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The Effect of the Community Eligibility Provision on the Ability of Free and Reduced-Price Meal Data to Identify Disadvantaged Students

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Abstract

The Community Eligibility Provision (CEP) is a policy change to the federally-administered National School Lunch Program that allows schools serving low-income populations to classify all students as eligible for free meals, regardless of individual circumstances. This has implications for the use of free and reduced-price meal (FRM) data to proxy for student disadvantage in education research and policy applications, which is a common practice. We document empirically how the CEP has affected the value of FRM eligibility as a proxy for student disadvantage. At the individual student level, we show that there is essentially no effect of the CEP. However, the CEP does meaningfully change the information conveyed by the share of FRM-eligible students in a school. It is this latter measure that is most relevant for policy uses of FRM data.

Note: Portions of this paper were previously circulated under the title "Using Free Meal and Direct Certification Data to Proxy for Student Disadvantage in the Era of the Community Eligibility Provision." We have since split the original paper into two parts. This is the first part.

1. Introduction

The use of free and reduced-price meal (FRM) eligibility as a proxy for student disadvantage is ubiquitous in education research. Moreover, policymakers at the federal, state, and local levels have historically relied on FRM data in their efforts to monitor and regulate educational outcomes and allocate funding.[1] It is common knowledge that FRM-eligibility is a noisy and coarse proxy for student poverty (Bass, 2010; Chingos, 2016; Harwell and LeBeau, 2010; Michelmore and Dynarksi, 2017); but while imperfect, it has been shown to be an effective indicator of disadvantage nonetheless (Domina et al., 2018).

The Community Eligibility Provision (CEP), which is a recent policy change to the National School Lunch Program (NSLP) administered by the United States Department of Agriculture (USDA), gives cause for concern about the continued use of FRM data to identify disadvantaged students. The CEP allows all students in participating schools and districts to receive free meals, regardless of students' individual circumstances. Setting aside the substantive impacts of the CEP on student outcomes, which have been studied elsewhere, our focus is on understanding the effects of the CEP on data quality.[2] The extent to which FRM data can continue to be used to identify high-need students in the post-CEP era is a question of critical importance, as researchers and policymakers have become dependent on using these data in this capacity. Concerns about the data effects of the CEP have been raised in recent policy reports and in the popular press (Camera, 2019; Chingos, 2018; Greenberg, 2018), but to the best of our knowledge we provide the first causal

[1] Most federal funding programs do not rely directly on FRM data, although there are exceptions (such as the e-rate program, which provides support to schools for telecommunications and internet equipment and access). Even so, indirectly, the disbursement of federal funds depends on FRM data in the sense that state and local governments often use FRM information internally to allocate federal support, such as from Title-I (Hoffman, 2012).
[2] Gordon and Ruffini (2018) evaluate the effect of the CEP on students' disciplinary outcomes, and Schwartz and Rothbart (forthcoming) study the effect of a precursor program within the NSLP on academic outcomes. Recent studies on the effects of providing universal meals outside of the NSLP include Dotter (2013) and Altindag et al. (forthcoming).

evidence documenting how the CEP has affected the ability of FRM data to identify disadvantaged students.

Our research design is based on empirical models that predict key student outcomes—test scores and attendance—using FRM data. This approach follows on recent, related work by Domina et al. (2018) and Michelmore and Dynarski (2017). Like in these previous studies, our interest is not in understanding how outcomes compare between FRM and other students in the models *per se*. Rather, it is in how these comparisons change when the CEP is adopted and what is implied by the changes. Evidence that students coded as FRM-eligible gain in the performance distribution relative to their more advantaged peers with the CEP in place, holding all else equal, would imply that the CEP has reduced the ability of FRM data to identify student disadvantage.

Our analysis is based on administrative microdata from Missouri. The CEP was first adopted by schools and districts in Missouri during the 2014-15 school year, and we construct a student data panel from 2011-12 to 2016-17, spanning three pre-CEP and three post-CEP years. Given our research question, in the ideal evaluation scenario we could construct a data experiment during the post-CEP period in which we would know which students in CEP schools would have been coded as ineligible for free or reduced-price meals in the absence of the CEP. With this information, we could estimate models of student outcomes using data that peel back the CEP data conditions, then compare the results to those from models that use the actual CEP data to assess the data effects of the CEP.

Unfortunately, in Missouri and most other states of which we are aware, state administrative microdata on FRM status are inclusive of CEP coding.[3] This means that students who are not FRM eligible based on individual circumstances, but who attend CEP schools, cannot be separately

[3] New York State is the one exception of which we are aware, although there may be others. Many states lose information about individual FRM eligibility when a school adopts the CEP because there is no longer the need locally to collect individual applications for free and reduced price meals and it is costly to do so.

identified, making the ideal research design described in the previous paragraph infeasible. However, our data permit the next best approach, which is to look backward in time and perform the reverse exercise. Specifically, we can identify CEP-adopting schools in Missouri in 2014-15 and later, then use the pre-CEP data from 2011-12 to 2013-14 to compare their actual FRM data to a scenario where we re-code the data *as if the CEP were already in place* during the pre-policy years. By comparing the results from models of student outcomes with and without CEP data coding in place from 2011-12 to 2013-14, holding all else equal, we causally identify the data effects of the CEP.

