in the specifications used in Tables 4 and 5 are respectively presented in Appendix Tables A1 and A2, available on the journal website. The event study provides suggestive evidence about whether, conditional on district and year fixed effects, outcomes of districts that were a number of years away from switching ("1-year lead," "2-year lead," etc.) to a 4-day week were trending differently from those of districts that would not adopt a 4-day week or were 6 or more years (Table A1) or 3 or more years (Table A2) preadoption. In support of the parallel trends assumption, the results in Tables A1 and A2 respectively depicted in Appendix Figures A2(a–g) and A3(a–b), available on the journal website, fail to reject the null hypothesis that there are no significant differences between the treatment and control districts before treatment for any of the examined outcomes. However, it is important to note that, as seen in Table A1, 4-day week districts had marginally significantly ($p<.10$) lower operations expenditures per pupil 1 and 2 years before they adopted a 4-day week.

A second set of robustness checks considers the associations between 4-day week adoption and district characteristics over time. Because districts voluntarily choose to adopt the 4-day week (i.e., the schedule is not randomly assigned), selection bias

Table 5
Effects of the 4-Day School Week on Student Achievement

	Dependent Variables			
	Standardized Math Scores		Standardized ELA Scores	
Independent Variable	(1)	(2)	(3)	(4)
4-day	–0.052		–0.032	
	(0.048)		(0.041)	
Adoption year		–0.031		–0.020
		(0.042)		(0.039)
1+ year lag		–0.060		–0.036
		(0.055)		(0.046)
Adj. R^2	.679	.679	.699	.699
p-value: $(H_0 : \beta_0 = \beta_{1+})$	—	.546	—	.734

Note. Standard errors, clustered at the district level, are in parentheses. All models include district fixed effects (FE), year FE, and the following district-level covariates: the percentage of students eligible to receive free or reduced-price lunch, the percentage of English learners, and the percentage of special education students (coefficients suppressed). The panel data in this table include the 338 rural districts (Grades 3–8) that adopted a 4-day school week between 2011 and 2015 or never had a 4-day week schedule. The districts are observed annually from 2009 to 2016 ($N = 2{,}704$). Schedule data are from the Oklahoma State Department of Education, demographic data are from the National Center for Education Statistics Common Core of Data (CCD), and school finance data are from the CCD Local Education Agency (School District) Finance Survey (F-33). ELA = English language arts.
*$p < .1$. **$p < .05$. ***$p < .01$.

is a chief threat to the validity of the present study. The inclusion of district fixed effects in my models controls for unobservable heterogeneity between districts (e.g., differences in the percentage of students NSLP-eligible) averaged over the total time period of the study. However, selection bias could still exist if treatment districts are experiencing changes within that time period that are different from the changes experienced by control districts that lead to or are results of switching to the 4-day week.

As specified in Equations (4) and (5), I test for selection bias related to students entering or exiting 4-day schools by regressing time-variant district characteristics on the 4-day week conditional on time and district fixed effects. The time-variant district characteristics include (a) the racial demographic composition of districts (Appendix Table A3, available on the journal website): the percentage of students who are Native American, the percentage of students who are White, and the percentage of students who are Hispanic; and (b) other district characteristics (Appendix Table A4, available on the journal website): the natural log of district enrollment, the percentage of students who are NSLP-eligible, the pupil-teacher ratio, and total per-pupil revenue. All of the point estimates in Appendix Tables A3 and A4 are substantively small and statistically insignificant, indicating that I fail to reject the null hypotheses that (a) districts are not selecting into treatment due to changes in district characteristics during the pretreatment period and (b) districts are not changing with respect to those same characteristics during the posttreatment period. Therefore, I find no strong evidence for selection into or out of treatment based on observables using this method.

