Evaluating "breakfast after the bell" with regression discontinuity extrapolation

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Abstract

In recent years, several states have mandated Breakfast After the Bell (BATB) programs for schools with sufficiently high shares of low-income students, hoping to increase school breakfast participation. The presence of strict thresholds in these policies allow for plausible identification of causal effects using regression discontinuity designs (RDDs). However, standard RDD procedures only yield results for schools near the threshold and therefore face external validity concerns, including among lower-income schools, which are of particular interest. In this paper, I investigate the effects of an Illinois mandate among *all* schools above the enrollment cutoff. To do so, I first use an RDD to identify local effects and then impose a range of additional "parallel trends"-like assumptions, allowing for extrapolation to lower-income schools. The local results suggest that the BATB mandate had negligible impacts on attendance-related measures and academic performance near the cutoff. However, extrapolated estimates provide suggestive evidence of increased attendance and decreased truancy beyond the policy threshold, emphasizing the importance of extrapolation in this setting.

Contents

1 Introduction

School breakfast programs have generally faced low participation rates relative to comparable school lunch programs. Consequently, over the past decade, several states have adopted policies which mandate Breakfast After the Bell (BATB) programs for schools above a certain free and reduced lunch (FRL) qualifying enrollment percentage, in an effort to increase breakfast participation. Traditional school breakfast programs are often only offered early in the morning, when students would need additional transportation accommodations to actually receive a meal—an infeasible requirement for many families, especially those in lower-income neighborhoods. In contrast, under BATB, schools must provide free and/or reduced breakfast to qualifying students after the school day begins, making the pickup process much more accessible. Furthermore, by having all children pick up breakfast at the same time during the school day, BATB programs aim to reduce the social stigma of accepting an additional free meal.

Compared to other states, Illinois has had low school breakfast participation rates: in the 2016-2017 school year, Illinois ranked 42nd with a 47.6% participation rate, according to a scorecard published by the Food Research and Action Center. In response to this, in 2016, the Illinois General Assembly passed legislation that would mandate BATB programs in schools with over 70% FRL-qualifying students beginning in the 2017–2018 school year. Many outcomes of interest, from student behaviors to academic performance, are strongly correlated with income, so it is not sufficient to directly compare schools affected by the policy (i.e. above the cutoff) to those unaffected. Regression discontinuity methods are particularly suitable to investigate the effects of this policy, since it is designed around a cutoff of a continuous variable (FRL enrollment).

However, while treatment effect estimates given by standard RDD methods may credibly apply to schools in a neighborhood of the threshold, they need not extrapolate well to schools with significantly higher percentages of FRL-qualifying students. Given that free and reduced meal programs are specifically targeted to aid disadvantaged families, these are likely the exact schools we are most interested in. Furthermore, there are a number of reasons to suspect that schools with around a 70% FRL enrollment could be affected differently than those in lower-income neighborhoods. On the one hand, one might expect that the marginal benefit of an additional free meal would be higher, on average, in schools with a higher percentage of low-income students. At the same time, the factors influencing student decisions may be fundamentally different across neighborhoods of varied socioeconomic statuses; for instance, if the costs of attending school (instead of, say, getting a job) were sufficiently high in lower-income neighborhoods, then students would actually experience fewer benefits of BATB, because many of them would not be receiving the additional meal as often in the first place.[1]

Using data on Illinois public schools, I first identify local causal effects of the BATB legislation, where estimates suggest that the policy had negligible impacts on a range of student outcomes of interest, including absenteeism, truancy, and academic proficiency measures. This stands in contrast to some of the previous work on BATB policies implemented in other states, which has found that they lead to declines in chronic absenteeism rates (Kirksey and Gottfried 2021) and increases in attendance (Chandrasekhar et al. 2023), but is consistent with other research that also finds null results on academic performance (e.g. Cuadros-Meñaca et al. 2022a). Notably, these results also contradict surveys of school principals that report significant decreases across these outcomes (Works 2015). Then, motivated by the reasoning outlined above, I propose three progressively weaker assumptions that allow me to extrapolate treatment effects for schools beyond the 70% cutoff. Under this framework, I find evidence that lower-income schools experience decreases in truancy

[1]A substantial body of work has looked into factors influencing student decisions to attend school. Several papers find that student employment, for example, increases truancy and dropout rates (Eckstein and Wolpin 1999, Dustmann et al. 1997, Staff et al. 2020).

rates and mild increases in attendance rates, suggesting that the students missing school frequently to begin with were most affected by the policy. This result is particularly important considering that I do not estimate significant local changes on attendance and truancy, emphasizing the need for extrapolation in this setting.

This paper contributes to several strands of literature. A large body of previous education research has investigated the impacts of school breakfast programs on a range of student outcomes, from obesity (Millimet and Tchernis 2013) to attendance and academic achievement (Meyers et al. 1989, Bartfeld et al. 2019, Frisvold 2015). In recent years, a number of studies have focused on the impacts of BATB policies specifically (see Olarte et al. 2023 for a review). Ferris et al. (2022) presents evidence that BATB increases breakfast participation in Missouri schools; other work generally finds mixed effects on exam scores (Cuadros-Meñaca et al. 2022a, Chandrasekhar et al. 2023, Cuadros-Meñaca et al. 2022b), but significant decreases in absenteeism and behavioral infractions (Kirksey and Gottfried 2021, Cuadros-Meñaca et al. 2023).

To the best of my knowledge, though similar BATB policies have been studied in other states, the mandate in Illinois has yet to be investigated. In particular, compared to most of the states where BATB has been studied (Arkansas, Colorado, Nevada, Texas), Illinois has nearly twice the amount of students enrolled in public schools, presenting a unique opportunity to see how BATB programs apply on a larger scale (U.S. Department of Education 2023). Furthermore, particularly due to the presence of Chicago, Illinois public schools face unique racial, economic, and geographic diversity; the Chicago Public Schools (CPS) system is the fourth-largest school district and the most racially segregated in the country (Fessenden 2012). During the period that we investigate, CPS also saw notably high usage of its school choice programs that made it easier for students to enroll in different public schools within their district (Barrow and Sartain 2017). These demographic and operational differences in Illinois' public school systems could lead to heterogeneous effects of BATB programs compared to previously-investigated states. In addition to a new setting, while I estimate effects on some of the same outcomes (e.g. overall truancy), I bring in additional and previously unexplored low-income counterparts of interest (i.e. outcomes computed over only the low-income population at a school), such as low-income absenteeism. These additional outcomes allow for further investigation of the target demographic of the policy.

Most importantly, many of the existing studies investigating BATB rely on RDDs to identify treatment effects, and so it is important to note that their results are only guaranteed to be valid in a neighborhood of the policy threshold. However, researchers and policymakers are likely interested in effects that extend beyond the cutoff as well, especially for schools with the highest FRL-eligible enrollments. By exploring ways to extend estimates beyond the threshold, this paper both contributes new findings about the impact of BATB on all schools affected by the mandate and provides a framework for RDD extrapolation that might improve our understanding of a wider range of cutoff-based policies.

The external validity of RDD estimates has been a point of concern in the literature for many years. To address this, a number of extrapolation methods have been suggested (e.g. Angrist and Rokkanen 2015, Dong and Lewbel 2015). My proposed approaches are most related to other extrapolation methods that rely on parallel trends-style assumptions. In the case of multiple cutoff RDD settings, Cattaneo et al. (2021) utilizes such assumptions to estimate treatment effects on post-education attendance from between the different thresholds of a subsidized loan program in Colombia. Most relevantly, Wing and Cook (2013) includes pre-treatment data to serve as an untreated comparison group under parallel trends, later illustrating their method through data from an RCT on the Head Start program. Mealli and Rampichini (2012) uses similar reasoning to investigate the effects of university grants, but utilizes the existence of a treatment-ineligible group instead.

Following this line of work, my proposed extrapolation methods will also rely on pre-treatment data and similar assumptions; however, I expand on previous research in two central ways. First, I aim to conduct more thorough tests of the underlying assumptions, providing an application for the shape restrictions test proposed by Cattaneo et al. (2019). Then, I investigate ways to relax or adjust the parallel trends assumption to accommodate other functional forms for the differences in trends. This is related to a collection of research involving differences-in-differences (DID) settings that also looks into relaxing the parallel trends assumption (see Ban and Kédagni 2022, Rambachan and Roth 2023). Dobkin et al. (2018), which applies a linear parameterization for the trends over time, is particularly relevant, and lays the foundation for one of the methods I suggest. To the best of my knowledge, this paper is the first to utilize the literature on DID extensions to extrapolate RDD estimates, providing a novel application for these methods.

The rest of the paper is organized as follows. Section 2 covers the Illinois schools data used. Then, Section 3 presents the standard (local) RDD estimates for a range of student outcomes. Section 4 discusses three sets of assumptions and how they can be applied to identify treatment effects away from the cutoff; I include the results of each approach for the Illinois BATB mandate. Section 5 concludes.

2 Data sources

I combine several datasets from the Illinois State Board of Education (ISBE), all of which are publicly available for download. The Illinois Report Card provides information on various school characteristics, as well as student outcomes aggregated at the school level. I use Report Card data ranging from the 2014–2015 to 2017–2018 school years and include a number of school characteristics, which are defined below. Importantly, note that students in Illinois are classified as low-income if they either (1) receive or live in households that receive Supplemental Nutrition Assistance Program (SNAP) or Temporary Assistance to

Needy Families (TANF), (2) are classified as homeless, migrant, runaway, Head Start, or foster children, or (3) live in a household where the household income meets the USDA income guidelines to receive free or reduced-price meals. Consequently, FRL student enrollment is a subset of low-income enrollment.