We focus our analysis on two FRM-based variables and how they are affected by the CEP. First, we examine individual student FRM designations, which are commonly used to control for student disadvantage in education research. The second variable is the share of FRM-eligible students in a school. This variable is sometimes used by researchers to control for schooling context and has historically played an important role in education accountability and finance policies.

For students' individual FRM designations, we find that the CEP has essentially no effect on their informational content. There are two factors that drive this null finding. First, students who experience a change in coding status due to the CEP are not a random sample—they are already a disadvantaged group, as evidenced by their attendance at high-poverty schools. While these students are "miscoded" in a technical sense because of the CEP, the substantive effect of the miscoding is modest. Second, and more importantly, we show that the number of students who experience a FRM status change due to the CEP—even in the extreme hypothetical scenario in which all eligible schools in Missouri adopt the CEP—is small. This result is not widely understood and may seem initially surprising. The explanation lies in the CEP rules, which are such that eligible schools and districts already have high shares of FRM-eligible students—about 80 percent on average. This means that relatively few students switch status when a school adopts the CEP. Note that this is not a Missouri-specific result, but rather it is a product of the rules that govern CEP eligibility nationally, which we elaborate on below.

In contrast, the CEP meaningfully affects the information contained by the share of FRM-eligible students in a school. Specifically, we show that the strong signal of student disadvantage conveyed by a very high FRM-share in the pre-CEP period is obscured substantially with the CEP in place because a set of relatively better off schools are coded with a 1.0 FRM share. This finding is notable because it is the school share of FRM-eligible students that is focal to finance and accountability policies targeted toward low-income students.[4]

Our findings inform contemporary research and policy applications of FRM data. For researchers, the precise nature of the informational degradation in FRM data resulting from the CEP—i.e., its effect on aggregate FRM measures but not individual measures—guides appropriate use of these data. From a policy perspective, the results increase the appeal of finding new measures of disadvantage to aid in the identification of high-need schools. In the discussion section we review available alternatives and efforts in some states to respond to the new data conditions brought on by the CEP. The alternatives that some states are using offer benefits relative to FRM data but also have limitations. Importantly, research has fallen behind policy in this area: states are reacting to the CEP by shifting away from FRM data—some more than others—but the alternatives they are shifting to have not been rigorously evaluated.

2. The Community Eligibility Provision and Missouri Context

2.1 CEP Program Rules

The CEP allows high poverty schools and districts to provide free meals (breakfast and

[4] Similarly, the district FRM share is also used in these types of policies. Our findings for the FRM-eligible school share are similar to findings for the FRM-eligible district share (results omitted for brevity), as analyses at these different levels of aggregation have common properties (especially in Missouri, which is a "small district" state—see below for details).

lunch) to all students without collecting individual household applications.[5] Eligibility for the CEP is based on the Identified Student Percentage (ISP). The ISP is calculated as the number of students who are directly certified for free meal receipt—via participation in other means-tested programs such as the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and the Food Distribution Program on Indian Reservations—divided by total enrollment. Students can also be grouped with the directly certified population (i.e., in the ISP numerator) if they are classified as belonging to a particular disadvantaged group, such as foster, migrant, homeless, or runaway youth.

The income threshold for SNAP, a key program that leads to direct certification, is the same as for free meals under the NSLP, at 130 percent of the poverty line. However, under the NSLP, students from households with incomes above 130 percent of the poverty line can also be eligible for *reduced-price* meals, for which the income threshold is higher, at 185 percent of the poverty line. In education research and policy applications, students eligible for free and reduced-price meals are typically grouped together as "low income" students. This results in a larger population of students identified as FRM-eligible relative to the directly-certified population (Massachusetts Department of Elementary and Secondary Education, 2017).[6]

Conditional on eligibility, schools and districts choose whether to participate in the CEP. Participants are reimbursed for the free meals by the USDA using a kinked formula based on the ISP. The USDA reimburses free meals at a rate of 1.6 times the ISP, with an ISP of at least 40 percent required for baseline CEP eligibility. Once the ISP reaches 62.5, the reimbursement rate plateaus at 100 percent. After a school or district is accepted into the CEP, it can offer free meals

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[5] In fact, groups of schools can adopt the CEP together regardless of district boundaries if they are eligible collectively, but this practice is uncommon.

