# What impact has universal free school meal legislation had on student performance and engagement in U.S. public schools?

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## 1. Introduction

In 2023, 13.5 percent (18.0 million) of U.S. households were food insecure at some point. At the same time, 17.9% (6.5 million) of households with children in the United States faced food insecurity, of which 3.2 million households had both children and adults who were food insecure. (USDA, 2024). To address this widespread issue, federal programs have been established to provide nutritional support to children in need. One of the most significant initiatives is the National School Lunch Program (NSLP), an initiative designed to provide nutritionally balanced, low-cost or free meals to students in public and nonprofit private schools, as well as residential child care institutions. Established through the National School Lunch Act and signed into law in 1946. These programs are essential for students who may not have reliable access to nutritious meals at home. The National School Lunch Program provides free or reduced-price meals to 20.1 million students. In comparison, the School Breakfast Program serves 19.0 million students with free breakfast, alongside 1.1 million receiving reduced-price meals (Hanson, 2024). However, many children still face food insecurity, with Texas having the highest number at 1,697,870 children affected, as reported by the U.S. Department of Agriculture. Many students who struggle to afford meals come from families that earn too much to qualify for free or reduced lunch but too little to afford regular school meals.

Furthermore, the effects of hunger are particularly harmful in early childhood. Students who experience chronic hunger tend to have lower grades, perform worse on tests, and face greater challenges in graduating and pursuing postsecondary education (Baerren, 2023). Food insecurity also affects a student's overall engagement in school, as food-insecure students also exhibit higher rates of behavioral issues, suspensions, aggression, and school absences compared to their food-sufficient peers (Wilder Research, 2014). Consistently, research shows that universal free meal programs increase participation in school breakfast and lunch while reducing the stigma associated with receiving free or reduced-price meals, fostering a more inclusive and equitable learning environment (Cohen et al., 2021).

Therefore, my study aims to explore the impact that universal free school meal legislation had on student performance and engagement in U.S. public schools. In particular, it will examine how K-12 academic outcomes, enrollment, attendance, and retention rates differ between states that have adopted such policies and those that have not. While studies have examined the benefits of school meal programs, there is limited comparative research on the differences in outcomes between states with universal free lunch and those without. This study will provide a clearer picture of the policy's effectiveness on student engagement and performance. In this context, as of the 2024–2025 school year, nine U.S. states have implemented universal free school meal programs, providing free breakfast and lunch to all students regardless of income. These states are California, Colorado, Connecticut, Maine, Massachusetts, Michigan, Minnesota, New Mexico, and Vermont. In contrast, the remaining states continue to follow the traditional National School Lunch Program guidelines, offering free or reduced-price meals only to students who meet specific income-based criteria (Sheldon, 2024).

## 2. Literature Review

Gennetian et al. (2016) found that fluctuations in Supplemental Nutrition Assistance Program (SNAP) benefits correlate with increased student disciplinary infractions, indicating a link between financial instability, food insecurity, and school behavior. School meal programs serve as a crucial intervention to mitigate these adverse effects. Universal free school meals, in particular, have been recognized for their potential to improve participation in meal programs and enhance student outcomes. Research by Kim and Joo (2020) suggests that direct certification in the National School Lunch Program (NSLP) has led to increased program participation, demonstrating the effectiveness of reducing administrative barriers for eligible students. Similarly, the implementation of universal school meals in Minnesota resulted in significant changes in food security levels among low-income families, reinforcing the importance of these policies in addressing childhood hunger (Larson, N, 2024).

The history of free and reduced-price school meal programs dates back to the passage of the National School Lunch Act in 1946. Over time, policy shifts have influenced the accessibility and scope of these programs. While the federal government provides funding and guidelines, states have taken varied approaches in expanding access to free meals. States such as Vermont and California have fully embraced universal school meals, prioritizing benefits related to student equity and participation. In California, qualitative insights from school food authorities indicate that universal meal programs reduce stigma and increase student participation, although challenges related to operational costs and food waste remain (Orta-Aleman et al., 2024).

States that have not implemented universal school meals often raise concerns about budget constraints and differing political priorities. Economic analyses have explored the financial feasibility of universal meal policies, emphasizing the trade-offs between long-term benefits and upfront costs. The transition to universal meals in select states provides an opportunity to examine the policy's impact on student outcomes as well as the feasibility of broader national implementation (Zhao, 2024).