- **Attendance**: The student attendance rate for a school is computed as the aggregate days of student attendance, divided by the sum of the aggregate days of student attendance and aggregate days of student absence, multiplied by 100. The low-income student attendance rate is the student attendance rate but computed among only low-income students.

- **Enrollment**: The enrollment measure describes the total student enrollment in the school as of October 1st. The measure does not include students who do not spend the majority of their school day at the reported school. Low-income enrollment is the same, but for low-income students only.

- **Chronic absenteeism**: A student is considered chronically absent if they have absences (both with and without valid cause) that total 10% or more of school days of the most recent academic school year, excluding any who are medically homebound. The chronic absenteeism rate for a school is computed as the number of chronically absent students, divided by average daily enrollment, multiplied by 100.

- **Truancy**: A student is considered a chronic truant if they are subject to compulsory attendance and have been absent without valid cause for 5% or more of the previous 180 regular attendance school days. The truancy rate for a school is the number of chronic truants, divided by average daily enrollment, multiplied by 100.

- **ELA/Math Proficiency**: Proficiency in English Language Arts (ELA) and Math is defined as the proportion of students who have proficient scores on standardized state and national assessments in the respective subjects. In Illinois, this includes performance levels 4 and 5 on the Partnership for Assessment of Readiness for College and Careers (PARCC) as well as levels 3 and 4 on the Dynamic Learning Maps (DLM) and SAT exams[2].

- **ELA/Math Growth**: Each student is given a growth percentile, measured by their performance over time relative to similarly-performing Illinois students from previous years, using data for up to two previous years. These scores range from 1-99, with 50 representing average growth. The ELA/Math growth measure takes the average of all students' growth percentiles in the respective subject areas.

Importantly, although absenteeism, truancy, and attendance rates measure similar concepts—that is, whether or not students are showing up at school—there are subtle differences in the information conveyed by each. Attendance rates are a general measure of the proportion of school days attended by students overall and do not focus on which students are attending. Absenteeism and truancy, in contrast, describe the proportion of students who are classified as chronically absent or truants. Therefore, it would be possible for attendance rates to fluctuate without any change in the number of chronically absent or truant students at the school, so long as the changes stem from the behavior of students who were not missing much school to begin with. Truancy can be thought of as a more extreme version of chronic absenteeism, as a student is only classified as a truant if they are missing school without valid cause. For instance, a student who takes an extended absence for a religious holiday would be considered chronically absent, but not a truant. The associated costs are also very different: in the time range that we investigate, students could face fines for truancy.

Because the attendance and academic performance outcomes are heavily related to

[2]The scores needed on each exam for specific performance levels are provided online by the ISBE.

each other, I also create indices for both attendance and performance, each including their respective relevant outcomes. Results estimated with the indices are largely unchanged from those estimated from the raw outcomes. More information on index construction and results are included in Appendix Section B.2.

FRL eligibility and breakfast participation data are available through the Nutrition Department of the ISBE. FRL eligibility data is collected annually on October 31, after the school year has begun, so I match eligibility data to the following year's Report Card and participation data. Furthermore, the FRL eligibility reports consist of self-reported data from National School Lunch Program (NSLP) sponsors, so schools in Illinois that do not participate in NSLP are not included. To make up for this, following the state's school breakfast program guidelines, I use low-income percentages reported in fall enrollment counts available through the Illinois Student Information System in place of FRL enrollment for any schools that do not show up in the NSLP data. Within the participation data available, Illinois only began to collect information on the types of school breakfast programs being offered (e.g. after the bell) after the passage of the BATB legislation, starting in the 2016-2017 school year.

In some of my specifications, I include additional covariates to control for neighborhood characteristics. I use data from the National Neighborhood Data Archive (NaNDA), which draws from the 2013-2017 American Community Survey to include a number of socioeconomic status and demographic variables by ZIP code tabulation area (ZCTA) (Melendez et al. 2020). In particular, I make use of neighborhood disadvantage and affluence indices, which are defined as follows:

- **Disadvantage index**: This index is constructed as the average of (1) proportion of female-headed families with kids, (2) proportion of households with public assistance income, (3) proportion of people with their reported income from the past 12 months below poverty level, and (4) proportion of people above the age of 16 in the civilian labor force who are unemployed.

- **Affluence index**: This index is constructed as the average of (1) proportion of families with an annual reported income of greater than 75k, (2) proportion of people with a Bachelor's degree or higher, and (3) proportion of employed civilians above the age of 16 who are in management, business, the sciences, or the arts.

I use fuzzy matching on school names to connect the NaNDA data to the Illinois Report Card, with a match rate of 90.5%. It is important to note that ZCTAs are less granular than the school data, so there are multiple schools that are assigned the same neighborhood characteristics.

For my local results, I focus on the 2017–2018 school year, the first year in which qualifying schools were subject to the BATB requirements, as it is the year in which the jump in treatment probability at the threshold was highest—in the 2018-2019 year, the program was faced with lower compliance on both sides of the threshold, since schools had more time to apply for exemptions. I do not include data from 2019 onwards due to changes in school meal operations during the COVID-19 pandemic. Summary statistics for the main outcomes and controls are reported in Table 1[3].

Both the attendance-related and performance-related outcomes at schools are correlated with FRL enrollment share, motivating the use of RDD to estimate effects. To visualize these differences, summary statistics computed only among schools above the 70% FRL cutoff are depicted in Table 2.

In general, schools with higher proportions of FRL-eligible students experience lower attendance, higher truancy and absenteeism, and lower ELA/math proficiency and growth. Lower-income schools do not differ much on average enrollment size, but are located in neighborhoods with higher disadvantage and lower affluence indices. These patterns largely

[3] I do not include charter schools, which were not subject to the mandate, or schools with 100% FRL enrollments in this analysis, which comprise a disproportionate number of schools in the sample.

Table 1: Summary statistics, overall

Variable	No. Obs	Min	Max	Median	Mean	StDev
Attendance Outcomes						
Attendance	2858	56.60	99.30	95.00	94.64	1.99
Truancy	2858	0.00	100.00	2.40	5.19	8.47
Absenteeism	2858	0.00	100.00	9.50	11.69	10.76
Performance Outcomes						
ELA Proficiency	2701	0.00	99.10	39.30	40.54	17.27
Math Proficiency	2701	0.00	97.20	33.30	35.32	17.97
ELA Growth	2118	11.00	89.00	51.70	51.79	9.20
Math Growth	2120	8.00	99.00	51.60	51.59	8.85
Controls						
Pre-Period Enrollment	2784	26.00	4195.00	412.00	525.56	474.86
Disadvantage Index	2807	0.01	0.38	0.09	0.11	0.07
Affluence Index	2807	0.09	0.88	0.34	0.38	0.15

Note: Summary statistics from the 2017–2018 school year, computed over Illinois public schools.

Table 2: Summary statistics, above cutoff

Variable	No. Obs	Min	Max	Median	Mean	StDev
Attendance Outcomes						
Attendance	455	66.80	98.50	94.30	93.59	2.91
Truancy	455	0.00	100.00	8.40	12.82	15.44
Absenteeism	455	0.00	100.00	14.60	18.09	13.59
Performance Outcomes						
ELA Proficiency	438	2.10	71.80	22.10	24.04	12.42
Math Proficiency	438	0.00	66.80	16.70	19.26	11.51
ELA Growth	352	11.30	80.50	47.35	47.59	8.59
Math Growth	353	11.00	71.30	47.70	47.64	8.03
Controls						
Pre-Period Enrollment	622	53.00	3518.00	414.00	530.27	401.96
Disadvantage Index	633	0.01	0.38	0.15	0.16	0.07
Affluence Index	633	0.09	0.77	0.28	0.31	0.14

Note: Summary statistics from the 2017–2018 school year, computed over Illinois public schools with at least a 70% enrollment share of FRL-qualifying students.

Table 3: Summary statistics, low-income vs. non low-income

Variable	No. Obs	Min	Max	Median	Mean	StDev
LI Absenteeism	2773	0.00	100.00	14.80	16.77	12.59
Non LI Absent.	2755	0.00	100.00	6.06	8.77	11.35
LI ELA Proficiency	2578	0.00	100.00	26.00	27.94	14.07
Non LI ELA Prof.	2570	0.00	100.00	47.23	47.12	17.15
LI Math Proficiency	2579	0.00	95.50	19.70	22.00	13.16
Non LI Math Prof.	2570	0.00	100.00	41.27	41.60	17.79
LI ELA Growth	2103	11.00	98.30	49.80	50.30	9.83
Non LI ELA Growth	2053	0.00	97.30	52.48	52.46	9.98
LI Math Growth	2105	7.50	99.00	49.60	49.90	9.38
Non LI Math Growth	2055	0.00	100.00	52.61	52.41	9.75

Note: Summary statistics from the 2017–2018 school year, computed over Illinois public schools. The low-income (LI) measures are provided in the Illinois Report Cards, while the non LI measures are calculated from the LI measures and the low-income enrollment shares.

follow expectation, though the lower growth measures among schools above the cutoff is particularly notable. Because the "growth" academic measure is already only computed against similarly-scoring students from previous years, lower-income schools experiencing lower growth suggests spreads in academic performance are increasing between income groups over the time period I investigate.