[6] The income-threshold difference explains part but not all of the gap between FRM and direct-certification rates. This is because schools and districts identify more students as FRM-eligible than would be predicted based purely on income (even in the absence of the CEP). It is beyond the scope of our study to go into this data issue in detail.

and receive reimbursement for four years without the need to re-apply. Our data panel covers the first three years of CEP implementation in Missouri—therefore, schools that we observe implementing the CEP remain covered throughout the timeframe we study.[7]

For portions of our analysis we leverage CEP program rules to identify all CEP-eligible schools, regardless of actual participation. We define eligible schools as those with at least 40 percent of students who are directly certified. We use this approximation of the ISP based on direct certification data because not all schools and districts in Missouri report an ISP value. In 2013-14, the year before any Missouri schools adopted the CEP—and, as a result, the last year in which the full informational value of FRM data is preserved—schools where at least 40 percent of students were directly certified had 79 percent of students coded as FRM-eligible, on average. This basic descriptive statistic previews the finding below that relatively few student change FRM status as a result of the CEP.

2.2 Missouri Context

Prior to the introduction of the CEP, Missouri was a middle-ranked state (25th) in terms of the fraction of students eligible for free and reduced-price meals via the NSLP.[8] Missouri is just below average in terms of CEP coverage (30th in state rankings), with about 13 percent of students in CEP schools.[9] Based on data from 2016-17, Missouri ranked 32nd and 36th among the 50 states in terms of the fraction of CEP-eligible schools and districts participating in the program.[10] Thus, Missouri is slightly below average among states in terms of total participation and participation

[7] This feature of the program means that if an eligible school or district adopts the CEP in year *t* and undergoes a significant compositional change such that three years later much wealthier students attend the school, the students will still be coded as FRM-eligible. Although we cannot rule out individual instances of this, in results omitted for brevity we find no evidence that such changes are happening at a high enough rate to be detectable in our empirical analysis.
[8] During the 2010-11 school year Missouri had 45 percent of students eligible statewide. This is the most recent pre-CEP year for which this information is available in the Digest of Education Statistics (Snyder, de Brey, and Dillow, 2019).
[9] Source: data tabulated in 2019 by the Urban Institute (link: https://www.urban.org/features/measuring-student-poverty-dishing-alternatives-free-and-reduced-price-lunch?).
[10] Source: Food Research & Action Center (2017).

conditional on eligibility, but it is not an outlier (i.e., there are many states with similar participation patterns).[11]

It is well-documented nationally that conditional on eligibility, schools with higher ISPs participate in the CEP at higher rates. This follows from the kinked incentive structure of meal replacement rates documented above. In national data and again focusing on the 2016-17 school year, schools with ISPs in the 40-49, 50-59, and 60+ ranges had CEP participation rates of 20.7, 57.5, and 74.2 percent, respectively (Food Research & Action Center, 2017). Using schools' direct certification shares to proxy for their ISPs, we find that the selection pattern in Missouri is similar, with analogous participation rates within these same bands in 2016-17 of 23.2, 52.2, and 80.0 percent.

It is also important to address the efficacy of districts' direct certification processes in Missouri, given the importance of direct certification data for defining CEP eligibility. One way to measure districts' direct certification processes is simply to count how many districts have a process in place at all. As of the 2014-15 academic year, the first year the CEP was available in Missouri, 96 percent of school districts were directly certifying students, which is slightly above the national average rate of 95 percent (Moore et al., 2016). Another way to measure this is to identify the fraction of school-aged SNAP participants statewide who are directly certified. As of the 2016-17 academic year, 95 percent of these students were directly certified in Missouri, which is again above the national average rate of 92 percent (United States Department of Agriculture, 2018). Our summary assessment of the direct certification processes in Missouri is that they are about average, or slightly above average, along measured dimensions.

[11] The CEP participation rate in Missouri is also closer to the national average when the denominator includes "near eligible schools" as defined in the Food Research & Action Center's CEP database (link: http://frac.org/community-eligibility-database/).

The analysis that follows quantifies the impact of the CEP in Missouri based on actual school and district participation. The results should translate well for the many states with similar take-up rates and selection patterns. We also provide an analysis that circumvents the selection issue entirely by examining a "maximum-effect scenario" in which we hypothetically allow all eligible Missouri schools to participate in the CEP.

3. Data

We use student-level administrative microdata provided by the Missouri Department of Elementary and Secondary Education (DESE) for the analysis. As noted above, our data panel covers a period from 2011-12 through 2016-17, spanning 3 years in both directions from the first year of CEP adoptions in Missouri, 2014-15 (hereafter we refer to school years by the spring year; e.g., 2014-15 as 2015). A separate school-level data file provided by DESE indicates which schools are participating in the CEP in each year from 2015 onward. Data on student direct certification are also available from 2013 onward from DESE—again, we use these data to identify CEP-eligible schools.

The most critical data element is the FRM indicator variable, which is available throughout the data panel. When a school adopts the CEP, all students are coded in the data as eligible for free meals.[12] We follow the standard practice in research and policy applications of combining free and reduced-price meal students into a single group of "FRM-eligible" students. We then assess the implications of the CEP with respect to student disadvantage as conveyed by belonging to this group. We also aggregate FRM data to the school level to assess the effect of the CEP on school-level FRM information. In addition, we briefly expand our framework to identify "free" and

[12] Again, to the best of our knowledge this is the typical way that CEP adoptions are handled in administrative data, and is consistent with concerns that have been raised about how the CEP affects education policies targeted toward high-need students ((Blagg, 2019; Chingos, 2016; Greenberg, 2018; Greenberg, Blagg, and Rainer, 2019). New York State is the one exception of which we are aware, although there may be others.