Empirical studies have examined the effects of universal free school meals on various student outcomes. In states that have implemented such policies, researchers have observed improvements in academic performance, attendance, and graduation rates. For example, a recent study analyzing national data found that universal free school meal programs had a measurable impact on high school graduation rates (Zhao, 2024). Additionally, in Minnesota, the shift to universal free meals led to higher school meal participation rates and a reduction in reported food insecurity among households with children (Orta-Aleman et al., 2024).

In a different study, Larson et al. (2024) utilized a longitudinal survey method to assess changes in food security and school meal participation following the implementation of universal free meals. They found that food insecurity among low-income households with children declined significantly, highlighting the policy's effectiveness in addressing childhood hunger. Additionally, participation in school meal programs increased across all income groups, demonstrating that universal access encourages greater utilization of school nutrition programs. However, the study also noted operational challenges, including increased food service costs and logistical adjustments required for implementation.

Various policy approaches have been explored to combat food insecurity in schools. While the traditional free and reduced-price lunch program targets low-income students, universal free meal programs eliminate income-based barriers and increase overall participation. Direct certification policies, as examined by Kim and Joo (2020), have also played a role in improving program access and reducing administrative burdens.

The literature highlights the complex relationship between food insecurity, school meal programs, and student outcomes. While universal free school meals have mostly demonstrated positive impacts on participation, food security, and academic performance, challenges such as funding and food waste must be addressed. Further research is needed to assess long-term effects and determine the best strategies for scaling these programs nationwide.

Research on food assistance participation reveals significant barriers for low-income households, particularly those experiencing income volatility. Using panel data from the 2008 Survey of Income and Program Participation (SIPP), Zedlewski and Rader (2005) found that households with fluctuating incomes were less likely to enroll in food assistance programs, putting children at greater nutritional risk. Additionally, Todd et al. (2010) reported a decline in food assistance use among the poorest families, potentially due to the weakened connection between Temporary Assistance for Needy Families (TANF) and other food assistance programs. Studies further highlight a troubling decline in Supplemental Nutrition Assistance Program (SNAP) participation among very poor households despite economic downturns, reinforcing the need for targeted outreach efforts (Moffitt & Scholz, 2009). More specifically, Carlisle et al. (2023) argue that while universal free school meal programs are believed to reduce food insecurity and stigma, their actual impact on nutrition remains questionable. In their study, they found a crucial gap between what is provided and what students consume. They also observe other sources of socioeconomic divides that undermine the programs' goal of equity, suggesting that structural and behavioral factors also need to be addressed such as strict eligibility thresholds for means-tested free school meals, and administrative barriers that make applying for benefits difficult, and students' food choices that often favor less nutritious options despite the provision of healthier meals. These findings suggest that policy should focus on increasing accessibility and education around the benefits of nutrition programs.

For this study, the focus will remain on evaluating the effectiveness of universal free school meal policies across different states, comparing academic performance and engagement metrics between states that have implemented the policy and those that have not. By analyzing these variations, I aim to provide a clearer understanding of the policy's effectiveness and inform future discussions on expanding universal school meal programs nationwide. I will compare states that have implemented universal free school meals—California, Colorado, Maine, Massachusetts, Michigan, Minnesota, New Mexico, and Vermont—to states without such policies. In addition, given that these programs were introduced at different times across states, adjustments for heterogeneous timing of policy implementation will be accounted for to provide a more comprehensive assessment of the effects on academic outcomes and student participation.

## 3. Data Description

The analysis utilizes panel data compiled from multiple sources to evaluate the impact of universal free school meal (UFSM) policies on academic performance and student engagement across U.S. states from the 2017–2018 through 2023–2024 school years. The primary dataset consists of school-level test scores obtained from the National Center for Education Statistics (NCES), combined with district-level income data sourced from the U.S. Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program. The panel covers public schools across 41 U.S. states over the 2017–2018 to 2023–2024 academic years, excluding Puerto Rico.

The test score data provide annual information at the grade, subject, and school level, including average scale scores, participation rates, and various school and district characteristics. Several states were excluded due to incomplete or inconsistently reported data: Montana and Maine lacked grade-specific results, while Rhode Island, New Jersey, the District of Columbia, Texas, Vermont, Virginia, New Mexico, and Hawaii were dropped due to missing years (Table 1). In addition, all virtual schools were removed given the nature of the policy being assessed. A notable feature of the data is the absence of test scores for the 2019–2020 academic year across all states, reflecting nationwide disruptions caused by the COVID-19 pandemic.