The same patterns that appear *across* schools with different FRL enrollments also persist even *within* the same school when comparing their low-income to non low-income student populations. The Report Card data for the 2017–2018 school year includes absenteeism and performance outcomes computed only over low-income students. In combination with data on low-income enrollment shares at these schools, I compute the same measures among the non low-income population to compare the measures between students at the same school. Summary statistics of these outcomes by income group (low-income and non low-income) are shown in Table 3. Once again, chronic absenteeism is substantially higher, while proficiency and growth are lower among low-income student populations.

3 Local effects of Illinois BATB

3.1 Methodology

Beginning with the 2017–2018 school year, Illinois schools with at least a 70% FRL-qualifying enrollment percentage were legally mandated to implement BATB. Importantly, a sizable portion of schools with lower FRL enrollments still chose to offer BATB options, though they were not required to. Schools beyond the threshold could also avoid the requirement if they fulfilled one of two conditions: (1) they could provide evidence that their breakfast participation among FRL-qualifying students was already above 70%, or (2) they had district-specific reasons why the provided funding for BATB would not be sufficient to implement the program, for which they would need to host a public town hall meeting to vote on the issue.[4]

As a result of this imperfect compliance, I apply both sharp and fuzzy RDD methods to this setting, though there are important distinctions in the interpretation of their results. The sharp RDD estimates will provide the causal effect of the legislation itself (i.e. an ITT estimate), whereas the fuzzy estimates will give the causal effect of actually implementing BATB.

I first consider the sharp case, following notation given by Cattaneo and Titiunik (2022). Let schools be indexed by $i = 1, \ldots, n$ with FRL enrollment given by X_i, and let $c = 70$ denote the BATB cutoff. Being mandated to implement BATB is then given by $Z_i = \mathbb{1}[X_i \geq c]$, which satisfies both $\mathbb{P}[Z_i = 1|X_i < c] = 0$ and $\mathbb{P}[Z_i = 1|X_i \geq c] = 1$. For the sharp RDD case, each school has potential outcomes $Y_i(0)$ and $Y_i(1)$; the observed outcome can be written as $Y_i = Y_i(1)Z_i + Y_i(0)(1 - Z_i)$. The average treatment effect at the cutoff is identified under two identifying assumptions, as part of the RDD continuity

[4] It was much more likely for a school to offer BATB when it was not required to than vice versa, as can be seen in Appendix A.

framework originally outlined by Hahn et al. (2001):

Assumption 1: $\mathbb{E}[Y_i(0)|X_i = x]$ and $\mathbb{E}[Y_i(1)|X_i = x]$ are continuous in x at c.

Assumption 2: The density of X_i near c is positive.

I check these assumptions in this setting in two ways. I run balance tests locally around the threshold on a few covariates of interest[5] to see that, on average, schools appear similar above and below the cutoff. Additionally, because Assumption 1 would be violated if schools were intentionally misreporting their FRL enrollment, I implement a density test developed by Cattaneo et al. (2018) in order to test for manipulation around the cutoff. This test fails to reject the null of no sorting on my data.[6] Together, these tests provide some evidence for the validity of Assumptions 1-2 in this setting.

Intuitively, under these assumptions, any discontinuous jump in average outcomes over the 70% cutoff should be the result of the legal requirement for BATB programs. Thus, the causal effect of the policy is identified and given by

$$\tau_{\text{srd}} = \mathbb{E}[Y_i(1) - Y_i(0)|X_i = c] = \lim_{x \to c^+} \mathbb{E}[Y_i|X_i = x] - \lim_{x \to c^-} \mathbb{E}[Y_i|X_i = x]$$

Now I consider the fuzzy RDD setting, where I am interested in identifying the effect of offering BATB, not just the legislation itself. Because treatment assignment (Z_i) differs from actual treatment in this case, I define implementation of BATB by D_i; each school has potential treatments $D_i(0)$ and $D_i(1)$ representing its BATB status below and above the cutoff, respectively. With the following additional assumptions, the causal effect of compliers around the cutoff is identified:

[5] I test balance on total enrollment (i.e. school size) as well as NaNDA's disadvantage and affluence indices; all tests fail to reject the null at a 0.05 significance level.

[6] For visual reference, the density is provided in Appendix A.

Assumption 3: There are no defiers: i.e. $D_i(1) \geq D_i(0) \quad \forall i$.

Assumption 4: $\lim_{x \to c^-} \mathbb{P}[D_i = 1 | X_i = x] \neq \lim_{x \to c^+} \mathbb{P}[D_i = 1 | X_i = x]$.

Assumption 3 seems fairly believable in this setting. If a school provides a BATB program when they are not legally obligated to, they would probably still provide one when required; similarly, if they would not offer BATB even when the law did require them to, then they likely never would. I check Assumption 4 by estimating a sharp RDD on the data using BATB participation as an outcome. Probability of participation increases by slightly over 20% around the threshold, which can be seen visually in Figure 1. These assumptions allow me to identify the effect of BATB programs on complier schools near the threshold, essentially as the ratio of two sharp RDD estimates:

$$\tau_{\text{frd}} = \frac{\lim_{x \to c^+} \mathbb{E}[Y_i | X_i = x] - \lim_{x \to c^-} \mathbb{E}[Y_i | X_i = x]}{\lim_{x \to c^+} \mathbb{E}[T_i | X_i = x] - \lim_{x \to c^-} \mathbb{E}[T_i | X_i = x]}$$

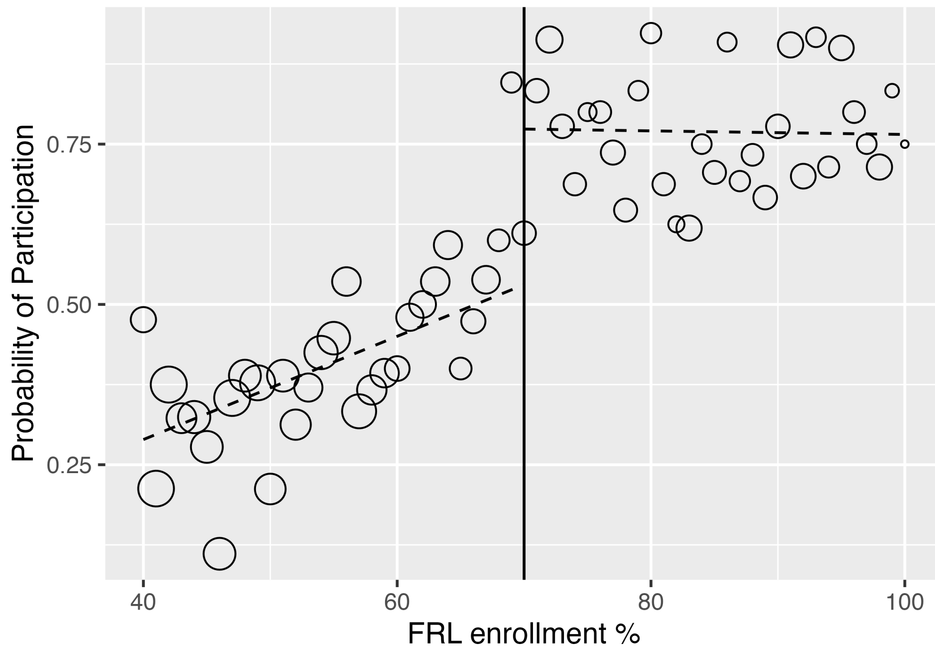
In order to implement both the sharp and fuzzy designs, I estimate τ_{srd} as the difference in intercepts of two local linear regressions on either side of the cutoff. I compute these estimators using a symmetric, MSE-optimal bandwidth[7] and a triangular kernel. Furthermore, in order to construct confidence intervals, I include a bias correction to ensure appropriate coverage (see Calonico et al. 2014 for more details on the RDD estimation procedure).

As a robustness check, I also estimate results where I include neighborhood characteristics and pre-period enrollment as covariates. These specifications rely on largely the same assumptions listed above, but include one additional assumption:

Assumption 5: $\lim_{x \to c^+} \mathbb{E}[W_i | X_i = x] - \lim_{x \to c^-} \mathbb{E}[W_i | X_i = x] = 0$.

[7] Bandwidth sensitivity tests are provided in Appendix C.

Figure 1: BATB participation by FRL enrollment share



Note: This figure plots the probability of offering a BATB program over the share of FRL-qualifying students. Probabilities are computed over bins at each percentage point. The policy cutoff of 70% FRL enrollment is plotted as a vertical line, while the OLS lines, computed separately for each side, are plotted as dashed lines. Each dot is sized proportionally to the square root of the number of schools falling into the respective enrollment bin.

for any covariate W_i. Intuitively, when including covariates, RDD estimates are the sum of the estimated discontinuity in outcomes as well as the estimated discontinuity in covariates; Assumption 5 holds that there is no RDD treatment effect on the covariates. Both the neighborhood characteristic indices as well as pre-period enrollment are measured over a time period before the Illinois mandate was put into place, and RDD estimates on these controls are not distinguishable from zero, so this assumption should hold in this setting.

