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School Calendars and Student Obesity

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Ample evidence documents rising student obesity in summer months and falling student obesity during the school year. One theory for this pattern is that out-of-school days lack some of the structure and health-promoting behaviors that schools provide. Given this observed seasonal pattern, a natural question is whether there is room for policies that alter the summer vacation to serve as an intervention. Compared to traditional calendars, year-round school calendars redistribute the same total number of school days more evenly across the calendar year, reducing the length of the summer break and lengthening breaks elsewhere. Such calendars, therefore, offer the unique opportunity to disentangle whether it is total time in school or the distribution of that time that matters for obesity. Using detailed data for all 5th graders in public schools in California, we take advantage of calendar changes between year-round school calendars and traditional calendars across 20 years to estimate the impacts of school calendar type on obesity. We find evidence of no change in obesity from various year-round school models that redistribute school time. Our findings indicate that there is nothing specific about the particularly long break at summer that matters for obesity accumulation. It is simply time spent in the school environment and not the distribution of that time driving changes in childhood obesity across the year.

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Abstract

Ample evidence documents rising student obesity in summer months and falling student obesity during the school year. One theory for this pattern is that out-of-school days lack some of the structure and health-promoting behaviors that schools provide. Given this observed seasonal pattern, a natural question is whether there is room for policies that alter the summer vacation to serve as an intervention. Compared to traditional calendars, year-round school calendars redistribute the same total number of school days more evenly across the calendar year, reducing the length of the summer break and lengthening breaks elsewhere. Such calendars, therefore, offer the unique opportunity to disentangle whether it is total time in school or the distribution of that time that matters for obesity. Using detailed data for all 5th graders in public schools in California, we take advantage of calendar changes between year-round school calendars and traditional calendars across 20 years to estimate the impacts of school calendar type on obesity. We find evidence of no change in obesity from various year-round school models that redistribute school time. Our findings indicate that there is nothing specific about the particularly long break at summer that matters for obesity accumulation. It is simply time spent in the school environment and not the distribution of that time driving changes in childhood obesity across the year.

1. Introduction

The epidemic of childhood obesity is a major public health concern. Between 1976 and 2020, the prevalence of childhood obesity in the US quadrupled, from 5% to 20% (Trust for America's Health, 2024), then increased a further 2% during the COVID-19 pandemic (Anderson et al, 2023). Childhood obesity is associated with various health conditions in childhood, such as type II diabetes, early puberty and metabolic syndrome, and also increases the risk of adulthood obesity and subsequent health issues such as cardiovascular disease, type 2 diabetes, arthritis and other joint problems, and some cancers (Biro and Wein, 2010; Geserick et al, 2018).

The social and economic costs of obesity are substantial as well. In addition to the economic costs of treating obesity-related health conditions, children and adults with obesity suffer from social stigma, higher risk of dropping out of school (von Hippel & Lynch, 2014; Benson et al, 2018), wage discrimination (Dolado et al., 2023), and lower stability in romantic relationships, especially among girls and women (Côté & Bégin, 2020). All these risks motivate the intense focus on childhood obesity specifically and highlight the need for early intervention (Biro and Wein, 2010; Geserick et al, 2018).

Policymakers have long acknowledged the potential for schools to impact student health, as evidenced by long-standing policies such as hiring school nurses, offering vision and hearing exams, incorporation of physical education in the curriculum, school meals programs, and after-school sports. As concerns about student obesity grew in the United States and other countries, many schools implemented programs directly aimed at reducing child obesity, such as school soda bans, tighter regulations on the content of school meals (Belot et al., 2016; Chriqui et al., 2014; Datar and Nicosia, 2012), school-based programs to encourage physical activity and increases in required physical education (Cawley at al., 2013).[1] While compliance with some school obesity policies is limited (von Hippel & Frisvold, 2023), some programs do change consumption and activity patterns but often fail to reduce obesity significantly, showing child obesity to be a difficult metric to impact (Cawley, 2015; von Hippel & Bradbury, 2015; von Hippel & Bogolasky, 2023).

Although school-based anti-obesity policies have had little success, there is evidence that simply being in school protects children against obesity. Various studies have established that children's overweight and obesity prevalence rise during summer vacation and fall during the academic year (von Hippel et al., 2007; von Hippel and Workman, 2016; Moreno, et al., 2015; Baranowski, et al., 2014). The "structured day hypothesis" posits that schools structure children's time in ways that encourage behaviors leading to fitness and healthy BMI (Brazendale, et al., 2017). Schools limit screen time, limit opportunities to snack, and cause families to enforce regular sleep schedules. Schools schedule regular, if not always vigorous,

[1] While implemented more generally for child nutrition than obesity, standard school meals programs, such as the National School Lunch Program and Breakfast in the Classroom have also been evaluated for their potential impacts on BMI and obesity, finding no notable effect (see Corcoran et al., 2016 ; Hinrichs, 2010).

physical activity, and schools provide meals that, while not ideal, are on average more nutritious than meals that children bring from home (Nader, et al. 2008; Au, et al. 2016). When school lets out, constraints loosen, and children are freer to behave in ways that increase obesity and reduce fitness.[2]

In light of summer weight gain, a natural question is whether policies that shorten the summer vacation might reduce obesity. In this study, we estimate the impact of year-round school calendars, which shorten summer vacation, on student body mass index (BMI), overweight, and obesity. Although their name might suggest otherwise, year-round school calendars do not increase the days that children spend in school, but instead redistribute the same total number of school days more evenly across the seasons, with a shorter summer break but more frequent two- and three-week breaks during fall, winter, and spring (Figure 1). Whether such calendars can impact student obesity therefore depends on whether simply the total number of unstructured days matters or whether the distribution of unstructured days is also important for obesity and fitness. For example, is there something unique to the particularly long summer break that leads to behavioral changes, where two shorter breaks would not? If so, then it is possible that schools could reduce obesity and improve fitness by adopting year-round school calendars that shorten summer vacation.

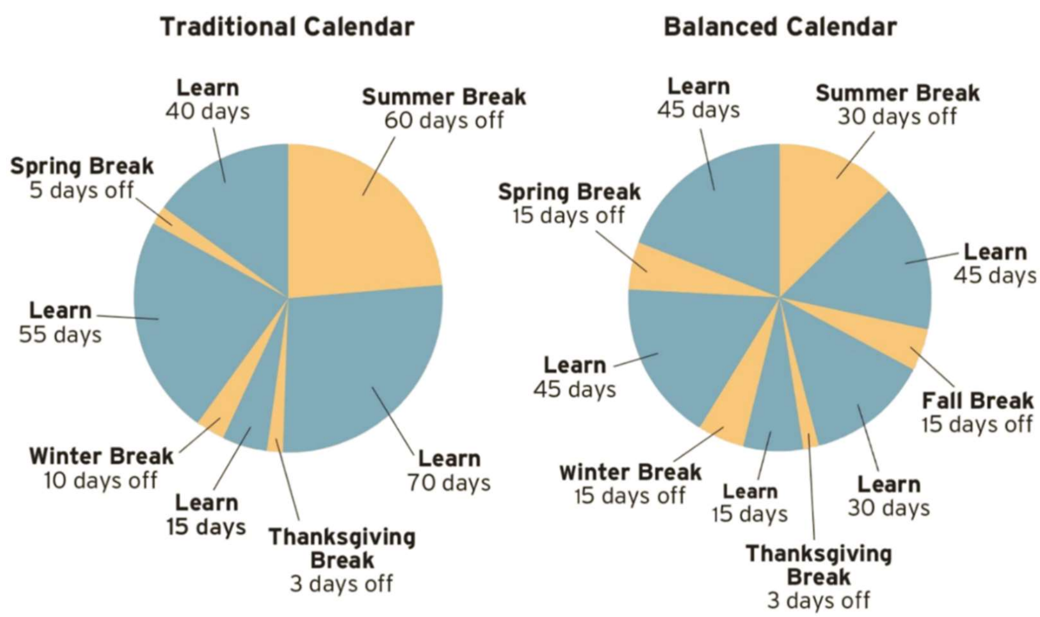

Figure 1. The distribution of time in and out of school under a traditional school calendar (left) compared to a popular type of year-round calendar (right). Source: von Hippel and Graves (2023).

[2] Other estimated policy impacts show that educational environments provide health-promoting structure. For example, access and length of daily exposure to Head Start has been shown to have lasting impacts on reduction of obesity through adolescence (Carneiro and Ginja, 2014; Frisvold and Lumeng, 2011).

While year-round calendars were not designed or adopted with any student health benefits in mind, they do shorten summer vacation while adding mini-vacations in fall, winter, and spring. If summer vacation increases the risk of obesity, then year-round calendars might reduce it.

On the other hand, some types of year-round calendars might actually increase student obesity. Many year-round schools use a "multi-track" calendar that staggers student schedules so that different children are in and out of schools at different times. These multi-track calendars are challenging for families with children on different schedules, as evidence by decreases in maternal employment (Graves, 2013), difficulty in retaining experienced teachers, who are often working parents with children on different schedules (Graves, McMullen and Rouse, 2018), and declines local housing values, reflecting a negative homebuyer valuation of the school model (Depro and Rouse, 2015). In short, if schools protect children from obesity by offering a more "structured day," then the multi-track calendar, by disrupting that structure, may reduce schools' protective effect.

Finally, it is possible that year-round calendars might have no impact on obesity, since although they reduce summer vacation, they do not actually change the amount of time that students spend in school. If what matters is not summer vacation, per se, but total exposure to school and non-school environments, then the net effects of year-round calendars may in fact be zero.

Indeed, past research on year-round calendars has often found null effects on academic outcomes, again because year-round calendars do not increase total time spent in school. Compared to traditional school calendars, year-round calendars do increase learning during summer, but they reduce learning during spring and fall, resulting in no increase in the total amount learned over a 12-month period (von Hippel, 2016). Difference-in-difference analyses in North Carolina suggest that year-round calendars have no effect on learning, while similar analyses in California suggest a small negative effect, perhaps because year-round calendars increase teacher turnover and reduce retention of experienced teachers (McMullen and Rouse, 2012; Graves, 2010; Graves, McMullen, & Rouse, 2018).

More broadly, school policies that alter time in school, even when not aimed at affecting student behavior or health, sometimes produce notable impacts on health-related outcomes. For example, the school policy of a 4-day school week has had negative impacts on adolescent diet, physical activity and drug use, yet no statistically significant changes in obesity (Tomayko et al., 2020 and Tomayko et al., 2021). School policy mandating later school start times has been found to have positive impacts on sleep and fewer absences for teenagers (e.g. Edwards, 2012; Bastian and Fuller, 2023). Some relationships between later school start times and lower body weight and lower depressive symptoms have also been documented (Gariépy et al., 2018; Wheaton et al., 2015). While these studies primarily focus on adolescents rather than elementary-aged students, it is notable that policies that alter the organization of time in school can have large behavioral impacts, making potential health impacts of year-round schools a plausible and open empirical question.

Prior evidence on the obesity effects of year-round calendars is quite limited. In a two-year study, Brusseau et al. (2019) reported that summer weight gain was slower at a year-round school than at a nine-month school, but their study was limited to two schools observed for a single summer, with no measure of weight gain during fall, winter or spring, Weaver et al. (2021) reported that elementary students (grades K-6) gained more during school and less during summer on a year-round calendar. But their sample of schools was small (2 traditional, 1 year-round) and though year-round schools changed the seasonality of weight gain, it is not clear whether they reduced total weight gain over periods of a year or more. In both studies, it was unclear whether the year-round and nine-month schools were comparable with respect to other characteristics.

In this study, we present the first large-scale, multi-year, causal estimation of the effects of year-round calendars on BMI, overweight, and obesity. In a staggered difference-in-difference analysis, we exploit the fact that hundreds of California schools switched between traditional and various types of year-round calendars over a two-decade period from 2000-2001 through 2018-2019. We find no impacts on BMI, overweight or obesity relative to a traditional school schedule from various year-round school calendars that redistribute school time, indicating that it is not the length of continuous unstructured time under the long summer break that increases obesity in summer months. Instead, our findings suggest that it is simply time spent in the school environment and not the distribution of that time driving changes in childhood obesity across the year.

2. Data and Policy Context

We use a mixture of publicly available and restricted data for our study.

School calendar data

First, we use publicly available panel data from the California Basic Educational Data System (CBEDS), which contains detailed school-level information, including data on which school calendars were used in every school and year.