"reduced-price" meal students separately and assess the implications of the CEP for each data element. Finally, we use data on student race/ethnicity and gender, whether students are English language learners (ELL), and whether students have individualized education programs (IEP), for portions of our analysis.

We evaluate changes to the informational content of FRM data using predictive models of student attendance and achievement in math and English language arts (ELA) in grades 3-8.[13] We define the student attendance rate as the total number of days attended divided by the total number of days enrolled, on a 0-1 scale.[14] All test scores are standardized to have a mean of zero and a variance of one within subject-grade-year cells. We also extend portions of our analysis to examine students in high school grades (9-12).

Figure 1 documents the rollout of the CEP in Missouri during our data panel for schools serving at least one grade in the 3-8 range. The changes over time in CEP implementation are cumulative and shown as (1) the count of schools, (2) the share of schools, and (3) the share of enrollment. The enrollment share is consistently below the share of schools, reflecting the fact that the average CEP-adopting school in Missouri is smaller than the average school statewide. This, in turn, reflects the fact that many eligible schools are in rural areas.

Table 1 provides summary statistics for the student data. Our data include over 1,700 schools with at least some coverage of tested grades and subjects (e.g., K-5, K-8, 6-8, etc.) and more than 1.8 million student-year observations summed over the pre- and post-CEP years of the data panel. On average during the post-CEP portion of the data panel, 11.6 percent of students in grades 3-8 in Missouri attended a CEP school.

[13] We focus on grades 3-8 due to the statewide testing in these grades in math and ELA over the course of our data panel. The attendance models focus on the same grades to ensure that comparisons across the models are not confounded by changes to the sample composition.

[14] For students who are enrolled in more than one school in a year, attendance is calculated across all schools.

4. Methodology

4.1 Individual FRM-eligibility

We first aim to determine how much the CEP has degraded the proxy value of individual FRM-eligibility as an indicator of student disadvantage. We focus on contemporaneous FRM information, which is the information typically used by researchers and policymakers.[15]

First, consider an initial regression of the following form:

$$Y_{igst} = \delta_0 + FRM_{it}\psi_1 + \mathbf{X_{it}}\boldsymbol{\psi_2} + \omega_g + \xi_t + e_{igst} \tag{1}$$

In equation (1), Y_{igst} is the outcome of interest—either a math test score, an ELA test score, or the attendance rate on a 0-1 scale—for student *i* in grade *g* at school *s* in year *t*. FRM_{it} is an indicator equal to one if student *i* is coded as FRM-eligible in year *t*, and $\mathbf{X_{it}}$ is a vector of indicators for the other student characteristics shown in Table 1. Conceptually, the variables in the X-vector are no different than the FRM-eligibility indicator, but we separate out FRM_{it} visually because its coefficient, ψ_1, is focal to our analysis. ω_g, ξ_t, and e_{igst} are grade fixed effects, year fixed effects, and idiosyncratic errors clustered at the school level, respectively.[16]

We use different datasets that reflect different CEP conditions in estimating the model in equation (1), as discussed in Section 4.3 below. Our interest is in how the estimate of ψ_1 changes as CEP coverage increases. It is intuitive that the CEP will reduce the ability of FRM_{it} to identify individually disadvantaged students, and accordingly the estimate of ψ_1 should be biased positively

[15] We acknowledge that this is not the most efficient way to use the data, despite its prevalence, as documented by Michelmore and Dynarski (2017). These authors show that cumulative measures of FRM participation are more effective at identifying student disadvantage. The CEP likely reduces the informational content of cumulative FRM-based measures of disadvantage following the same basic arguments made here, but an investigation of the effect on cumulative FRM measures would benefit from a longer post-CEP data panel than is available for this project.

[16] Because tests are standardized within subject-grade-year cells, the grade and year fixed effects are of no practical importance in the models, but we include them for completeness.

by the CEP. However, the magnitude of the effect is unclear. Moreover, inference from equation (1) is complicated because FRM_{it} is a student-level indicator that categorizes students into one of two exhaustive groups, resulting in an example of Simpson's paradox in a direct comparison of FRM-eligible and -ineligible students (Simpson, 1951).