3.2 Local results

I begin with the sharp RDD estimates, which are reported in Table 4: the first column reports the unconditional estimates and the second includes controls for school size in the previous year and neighborhood characteristics.[8] The inclusion of additional covariates does not significantly affect the results.

Table 4: Sharp RDD Results

	No Controls		With Controls	
Outcome	Estimate	(SE)	Estimate	(SE)
Attendance	0.527	(0.675)	0.450	(0.455)
Truancy	-0.787	(3.174)	-3.358	(2.715)
Absenteeism	-1.944	(3.195)	-1.541	(3.074)
LI Absenteeism	-2.305	(3.634)	-0.167	(2.791)
ELA Proficiency	-2.076	(3.962)	-4.897	(3.455)
LI ELA Proficiency	-1.825	(3.436)	-4.386	(3.051)
Math Proficiency	1.252	(3.411)	-2.007	(3.062)
LI Math Proficiency	-0.627	(2.464)	-2.928	(2.712)
ELA Growth	-0.662	(2.588)	-1.088	(2.735)
LI ELA Growth	0.780	(2.316)	0.135	(2.490)
Math Growth	2.799	(2.576)	2.157	(2.446)
LI Math Growth	3.598	(2.674)	1.190	(2.181)

Note: Robust SEs reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. This table presents the local sharp RDD estimates under two specifications: without any controls on the left, and including neighborhood and enrollment controls on the right.

These estimates suggest that the impact of the 2016 legislation was negligible across attendance, absenteeism and truancy, which may reveal some information about why results show limitations on the reach of BATB: clearly, because the meals are served in school during the day, any eligible student who is absent will not receive a breakfast to begin with. ISBE data also shows that truancy and chronic absenteeism rates are higher among low-income students, who are the main intended beneficiaries of BATB. Thus, the lack of a

[8] For neighborhood characteristics, I use the disadvantage and affluence indices offered in NaNDA, as defined in Section 2.

change on these outcomes is somewhat concerning, as it could suggest that the free meals being offered are often not actually reaching the FRL-eligible students who are regularly missing school in the first place.

These results also differ from existing work on BATB mandates in other states which find significant decreases in chronic absenteeism near the cutoff (e.g. Kirksey and Gottfried 2021). Though the legislation itself is designed similarly across states (previous research investigates Colorado and Nevada), the composition of students and schools might have substantial differences, which could account for the varied effects of a BATB mandate. My estimates here could point to the existence of differing impacts of free meal programs between different states and their public school systems; further work would be needed to confirm this.

Finally, consistent with the existing literature on school breakfast and BATB programs, I find null results on a range of academic proficiency measures, specifically in English/Language Arts (ELA) and Math. In line with Cuadros-Meñaca et al. (2022a), this suggests that although students eat an additional meal during the school day under BATB—potentially reducing or interrupting class time—there are no adverse effects on academic performance.

I leave the fuzzy RDD results, which describe the impact of offering a BATB program (as opposed to the effect of being mandated to, as measured in the sharp design) to Appendix B.1. Importantly, though the first stage estimates (i.e. the jump in BATB participation at the threshold) are significant, as reported in Appendix A, the increase is only around 20 percentage points. Therefore, it is important to keep in mind that the magnitude of our point estimates are likely unreliable due to a noisy first stage. Interpretations remain largely consistent, though, as I am unable to reject the null of no impact across the range of student outcomes that I investigate.

4 Extrapolation

4.1 Background

The local results reported above, under the assumptions listed, identify effects on schools with near-70% FRL enrollments. However, standard RDD estimates do not necessarily extrapolate well to the schools we are likely most interested in: those with the highest low-income populations in Illinois. Without additional information regarding the impacts on these schools, it could be difficult to draw reliable conclusions about the BATB mandate and its effectiveness. To address this, I impose additional assumptions in order to estimate the impact of the legislation on all affected schools.

I focus specifically on two outcomes of particular importance to understand across the full range of schools: attendance and truancy. These two outcomes are especially relevant as they relate to the overall effectiveness of BATB in reaching students: namely, if BATB does not motivate students missing many school days to attend more often, then they often do not receive the additional free meal.

My setup, outlined below, resembles a difference-in-differences (DID) setting. More specifically, while DID specifications rely on the assumption that the relationship between outcomes and *time* would remain similar (parallel) in the absence of treatment, I consider the idea that the relationship between outcomes and *FRL enrollment* would remain similar in the absence of the BATB mandate. I expand on Wing and Cook (2013) and Mealli and Rampichini (2012), who similarly propose using a parallel trends assumption in order to extrapolate treatment effects away from the threshold in RDDs.[9]. The general model they consider, however, can be made more flexible, where the difference in trends need not

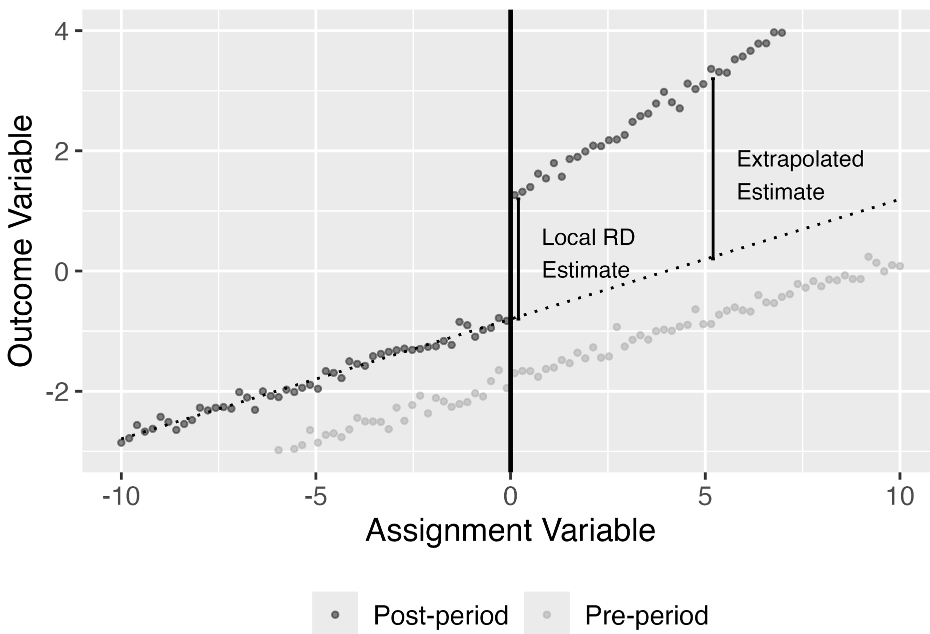
[9] My approach more closely resembles Wing and Cook (2013), since I will also use pre-treatment data (from before the mandate was passed) as the untreated comparison group. Mealli and Rampichini (2012) uses a comparison group of individuals who were ineligible for treatment; I do not have an analogous group in this setting.

be constant so long as it is an estimable function of the running variable. Following this framework, I propose methods that will rely on some assumption of the form

$$\mathbb{E}[Y_{i,t+1}(0)|X_{i,t} = x] - \mathbb{E}[Y_{i,t}(0)|X_{i,t} = x] \equiv f(x)$$

where the outcome and FRL enrollment $(Y_{i,t}, X_{i,t})$ are now indexed both by school and by time t.

Figure 2: Extrapolation illustration, parallel trends



Note: This figure illustrates the extrapolation methods used below, particularly for the simplest case of constant differences (i.e. parallel trends). The dotted line extending beyond the cutoff represents the assumed counterfactual, computed based on a constant difference over the range of the assignment variable.

The simplest case of parallel trends is visualized in Figure 2. Intuitively, in this case, extrapolation relies on the assumption that the relationship between the assignment and outcome variables stays similar (though may differ by a constant) in the pre- and post-periods. From this assumption, a post-period untreated counterfactual can be constructed past the cutoff, allowing for extrapolation. The weaker assumptions follow similar reasoning, though may not have only a constant difference between the pre- and post-period data.

In the following sections, I aim to identify causal effects of the BATB legislation beyond the cutoff[10]. I achieve this by considering three progressively weaker restrictions on the form of $f(x)$. First, I follow Wing and Cook (2013) and Mealli and Rampichini (2012) and assume f is constant. Then, I relax this by considering the case where f is linear, but not necessarily constant; this is analogous to the parametric difference-in-differences setting proposed by Dobkin et al. (2018). Finally, I suppose only that f is non-increasing, but make no further assumptions on its form: under this assumption, I do not point identify the treatment effects, but instead provide a bound. Proofs for each of the propositions introduced below can be found in Appendix D.

Though I cannot test any of the following assumptions directly, I check them in a number of ways. For the first two sets of assumptions, I implement tests analogous to checking for parallel pre-trends in DID settings. Then, I test for monotonicity by estimating f using data from two pre-treatment years (i.e. before the mandate passed) and running shape restrictions tests proposed in Cattaneo et al. (2019). Intuitively, if f satisfied the necessary shape restrictions before the BATB legislation, it provides evidence that it would also satisfy the assumptions in the years I investigate. I leave the details of this testing procedure for the reader in Appendix E.