We focus on three main types of year-round school calendars in California: single-track, multi-track and Concept 6. The single-track year-round school redistributes the school days across the calendar year differently than on a traditional calendar but does not alter anything else. A second type of year-round school is the multi-track model. The multi-track year-round school alters the distribution of days across the calendar year in the same way as the single-track model, but also splits the student body into groups (i.e. tracks) that rotate on and off breaks in an alternating fashion. This allows more students to be taught in the same school building, as the entire student population is never simultaneously in session. For this reason, the multi-track model has been widely adopted to aid with school crowding. Like the single-track model, whether multi-track year-round schools can impact obesity depends on whether it is just the total or the distribution of unstructured days that matter, but also may be impacted by various organizational challenges to school programs. For example, both a single-track and multi-track year-round calendar will result in breaks occurring in non-summer months, where activities like summer camps are less readily available, potentially altering student health-affecting behaviors during non-school time. While a single-track year-round calendar could in theory offer equivalent "camp-like" activities on site during the more frequent breaks throughout the year, a multi-track calendar could not, as the building is continuously used to full capacity.

A variant of the year-round calendar called a "Concept 6 calendar" in California is a type of multi-track calendar. However, it comes with an additional change. Concept 6 schools do not just redistribute school days more evenly and use tracks, but also reduce the total number of school days by 10 percent, from 180 to 163. However, Concept 6 schools were also only implemented in schools experiencing severe levels of school crowding.

Data on BMI, overweight, and obesity

Throughout our study period, all California fifth graders were required to take the FitnessGram assessment designed by the Cooper Institute in Dallas, Texas. FitnessGram data were recorded by physical education teachers. For each school, the state published the percentage of fifth graders whose BMI was in the "healthy fitness zone" (HFZ), but the Cooper Institute changed the definition of the HFZ three times during the authors' study period—first in 2005, then in 2010-11 (Welk et al, 2011a, 2011b), and then in 2013-14 when the Cooper Institute aligned the FitnessGram BMI standards with standards used by the Centers for Disease Control and Prevention (CDC). Changes to the HFZ made it impossible to estimate trends over time. It was also unclear how well the state had managed data quality issues—for example, whether it had screened out biologically implausible values of 5th graders' heights and weights.

We therefore requested and obtained restricted individual student-level data from the FitnessGram, which included measures of students' gender (M/F), height (m), weight (kg), birth date, and date of measurements, which we used to construct the students' age at the time of measurement. From height and weight, we calculated body mass index (kg/m^2), then used CDC standards to calculate each student's BMI z-score with respect to a "reference population" of children of the same gender and age whose BMIs were measured in the 1960s-80s (Kuczmarski, 2000). Finally, we calculated whether each student had overweight or obesity according to CDC cutoffs for BMI. Cutoffs vary by gender and age; the cutoff for overweight corresponds to the 85th percentile of the reference population, and the cutoff for obesity corresponds to the 95th percentile (Ogden & Flegal, 2010).

To construct our analytic sample, we start by restricting the sample to children with non-missing values for height, weight and gender. We deliberately removed values that were biologically implausible for 5th graders and represented clear outliers in the distribution. In particular, we limited the data to heights between 47 and 73 inches, weights between 40 and 225 pounds, BMIs between 10 and 40 kg/m^2, and Z scores for height, weight, and BMI that exceeded 5 in absolute value. These steps were consistent with WHO guidelines and inspection of histograms for smoothness and outliers.

We calculated age as the difference between the child's birth date and measurement date. Unfortunately, birth date was missing for roughly 40% of students in the early years of our sample and test dates are missing for these same years (see Appendix Table 1).[3] As no dates or ages were missing in later years of our sample, we estimated their relationship with height, gender, and other variables, and used that relationship to impute age at measurement in earlier years.

Our complete dataset is comprised of individual students in 5th grade cohorts within a panel of all public schools in California over the range of school years from 2000-2001 through 2018-19.

3. Empirical Strategy and Identifying Variation

We use a staggered differences-in differences (D-D) approach to identify the causal impact of three distinct year-round school calendars—single-track, multi-track, and Concept 6—on four outcomes of interest: BMI, BMI z-score, overweight, and obesity. We also include height and sex as "placebo outcomes." Since there are no plausible mechanisms by which school calendars could affect height or sex, we expect that a correctly specified model would find no significant effect of calendar-switching on either variable.[4]

Recent econometric developments have altered the standard approach for estimating D-D with differential timing in policy implementation. Until recently, the standard approach for estimating D-D with this type of staggered implemented had been using a two-way fixed-effects approach. Recent evidence (e.g. Goodman-Bacon, 2021) has shown that this approach requires an assumption of no policy impact heterogeneity over time to produce unbiased average estimates. Unfortunately, this is not a reasonable assumption in our setting, as it may take time for BMI or obesity effects to accumulate (diminish) if conditions worsen (improve). It is also normal for a school to take time to fully phase in a policy. For example, if a calendar were to cause disruptions to sports programming at first, but not later, then we may see early impacts differ from later impacts.

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[3] Test dates are only available in our data from the 2008-2009 school year onward (missing in 41% of the sample). For these later years, March is the most common testing month (at 33%), followed by April and February (each at 23%).

[4] Height in centimeters in included as a placebo outcome, as it is an alternative biological measure that one would not expect to be affected by school calendars. While we view it as unlikely, one could worry that nutrition and activity from calendar changes could directly impact height, or that obesity-related outcomes like early puberty could impact height of 5th graders. We therefore also provide estimates with a dummy for male sex as an alternative placebo outcome, excluding male as a covariate in such specifications.

We therefore use the method proposed by Callaway and Sant'Anna (CS, 2021) that is designed for potentially heterogenous impacts over time. The CS method compares schools that change calendars in any given year only to those schools that have "not-yet-changed" but do eventually make the same change later. In this way, the pre-policy years for the "late-adopting" schools serve as a comparison or "control" group for those "early-adopting" schools, while limiting comparison to schools that are similar in having at some point taken the same school policy choice.[5] Therefore, for all the students who experience a calendar change in a specific year, such as 2005, their group-time differences are compared to the same group-time differences for similar students in schools that have not yet changed for that year (e.g. a school that changes in 2010 is still a comparison through 2009). Standard errors are clustered at the school level.

The group-year treatment effects can be presented visually (as with an event study analysis) or aggregated to an average treatment on the treated (ATT) policy effect. We present both results for each year-round to traditional calendar change type in the next section. All estimation is run on student data and includes school effects, controls for race (dummies for Asian, Black, Hispanic, other, each relative to white), age and gender.[6]

Since schools do not randomly adopt year-round calendars, the inclusion of school fixed effects is particularly important for identification of a causal effect of the calendar type.[7] Controls for race account for the fact that some races have higher or lower levels of BMI or obesity overall, regardless of any policy intervention. Using a D-D estimation strategy, our estimates would not measure that one calendar type has higher obesity, but rather whether schools with a certain year-round calendar type experienced differential changes in obesity compared to a traditional one. We also control for gender. Boys and girls differ in their height- and weight- for-age, especially in early puberty. Gender and age are implicitly controlled in the CDC's age- and gender-specific standards overweight, obesity, and BMI Z scores, but these standards refer to a reference population of children who grew up in the 1960s to 1980s, and the associations between gender, age, and weight have shifted during the obesity epidemic.

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[5] More formally, the CS approach estimates group-time average treatment effects; that is, for each policy change year, separate impacts are calculated and then aggregated for the overall causal policy effect. This method can be written as: $ATT(g,t) = E\left[Y_t - Y_{g-1} | G_g = 1\right] - E\left[Y_t - Y_{g-1} | D_t = 0\right]$ (Equation 2.9, CS, 2021), where the time period when a student first becomes treated (i.e. calendar change) is denoted by G and is the same for groups (g) of individuals treated in the same time period (t). The term D_t is equal to one for individuals treated in period t and zero for those not-yet-treated in year t.

[6] Evidence shows that because BMI does not distinguish between fat and fat-free mass, relative to percent body fat, BMI misidentifies substantial fractions of individuals as obese or non-obese, depending on gender and race (e.g., Burkhauser and Cawley, 2008). Over a 25-year period, Baum (2007) estimated that changes in the distribution of race and age alone account for roughly 10% of the observed obesity increase.

[7] Recall that we have a panel of schools with policy changes taking place at the school level, but that our data is at the individual student level (as a repeated cross-section of 5th graders within schools). We therefore can still include school fixed effects within the CS framework.

4. Results

4.1. Descriptive statistics

In California, schools experienced significant changes in calendar type over time. Evidence of these changes can be seen in Figure 2, where multi-track year-round and Concept 6 calendars were at their height in the early 2000s (making year-round calendars roughly 25% of public schools in California) and fading out rapidly over a decade. This rapid phasing out of multi-track year-round calendars was driven by a lawsuit (*Williams v. State of California*), which required Concept 6 calendars to be phased out, but that also resulted in the same occurring for other multi-track year-round calendars. This did not impact the overall use of single-track year-round calendars in the long-run, but such calendars were sometimes used as a bridge between multi-track and traditional in the phase-out period, resulting in various calendar changes using single-track year-round calendars during this time period as well.

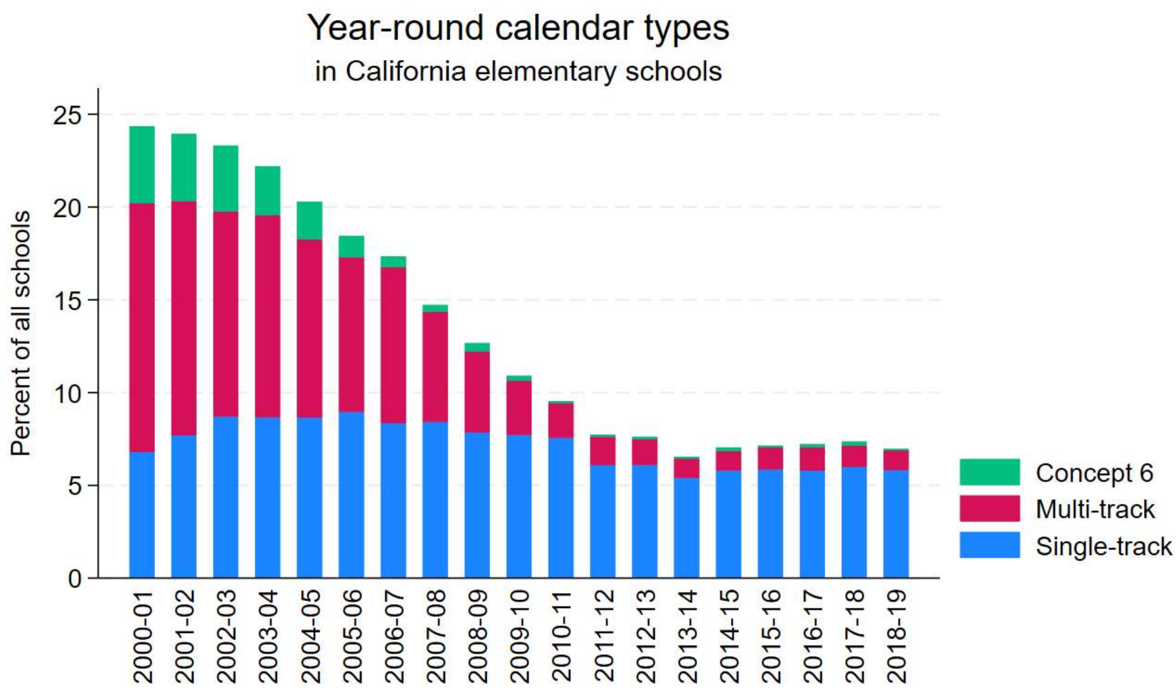

Figure 2. Trends in the use of year-round calendars in California elementary schools. Source: California Basic Educational Data System (CBEDS).

It is important to note that our sample is a non-balanced panel, with schools opening and closing across time as well as existing schools changing calendars. A school that opens under a particular school type and then later closes but never changes calendar type will factor into Figure 2. However, for our analysis, we are interested in calendar changes *within schools*, which occur in our data with differential timing across schools and between different pairings of calendar types.