The inference problem with equation (1) caused by Simpson's paradox is easiest to illustrate if we make two assumptions, both of which are reasonable. First, assume that students whose individual FRM designations change as a result of the CEP are, on average, (a) more advantaged than students who would be coded as FRM-eligible even without the CEP (i.e., always-eligible students), and (b) less advantaged than students who are never coded as FRM eligible regardless of the CEP (i.e., never-eligible students). Second, assume that the characteristic of "advantage" referenced in the preceding sentence is positively related to student achievement and attendance. Under these two assumptions, when the CEP takes effect, the achievement and attendance outcomes for the groups of FRM and non-FRM designated students will both rise, on average. This is because the non-FRM group loses its least advantaged members and the FRM group gains new members who are relatively advantaged within the group. Depending on the shapes of the functions mapping "advantage" to outcomes at different places in the distributions and the weight of the data in each category, the outcome gaps between FRM-eligible and -ineligible students—which are captured conditionally by ψ_1 in equation (1) —could rise, fall, or remain the same when the CEP is implemented.

The fundamental reason for the ambiguity is that there is no reference group of students whose membership is unaffected by the CEP in equation (1). When a student is re-coded as FRM-eligible due to the CEP, the student necessarily leaves the FRM-ineligible group, resulting in changes in composition to both groups. To avoid Simpson's paradox and permit appropriate inference, we require a reference group of students unaffected by the CEP recoding that can be used to benchmark the change in performance of FRM-identified students after the CEP takes effect. We construct such a reference group using students who are themselves FRM-ineligible and who attend low-poverty schools, which we define as schools with FRM shares below 0.25 during the three pre-policy years 2012-2014. None of these schools should be close to eligible for the CEP based on their ISPs—recall that the average pre-policy FRM share of CEP-eligible schools in Missouri is 0.79—and indeed, empirically none are observed adopting the CEP during our data panel.

We build the reference group into equation (1) with the following modification, as shown by equation (1a):

$$Y_{igst} = \tilde{\delta}_0 + FRM_{it}\tilde{\psi}_1 + NFRM_{it}^{75}\tilde{\psi}_2 + \mathbf{X_{it}}\tilde{\boldsymbol{\Psi}}_{\mathbf{3}} + \tilde{\omega}_g + \tilde{\xi}_t + \tilde{e}_{igst} \tag{1a}$$

Equation (1a) largely replicates equation (1) and like terms and coefficients are similarly defined. The difference is that we divide students into three groups based on their individual FRM status and the FRM share of the school attended during the pre-CEP years 2012-14: (1) FRM_{it}, which is equal to one if the student is individually coded as FRM eligible and zero otherwise, (2) $NFRM_{it}^{75}$, which is equal to one if the student is not individually coded as FRM eligible *and* the student attends a school with an FRM share between 0.25-1.0, and (3) $NFRM_{it}^{25}$, which is equal to one if the student is not individually coded as FRM eligible *and* the student attends a school with an FRM share below 0.25. The latter group is the omitted reference group in equation (1a). Thus, $\tilde{\psi}_1$ indicates how FRM-coded students statewide perform compared to the subpopulation of FRM-ineligible students at low-poverty schools (i.e., the group identified by $NFRM_{it}^{25}$), on average. The interpretation of this difference is now straightforward because the students flowing into the FRM pool due to the CEP (i.e., individually-ineligible students at high-poverty schools) are not flowing out of the reference population; instead they are flowing out of the $NFRM_{it}^{75}$ group. If the CEP-induced coding results in a less-disadvantaged pool of FRM-eligible students, we would predict $\tilde{\psi}_1$ to become less negative as more schools adopt the CEP.

4.2 The School FRM Share

Next we estimate a version of equation (1) where the right-hand-side variables are aggregated to the school level:

$$Y_{igst} = \delta_0 + \overline{\mathbf{FRM}}^{\mathbf{b}}_{\mathbf{st}}\boldsymbol{\delta}_{\mathbf{1}} + \overline{\mathbf{X}}_{\mathbf{st}}\boldsymbol{\delta}_{\mathbf{2}} + \lambda_g + \phi_t + \varepsilon_{igst} \tag{2}$$

Equations (1) and (2) differ only in that equation (2) regresses individual student outcomes on school-average student characteristics, rather than student-level characteristics. For ease of exposition, our primary model does not include the FRM share linearly but rather divides schools into bins based on the school-average FRM share. Specifically, $\overline{\mathbf{FRM}}^{\mathbf{b}}_{\mathbf{st}}$ is a vector of indicator variables that identifies the bin, *b*, to which school *s* belongs in year *t*. The bins are as shown in Table 1 and categorize schools with shares of FRM-eligible students of (1) 0.90 or more, (2) 0.75-0.89, (3) 0.50-0.74, (4) 0.25-0.49, and (5) less than 0.25. Table 1 shows summary statistics for each bin in the pre- and post-CEP periods. We omit bin-5 schools in the model as the comparison group. We continue to cluster our standard errors at the school level.