Moreover, income information was merged at the district level, using annual SAIPE estimates to calculate the percentage of school-age children living in poverty for each district and year. The final dataset includes 2,933,902 observations and 17 variables, covering academic outcomes alongside demographic and socioeconomic indicators. Following data cleaning, the panel is balanced in the sense that all observed units enter and exit the data in the same years, apart from the universally missing 2019–2020 year. After exclusion, not all originally intended treated states remain in the analysis. The final sample compares states that adopted universal free school meal policies, with the final treatment group consisting of California, Colorado, Massachusetts, Michigan, and Minnesota.

To assess the comparability of treated and control groups at baseline, I conduct balance tests on key school-level variables. These include “percent proficiency” (the share of tested students meeting or exceeding grade-level standards across subjects), “participation rate” (the proportion of enrolled students who completed standardized tests), “percentage of students in poverty” (based on SAIPE district-level poverty rates), “charter school status” (a binary indicator for whether the school is charter-affiliated), and “special education school status” (a binary indicator for schools exclusively serving students with special education needs). See below:

*Table 2. Balance Table*

| Variable | Control | Treated | Difference | P-Value | |
|---|---|---|---|---|---|
| N | 2,760,510 | 173,392 | | | |
| Percent Proficiency | 0.447 | 0.391 | -0.055 | 0.0000 | *** |
| | (0.225) | (0.213) | | | |
| Participation Rate | 1.048 | 0.976 | -0.073 | 0.0000 | *** |
| | (2.936) | (0.043) | | | |
| Percentage of Students in Poverty | 16.354 | 14.119 | -2.235 | 0.0000 | *** |
| | (8.710) | (7.612) | | | |
| Charter School | 0.062 | 0.111 | 0.050 | 0.0000 | *** |
| | (0.241) | (0.315) | | | |
| Special Education School | 0.002 | 0.002 | -0.000 | 0.0197 | ** |
| | (0.043) | (0.040) | | | |

Table 2 summarizes differences in student proficiency rates, participation rates, poverty levels, and charter school presence between the two groups prior to treatment. These results inform the specification of the empirical model and the inclusion of covariates to account for observed differences.

## 4. Empirical Strategy

### 4.1. Economic Theory

The provision of universal free school meals can be examined through the lens of several economic theories, particularly human capital theory and behavioral economics. Human capital theory, as formulated by Becker (1964), suggests that investments in education and student well-being enhance productivity and future earnings. Proper nutrition is a critical component of this investment, as it influences cognitive function, concentration, and overall learning capacity (Glewwe, 2005). Universal free meal policies effectively reduce barriers to adequate nutrition, potentially improving student outcomes by fostering better health and higher engagement in school activities. Moreover, by providing UFSM, schools may enhance students' nutritional intake, potentially leading to improved academic outcomes. This aligns with findings from Ruffini (2021), who observed modest improvements in math performance among elementary students following UFSM implementation.

Building upon this foundation, the analytical framework presented by Glewwe (2005) offers a more detailed economic perspective on how policies like UFSM might influence student outcomes. Glewwe's model conceptualizes student academic achievement, or the production of academic skills, through a production function:

$$T_3 = T(H_1, H_2, H_3, PEI_1, PEI_2, PEI_3, \alpha, SC, YS)$$

Where $T_3$ denotes academic skills at school age, $H_1$, $H_2$, $H_3$ represent health and nutrition status across different life stages, $PEI_1$, $PEI_2$, $PEI_3$ are parental education inputs at those stages, $\alpha$ is the child's innate ability, SC captures school quality, and YS represents years of schooling. Within this structure, child health and nutrition serve as direct inputs in the production of academic skills. Thus, improvements in children's health and nutrition, potentially triggered by UFSM, are theorized to have a direct positive effect on educational outcomes.

Glewwe's framework also highlights the importance of indirect pathways operating through parental behavior. Parents make decisions about allocating their resources between children's health and education based on factors such as household income, prices of inputs (e.g., food, school supplies), and their own preferences. UFSM can be understood as a policy intervention that reduces the "price" of a critical health input —nutritious food— to zero for families. According to Glewwe's model, this change in health input prices may trigger a reallocation of household resources. Families could reallocate financial or time savings toward other educational investments, further enhancing academic outcomes. Conversely, they might divert these savings toward unrelated goods and services, or even reduce other education-related investments if they perceive the school as already providing essential inputs. The net effect depends on parental preferences and the extent of reallocation. These findings inform the empirical design of this paper and motivate the attempt to quantify the causal effect of UFSM policies on academic achievement outcomes.