Table 1: Variation in calendar changes in California, 2000-2001 to 2018-2019

Number of schools by number of calendar changes observed

Calendar changes	Schools	Percent
No changes	5340	78
1 change only	770	11.2
2 or more changes	741	10.8
Total	6851	100.00

Number of schools that did not change calendars

Calendar type	Schools	Percent
Concept 6	33	0.62
Multi-track year-round	68	1.27
Single-track year-round	161	3.01
Traditional	5078	95.09
Total	5340	100.00

Among schools that changed calendars once, how many made each type of change?

	Schools	Percent
Concept 6 -> Multi-track year-round	4	0.52
Concept 6 -> Single-track year-round	1	0.13
Concept 6 -> Traditional	36	4.68
Multi-track year-round -> Single-track year-round	23	2.99
Multi-track year-round -> Traditional	369	47.92
Single-track year-round -> Multi-track year-round	1	0.13
Single-track year-round -> Traditional	270	35.06
Traditional -> Multi-track year-round	12	1.56
Traditional -> Single-track year-round	54	7.01
Total	770	100.00

Source: California Department of Education CBEDS data

Table 1 provides a detailed break-down of calendar change types during our sample period. Roughly 78% of schools experienced no change in calendar type, and these were predominantly traditional calendar schools (95%). Of those schools that experienced a calendar change, roughly half changed one time only and the other half experienced two or more changes over the sample period. For the purposes of our analysis, we use data only for those schools that experienced only one change to establish a clear “before” and “after” period for the policy change. In schools that experienced only 1 calendar change between 2000 and 2019, the majority were schools changing from multi-track year-round to traditional calendars (48%, 369 schools) or from single-track year-round to traditional (35%, 270 schools). There are also observed changes from Concept 6 to traditional (as mandated by law), but with fewer observations given that Concept 6 calendars were less widely used across the state to begin with (4.7%, 36 schools). Given that adoption of year-round schools in California started in the 1990s, before our data began in the 2000-01 school year, we are observing mostly changes back to traditional calendars in our time frame. In separate regressions, we consider changes to a traditional calendar from: (i) single-track year-round, (ii) multi-track year-round, and (iii) Concept 6 calendars. In some specifications, we will also use schools that are always observed on a year-round calendar throughout our full data as comparison groups.

Table 2: Summary of student characteristics (means and standard deviations) by school calendar type

	Year-round calendars			Traditional
	Concept 6	Multi-track	Single-track	calendars
Male	0.514	0.508	0.511	0.510
	(0.500)	(0.500)	(0.500)	(0.500)
White	0.0285	0.225	0.239	0.290
	(0.166)	(0.418)	(0.427)	(0.454)
Asian	0.0167	0.0486	0.0395	0.0478
	(0.128)	(0.215)	(0.195)	(0.213)
Black	0.0482	0.101	0.0825	0.0659
	(0.214)	(0.302)	(0.275)	(0.248)
Hispanic	0.893	0.533	0.515	0.471
	(0.309)	(0.499)	(0.500)	(0.499)
Other	0.0139	0.0915	0.123	0.125
	(0.117)	(0.288)	(0.329)	(0.331)
Age	10.99	10.97	10.93	10.96
	(0.399)	(0.396)	(0.392)	(0.394)
BMI	21.78	20.76	20.33	20.28
	(4.846)	(4.666)	(4.507)	(4.528)
BMI zscore	0.971	0.726	0.628	0.608
	(1.078)	(1.138)	(1.151)	(1.155)
Overweight	0.537	0.440	0.405	0.397
	(0.499)	(0.496)	(0.491)	(0.489)
Obesity	0.321	0.244	0.218	0.213
	(0.467)	(0.430)	(0.413)	(0.410)
Height (cm)	145.3	146.2	146.0	146.4
	(8.475)	(8.552)	(8.269)	(8.265)
Observations	40,287	288,173	322,035	5,667,982

Note: Variable means and standard deviations (in parenthesis) are reported for the full sample of students across years (repeated cross-sections of 5th graders within a panel of schools) restricted in each column to the sample that are observed in that year on a specific calendar type.

Table 2 summarizes key variables for the full available estimation sample, comprised of schools that changed at most once, broken out by calendar types. Summarized observations in Table 2 are at the student level pooled for all years. It is evident that schools that adopt year-round calendars in California differ from those that do not. Notably, there is a clearly different racial make-up across calendar type. There is a dramatically higher percent of Hispanic students in Concept 6 schools, which were widely used in the Los Angeles Unified School District. These disparities were part of the impetus for the *Williams* lawsuit alleging that the use of multi-track and Concept 6 calendars was discriminatory. There is also a higher observable percent of student that are Hispanic and black and a lower percent that are white in multi-track year-round schools. While less pronounced, this minority concentration is also higher in single-track year-round compared to traditional calendar schools. We also see notable differences in obesity across calendar types. Specifically, we can observe higher BMI and obesity corresponding to calendars in a way that is positively correlated with minority concentration in the school. For this reason, it will be particularly important for us to consider race in our analysis.

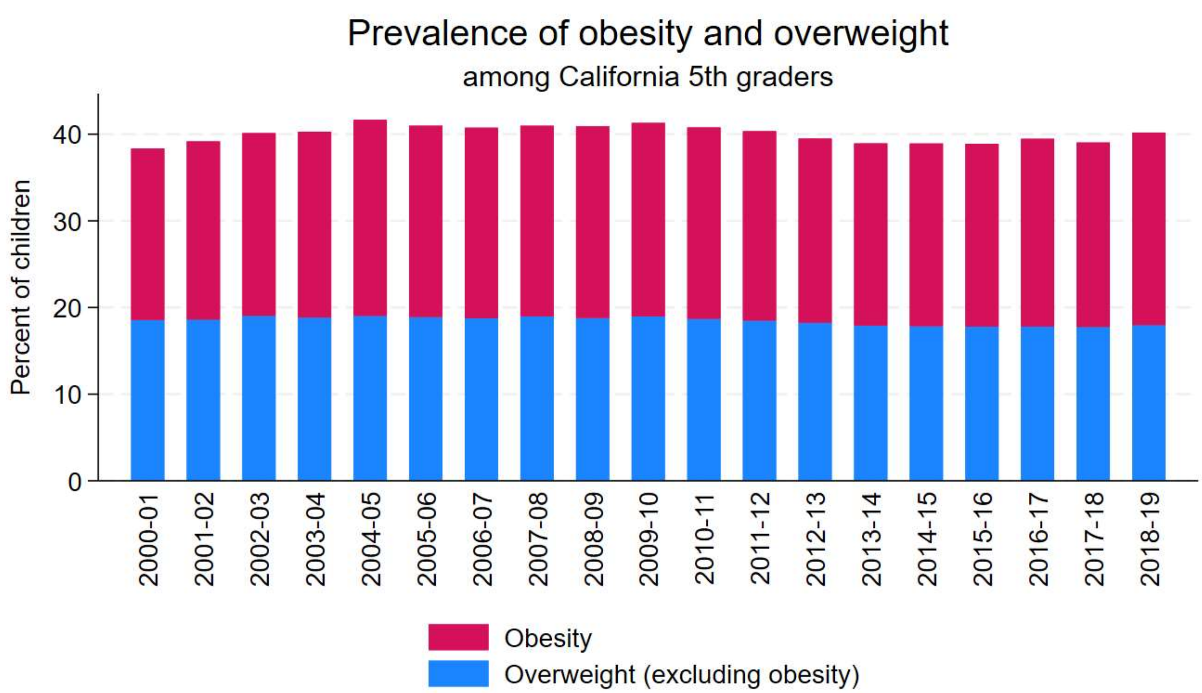

Figure 3. Fifth-grade trends in overweight and obesity prevalence.
Source: California Basic Educational Data System (CBEDS), California Department of Education.

Figure 3 shows the evolution of the percentage of 5th grade Californians who were categorized with overweight and obesity. What is clear is that even with many calendar changes across time, the overall prevalence of overweight and obesity did not change much.

What matters for calendar impacts, however, is not the overall trajectory of obesity in the pooled sample of schools, but whether the trajectory varied by calendar types. We therefore also summarize changes in our outcome variables across time by calendar change type. We limit the sample to schools that appear in both the first and last year in our data to avoid documenting trends resulting from sample composition changes over time. We then summarize BMI, BMI z-score and overweight and obesity status across time by sub-samples of schools making a particular year-round to traditional calendar change. This descriptive evidence for schools changing from multi-track year-round to a traditional calendar for the outcomes of BMI z-score and obesity are shown as an example in Figure 4.

From this descriptive evidence, we do not see notable evidence of a distinct change in any obesity-related outcome before and after the calendar change. Together our descriptive evidence would suggest minimal room for calendar effects on obesity. Such evidence, however, does not represent causal evidence on its own, as it does not address any potentially important student and school characteristics that might influence both obesity and switching away from year-round calendars.

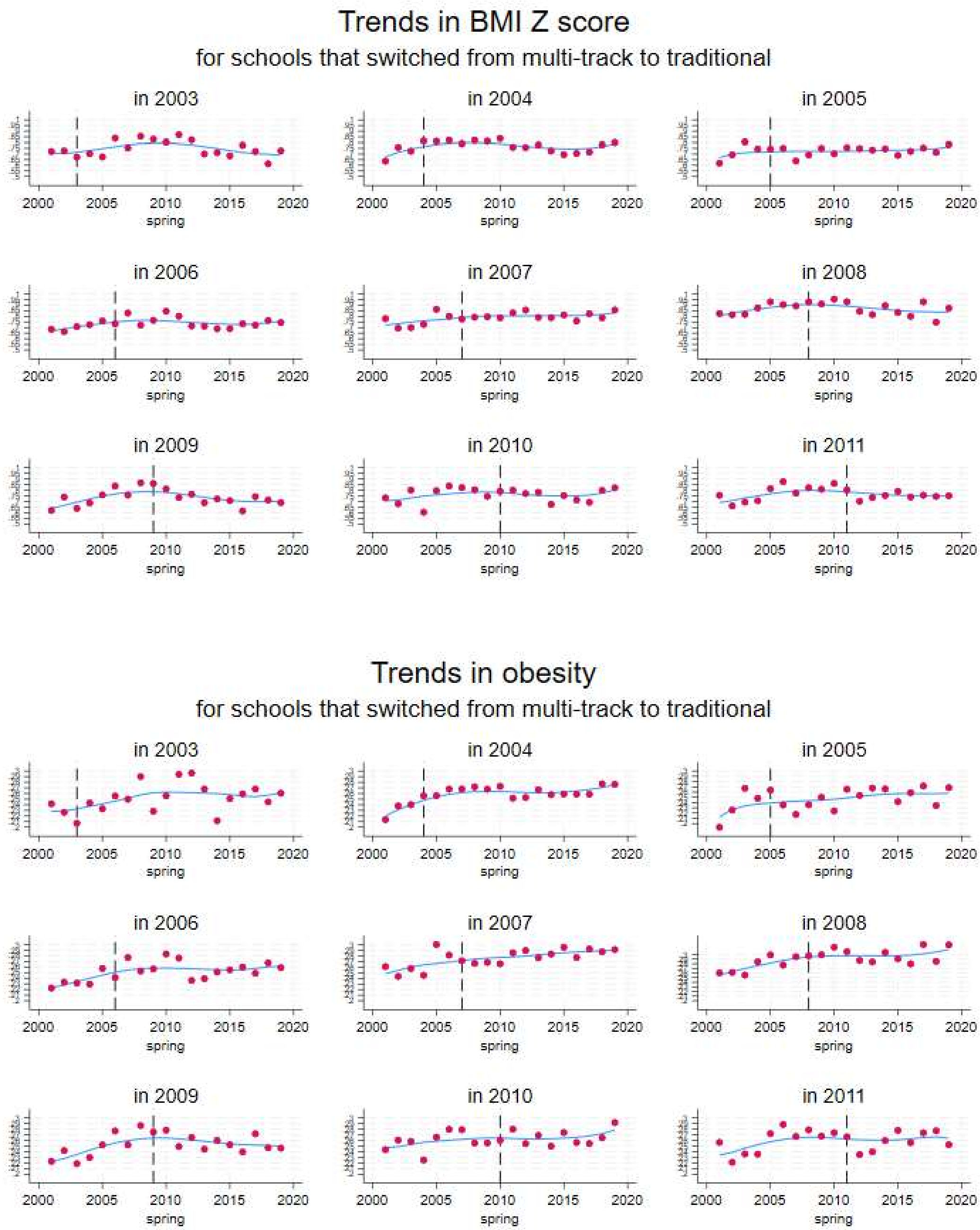

Figure 4. Average BMI z-score and obesity rate across year by calendar change year for calendars that changed from multi-track year-round to a traditional calendar in our sample period. Analysis in this figures is done on a sample limited to schools that appear in both the first and last year in our data to avoid documenting trends resulting from sample composition changes over time.