To interpret changes in $\boldsymbol{\delta}_{\mathbf{1}}$ in response to the CEP, note that the CEP increases the value of the continuous variable $\overline{FRM}_{st}$ for some schools with pre-CEP values below 1.0, which are then pushed into bin-1. The schools that shift into bin 1 due to the CEP are less impoverished than other bin-1 schools. Thus, the CEP should attenuate the outcome gap between students who attend bin-1 and other schools (i.e., make it less negative), although the magnitude of the effect is difficult to predict *ex ante*. A similar Simpson's paradox phenomenon may apply to the comparisons of bin-1 to bins 2 and 3 in equation (2), and to a lesser extent bin-4 (because a small number of schools move between bin-1 and bin-4 under the various scenarios we consider below), but schools in bin 5 do not change CEP status and thus comparisons that use bin 5 as the anchor are straightforward.

We make three additional comments about equation (2). First, the bin ranges we use to construct $\overline{\mathbf{FRM}}_{\mathbf{st}}^{\mathbf{b}}$ are more differentiated at the high end of the FRM distribution to better illuminate the effects of the CEP, which primarily pushes schools from bins (2) and (3) into bin (1) based on program rules. A type of impact that is missed by the binned approach occurs for schools with FRM shares at or above 0.90 that adopt the CEP, which are coded as bin-1 throughout. In an extension, we also estimate an analog to equation (2) that enters the FRM school share as a linear, scalar variable to capture the effect of these moves, which we elaborate on below.

Second, we do not aggregate the outcome variables in equation (2)—i.e., we use student-level outcomes as in equations (1) and (1a). This is because our objective is to assess the effect of the CEP on the ability of FRM data to identify disadvantaged students, which is inherently an individual-level prediction problem. The coefficients in equation (2) appropriately indicate the ability of the FRM school share to predict student disadvantage as measured by the outcomes we consider.

Third, we also estimate variants of equation (2) that include the individual-student variables, FRM_{it} and $\mathbf{X}_{\mathbf{it}}$, from above. The student-level variables improve the explanatory power of the models substantially because they are predictive of student outcomes. Moreover, because they are correlated with the school FRM share for individual students, they also reduce the predictive power of the FRM-bin indicators. However, their inclusion does not yield new substantive insights regarding the effects of the CEP on the information contained by either the student- or school-level *FRM* variables.

4.3 CEP Data Conditions and Estimation Issues

We estimate equations (1a) and (2) on multiple datasets structured to reflect different CEP conditions. The first two datasets are the pre-CEP dataset, from 2012-2014, and the post-CEP dataset, from 2015-2017. The pre/post comparison is a natural first step toward assessing the data impacts of the CEP. However, a limitation of the pre/post analysis is that other factors may also be changing over the timespan during which the CEP was adopted by schools and districts in Missouri, confounding causal inference.

We improve our comparisons by using a data-censoring procedure that effectively implements the CEP *data change* prior to the actual policy change. We refer to our data-censoring procedure as "pseudo-coding" the CEP. In the first pseudo-coded scenario, we modify the pre-CEP data for all schools that we observe adopting the CEP during the first year in Missouri (2015). Specifically, we go backward in the data to the period from 2012-2014 and overwrite the FRM data for these schools as if they had adopted the CEP during those years; i.e., all students in these schools are recoded as FRM-eligible. Noting that no school had actually adopted the CEP during the pre-CEP years, these schools are "pseudo-coded" to have adopted the CEP prior to actual adoption.

Using the 2012-2014 data, we re-estimate equations (1a) and (2) twice: once with the real pre-CEP data, where students in schools that adopted the CEP in 2015 are still distinguishable by their individual FRM status, and once with the pseudo-coded data where all students in 2015 CEP schools are coded as FRM eligible. By re-estimating the models on the same exact data for the same exact schools in the same exact years, where the only difference is whether CEP coding rules are implemented, we can directly assess the data consequences of the CEP. Our approach holds all else constant, without ambiguity. This research design is well-suited to support causal inference with regard to the data effects of the CEP and is superior—in the sense that the identifying assumptions are weaker—to a conceptually similar difference-in-differences research design.

We also extend the above-described exercise to two more pronounced scenarios. In the first, we pseudo-code all schools that adopted the CEP within the first three years in Missouri, rather than just the first year, in the 2012-14 data. Based on the slow growth in CEP adoptions after 2015 illustrated by Figure 1, we do not expect this change to have a significant impact on the findings. For the final scenario, we use the fraction of directly certified students to identify a sample of CEP-*eligible* schools, regardless of future adoption decisions, then pseudo-code all of these schools as adopters in the pre-CEP period.[17]

The final scenario makes endogenous adoptions irrelevant because all eligible schools are coded as adopters. In contrast, in the first two pseudo-coded scenarios, the pseudo-coding is inclusive of endogenous uptake of the CEP. Both sets of results are informative. As described above, selection into the CEP conditional on eligibility is negative, and this is a feature of the national program driven by the incentive structure. Correspondingly, results conditional on observed selection have real-world applicability.