I also test for selection bias by conducting the same DID and semidynamic DID analyses respectively specified in Equations (1) and (2) with the previously defined two alternative, more restrictive control groups: the matched comparison control group and the future 4-day school week adopters (i.e., 2018 or 2019) control group. Descriptive statistics of the original treatment and control groups as well as the two alternative control groups are presented in Appendix Table A5, available on the journal website. The alternative control group DID and semidynamic DID analyses are presented for the granular district expenditures outcomes in Appendix Table A6 and for the achievement outcomes in Appendix Table A7, both available on the journal website. The point estimates for each specification are similar in both magnitude and statistical significance to those of the original analyses presented in Tables 4 and 5. If selection bias into treatment were present in the original analysis, one would expect that the analyses with more restrictive control groups would find comparatively smaller and weaker effects of the 4-day school week because the treatment and control groups are more similar; thus, these results also provide no evidence for selection into treatment based on observables.

Discussion

As education budgets have tightened across the United States, increasing numbers of districts have turned to the 4-day school week as a cost-saving strategy. The present study used panel data from Oklahoma public schools and a DID research design to provide the first rigorous, quasi-experimental analysis of the effects of the 4-day school week on both school finance and academic achievement. The results suggest the limited savings from 4-day weeks are more likely concentrated in federal revenues (95% confidence interval [CI]: –$190.18, –$8.77) than state (95% CI: –$452.93, $98.26) or local (95% CI: –$515.28, $527.43) revenues. More specifically, districts' reduced spending is concentrated in the following three categories: (a) operations expenditures, which decrease by approximately $86.70 (in 2010 dollars; 95% CI: –$148.80, –$24.62) per pupil (a 7% decrease); (b) transportation expenditures, which decrease by approximately

$40.10 (in 2010 dollars; 95% CI: –$70.06, –$10.14) per pupil (an 11% decrease); and (c) food services expenditures, which decrease by approximately $77.25 (in 2010 dollars; 95% CI: –$108.69, –$45.81) per pupil (a 12% decrease) when a district switches to a 4-day school week.

Because food services expenditures are largely reimbursed by the federal government in many of these communities (>70% of students are NSLP-eligible in 4-day week districts), the "savings" for the district caused by reducing expenditures on food services are likely less than they appear. As seen in the results presented herein, expenditures on food services and federal revenue designated for nutrition simultaneously decrease in districts with 4-day school weeks. Therefore, the "savings" generated by reducing spending on food services appear to be realized only partially by the district, as much of the savings (~55%) are coming out of districts' federal revenue and effectively being returned to the federal government in the form of subsidies for school meals the government no longer has to provide on the 5th day. If and how students are accessing healthy meals on the 5th day as well as who absorbs the cost of that meal remain open and important questions. Furthermore, there is no evidence of savings regarding districts' expenditures for administration or student support, two of the categories of expenditures that Griffith (2011) argues are likely to generate the most savings after operations.

Based on these analyses, there is not convincing empirical support for one of the primary motivations for making the switch to a 4-day week: that it will preserve significant *local* revenue. There is limited direct financial benefit to the district; rather, there is perhaps a small benefit to the federal government due to savings from providing one fewer day per week of school meals to NSLP-eligible students. The combined decreases in operations, transportation, and food services expenditures comprise approximately 2.03% of the average 4-day district's total expenditures before they adopted the 4-day week (2.03% = ~$68,000 in 2019 dollars). Although possible, it is unlikely that such savings are of practical significance. Furthermore, in this study I find no support for the argument that the savings are being redirected to instruction or other types of expenditures, as there is no significant positive effect of the 4-day week on any expenditure category.

I also find no detectable effect of the schedule on math or ELA achievement. This finding counters that of both Anderson and Walker (2015), who find a generally small (4%–7%) increase in the percent of fourth- and fifth-grade students scoring above the proficient threshold in Colorado; and Thompson (2019b), who finds a small (5%–7%) decrease in the in the percentage of students scoring above the proficient threshold among students in Grades 3 through 8 in Oregon. Although these different results could reflect true differences in the average effect of the policy across these states, they more likely reflect the generally large amount of variation in the implementation of the policy and its effects on academics by district, resulting in statistical noisiness in the estimations. One can imagine that whether adopting the 4-day school week impacts key factors related to student achievement, such as student attendance, instructional time, teacher quality and retention, or student fatigue, could vary greatly based on a district's specific implementation of the policy.