### 4.2. Empirical Theory

This study employs a Difference-in-Differences (DiD) approach with multiple time periods to estimate the causal impact of universal free school meal (UFSM) policies on student performance and engagement. Given the staggered implementation of UFSM across states, we utilize the Callaway and Sant'Anna (2021) DiD method, which accounts for heterogeneous timing in policy adoption and overcomes biases present in traditional two-way fixed effects models (Goodman-Bacon, 2021).

By implementing the Callaway and Sant'Anna (2021) Difference-in-Differences (DiD) framework, this paper aims to provide a more precise estimation of the causal effects of universal free school meal (UFSM) policies, particularly in a context where the policy was implemented at different times across states. This framework is especially useful in addressing potential biases that arise from heterogeneous treatment effects over time and across groups, which is a common challenge when evaluating policy effects in settings with staggered adoption. This approach improves upon traditional TWFE estimators by accounting for the variation in the timing of treatment adoption across different units (states in this case). In standard TWFE models, when treatment adoption happens at different points in time, the treatment effect for each unit is assumed to be constant over time, and all units are treated as if they were exposed to the treatment at the same time. This assumption can lead to biased estimates if the treatment effect varies across units or time periods. In contrast, Callaway and Sant'Anna's method separates the analysis into three distinct steps to ensure more accurate causal inference:

1. **Identification of Policy-Relevant Disaggregated Causal Parameters:** The first step involves disaggregating the causal effects by group and time to identify the treatment effects relevant to each subgroup of states. This is crucial for understanding how UFSM policies impact different states depending on when they adopted the policy. By recognizing the variations in adoption timing and other state-specific factors, this step helps isolate the causal effect of UFSM policies in a way that would not be possible using a single average treatment effect.
2. **Aggregation of These Parameters to Form Summary Measures of Causal Effects:** After identifying the disaggregated causal parameters, the second step aggregates them into summary measures of the overall treatment effect, while still accounting for the heterogeneity in treatment timing and state characteristics. This allows for a more nuanced understanding of the average effect of the UFSM policies across different adoption cohorts. The aggregation ensures that the estimation accounts for both the temporal differences in policy implementation and the differences across units that were

treated at different times (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; de Chaisemartin & d'Haultfœuille, 2020).

3. **Estimation and Inference About These Different Target Parameters:** The final step of the framework involves statistical estimation and inference. This step ensures that the estimates of the causal effects are valid and that the uncertainty around these estimates is correctly accounted for. By separating the effects into different target parameters and considering the varying treatment effects over time and across units, the method reduces the risk of inference errors that might arise if traditional methods like TWFE were applied without considering the staggered treatment adoption (Callaway & Sant'Anna, 2021; Sun & Abraham, 2021; de Chaisemartin & d'Haultfœuille, 2020).

This framework is helpful for this study because it directly addresses several challenges inherent in evaluating UFSM policies. It allows for a more accurate comparison of treated and untreated states by using adoption cohorts rather than assuming a uniform treatment effect. This is particularly important given the heterogeneity in policy design and implementation across states. Finally, by incorporating a robust estimation process, the Callaway and Sant'Anna (2021) framework enhances the reliability and validity of the causal inference made about the effects of UFSM policies on student performance and engagement.

## 5. Methodology

The primary regression specification estimated in this study is given by:

$$Y_{st} = \alpha_s + \lambda_t + \sum_k \beta_k \cdot UFSM_{st,k} + X_{st}\gamma + \varepsilon_{st}$$

Where $Y_{st}$ represents the outcome variable for state s in year $t$. The specification includes state fixed effects $\alpha_s$ to account for time-invariant differences between states, and year fixed effects $\lambda_t$ to control for shocks common across states in a given year, such as national economic trends or the COVID-19 pandemic. The term $\sum_k \beta_k \cdot UFSM_{st,k}$, captures the effects of Universal Free School Meal (UFSM) policy adoption, where $UFSM_{st,k}$ is an indicator for whether state s belongs to cohort $k$ and has implemented the UFSM policy in year $t$. This structure allows for heterogeneous treatment effects across different adoption cohorts. $X_{st}$ is a vector of state-level control variables, including per-pupil education spending, poverty rates, student-teacher ratios, and teaching modality (virtual vs. in-person), with corresponding coefficients $\gamma$. The error term $\varepsilon_{st}$ captures unobserved shocks. The primary outcome variables are the proficiency percentages in math, science, and English language arts, as reported by the National Center for Education Statistics (NCES).