4.2 Parallel trends

To begin, I impose the following:

Assumption 5.a: $f(x) = \mathbb{E}[Y_{i,t+1}(0)|X_{i,t+1} = x] - \mathbb{E}[Y_{i,t}(0)|X_{i,t} = x] = \delta$ for some

[10] Treatment is defined as in the sharp RDD case, and so results are ITT estimates; importantly, though, noncompliance is significantly lower beyond the cutoff, as shown in Figure A.1.

constant δ.

This is analogous to the parallel trends assumption in DID specifications, except that the running variable is FRL enrollment instead of time. Intuitively, here, I am assuming that the relationship between FRL enrollment and outcomes of interest would have stayed constant across years (with potential differences in levels) in the absence of treatment. This then allows me to identify an average treatment effect for schools with $X_i > c$:

PROPOSITION 1: Under 5.A, treatment effects are identified for FRL enrollments beyond the cutoff (i.e. $x \geq c$) and are given by

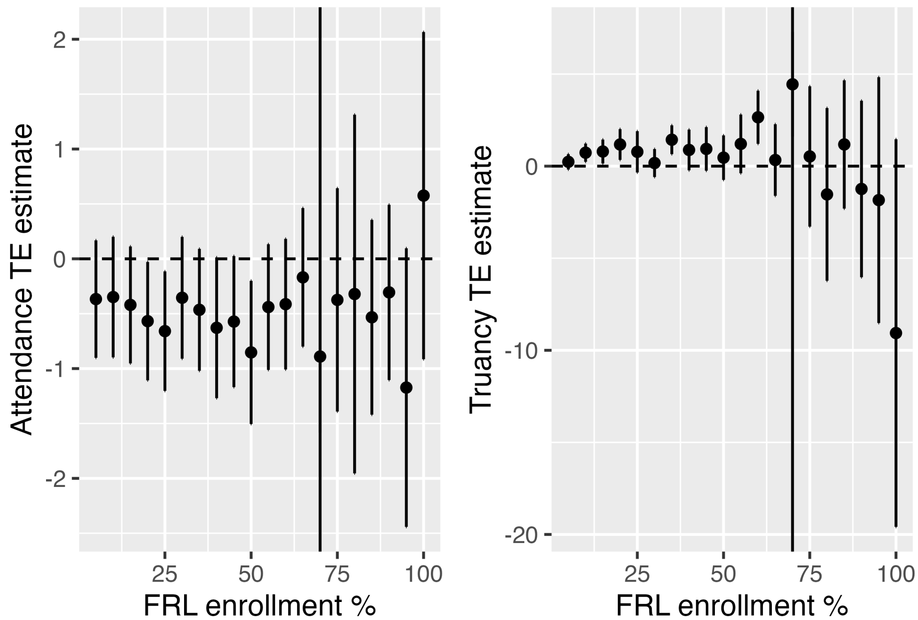
$$\begin{aligned} \mathrm{ATT}_x =& \Big(\mathbb{E}[Y_{i,t+1}(1)|X_{i,t+1} = x] - \mathbb{E}[Y_{i,t}(0)|X_{i,t} = x] \Big) \\ & - \Big(\mathbb{E}[Y_{i,t+1}(0)|X_{i,t+1} < c] - \mathbb{E}[Y_{i,t}(0)|X_{i,t} < c] \Big). \end{aligned} \tag{1}$$

This is essentially a DID setting, though FRL enrollment is continuous, while time periods in DID specifications are generally discrete. To address this, I discretize FRL enrollment into 20 bins[11]. I compute estimates for each bin as presented in Table 5; results are presented visually in Figure 3. The points below the 70% threshold are tests of the parallel trends assumption, using the same pre-trends testing as a standard DID specification. The points above the cutoff give extrapolated treatment effects along with 95% confidence intervals.

Across the three outcomes investigated here, the extrapolated results suggest that the BATB legislation had negligible results on attendance and truancy, consistent with the local results.

[11]Our results are generally robust to the choice of bins, albeit with more noise as the number of bins grows. Estimates using alternative bin amounts are presented in Appendix C.1.

Figure 3: Extrapolated estimates, constant assumption



Note: This figure illustrates the results of extrapolating while assuming parallel trends (i.e. constant differences) along with 95% confidence intervals. Estimates to the left of the cutoff show violations of parallel trends, so results should be interpreted with caution.

4.3 Linear trends

One way to relax Assumption 5.A is to suppose that the relationship between outcomes and FRL enrollment does change across years, but in a way that can be parameterized. Let G_i be a binary variable representing the year (in our case, suppose $G_i = 1$ for the 2017–2018 school year). Then:

Assumption 5.b: $\mathbb{E}[Y_i(0)|G_i = g, X_i = x] - \mathbb{E}[Y_i(0)|G_i = g, X_i = k] = \alpha_{1,x} + \delta(x-k)g$ for constants $\alpha_{1,x}, \delta$.

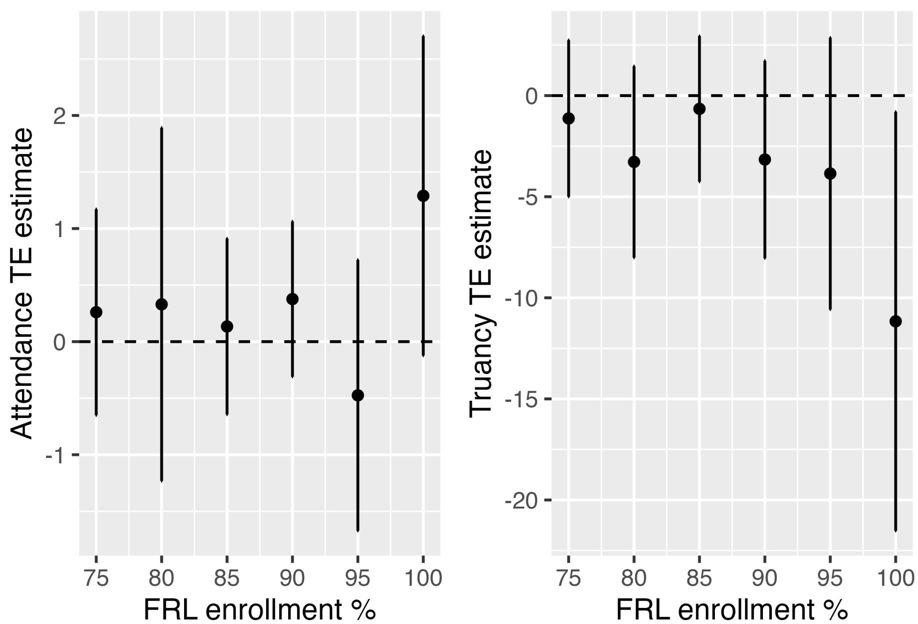
Proposition 2: Under Assumption 5.B, treatment effects ATT_x are identified for

FRL enrollments $x \geq c$ with the following specification:

$$Y_i = \gamma_0 + (\gamma_1 - \delta)g + \delta x g + \left(\sum_k \alpha_{1,k} \, \mathbb{1}[X_i = k] \right) + \sum_{x \geq c} \mathrm{ATT}_x g \, \mathbb{1}[X_i = x] \tag{2}$$

This procedure resembles a two-way fixed effects specification, except that FRL enrollment is included linearly. The extrapolated results are shown in Figure 4 and estimates are provided in Table 5. Matching with previous results, I see negligible effects on all three outcomes throughout the range of FRL enrollments above 70%. Importantly, however, the linear results on truancy should be interpreted with caution, as one of the two shape restrictions tests on truancy rejects[12].

Figure 4: Extrapolated estimates, linear assumption



Note: This figure illustrates the results of extrapolating while assuming linear trends (i.e. differences that vary linearly) along with 95% confidence intervals. Importantly, truancy fails the pre-post shape restrictions test and should be interpreted with caution.

[12]See Appendix E.

4.4 Monotonic trends

To relax the assumptions on f even further, we may not want to parameterize the difference between untreated outcomes in the absence of treatment at all, but rather place restrictions on how f might behave. For instance, in this case, with regard to the relationship between student outcomes and FRL enrollment, we may be willing to impose the assumption that f is monotonic, even if we do not exactly know its form. In this section, I utilize the following to derive bounds for treatment effects (with analogous results for non-decreasing functions):

Assumption 5.c: f is non-increasing in x.