4.2. Difference-in-difference estimates

Main estimation results are provided in Table 3, separately for the samples of changers from single-track to traditional, multi-track to traditional and Concept 6 to traditional calendars. Here we show results using only those schools that experience a change during our sample period (e.g. only changers from single-track to traditional), as well as estimation that also includes always year-round schools included as an additional control group (e.g. observed on single-track over our full sample period).

Our main estimation shown in Table 3 restricts observations to within 6 years of the calendar change. We chose a 6-year window because it takes 6 years for a child to progress from the start of kindergarten to the end of 5th grade. With a window shorter than 6 years, we would be estimating effects using only 5th graders who had spent less than their full elementary career on the school's new calendar. With longer windows, unobserved school characteristics have more time to change and could confound the results. In addition, the number of schools that identify each year-from-change effect would shrink if we required longer exposures. While our panel of schools spans 20 years, our individual data is a repeated cross-section of 5th graders within that panel of schools. Given that we only observe 5th graders, our estimates automatically represent the effects averaged across various years of short-run exposure to different calendar types (0-6 years).[8] In the appendix, we provide estimation results using a 5- and 7- year window instead, as well as estimation with the full sample with no restrictions on time since the calendar change. Across specifications we consistently find no evidence of impacts of school calendar type on BMI, overweight or obesity.

[8] Our estimated effects limited to a 6-year window of time before and after the policy change therefore averages across effects for individuals with differing mixtures of exposure to calendar types. For example, an event study estimate for two years after a calendar change from multi-track year-round calendar to a traditional calendar would be identified off of 5th graders who had 2 years of exposure to a traditional calendar (e.g. 4th and 5th grade) and 4 years of multi-track year-round exposure before that (Kindergarten through 3rd grade), regardless of how long the school might have been on that calendar in prior years. Likewise, the event study estimate for 6 years post policy is identified off of 5th graders who have spent their entire time from kindergarten to 5th grade on a traditional school that had been on a muti-track calendar just before that student enrolled.

Table 3: Effects of changing from year-round calendars to traditional calendars, 6-year range from policy change, ATT estimate, s.e. and observations for separate regressions by outcome and sample.

BMI	BMI z-score	Overweight	obesity
Sample: Only schools that changed from single-track year-round to traditional calendars			
-0.021	-0.016	-0.007	-0.020
(0.780)	(0.200)	(0.090)	(0.054)
165,605	165,605	165,605	165,605
Sample: Including always-single-track schools			
-0.135	-0.014	-0.000	-0.016
(0.204)	(0.054)	(0.024)	(0.017)
347,982	347,982	347,982	347,982
Sample: Only schools that changed from multi-track year-round to traditional calendars			
-0.730	-0.153	-0.064	-0.047
(0.583)	(0.144)	(0.059)	(0.051)
366,160	366,160	366,160	366,160
Sample: Including always multi-track schools			
-0.053	-0.001	-0.011	-0.003
(0.394)	(0.094)	(0.055)	(0.044)
446,151	446,151	446,151	446,151
Sample: Only schools that changed from Concept 6 to traditional calendars			
-0.516	-0.117	-0.021	-0.012
(0.667)	(0.167)	(0.080)	(0.061)
33,705	33,705	33,705	33,705
Sample: Including always Concept 6 schools			
-0.907	-0.135	-0.099	-0.170
(2.080)	(0.551)	(0.193)	(0.290)
48,008	48,008	48,008	48,008

School-clustered standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1
Notes: Estimates are shown for changing from a multi-track year-round, a single-track year-round, or a Concept 6 calendar to a traditional calendar. Each analysis is repeated by whether the comparison group is comprised of only not-yet-changed schools or also includes never changing schools

From Table 3, one can see that we find robust evidence of no short-run impacts on BMI, BMI z-score, overweight or obesity. Appendix Table 2 also reports placebo outcomes for changes in student height and sex, both of which, as expected, are not impacted by school calendar changes. The event study graphs corresponding to our main estimation presented in Table 3 are presented in Appendix Figures 3-5, and for the placebo outcomes in Appendix Figure 6. One can clearly see a smooth zero effect, with no visual change at the calendar change year for both the main and placebo estimations.

These findings are robust to the window from the calendar change that is used in estimation. Results for a 5-year and 7-year pre-and post-policy range, as well as with no restrictions on time since the policy change (see Appendix Tables 3-5) all largely show no significant estimates for any of the health measures reported.[9] The full set of event study graphs are also provided for estimation using no restriction on time-since-the-policy-change (see Appendix Figures 7-10), showing just how consistently zero the impacts remain further from the calendar change.[10]

Recall that the single-track model does not alter the total number of school days and does not include dividing students into tracks. Since a single-track year-round calendar only alters the distribution of time across the calendar year relative to a traditional calendar, no impacts found for this specific year-round calendar therefore implies that there is no obesity-reducing benefit from specifically reducing the number of *continuous* unstructured days. The gains in obesity over the summer months and losses during the school year therefore likely just reflect total typical time spent in school during these seasons and not something unique to the length or timing of summer break.

Multi-track year-round calendars come with additional organizational burdens that could potentially impact student activity levels. Notably, the multi-track calendar, by also dividing students into separate tracks can impact other aspects of school organization. For example, it could complicate the organization of a substitute to the typical summer camps offered on a traditional summer break. On a multi-track year-round school calendar, the school building being in continuous use does not allow for any physical programs to be organized on-site during the more frequent breaks throughout the year. This could potentially impact student use of such programs overall and decrease physical activity during breaks.[11] That we find no impacts for the multi-track model implies that these hurdles do not reduce access to physical activity significantly enough to impact obesity measures for 5th graders. Together, the results for single-track and multi-track schools support the conclusion that it is the total number of days in the

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[9] The exception is one estimate significant at the 10% level when using the full sample. While often reported in economics, 10% significance is not typically considered statically significant in the public health literature when using obesity-related measures. Additionally, this is the singular exception and results from a specification that does not pass the placebo test for height as an alternative policy outcome.

[10] Standard errors are quite large across all specifications reported in this paper. If we instead run our estimation using a standard TWFEs model, standard errors are not as large, VIFs and correlations between covariates are small, indicating that this is unlikely to be due to multicollinearity and is more likely due to how demanding of the data is the Callaway and Sant'Anna (2021) estimation. It reduces the number of control units to those relevant for each treated group, leading to unbiased estimates, but with fewer observations contributing to comparisons. Notably, California is the US state with the largest historical use of year-round calendars, and we use data on all 5th grade students in public schools across the state. Our study therefore makes use of what is likely the most variation one can obtain for addressing our question of interest.

[11] Additionally, the multi-track model could complicate the organization of competitive sports with non-year-round schools, as team members might be on different tracks. To the extent that this decreases participation in sports in year-round schools, this could serve as an additional mechanism for observed obesity differences. This mechanism, however, is likely more applicable to ages older than 5th grade.

structured school environment and not the distribution of those days that matters for student health.

We also find no impact for Concept 6 schools, which differ from other multi-track year-round calendars by having longer and fewer school days. The null impacts for Concept 6 schools could be interpreted as further evidence that it is not even the *total number of days* in the structured school environment, but *just total overall time* that matters for student obesity. However, we would caution against putting much weight on this interpretation. Notably, the Concept 6 model differs from other year-round models also in terms of the context in which they were implemented, with Concept 6 calendars only being used in cases of extreme school crowding. If one wanted to compare the differences between Concept 6 schools and other multi-track year-round schools, we cannot unfortunately distinguish between these two potential mechanisms (fewer and longer school days versus the school crowding context). Taken together, however, our results do clearly indicate that it is not something unique to the long summer break that drives a rise in student obesity during that time, but rather it is just time spent out of school that occurs even when breaks vary in length and season.

5. Conclusions

It is well-documented that student obesity rises faster in summer months than in the rest of the year, and the leading hypothesis for this pattern being that school structure promotes healthy behaviors. One might consider a policy that reduces the length of the summer vacation as a potential remedy. Under the structured day hypothesis, year-round school calendars could aid in obesity reduction if there is something particular about the length and/or timing of the summer break that is causing obesity to rise. However, if what matters just total time in a healthy structured school environment, and the distribution of that time is not fundamentally important, then one would expect no impact of year-round calendars relative to traditional school calendars on obesity. In this way, comparisons between year-round and traditional calendars serve as a clean test for the role of summer break specifically, as opposed to just time out of school, in contributing to the child obesity epidemic.

Our results support the importance of total time in school over any particular distribution of breaks and are therefore informative for understanding how this structure provided by schools might work in practice. From our results, we can better understand what we should – and potentially more importantly – what we shouldn't make of the summer break obesity rise: notably, it is not something unique to summer vacation, but rather just vacation from school more generally.

Since previous research has found some school time policies that have large behavioral impacts, the impacts of year-round school calendars on obesity were an open empirical question. This study contributes to academic literature as the first to estimate the impacts of school calendars on obesity using a large dataset and a methodology suited to causal policy analysis. We found robust evidence of no obesity-related impacts. Such results are also relevant for school equity considerations. California was once the nation's leading user of year-round calendars, but for twenty years California schools have been switching to traditional calendars. It is important to understand whether these calendar changes have improved or harmed children's health and fitness. This is especially so as obesity is most prevalent in the same disadvantaged communities where year-round calendars were most prevalent. Our results show that the calendar type used does not impact obesity, and therefore is not a consequential factor in exacerbating or remedying existing disparities in obesity rates.