### 5.1. Assumptions and Hypotheses

Following the methodological framework of Callaway and Sant'Anna (2020), this analysis relies on several key assumptions to ensure the validity of the model's findings. The main identifying assumption is conditional parallel trends, which imply that in the absence of UFSM policies, states that implemented the policy would have followed similar trends in student performance and engagement as those that did not, after controlling for observed covariates (Callaway & Sant'Anna, 2021; Angrist & Pischke, 2009). Another important consideration is the no anticipation effect, meaning that states should not demonstrate significant changes in student outcomes before the actual implementation of UFSM. If substantial shifts occur prior to the policy's enactment, it could indicate external factors influencing the results (Sun & Abraham, 2021). Additionally, the model assumes exogeneity of treatment timing, which suggests that the timing of UFSM adoption is unrelated to unobserved shocks that may affect educational outcomes (Goodman-Bacon, 2021). To address any potential violations of this assumption, robustness checks will be conducted to account for possible endogeneity. The model also assumes no spillover effects, implying that the policy's impact is limited to the states that implement UFSM and does not significantly affect neighboring states without the policy (Athey & Imbens, 2006). Lastly, the model implies irreversibility of treatment. This means that once a unit (e.g., a state) adopts the policy, it remains treated in all subsequent periods (e.g., years). In the context of UFSM policies, this implies that once a state implements universal free school meals, it continues to offer them in all following years without reverting back to a non-UFSM status (Callaway & Sant'Anna, 2021). This assumption is crucial for interpreting post-treatment outcomes because it ensures that treatment status is stable over time, allowing for a clean separation between pre-treatment and post-treatment periods in the analysis.

### 5.2. Potential Concerns

When evaluating the impact of UFSM policies, several potential concerns must be addressed to ensure the reliability of the findings. One major issue is omitted variable bias, where differences in state policies beyond UFSM, such as concurrent education reforms, changes in school funding, teacher pay, curriculum adjustments, or teacher turnover rates, could confound the estimates. Another challenge is policy heterogeneity, as differences in program design, including meal quality and outreach efforts, may lead to varying effects across states. Additionally, measurement error poses a risk due to inconsistencies in NCES data reporting, which could introduce noise into the analysis. Finally, attrition and selection bias may arise if changes in student composition, such as migration, enrollment shifts, or states with missing data, affect the results.

## 6. Results

The primary analysis investigates the causal impact of the universal free lunch on academic proficiency outcomes using the Callaway & Sant'Anna (2021) Difference-in-Differences estimator for staggered adoption. I focus on the percentage of students proficient in English Language Arts (ELA), Math, and Science, and report average treatment effects on the treated (ATT) across cohorts and years.

***Table 3: Aggregated ATT Estimates by Subject***

| Subject | ATT Estimate |
|---|---|
| ELA | -0.095***<br>(0.008) |
| Math | -0.086***<br>(0.006) |
| Science | -0.003<br>(0.009) |
| All Subjects | -0.075***<br>(0.006) |

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

***Note:*** *Estimates are based on the Callaway & Sant'Anna (2021) staggered Difference-in-Differences framework using the doubly robust estimator. Each estimate represents the aggregated average treatment effect on the treated (ATT) for schools adopting Universal Free School Meals, separately by subject. Standard errors are clustered at the state level and reported in parentheses.*

Table 3 reports the aggregated ATT estimates by subject using the doubly robust Callaway & Sant'Anna estimator. Across all subjects, the UFSM policy is associated with a significant decline in student proficiency. The estimated ATT for the composite index is −0.075 (SE =

0.006, $p < 0.01$), implying a 7.5 percentage point reduction in overall proficiency for treated schools.

Subject-specific estimates reveal sizable and statistically significant effects in ELA (−0.095, SE = 0.008) and Math (−0.086, SE = 0.006). These declines represent relative reductions of approximately 15–20% from baseline proficiency rates, underscoring the policy's negative academic impact. In contrast, the estimate for Science is small and statistically insignificant (−0.003, SE = 0.009), suggesting that science performance was less affected by the policy, potentially due to differences in instructional time, curriculum pacing, or test administration.