Proposition 3: Under Assumption 5.C, lower bounds on treatment effects for FRL enrollments $x \geq c$ are identified and given by

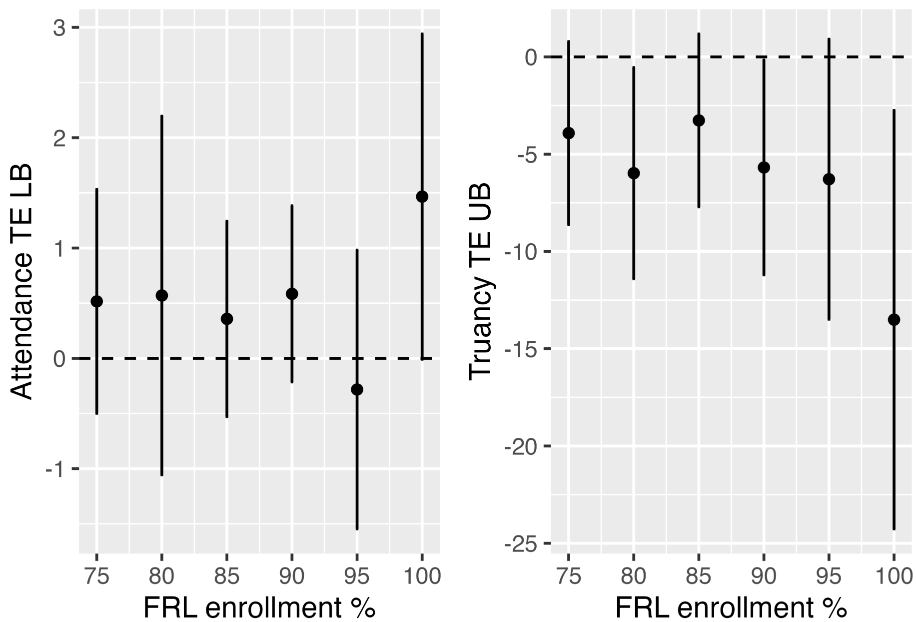
$$\mathrm{LB}_x = \mathbb{E}[Y_{i,t+1}(1)|X_{i,t+1} = x] - \Big(\mathbb{E}[Y_{i,t}(0)|X_{i,t} = x] + f(c)\Big). \tag{3}$$

Based on the outcomes of the shape restrictions test, I compute lower bounds for attendance and upper bounds for truancy and mobility, as shown in Figure 5; estimates are shown in Table 5.

The lower bounds on attendance are mixed, with light evidence of a positive lower bound around schools with the highest FRL enrollments. Most of the other estimates are also positive in sign, but are not significant. This could provide some suggestion of a very slight increase in attendance rates—around 0.5-1 percentage points—for schools with an FRL enrollment beyond the threshold. The increased availability of an additional free meal should provide incentives for students to come to school more often, so directionally, this makes sense.

For truancy rates, though estimates are noisy, the upper bounds are generally either

Figure 5: Extrapolated estimates, monotonic assumption



Note: This figure illustrates the results of extrapolating while assuming monotonic trends (i.e. differences that vary monotonically) along with 95% confidence intervals. Based on our assumptions, attendance estimates represent lower bounds, while truancy estimates represent upper bounds.

indistinguishable from zero or negative, suggesting that lower-income schools do experience decreases in the number of truants due to the additional breakfast offering. This differs from the insignificant impacts found in the local results and emphasizes the importance of extrapolation in this setting. Once again, a negative effect here also matches intuition: an additional and more accessible free meal that is only available to be picked up at school provides more incentive to go to school regularly, lowering the rate of truancy. This result suggests that lower-income populations are more responsive to the additional meal offering, which makes sense considering these are the target populations of the policy and facing higher rates of food insecurity. It is important to note, however, that these estimates are noisy and therefore consistent with both small and large decreases in truancy rates.

Furthermore, though attendance and truancy rates both capture some measure of students' decisions to come to school, attendance is more general—it captures the proportion

Table 5: Extrapolation Results

Assumption	Outcome	75%	80%	85%	90%	95%	100%
Constant	Attendance	-0.374	-0.321	-0.533	-0.306	-1.173*	0.576
		(0.519)	(0.833)	(0.453)	(0.407)	(0.647)	(0.760)
	Truancy	0.527	-1.534	1.179	-1.239	-1.843	-9.063*
		(1.944)	(2.392)	(1.777)	(2.445)	(3.405)	(5.368)
Linear	Attendance	0.261	0.330	0.134	0.377	-0.475	1.290*
		(0.469)	(0.805)	(0.401)	(0.353)	(0.617)	(0.737)
	Truancy	-1.129	-3.279	-0.655	-3.161	-3.855	-11.163**
		(1.993)	(2.441)	(1.855)	(2.513)	(3.462)	(5.411)
Monotonic	Attendance (LB)	0.516	0.569	0.358	0.584	-0.283	1.466*
		(0.518)	(0.829)	(0.452)	(0.407)	(0.644)	(0.752)
	Truancy (UB)	-3.918	-5.979**	-3.266	-5.683**	-6.288*	-13.507**
		(2.399)	(2.769)	(2.266)	(2.816)	(3.670)	(5.485)

Note: Robust SEs reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Estimates in gray are reported for consistency, but should be interpreted with caution based on shape restrictions tests.

of days attended by students over total possible days, as opposed to measuring the proportion of students who miss a substantial amount of school. The stronger negative effects on truancy, then, in tandem with the insignificant estimates on attendance at certain FRL enrollment shares, suggest that the BATB legislation increased the likelihood of attending school among the students missing the most school and had smaller impacts on the groups of students who were not missing school frequently to begin with. This result may follow expectation: because truancy is a more extreme measure of attendance, it is significantly higher among lower-income students that are the primary targets of the policy and are more likely to directly benefit from the BATB offering, as they qualify for the meal to be free or reduced.

5 Conclusion

The 2016 BATB legislation in Illinois was part of a national wave of similar mandates designed to help increase school breakfast participation, particularly among FRL-eligible students. Though the existing literature has provided some evidence of decreases in absenteeism and behavioral infractions as a result of BATB policies in other states, I find insignificant effects on related outcomes in Illinois, including truancy and chronic absenteeism, for schools near the 70% FRL enrollment threshold.

A major limitation of RDD frameworks—and therefore, the methods used by much of the previous work on BATB—is external validity: treatment effect estimates, even when credible in a neighborhood near the cutoff, are not guaranteed to hold well for units farther away. In order to investigate lower-income schools, given that the BATB policy is meant to target low-income students, I impose additional assumptions and extrapolate treatment effects for all Illinois schools beyond the 70% FRL enrollment threshold. As a result, I find some evidence that schools beyond the cutoff experience decreases in truancy and slight increases in attendance rates.

These results have a number of implications for the BATB policy in Illinois, as well as school breakfast policies on a broader scale. In particular, these estimates shed light on the students that BATB programs are managing to reach. Given that I find negative impacts on truancy among the lower-income schools affected by the mandate, it seems that the additional breakfast offerings lead to particularly substantial changes in the behaviors of students who missed school frequently. The extrapolated effects on attendance also provide potentially promising outlooks for BATB programs among lower-income schools. For both of these outcomes, these results emphasize the need for extrapolation in this setting, as both outcomes yield estimates indistinguishable from zero close to the cutoff.

There are still several open questions with regard to the BATB policy in Illinois that would require further investigation and additional data to pursue. The stronger impacts on truancy relative to attendance suggest that there could be some heterogeneity between students based on how much they were attending school to begin with, but due to data limitations, I am unable to further investigate heterogeneity within student populations.

As a final note, many of the results that I find here differ from the existing literature, which investigates similar BATB policies, but in other states, like Colorado. In the future, it could be worth investigating this difference across states and what factors might be contributing to it in order to better understand where BATB programs would be most effective.

Acknowledgments: I thank Jonas Enders, Francesco Ruggieri, Rebecca Sealy, and Alexander Torgovitsky for their helpful comments and suggestions. I am especially grateful to Kirill Ponomarev for his valuable guidance throughout the project.

A Additional descriptives

Figure A.1: BATB status by FRL enrollment

Figure A.2: School counts by FRL enrollment

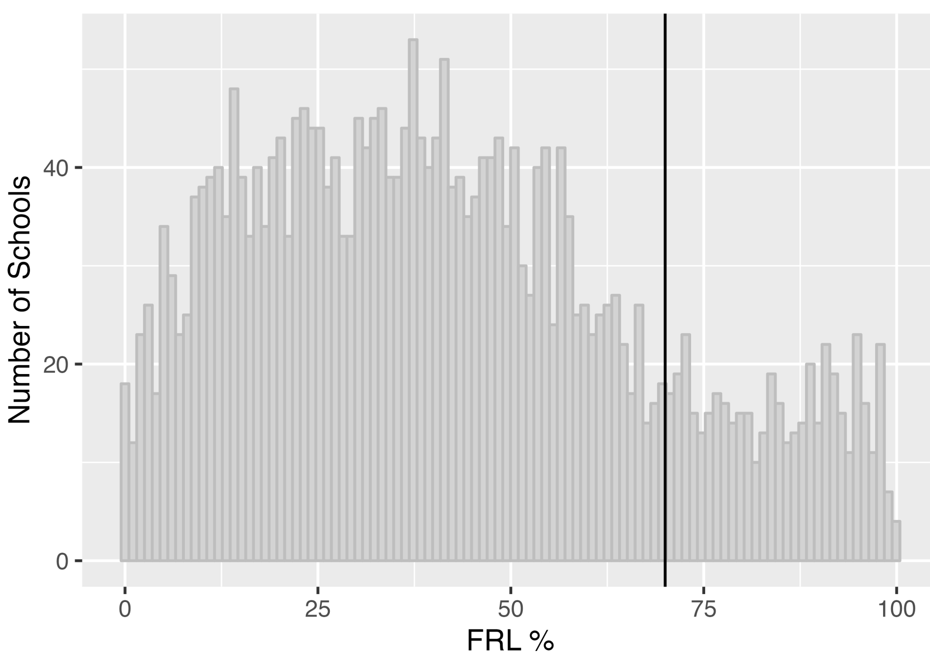


Table A.1: First Stage Estimates

	No Controls		With Controls	
Outcome	Estimate	(SE)	Estimate	(SE)
Attendance	0.207**	(0.104)	0.211***	(0.073)
Truancy	0.201**	(0.096)	0.200**	(0.088)
Absenteeism	0.201**	(0.098)	0.206***	(0.077)
Low-Income Absenteeism	0.197**	(0.088)	0.204**	(0.087)
ELA Proficiency	0.213*	(0.112)	0.185*	(0.101)
Low-Income ELA Prof.	0.211**	(0.107)	0.198**	(0.094)
Math Proficiency	0.209*	(0.115)	0.194**	(0.096)
Low-Income Math Prof.	0.204*	(0.115)	0.185*	(0.100)
ELA Growth	0.179*	(0.091)	0.202**	(0.099)
Low-Income ELA Growth	0.196**	(0.086)	0.195*	(0.102)
Math Growth	0.157	(0.114)	0.201**	(0.101)
Low-Income Math Growth	0.178*	(0.104)	0.201**	(0.101)

Note: Robust SEs reported in parentheses. $^{*}\,p < 0.1$; $^{**}\,p < 0.05$; $^{***}\,p < 0.01$.