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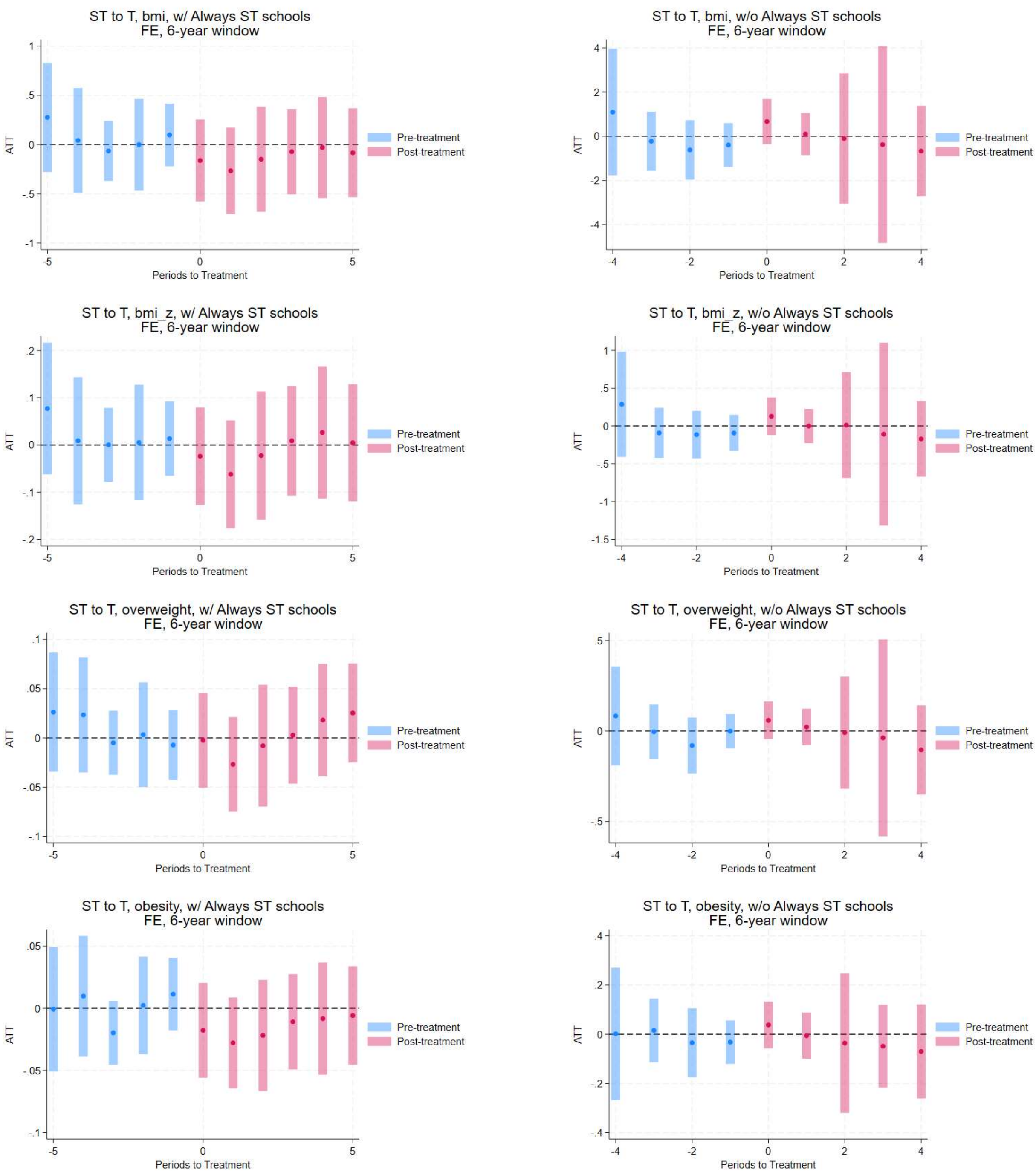
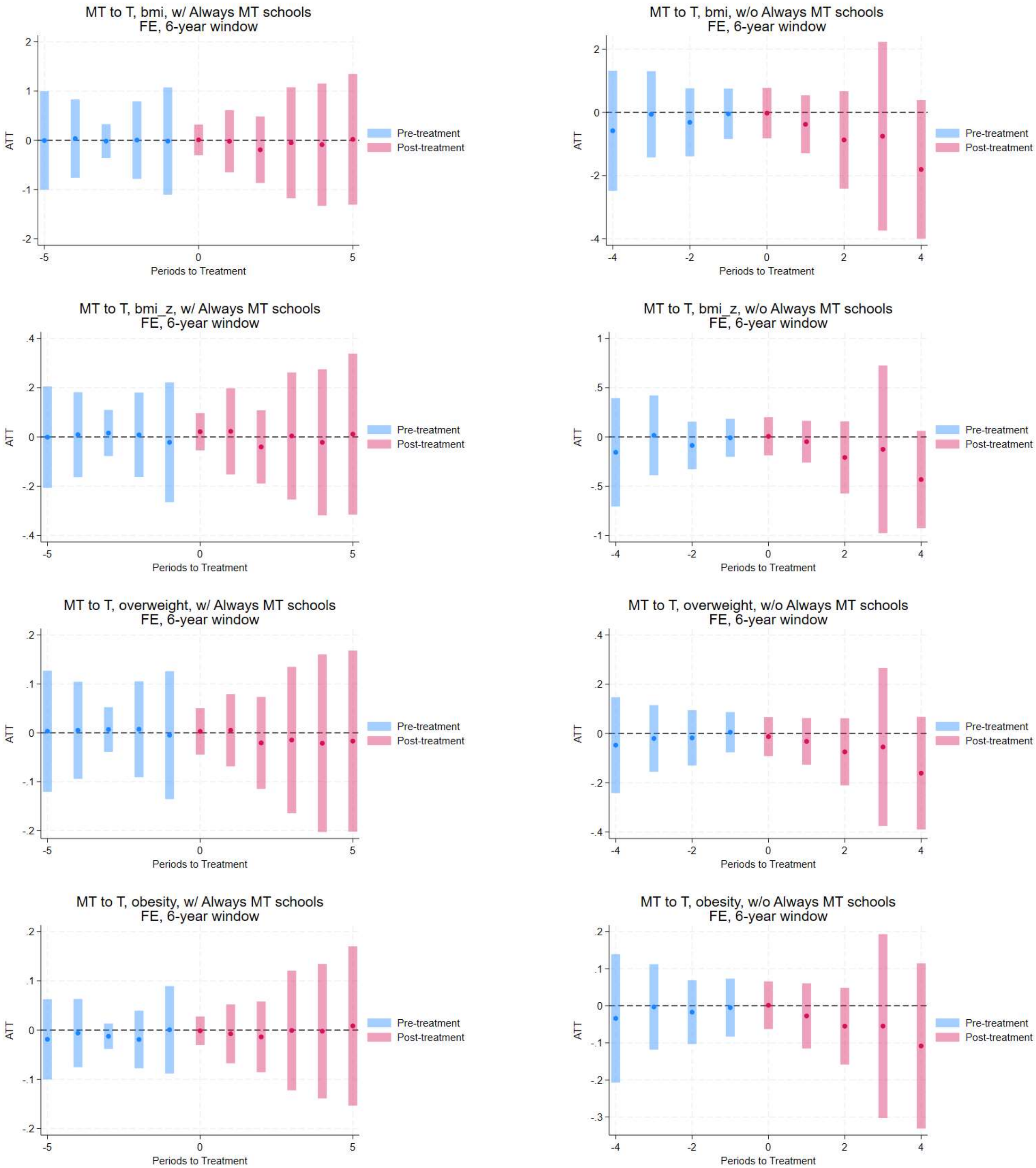
Appendix Figures and Tables

Appendix Figure 1: Event study graphs: single-track (ST) to traditional (T) school calendars, 6-year window



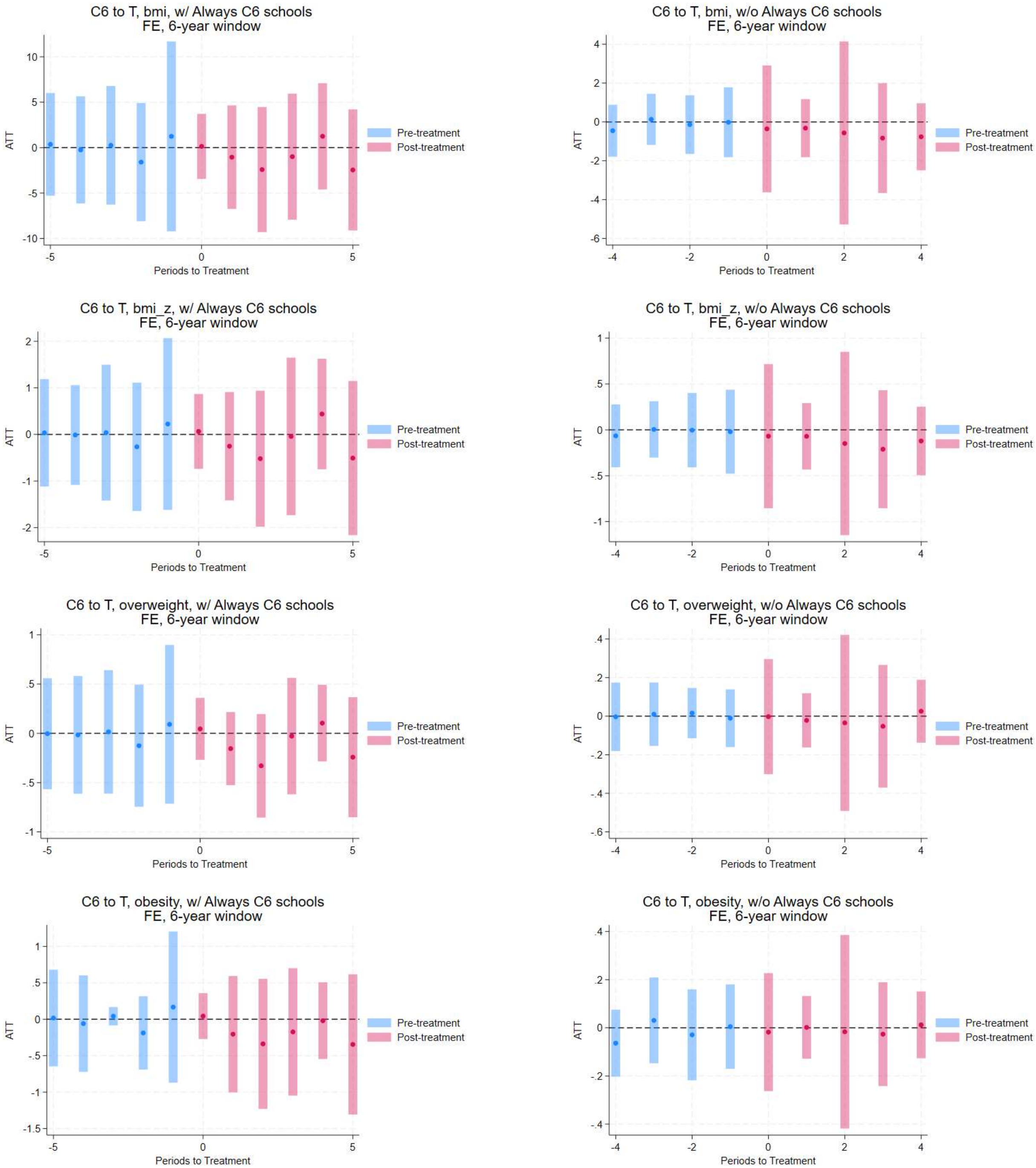
Note: event study estimates, corresponding to similar aggregate ATT estimates for Table 2.

Appendix Figure 2: Event study graphs MT to T, 6-year window



Note: event study estimates, corresponding to similar aggregate ATT estimates for Table 2.

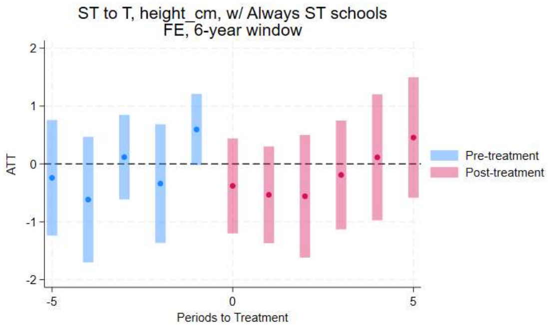
Appendix Figure 3: Event study graphs C6 to T

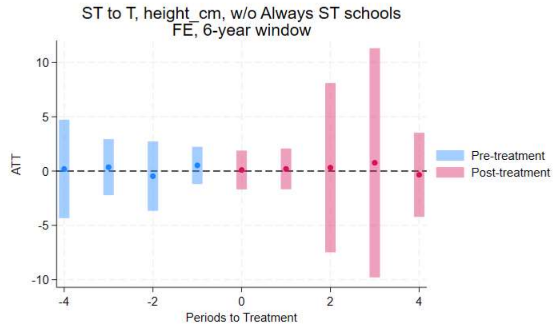


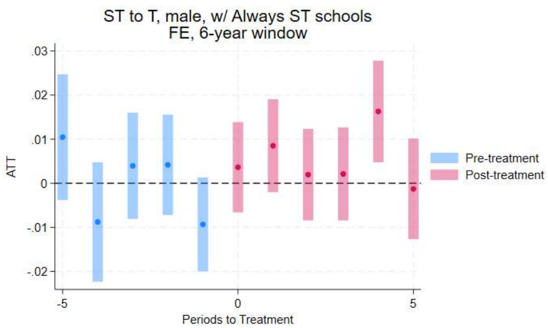
Note: event study estimates, corresponding to similar aggregate ATT estimates for Table 2.