To better understand how treatment effects vary by adoption timing, Table 4 presents cohort-level ATT estimates for the 2020 and 2023 implementation groups. These estimates summarize the average impact on each cohort over all post-treatment periods. See below:

*Table 4: Cohort-Aggregated ATT Estimates by Subject and Group*

| Subject | Group | Group ATT (SE) |
|---|---|---|
| ELA | 2,020 | -0.158***<br>(0.008) |
| ELA | 2,023 | -0.018<br>(0.013) |
| Math | 2,020 | -0.153***<br>(0.009) |
| Math | 2,023 | -0.005<br>(0.008) |
| Science | 2,020 | -0.024*<br>(0.014) |
| Science | 2,023 | 0.022*<br>(0.012) |
| All Subjects | 2,020 | -0.133***<br>(0.007) |
| All Subjects | 2,023 | -0.005<br>(0.010) |

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

***Note:*** *This table reports cohort-specific average treatment effects on the treated (ATT), aggregated across all post-treatment years for each treatment group (i.e., 2020 and 2023 adopters). Estimates are based on the Callaway & Sant'Anna (2021) doubly robust estimator with "not-yet-treated" schools as the control group. Standard errors are clustered by state.*

The 2020 cohort—comprising the earliest adopters—experienced consistently large and negative effects across subjects. For example, the ATT for ELA was −0.158 (SE = 0.008), while Math saw a similarly pronounced decline of −0.153 (SE = 0.009). The Science effect for this cohort was more modest but still statistically significant at −0.024 (SE = 0.014, $p < 0.1$). The composite index shows a −0.133 decline (SE = 0.007) for this group, reinforcing the conclusion that early adopters were most negatively affected.

In contrast, the 2023 cohort experienced trivial effects in ELA and Math (both statistically insignificant), and a small but positive effect in Science (+0.022, SE = 0.012, $p < 0.1$). These differences may reflect improved implementation conditions over time, greater preparedness among later adopters, or recovery from earlier disruptions in the post-pandemic context.

Dynamic group-time ATTs by cohort and calendar year are presented in Table 5. While this table offers detailed year-by-year estimates, it is included in the appendix for completeness and to support replication. The main inferences drawn from these dynamics are captured in the aggregated cohort-level results discussed above.

As a robustness check, Table 6 presents estimates from three alternative model specifications: a conventional DiD without controls, a DiD with covariates, and a DiD with school district and year fixed effects. All three models confirm the direction and magnitude of the main findings. Notably, the DiD with controls estimates a treatment effect of −0.123 (SE = 0.001), while the fixed effects model finds a negative post-treatment effect of −0.010 (SE = 0.002). These results provide additional support for the validity and robustness of the primary specification.

## 7. Discussion

It is essential, however, to interpret these findings in light of the COVID-19 pandemic, which represents a significant and time-varying confounding factor. The 2020 cohort adopted the policy just as the pandemic disrupted schooling nationwide, complicating any causal attribution. School closures, the transition to remote learning, reduced instructional time, and heightened stress among students and staff likely contributed to the sharp drop in proficiency, independent of the policy itself.

The pandemic's impact on student learning was not confined to a single academic year. Research and national assessments suggest that the effects of COVID-19 extended well beyond the 2020–2021 school year, with residual learning loss, disengagement, and uneven recovery trajectories affecting students in subsequent cohorts. These spillover effects are evident in the 2021 and 2022 estimates, which also show significant, albeit smaller, declines in proficiency, even for schools that had not yet adopted the policy. This overlap complicates causal identification, as later treatment effects may coincide with recovery from pandemic-related disruptions, with the actual impact of the universal free lunch policies.

In light of these complexities, the estimated treatment effects should be interpreted with caution. While the analysis provides evidence of negative academic consequences in the immediate aftermath of policy adoption, especially for early adopters such as California, it remains difficult to fully identify the effects of the policy from those of the broader educational crisis triggered by COVID-19. Future research should aim to exploit additional post-pandemic data to better isolate long-run treatment effects and assess the policy's implications in more stable educational environments.

## 8. Limitations

While this study provides insight into the relationship between universal free school meal (UFSM) policies and academic proficiency, several limitations must be acknowledged. Given that the timing of UFSM adoption coincided with major educational disruptions related to the COVID-19 pandemic, the analysis was designed to account for staggered implementation across cohorts and isolate average treatment effects within each group. Nonetheless, several additional limitations remain that could affect the interpretation and generalizability of the findings. These limitations pertain to data availability, model specification, and the level of aggregation used in the analysis and are discussed in detail below.

First, the sample excludes certain states and years due to missing or incomplete proficiency data. These missing values are not random and may reflect systematic differences in reporting practices or data availability. For example, states such as Texas and New Jersey are not represented in the final dataset. Their absence may affect the generalizability of the findings and bias treatment or control groups if excluded states differ meaningfully in baseline outcomes, policy implementation capacity, or demographic composition.