B Alternative specifications

B.1 Fuzzy specification

Results from the fuzzy RDD specification are included below. These estimates are substantially noisier than those in the sharp specification, though conclusions are largely the same.

Table B.1: Fuzzy RDD Results

	No Controls		With Controls	
Outcome	Estimate	(SE)	Estimate	(SE)
Attendance	2.579	(3.360)	2.385	(1.975)
Truancy	-2.897	(15.296)	-15.958	(15.648)
Absenteeism	-10.160	(16.898)	-5.553	(14.415)
LI Absenteeism	-4.763	(15.827)	-11.862	(16.211)
ELA Proficiency	-6.445	(17.455)	-22.568	(22.041)
LI ELA Proficiency	-7.069	(15.650)	-23.027	(18.007)
Math Proficiency	5.341	(16.307)	-11.579	(18.917)
LI Math Proficiency	1.493	(15.083)	-16.667	(20.005)
ELA Growth	-5.262	(11.629)	-5.216	(13.999)
LI ELA Growth	1.329	(9.395)	1.041	(12.657)
Math Growth	17.286	(18.695)	11.746	(15.992)
LI Math Growth	16.040	(16.687)	9.885	(16.226)

Note: Robust SEs reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

B.2 Index outcomes

Because the outcomes tested here can largely be grouped into two related categories, I also construct two indices, one for attendance and once for performance, and estimate local results using the indices as outcomes. To construct each index, I simply take the first principal component of the related outcomes. For attendance, I take the negative of truancy and absenteeism (such that more positive numbers are better) along with attendance rates as inputs for the index. For performance, I take ELA/Math proficiency and growth.

Table B.2: Index Outcome Results

Outcome	No Controls Estimate	No Controls (SE)	With Controls Estimate	With Controls (SE)
Sharp Specification				
Attendance Index	-0.343	(0.526)	0.193	(0.257)
Performance Index	0.195	(0.362)	-0.635	(0.515)
Fuzzy Specification				
Attendance Index	-1.728	(2.604)	0.935	(1.204)
Performance Index	0.567	(1.658)	-2.004	(2.260)

Note: Robust SEs reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Then, I compute local estimates using the same procedure as the other main outcomes. Results largely agree (i.e. are not statistically significant) and are shown in Table 8.

C Sensitivity checks

C.1 Alternative binning

For each proposed extrapolation method, I require that the running variable be discretized. To confirm that results are not sensitive to the number of bins, I run each method on 50 bins, with results shown in the figures below.

In the monotonic case, while results generally agree with the 20 bins case, this exercise provides further evidence that lower-income schools may be facing lower truancy and higher attendance as a result of the BATB mandate: several upper bounds for truancy are negative.