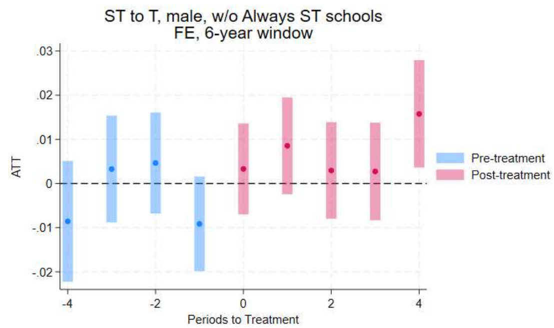
Appendix Figure 4: Event study graphs for placebo outcomes, 6-year window

Single-track (ST to T)

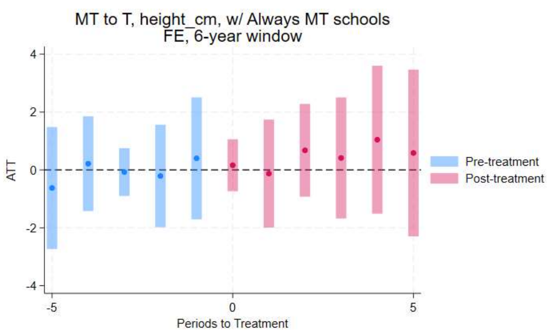


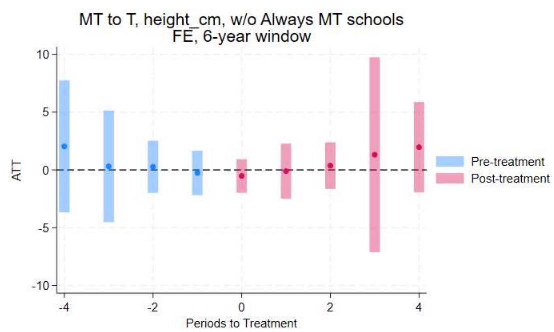


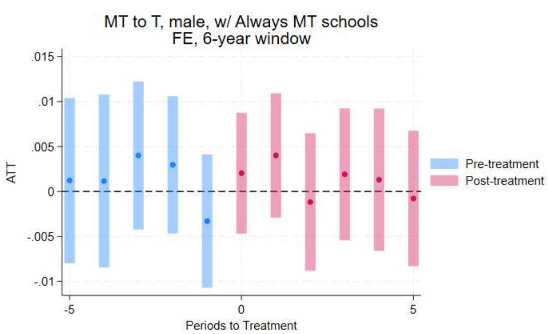


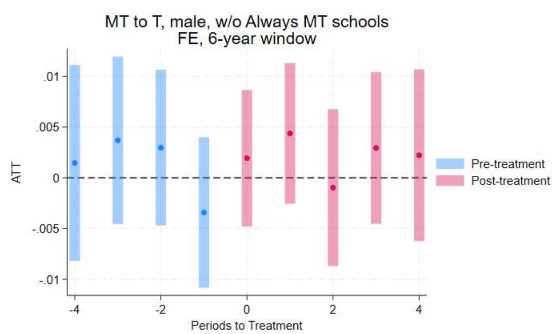


Multi-track (MT to T)

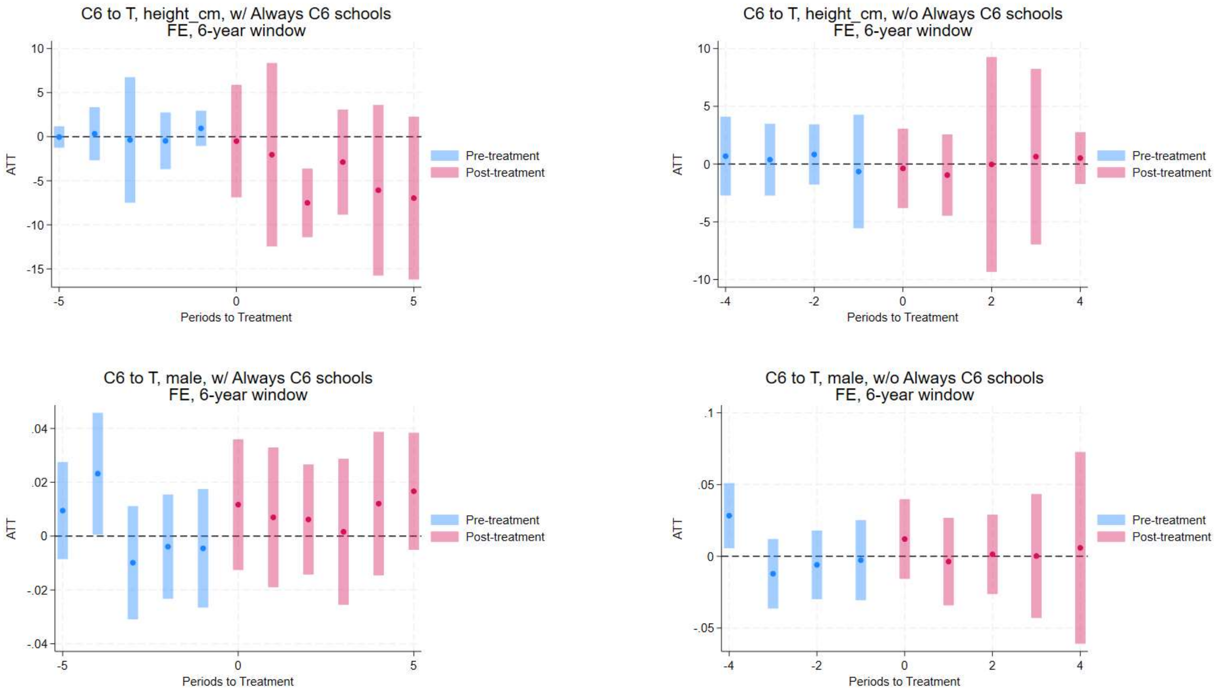






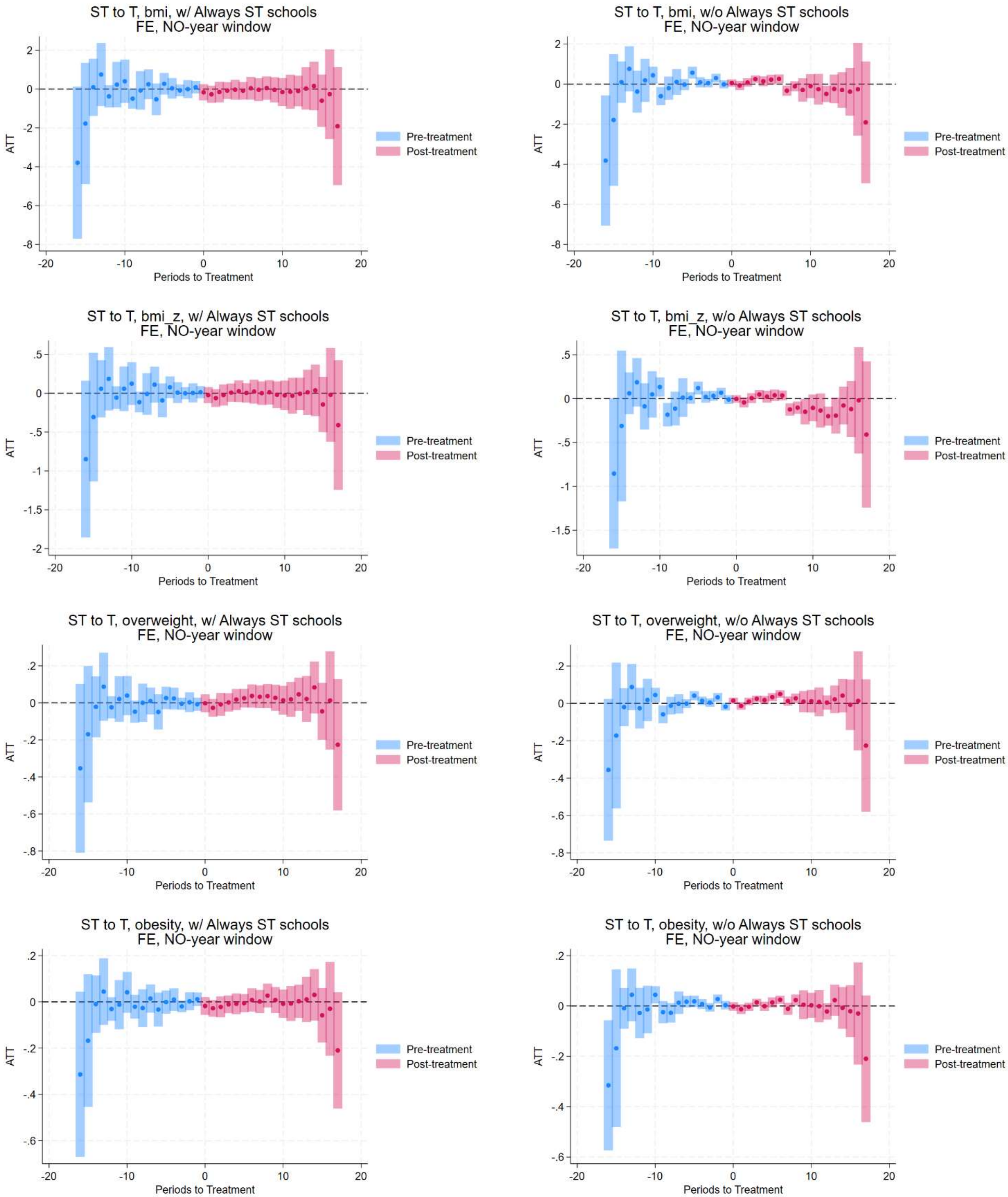


Concept 6 (C6 to T)

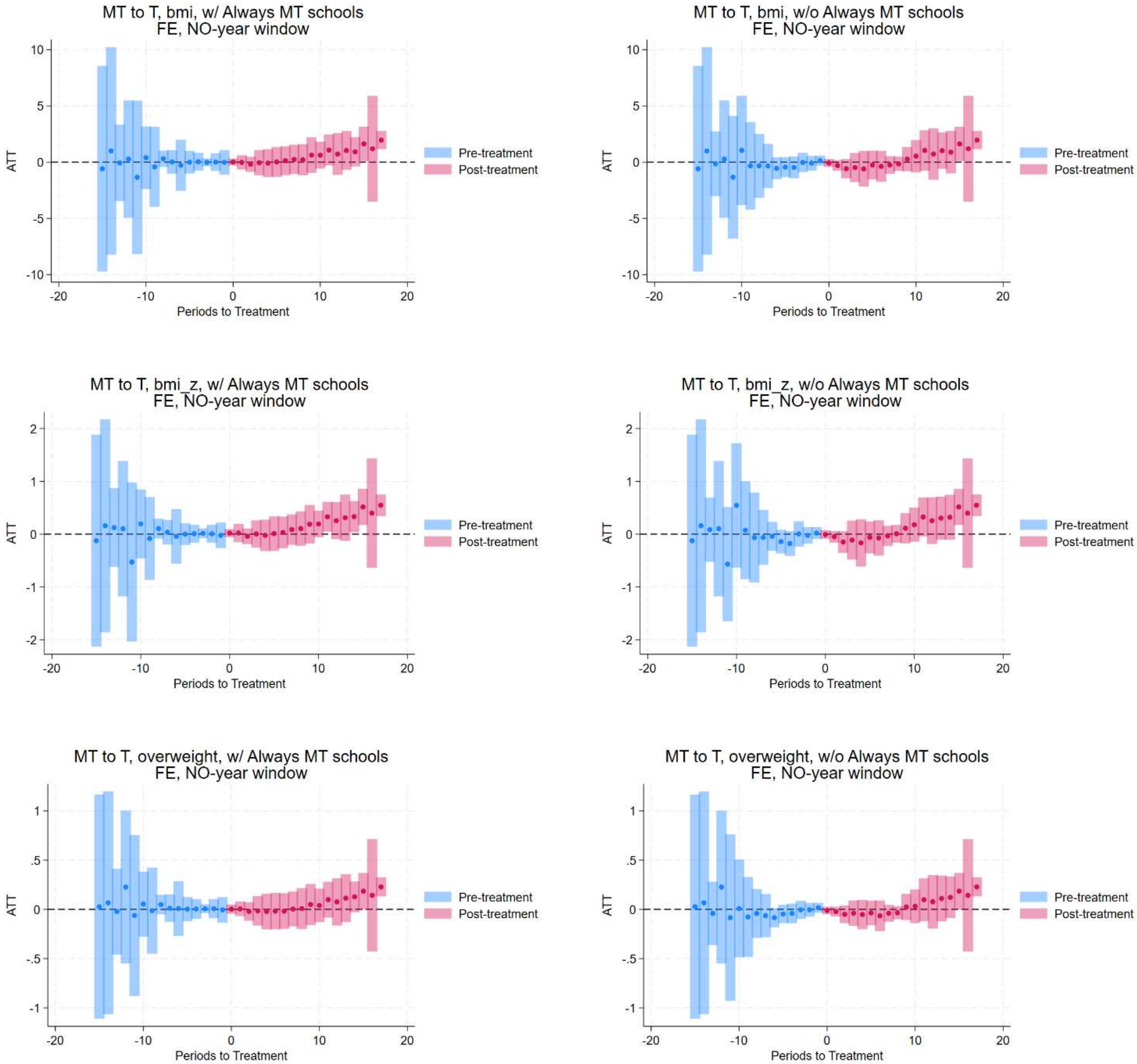


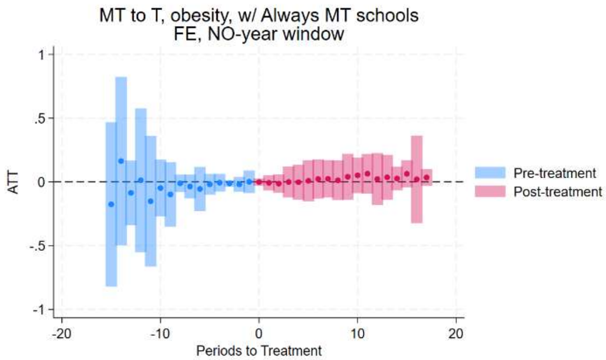
Note: event study estimates, corresponding to similar aggregate ATT estimates for Table 2, but instead using height as a placebo outcome.