Nevertheless, for districts motivated to switch to the 4-day school week for reasons other than academics, which seems to be most, if not all, districts, the conflicting evidence and null result presented herein may encourage them to persist with a 4-day school week. If a district is saving money, even if relatively little, or realizing some other benefit not observed in the present study, and student achievement is stable, perhaps the concerns about the 4-day school week's negative effects on student achievement are not justifiable. Overall, these findings suggest that the 4-day week may not reduce district expenditures or have the dramatic effects on student achievement that so many people expect it to.

The generalizability of the results presented herein to states outside of Oklahoma depends on the similarity of the implementation of the 4-day school week across states as well as the differences in the experiences of students on their "day off" across states. Thompson et al. (in press) provide the first descriptive evidence of adoption and implementation trends of the four-day school week nationally. Their work indicates, at a high level, that there is indeed variation in whether school buildings are open and whether programming for students or professional development happens on the day off; however, they do not disaggregate these findings at the state level. Although more research is needed to describe the precise differences in the 4-day week across states (e.g., perhaps certain states consistently offer a full day of programming to students on the day off, whereas other states offer reduced, irregular, or no programming on the day off), there is no *a priori* reason to expect that the 4-day week is being implemented and experienced by teachers, students, and families in dramatically different ways across states. Additionally, the similar National Center for Education Statistics (NCES)–designated rurality of most 4-day districts across states suggests certain commonalities in the experiences of these communities that would bolster the generalizability of the present findings. Nevertheless, Oklahoma is a somewhat unique state due to its previously described desolate state of education funding and teacher shortages, both of which are larger incentives for adopting the 4-day school week in Oklahoma than in other states (Thompson et al., in press).

The lack of knowledge about what happens on the day off and the variety of unexamined potential impacts the schedule likely has on students, families, and communities necessitate further empirical research. Future work should focus on (a) describing the impact of 4-day school weeks on students' out-of-school time; (b) parsing apart the relationship between the schedule and achievement into the many possible mechanisms driving the relationship (e.g., student attendance rates, teacher quality, teacher retention); (c) exploring the factors that cause the schedule switch to save more or less money (e.g., opening the school on the 5th day, start/end times, providing food for the 5th day); and (d) investigating the effect of the schedule on other student, family, school, and community outcomes (e.g., physical and mental health, social-emotional skill development, child care expenses, parental employment choices). The established connection between the 4-day school week and increased delinquency indicates the importance of considering how the schedule change impacts students' lives outside the school context. Developmental systems theory (Lerner & Castellino, 2002) supports such inquiry, arguing that development is an interactive process occurring as youth regulate and integrate their various relational contexts (e.g., family, school, society). In this case, the

student's role in regulating the interactions between these contexts might look like a student choosing to spend time on her or his day off of school to volunteer, to study, or, perhaps, to get into trouble. These different choices could lead to very different developmental outcomes (e.g., identity, motivation, sense of purpose, health, well-being, etc.) for the individual both within school and in other contexts (Larson et al., 2004). Investigating these effects of the 4-day week as well as the academic and financial effects is essential for informing policymakers and practitioners about the consequences, both positive and negative, of policies allowing districts to adopt the schedule.

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NOTES

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[1]A unique version of SEDA was created for this project in collaboration with Dr. sean reardon in order to provide estimates of achievement based on pooled averages of scores aggregated across Grades 3 through 8 for districts that have very small student populations. These districts' grade-level achievement data would necessarily be redacted in the original version of SEDA and other data sources based on privacy concerns due to the small sample size of students comprising each district-grade observation.

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