Figure C.1: Constant extrapolation, alternative bins

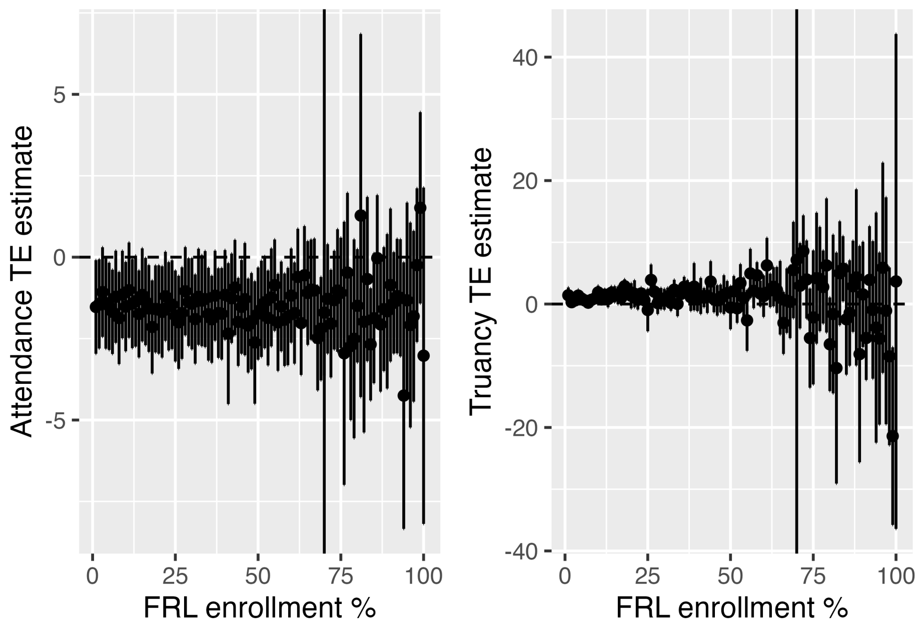


Figure C.2: Linear extrapolation, alternative bins

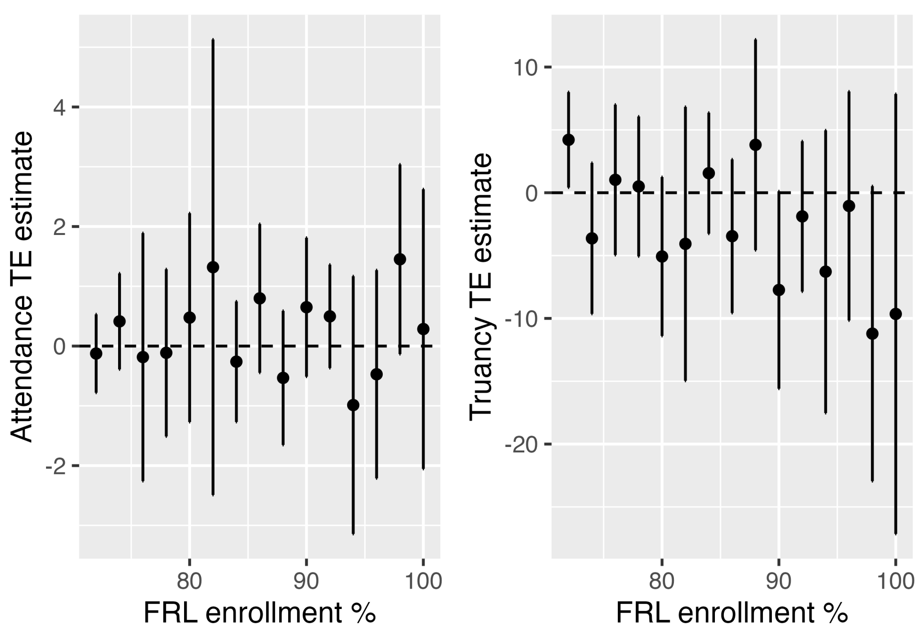


Figure C.3: Monotonic extrapolation, alternative bins

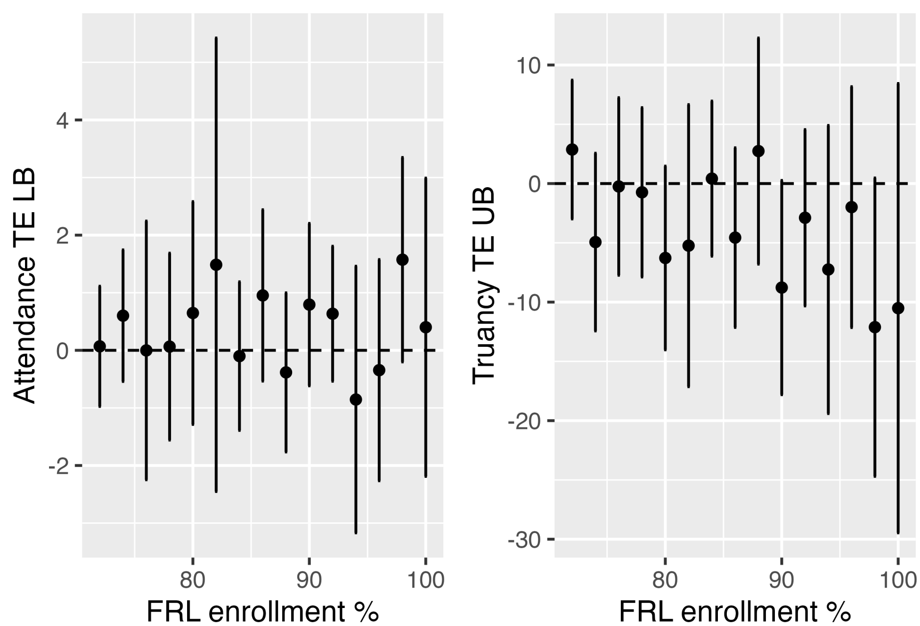


Table C.1: Bandwidth Sensitivity

Outcome	5	7.5	10	12.5	15	20	25
Attendance	0.30 (1.33)	0.40 (1.00)	0.41 (0.84)	0.48 (0.74)	0.56 (0.65)	0.49 (0.52)	0.41 (0.44)
Truancy	-3.83 (5.67)	-2.45 (4.55)	-2.34 (3.98)	-1.55 (3.56)	-0.86 (3.20)	0.02 (2.70)	0.30 (2.35)
Absenteeism	-2.26 (6.07)	-3.78 (4.97)	-3.99 (4.35)	-2.94 (3.86)	-2.23 (3.43)	-1.72 (2.85)	-1.34 (2.44)
Low-Income Absenteeism	-3.90 (6.14)	-5.52 (5.00)	-4.96 (4.36)	-3.45 (3.90)	-1.93 (3.51)	-1.22 (3.01)	-0.36 (2.66)
ELA Proficiency	-2.73 (5.37)	-0.88 (4.67)	-1.89 (4.18)	-1.64 (3.75)	-0.76 (3.37)	-0.90 (2.89)	-0.19 (2.56)
Low-Income ELA Prof.	-0.56 (4.76)	-0.08 (4.13)	-1.26 (3.71)	-1.69 (3.35)	-1.18 (3.01)	-1.45 (2.59)	-0.81 (2.31)
Math Proficiency	-3.19 (4.39)	-0.22 (4.13)	0.31 (3.84)	0.95 (3.53)	1.18 (3.21)	0.22 (2.76)	-0.02 (2.45)
Low-Income Math Prof.	-2.52 (3.78)	-0.12 (3.65)	0.19 (3.38)	0.26 (3.11)	0.11 (2.82)	-0.76 (2.42)	-0.90 (2.15)
ELA Growth	0.43 (4.13)	0.10 (3.67)	-0.58 (3.32)	-0.62 (3.01)	-0.62 (2.73)	-0.78 (2.36)	-0.95 (2.05)
Low-Income ELA Growth	3.32 (3.78)	3.25 (3.34)	2.28 (3.05)	1.86 (2.81)	1.41 (2.56)	0.70 (2.23)	0.33 (1.95)
Math Growth	1.77 (4.32)	1.33 (3.76)	1.78 (3.34)	2.59 (3.00)	2.70 (2.71)	2.07 (2.34)	1.14 (2.07)
Low-Income Math Growth	2.81 (4.78)	3.18 (4.10)	3.32 (3.60)	3.85 (3.18)	3.60 (2.84)	2.96 (2.42)	2.18 (2.11)

Note: Robust SEs reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

D Proofs of propositions

Proof of Proposition 1. Recall that our goal is to identify the quantity $\text{ATT}_x = \mathbb{E}[Y_{i,t+1}(1)|X_{i,t+1} = x] - \mathbb{E}[Y_{i,t+1}(0)|X_{i,t+1} = x]$ for $x \geq c$. The first mean can be estimated from the data, as units beyond the threshold are, by definition, treated. Thus, we simply need to identify the latter. Under Assumption 5.A, we have

$$\begin{aligned}\mathbb{E}[Y_{i,t+1}(0)|X_{i,t+1} = x] &= \mathbb{E}[Y_{i,t}(0)|X_{i,t} = x] + \delta \\ &= \mathbb{E}[Y_{i,t}(0)|X_{i,t} = x] + \mathbb{E}[Y_{i,t+1}(0)|X_{i,t+1} < c] - \mathbb{E}[Y_{i,t}(0)|X_{i,t} < c]\end{aligned}$$

and we are done.

Proof of Proposition 2. As defined above, our estimand of interest is still ATT_x. Observe that

$$\begin{aligned}\mathbb{E}[Y_i|G_i, X_i] &= \mathbb{E}[Y_i(0)|G_i, X_i] + \sum_x g\,\mathbb{1}[X_i = x]\,\mathbb{E}[Y_i(1) - Y_i(0)|G_i = 1, X_i = x] \\ &= \mathbb{E}[Y_i(0)|G_i, X_i] + \sum_x \text{ATT}_x g\,\mathbb{1}[X_i = x]\end{aligned}$$

where the sum is over all $x \geq c$. This holds because for all such x, when $G_i = 1$, we observe $Y_i(1)$; the $Y_i(0)$ quantities cancel out. In all other cases, the second term is zeroed out, and we observe $Y_i(0)$.

Now, note that under Assumption 5.B, we have

$$\mathbb{E}[Y_i(0)|G_i, X_i] = \mathbb{E}[Y_i(0)|G_i = g, X_i = 1] + \alpha_{1,x} + \delta(x-1)g$$

The first term here has a fixed $X_i = 1$, so it is actually just a function of g (that is, it takes two values for each of the possible values of g). Therefore, we can write it as a g term with a coefficient, like so:

$$
\begin{aligned}
&= \gamma_0 + \gamma_1 g + \alpha_{1,x} + \delta(x-1)g \\
&= \gamma_0 + \gamma_1 g + \left(\sum_x \alpha_{1,x} \, \mathbb{1}[X_i = x] \right) + \delta(x-1)g \\
&= \gamma_0 + (\gamma_1 - \delta)g + \delta x g + \left(\sum_t \alpha_{1,x} \, \mathbb{1}[X_i = x] \right)
\end{aligned}
$$

Our final regression specification is then given by

$$\mathbb{E}[Y_i | G_i, T_i] = \gamma_0 + (\gamma_1 - \delta)g + \delta x g + \left(\sum_x \alpha_{1,x} \, \mathbb{1}[X_i = x] \right) + \sum_{x \geq c} \mathrm{ATT}_x g \, \mathbb{1}[X_i = x]$$

Proof of Proposition 3. To begin, note that if $f(x)$ is non-increasing, then $\mathbb{E}[Y_{i,t}(0) | X_{i,t} = x] + f(c)$ gives an upper bound for $\mathbb{E}[Y_{i,t+1}(0) | X_{i,t+1} = x]$ for all $x \geq c$, since $f(c) \geq f(x)$ by assumption. Therefore, we get the following lower bound[13] for the treatment effect for schools with $X_{i,t} = x > c$:

$$\mathbb{E}[Y_{i,t+1}(1) | X_{i,t+1} = x] - \Big(\mathbb{E}[Y_{i,t}(0) | X_{i,t} = x] + f(c) \Big)$$

E Shape restrictions tests

Though I cannot test Assumptions 5.A-5.C directly for $X_{it} \geq c$ when $t \geq t^*$, I test whether the difference in trends is parallel, deviates linearly, or deviates monotonically for two sets of untreated group pairings: (1) schools below the threshold of 70%, between pre- and post-treatment years; and (2) schools both below and above the threshold between two different

[13] If we assume the opposite, that f is non-decreasing, then this will provide an upper bound.

pre-treatment years. The first is similar to testing pre-trends for DID specifications, with the idea that if a shape restriction on the trends holds for schools with an FRL enrollment below the threshold, then it would continue to hold beyond it. For (2), my aim is to check how the relationship between outcomes and FRL enrollment changed over time before treatment, with the idea being that if it remained parallel/deviated linearly/deviated monotonically between years previously (while potentially also differing by a constant), I could expect that it would have done the same after the legislation was passed, had the treatment not taken place.

I include comparison (1) in the results under the parallel trends assumption, following standard DID pre-trends testing procedures. For Assumptions 5.B-5.C, where cannot simply test coefficients, I focus on a separate shape restrictions testing procedure, proposed by Cattaneo et al. (2019).

To begin with, I face the concern of a continuous $X_{i,t}$, which poses difficulties for actually computing $f(x)$ in the data—that is, I do not have access to corresponding pairs $(\mathbb{E}[Y_{i,t+1}(0)|X_{i,t+1} = x], \mathbb{E}[Y_{i,t}(0)|X_{i,t} = x])$ to take differences between. Instead, I use nearest neighbor matching on the data to generate these pairs manually. In this process, each unit from the 2015–2016 pre-treatment data is matched to a unit from the 2014–2015 data with the closest FRL enrollment, measured by the Euclidean norm. Given all sets of these pairs, I take the differences in outcomes between both years, which gives an estimate for f in my data to run tests on. I then run the binscatter shape restrictions test presented in Cattaneo et al. (2019): first, data points for f are grouped into an MSE-optimal number of bins, within which $Y_{i,t}$ and $X_{i,t}$ are averaged; then, joint confidence intervals are computed as well as the best fit for a linear / non-increasing function to the data.

Our test then becomes whether or not the best fit linear / monotonic function falls within the bounds of the joint confidence intervals, in which case we fail to reject the null

Table E.1: Shape Restrictions Test Results

	Pre-Treatment		Below Threshold	
Outcome	Test Stat	p-value	Test Stat	p-value
Monotonic				
Attendance	-1.250	0.755	-2.082	0.214
Truancy	1.485	0.554	1.838	0.230
Linear				
Attendance	1.521	0.964	1.972	0.750
Truancy	13.360	0.000	2.460	0.337

Note: Computed using binscatter test from Cattaneo et al. (2019).

of a constant function. The test statistics and p-values for both outcomes are reported in Table 7.

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