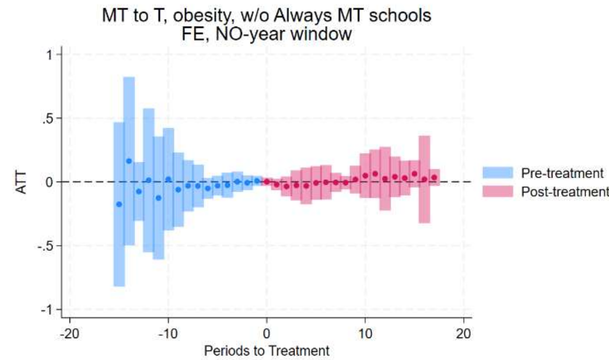
Appendix Figure 5: Event study graphs ST to T, no year window restrictions



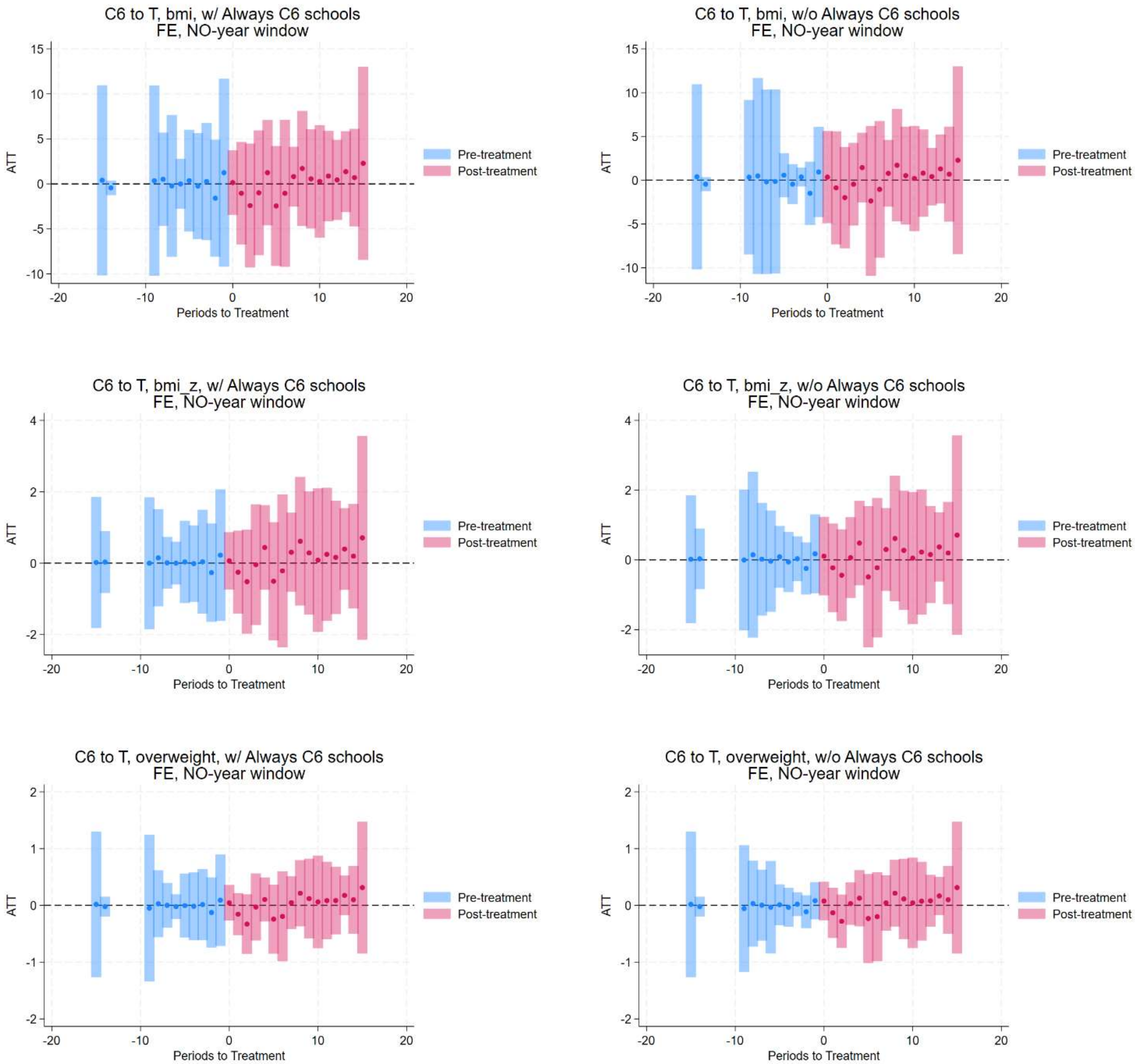
Appendix Figure 6: Event study graphs MT to T, no year window restrictions

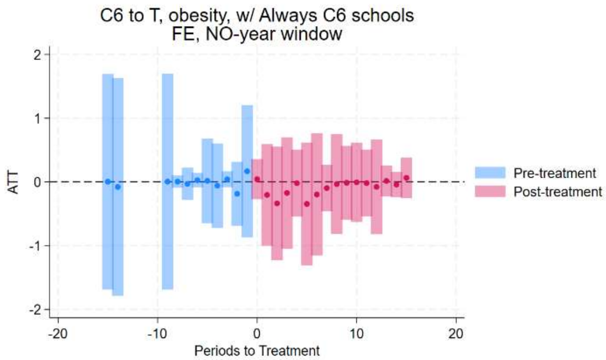


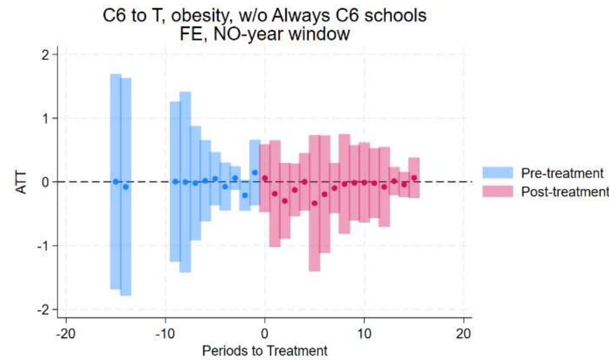

MT to T, obesity, w/ Always MT schools
FE, NO-year window
ATT
Periods to Treatment
Pre-treatment
Post-treatment


MT to T, obesity, w/o Always MT schools
FE, NO-year window
ATT
Periods to Treatment
Pre-treatment
Post-treatment

Appendix Figure 7: Event study graphs C6 to T, no year window restrictions

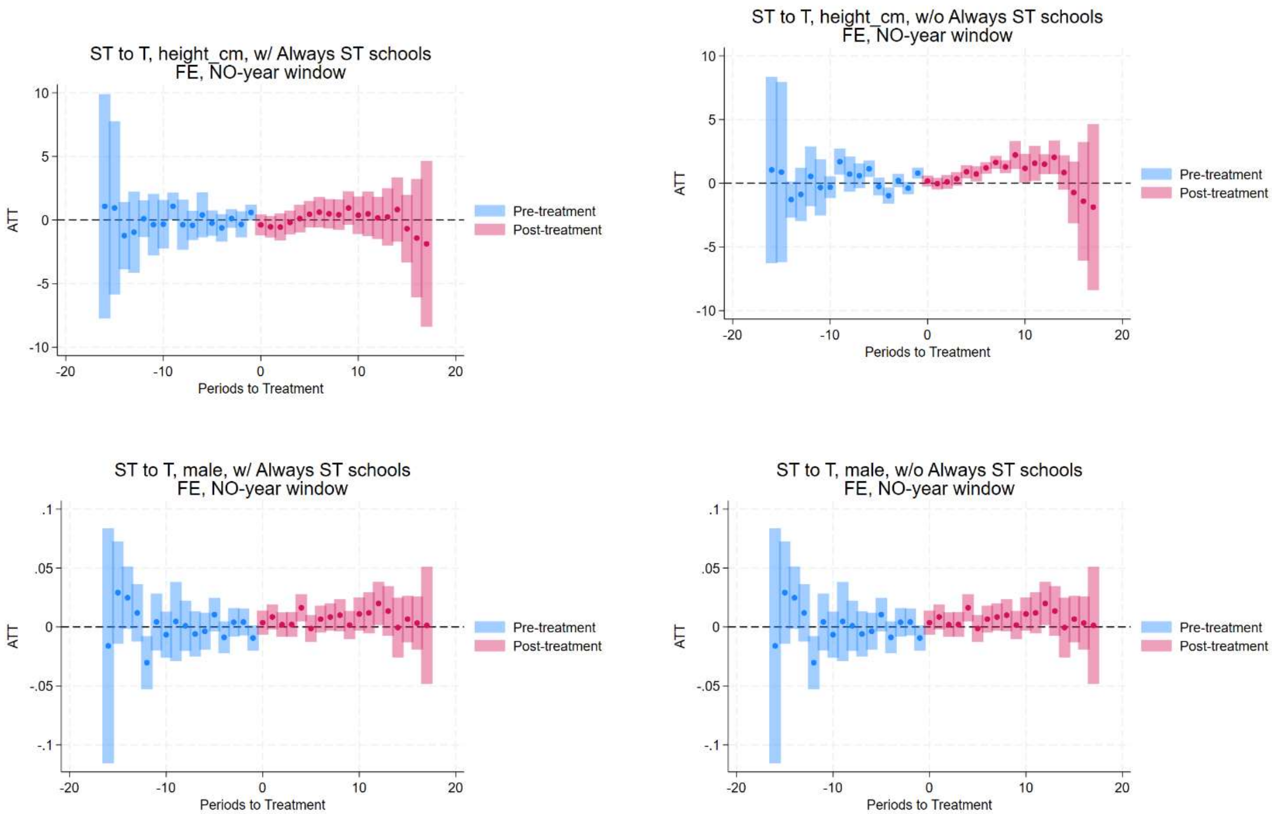



C6 to T, obesity, w/ Always C6 schools
FE, NO-year window
ATT
Periods to Treatment
Pre-treatment
Post-treatment

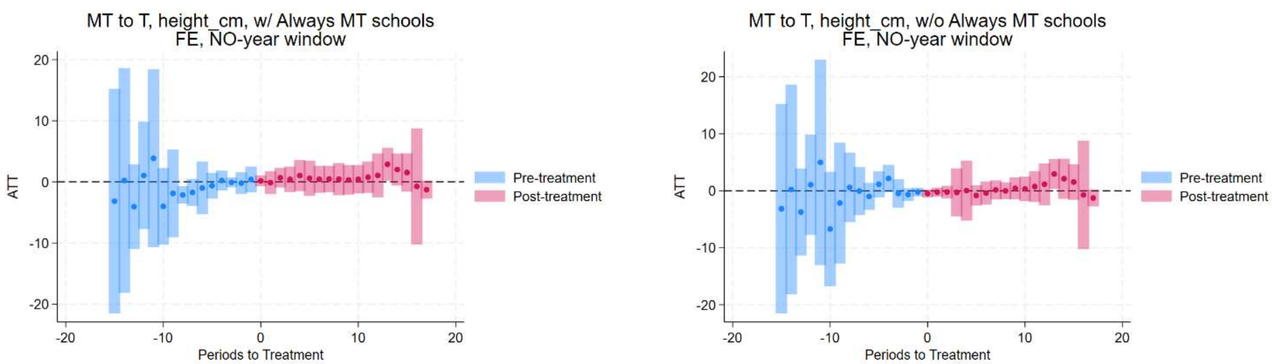

C6 to T, obesity, w/o Always C6 schools
FE, NO-year window
ATT
Periods to Treatment
Pre-treatment
Post-treatment