The selection-free, full-participation results are also of interest because they give a true upper bound on the data effect of the CEP, for two reasons. The first reason is obvious: when we pseudo-code all CEP-eligible schools, it allows for the highest level of school and student coverage based on program rules. The second, less-obvious reason is that the marginally-included schools in this scenario have relatively fewer high-poverty students, reflecting the fact that conditional on eligibility, schools with lower direct certification rates are less likely to choose to participate. The implication is that in the upper-bound scenario, more students are miscoded as FRM-eligible per school due to the CEP because (relatively) fewer are individually FRM-eligible. The significance of each individual miscoding is also accentuated at the marginally included schools because students who attend schools with lower direct certification rates will be less disadvantaged, on average.

Finally, recall from above that CEP adoptions can occur at the district or school level. For districts, each individual school does not need to be eligible for the CEP as long as the district is

[17] We perform the eligibility calculation using the 2016 data, which is the middle year of the post-CEP portion of the data panel.

eligible collectively.[18] The first two pseudo-coded scenarios capture real adoption decisions in Missouri and reflect the composition of district and school adoptions as it exists in practice. The third pseudo-coded scenario is based on identifying eligible *schools* to get the upper-bound effect; below we show that our results from this scenario do not differ substantively if we pseudo-code the data based on district-level eligibility instead.

5. Results

5.1 Individual FRM-eligibility

Table 2 shows results from the math-achievement version of equation (1a). The column headers indicate the different datasets used to estimate the equation, which reflect different CEP conditions. For each dataset, models with and without the *X*-vector controls are estimated. The first set of results in columns (1) and (2) use the real pre-CEP data and serve as the benchmark by which the effects of the CEP are assessed in later columns. The results for ELA achievement and attendance are substantively similar to the math results in terms of the implications of the CEP. For brevity and ease of presentation, we relegate them to the appendix (Appendix Tables A.1 and A.2).

In addition to showing the regression results, Table 2 also shows how the FRM-eligible share of students in Missouri evolves under the different CEP conditions. The first two conditions in the table, for the pre- and post-CEP years in columns (1)-(4), show that the percent of CEP-adopting schools grew from 0 in the pre-CEP period (by definition) to an average of 15.1 percent during the post-CEP period. But this increase in CEP-adopting schools corresponds to a much smaller increase in the share of Missouri students coded as FRM-eligible—just 1.7 percentage points. As noted above, there are two reasons for the small increase: (1) CEP-adopting schools typically have a small fraction of non-FRM-eligible students (those affected by the data change) owing to program rules

[18] We again note that groups of schools can adopt the CEP together regardless of district boundaries if they are eligible collectively, but this is uncommon in practice.

and (2) the average CEP-adopting school is smaller than the average school in Missouri. The first issue is the most important driver of the small increase in FRM coverage attributable to the CEP.

Using the pseudo-coded pre-CEP data, the first scenario in columns (5) and (6) also shows an increase in the FRM-eligible student share of 1.7 percentage points, to 52.9 percent.[19] The second scenario, in which we pseudo-code schools that adopted the CEP by the end of our data panel, only marginally increases the shares of CEP schools and FRM-coded students (to 16.4 and 53.5 percent, respectively), as predicted based on Figure 1. In columns (9) and (10), we pseudo-code all CEP-eligible schools as CEP adopters. While we calculate that just over 30 percent of Missouri schools are CEP eligible, even at this upper bound, the hypothetical effect of the CEP on the share of FRM-coded students in Missouri is modest. It rises just 5.3 percentage points to 56.5 percent. Columns (11) and (12) explore a variant of the upper-bound scenario presented in columns (9) and (10), which we will return to later.

Turning to the regression results in columns (1)-(10), we report estimates from equation (1a) of $\tilde{\psi}_1$ and $\tilde{\psi}_2$, which convey average outcomes relative to the holdout group of non-FRM students who attend low-poverty schools. We focus our discussion on the parameter of interest, $\tilde{\psi}_1$. The estimates of $\tilde{\psi}_1$ show that the CEP has essentially no effect on the ability of FRM-eligibility to identify disadvantaged students as measured by test performance. For example, in the first pseudo-coded scenario, comparing the results in column (1) to column (5) shows that the value of $\tilde{\psi}_1$ hardly changes with the CEP coding in place—it changes trivially from –0.791 to –0.782 student standard deviations. This implies a very small, inconsequential gain in performance for the FRM-coded population relative to the reference population due to the CEP data effect. The estimates of $\tilde{\psi}_1$

[19] The match with the pre/post comparison is coincidental, likely reflecting a combination of there being more CEP schools on average in the full post-CEP period, offset by improving economic conditions statewide over time from the pre- to post-CEP years (which affects the statewide FRM eligibility rate).

decline substantially in columns (2) and (6), relative to columns (1) and (5), because the X-vector of other student controls removes the influence of these controls over the parameter of interest. However, comparing columns (2) and (6) shows that the impact of the CEP remains negligible in the conditional models.