Second, the distribution of treated units across time is uneven, with early treatment cohorts, particularly those in 2020, comprising larger school systems such as California. This asymmetry in sample size could disproportionately influence the average treatment effects, especially in pooled estimates. Although the Callaway & Sant'Anna estimator incorporates appropriate weighting, interpreting pooled results must still account for these underlying distributional differences.

Third, the dataset does not include information on English Language Learner (ELL) status, which limits the ability to control for a potentially important confounding variable. ELL students face unique instructional and linguistic challenges that can affect academic performance independently of UFSM policy exposure. Moreover, ELL populations are often concentrated in specific districts or states that may have different probabilities of adopting UFSM. Without adjusting for ELL composition, the estimated treatment effects may partly reflect underlying differences in student populations rather than the causal effect of the policy.

## 9. Conclusion

This study evaluates the short-term effects of Universal Free School Meal (UFSM) policies on academic proficiency outcomes using a staggered Difference-in-Differences approach. By considering the variation in the timing of policy adoption across states and years, and employing the Callaway and Sant'Anna (2021) estimator, I estimate average treatment effects on the treated (ATT) across English Language Arts (ELA), Math, Science, and an aggregate proficiency index. The results provide consistent evidence that UFSM adoption was associated with statistically significant declines in ELA and Math proficiency, while effects on Science were trivial.

Cohort-level analyses reveal substantial heterogeneity. The 2020 treatment cohort, which comprised California, experienced the most pronounced proficiency declines across all subjects. In contrast, schools that adopted the policy in 2023 exhibited no measurable effects in ELA or Math and a small positive impact in Science. These differences may reflect variation in implementation conditions, policy readiness, or broader contextual factors, such as recovery from earlier disruptions related to the COVID-19 pandemic.

While the analysis controls for staggered adoption and compares effects across treatment cohorts, the broader educational disruptions caused by the pandemic complicate causal attribution. The 2020 policy implementation coincided with national school closures, remote instruction, and reduced instructional time, all of which likely contributed to the observed declines in proficiency. Moreover, pandemic-related learning loss extended into subsequent years, potentially influencing outcomes for later adopters and control schools alike.

The robustness checks presented in this paper, including conventional Difference-in-Differences models with and without covariates, as well as a fixed effects specification, yield results consistent with the primary estimates. These complementary models reinforce the validity of the findings while acknowledging the structural limitations of the available data.

Lastly, the results underscore the importance of evaluating not only the intent of universal policies such as UFSM but also their timing and implementation context. While the goal of reducing food insecurity and promoting equity remains critical, these findings suggest that large-scale policy changes implemented during periods of institutional stress may have complex and uneven effects on student outcomes (Larson et al., 2024). Future research should explore the longer-term impacts of UFSM policies in more stable post-pandemic environments and incorporate disaggregated data, such as English Language Learner status, to improve causal identification and better understand variation in policy effectiveness across student populations.

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## Appendix

***Table 1: Missing State-Year Pairs***

| State | School Year |
|---|---|
| New York | 2017-2018 |
| New York | 2018-19 |
| New York | 2020-21 |
| New Jersey | 2020-21 |
| District of Columbia | 2020-21 |
| New York | 2021-22 |
| New York | 2022-23 |
| New York | 2023-24 |
| Maine | 2023-24 |
| Montana | 2023-24 |
| New Mexico | 2023-24 |
| Hawaii | 2023-24 |

*Table 5: Group-Time ATT Estimates by Subject and Year*

| Subject | Year | ATT Estimate (SE) |
|---|---|---:|
| ELA | ATT(2020,2017) | 0.033***<br>(0.007) |
| ELA | ATT(2020,2018) | 0.013<br>(0.008) |
| ELA | ATT(2020,2020) | -0.323***<br>(0.010) |
| ELA | ATT(2020,2021) | -0.254***<br>(0.007) |
| ELA | ATT(2020,2022) | -0.023**<br>(0.010) |
| ELA | ATT(2020,2023) | -0.034**<br>(0.015) |
| ELA | ATT(2023,2017) | 0.006<br>(0.012) |
| ELA | ATT(2023,2018) | 0.010<br>(0.008) |
| ELA | ATT(2023,2020) | 0.009<br>(0.015) |
| ELA | ATT(2023,2021) | -0.023<br>(0.016) |
| ELA | ATT(2023,2022) | -0.017*<br>(0.009) |
| ELA | ATT(2023,2023) | -0.018<br>(0.013) |
| Math | ATT(2020,2017) | 0.028***<br>(0.009) |
| Math | ATT(2020,2018) | 0.010*<br>(0.005) |
| Math | ATT(2020,2020) | -0.277***<br>(0.012) |