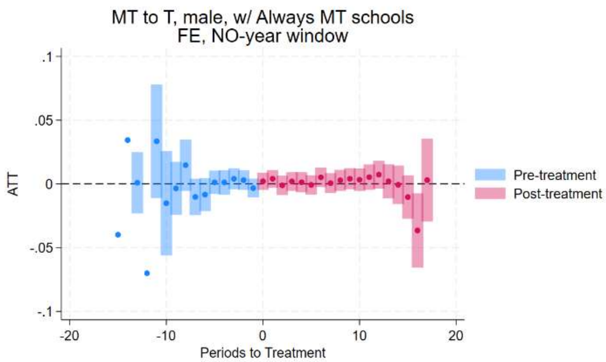
Appendix Figure 9: Event study graphs for placebo outcomes, no year window restriction

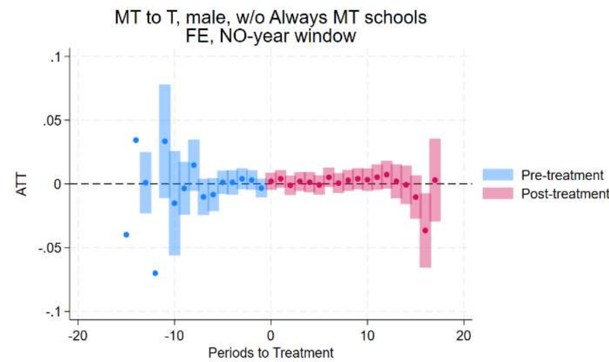
Single-track (ST to T)



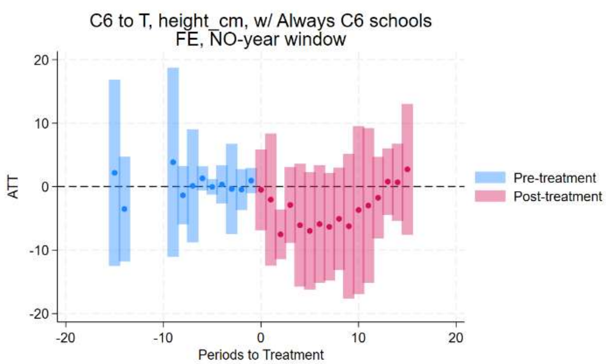
Multi-track (MT to T)

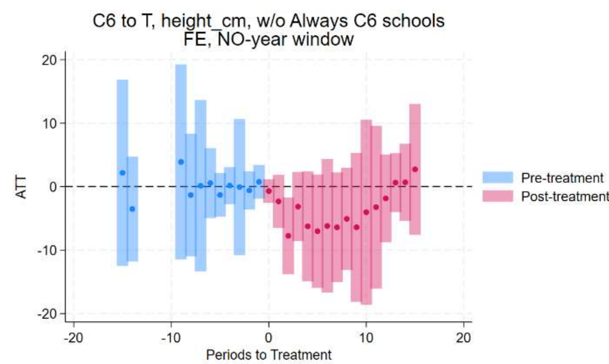


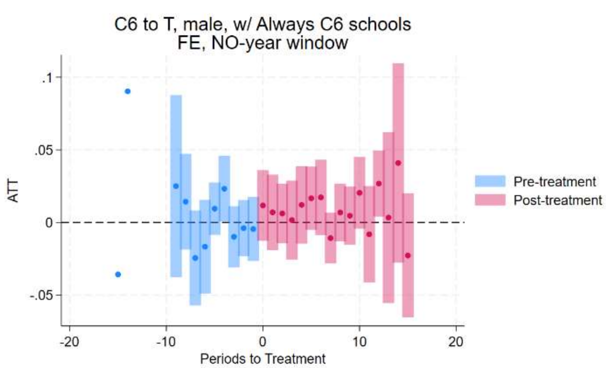


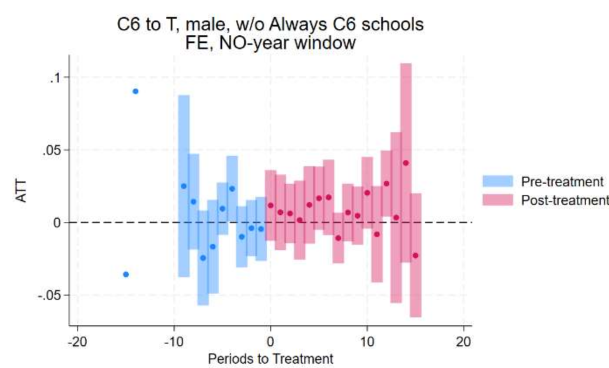


Concept 6 (C6 to T)









Note: event study estimates, corresponding to similar aggregate ATT estimates for Table 2, but instead using height as a placebo outcome.

Appendix Table 1

What fraction of dates are missing each year?

	Missing test date	Missing birth date
2000-01	1	.015
2001-02	1	.007
2002-03	1	.607
2003-04	1	.604
2004-05	1	.6
2005-06	1	.001
2006-07	1	.605
2007-08	1	.606
2008-09	0	.608
2009-10	0	.606
2010-11	0	1
2011-12	0	0
2012-13	0	0
2013-14	0	0
2014-15	0	0
2015-16	0	0
2016-17	0	0
2017-18	0	0
2018-19	0	0

Appendix Table 2: Placebo effects (corresponding to main effects in Table 3) of changing from year-round calendars to traditional calendars, 6-year range from policy change, ATT estimate, s.e. and observations for separate regressions by outcome and sample.

	Placebo outcomes	
	Height (cm)	male
Sample: Only changers ST to T	0.206 (1.810) 165,605	0.006 (0.004) 165,605
Sample: Including always ST schools	-0.218 (0.420) 347,982	0.005 (0.004) 347,982
Sample: Only changers MT to T	0.551 (1.285) 366,160	0.002 (0.003) 366,160
Sample: Including always MT schools	0.455 (0.903) 446,151	0.001 (0.003) 446,151
Sample: Only changers C6 to T	-0.172 (1.528) 33,705	0.003 (0.014) 33,705
Sample: Including always C6 schools	-4.233 (3.334) 48,008	0.009 (0.009) 48,008

School-clustered standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Estimates are shown for changing from a multi-track year-round (MT), a single-track year-round (ST) or a Concept 6 (C6) calendar to a traditional (T) calendar, repeated by whether the comparison group is comprised of only not-yet-changed schools or also includes never changing schools.

Appendix Table 3: Effects of changing from year-round calendars to traditional calendars, 5-year range from policy change, ATT estimate, s.e. and observations for separate regressions by outcome and sample.

Health outcomes				Placebo outcomes	
BMI	BMI z score	Overweight	Obesity	Height (cm)	Male
Sample: Only changers ST to T					
-0.014	-0.011	-0.011	-0.022	0.154	0.005
(0.657)	(0.156)	(0.074)	(0.069)	(1.374)	(0.004)
138,841	138,841	138,841	138,841	138,841	138,841
Sample: Including always ST schools					
-0.143	-0.018	-0.005	-0.018	-0.331	0.006
(0.206)	(0.054)	(0.024)	(0.017)	(0.418)	(0.004)
323,430	323,430	323,430	323,430	323,430	323,430
Sample: Only changers MT to T					
-0.487	-0.083	-0.050	-0.034	0.341	0.002
(0.450)	(0.107)	(0.041)	(0.041)	(0.812)	(0.003)
310,539	310,539	310,539	310,539	310,539	310,539
Sample: Including always MT schools					
-0.067	-0.003	-0.010	-0.005	0.430	0.002
(0.351)	(0.084)	(0.049)	(0.038)	(0.815)	(0.003)
389,410	389,410	389,410	389,410	389,410	389,410
Sample: Only changers C6 to T					
-0.488	-0.116	-0.025	-0.013	-0.214	0.003
(0.840)	(0.206)	(0.084)	(0.067)	(1.629)	(0.012)
29,093	29,093	29,093	29,093	29,093	29,093
Sample: Including always C6 schools					
-0.619	-0.066	-0.073	-0.137	-3.723	0.008
(1.904)	(0.506)	(0.179)	(0.260)	(3.133)	(0.010)
40,986	40,986	40,986	40,986	40,986	40,986

School-clustered School-clustered standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Estimates are shown for changing from a multi-track year-round (MT), a single-track year-round (ST) or a Concept 6 (C6) calendar to a traditional (T) calendar, repeated by whether the comparison group is comprised of only not-yet-changed schools or also includes never changing schools

Appendix Table 4: Effects of changing from year-round calendars to traditional calendars, 7-year range from policy change, ATT estimate, s.e. and observations for separate regressions by outcome and sample.

Health outcomes				Placebo outcomes	
BMI	BMI z score	Overweight	Obesity	Height (cm)	Male
Sample: Only changers ST to T					
0.087	-0.011	0.012	0.003	0.750	0.006
(0.429)	(0.106)	(0.052)	(0.044)	(1.443)	(0.004)
185,120	185,120	185,120	185,120	185,120	185,120
Sample: Including always ST schools					
-0.112	-0.010	0.004	-0.013	-0.113	0.005
(0.211)	(0.056)	(0.024)	(0.018)	(0.436)	(0.004)
371,206	371,206	371,206	371,206	371,206	371,206
Sample: Only changers MT to T					
-0.492	-0.107	-0.056	-0.033	0.596	0.011***
(0.565)	(0.127)	(0.048)	(0.045)	(1.025)	(0.003)
413,876	413,876	413,876	413,876	413,876	413,876
Sample: Including always MT schools					
-0.028	0.004	-0.012	0.001	0.452	0.002
(0.422)	(0.099)	(0.059)	(0.049)	(0.908)	(0.003)
497,510	497,510	497,510	497,510	497,510	497,510
Sample: Only changers C6 to T					
-0.555	-0.133	-0.025	-0.012	0.050	0.005
(0.624)	(0.157)	(0.077)	(0.059)	(1.490)	(0.014)
36,448	36,448	36,448	36,448	36,448	36,448
Sample: Including always C6 schools					
-0.926	-0.146	-0.112	-0.174	-4.460	0.010
(2.286)	(0.611)	(0.214)	(0.315)	(3.431)	(0.009)
53,222	53,222	53,222	53,222	53,222	53,222

School-clustered standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1
Notes: Estimates are shown for changing from a multi-track year-round (MT), a single-track year-round (ST) or a Concept 6 (C6) calendar to a traditional (T) calendar, repeated by whether the comparison group is comprised of only not-yet-changed schools or also includes never changing schools

Appendix Table 5: Effects of changing from year-round calendars to traditional calendars, full-year range from policy change, ATT estimate, s.e. and observations for separate regressions by outcome and sample.

Health outcomes				Placebo outcomes	
BMI	BMI_z	overweight	obesity	height_cm	Male
Sample: Only changers ST to T					
-0.048	-0.048+	0.015	0.001	0.808***	0.007
(0.096)	(0.026)	(0.010)	(0.008)	(0.197)	(0.004)
473,774	473,774	473,774	473,774	473,774	473,774
Sample: Including always ST schools					
-0.107	-0.013	0.012	-0.008	0.085	0.007
(0.271)	(0.073)	(0.031)	(0.023)	(0.567)	(0.004)
473,706	473,706	473,706	473,706	473,706	473,706
Sample: Only changers MT to T					
-0.049	0.014	-0.009	-0.001	0.059	0.002
(0.355)	(0.089)	(0.037)	(0.035)	(0.584)	(0.003)
695,605	695,605	695,605	695,605	695,605	695,605
Sample: Including always MT schools					
0.247	0.089	0.016	0.015	0.579	0.002
(0.492)	(0.109)	(0.071)	(0.057)	(0.940)	(0.003)
693,996	693,996	693,996	693,996	693,996	693,996
Sample: Only changers C6 to T					
-0.069	0.061	-0.008	-0.107	-4.540	0.008
(2.235)	(0.622)	(0.237)	(0.276)	(3.104)	(0.008)
70,243	70,243	70,243	70,243	70,243	70,243
Sample: Including always C6 schools					
-0.202	0.040	-0.022	-0.120	-4.356	0.008
(2.008)	(0.624)	(0.226)	(0.283)	(3.629)	(0.008)
70,243	70,243	70,243	70,243	70,243	70,243

School-clustered standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Estimates are shown for changing from a multi-track year-round (MT), a single-track year-round (ST) or a Concept 6 (C6) calendar to a traditional (T) calendar, repeated by whether the comparison group is comprised of only not-yet-changed schools or also includes never changing schools