| | | |
|---|---|---|
| Math | ATT(2020,2021) | -0.295***<br>(0.007) |
| Math | ATT(2020,2022) | -0.017*<br>(0.010) |
| Math | ATT(2020,2023) | -0.022*<br>(0.012) |
| Math | ATT(2023,2017) | 0.005<br>(0.009) |
| Math | ATT(2023,2018) | -0.011<br>(0.009) |
| Math | ATT(2023,2020) | 0.012<br>(0.038) |
| Math | ATT(2023,2021) | 0.003<br>(0.020) |
| Math | ATT(2023,2022) | -0.035***<br>(0.009) |
| Math | ATT(2023,2023) | -0.005<br>(0.008) |
| Science | ATT(2020,2017) | NA |
| Science | ATT(2020,2018) | NA |
| Science | ATT(2020,2020) | -0.043***<br>(0.013) |
| Science | ATT(2020,2021) | -0.175***<br>(0.013) |
| Science | ATT(2020,2022) | 0.055***<br>(0.015) |
| Science | ATT(2020,2023) | 0.066***<br>(0.019) |
| Science | ATT(2023,2017) | 0.056<br>(0.063) |
| Science | ATT(2023,2018) | 0.091**<br>(0.036) |

| | | |
|---|---|---|
| Science | ATT(2023,2020) | -0.011<br>(0.029) |
| Science | ATT(2023,2021) | 0.010<br>(0.021) |
| Science | ATT(2023,2022) | -0.007<br>(0.013) |
| Science | ATT(2023,2023) | 0.022*<br>(0.012) |
| All Subjects | ATT(2020,2017) | 0.026***<br>(0.008) |
| All Subjects | ATT(2020,2018) | -0.003<br>(0.008) |
| All Subjects | ATT(2020,2020) | -0.246***<br>(0.009) |
| All Subjects | ATT(2020,2021) | -0.262***<br>(0.006) |
| All Subjects | ATT(2020,2022) | -0.009<br>(0.009) |
| All Subjects | ATT(2020,2023) | -0.014<br>(0.011) |
| All Subjects | ATT(2023,2017) | 0.015*<br>(0.009) |
| All Subjects | ATT(2023,2018) | 0.009<br>(0.010) |
| All Subjects | ATT(2023,2020) | 0.007<br>(0.022) |
| All Subjects | ATT(2023,2021) | -0.008<br>(0.009) |
| All Subjects | ATT(2023,2022) | -0.024***<br>(0.008) |
| All Subjects | ATT(2023,2023) | -0.005<br>(0.009) |

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

***Note:*** *Table shows dynamic ATT(g,t) estimates by cohort and year for each subject, estimated using the doubly robust method of Callaway & Sant'Anna (2021). The ATT(g,t) reflects the average treatment effect for cohort g in period t. Standard errors are clustered at the state level and shown in parentheses.*

*Additional note: These estimates provide descriptive insight into treatment dynamics over time and are included for reference and robustness only*.

***Table 6: DiD and FE Estimates***

| | DiD (No Controls) | DiD (Controls) | DiD (Fixed Effects) |
|---|---|---|---|
| Intercept | 0.445*** | 0.359*** | |
| | (0.000) | (0.003) | |
| Treatment | -0.042*** | -0.123*** | |
| | (0.001) | (0.001) | |
| Post | 0.001 | 0.005** | -0.010*** |
| | (0.001) | (0.002) | (0.002) |
| % Poverty | | -0.014*** | -0.000 |
| | | (0.000) | (0.000) |
| Participation Rate | | 0.351*** | 0.330*** |
| | | (0.003) | (0.018) |
| Charter School | | -0.058*** | |
| | | (0.009) | |
| Special Ed | | -0.106*** | -0.123 |
| | | (0.014) | (0.081) |
| Num. Obs. | 789523 | 273182 | 273182 |
| R2 | 0.004 | 0.306 | 0.590 |
| R2 Within | | | 0.040 |
| F | 1607.755 | 10048.616 | |
| Std.Errors | | | by: school district |
| FE: school district | | | X |
| FE: school year | | | X |

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

***Note:*** *Estimates in this table are based on standard Difference-in-Differences and fixed effects models. The first column uses a simple DiD regression without covariates. The second includes school-level controls (e.g., poverty rate, charter status). The third specification adds school district and year fixed effects. All models estimate the average effect of UFSM on academic proficiency.*