The results in columns (7) and (8), using the second pseudo-coded scenario, are similar to the first. Moreover, even in columns (9) and (10), where we pseudo-code the upper-bound CEP condition, there is only a very small effect of the CEP on the value of $\tilde{\psi}_1$. It declines trivially by 0.03-0.04 student standard deviations on a base of 0.60-0.80 standard deviations, depending on whether we use the sparse or full model. All of these results point toward the conclusion that the CEP does not meaningfully affect the ability of the FRM-eligibility indicator to identify disadvantaged students at the individual level.

Given the limited effect of the CEP documented in columns (1)-(10), the purpose of columns (11) and (12) in Table 2 is to disentangle the two previously-mentioned mechanisms that dull the CEP effect. The first mechanism is that the CEP changes FRM status for students who already attend high-poverty schools, reducing the substantive importance of miscoded FRM values. The second is that a relatively small number of students experience a status change as a result of the CEP.

The results in columns (11) and (12) are from a modified version of the upper-bound scenario in columns (9) and (10). The bottom rows of the table show that we hold the number of schools and students affected by CEP fixed at the same levels as in columns (9) and (10) (i.e., 5.3 percent of students and 30.7 percent of schools). However, instead of pseudo-coding eligible CEP schools based on their direct certification shares as in columns (9) and (10), in columns (11) and (12) we randomly pseudo-code schools as CEP adopters. Thus, the number of miscoded students is held constant, but the students who experience an FRM status change in columns (11) and (12) are no longer concentrated in high-poverty schools.

The estimates of $\tilde{\psi}_1$ in columns (11) and (12) are somewhat less negative than in columns (9) and (10), as expected, but they change very little substantively. Put another way, holding the scope of the data change constant in terms of the number of students impacted, even when we pseudo-code a much more advantaged student population as FRM eligible, our estimates of $\tilde{\psi}_1$ are essentially unaffected. This indicates that the primary driver of our null results in Table 2 is the small number of students who experience an FRM status change due to the CEP.

5.2 The School FRM Share

Table 3 follows the structure of Table 2 but shows output from equation (2). Again, we show results for math achievement in the main text and relegate the findings for ELA achievement and attendance to the appendix because of their similarity (Appendix Tables A.3 and A.4). The data scenarios are the same as in Table 2. Recall that schools are binned by the school-level FRM share in each year, with low-poverty schools (FRM share below 0.25) serving as the comparison group for the other groups of schools.

Unlike in the student-level models, there is a clear attenuating effect of the CEP in Table 3. In both the sparse models (without $\overline{\mathbf{X}}_{\mathbf{st}}$) and the full models, the coefficient on the bin-1 indicator consistently declines as the influence of the CEP increases. Comparing columns (1) and (2) to the upper-bound scenario in columns (9) and (10) reveals a sharp change. The magnitude of the bin-1 to bin-5 gap—which compares schools coded with FRM shares at or above 0.90 to schools with FRM shares below 0.25—falls by about 0.40 student standard deviations. This is a very large change and confirms that the informational content of a high school-level FRM share, with respect to the level of disadvantage of the student population, is clearly degraded by the CEP. We emphasize that the effects documented in Table 3 occur without any true changes in the world—they are driven entirely by whether we code FRM status using the CEP rules, holding everything else constant. This assures that the effects reflect the causal impacts of the CEP on the data.

Columns (11) and (12) show results from the random-assignment analog to columns (9) and (10) for equation (2). Mirroring the generally greater impact of the CEP in the school-aggregated models in Table 3, the effect of randomly assigning CEP eligibility is also somewhat larger. Thus, a takeaway from Table 3, which is present but much less visible in Table 2 due to the small overall changes in the effect sizes, is that the students who are miscoded as FRM-eligible due to the CEP are already disadvantaged in a meaningful way. Intuitively, this aspect of the CEP reduces the loss of information relative to the case where the miscoded students are from randomly-selected schools.

6. Sensitivity Analysis and Extensions

6.1 Sensitivity Analysis

We examine the sensitivity of our findings along two dimensions. First, in equation (2) we replace the binned variable vector, $\overline{\mathbf{FRM}}_{\mathbf{st}}^{\mathbf{b}}$, with a continuous FRM school share variable, $\overline{FRM}_{st}$. An appealing feature of this extension is that the continuous variable captures some changes in schools' FRM shares missed by the binned variables. Namely, if a school adopts the CEP and moves from a 0.90-0.99 FRM school share to a 1.0 FRM school share, this variation will not be captured by the binned variables but is captured by the continuous variable.

The results from the continuous-variable version of equation (2) are reported in Table 4.[20] Like the results from the binned model, they point to a clear decline in the ability of the FRM school share to identify schools serving more disadvantaged student populations. The magnitudes of the coefficient changes are difficult to compare across models, but the changes between columns (1)/(2)

[20] In Table 4 and all subsequent tables, we omit the random-assignment scenario shown in columns (11) and (12) of Tables 2 and 3. This scenario is useful for assessing the relative importance of the mechanisms that drive our findings in Tables 2 and 3 but is of little value otherwise as it does not correspond to a realistic policy.

and (9)/(10) in Table 4 are large. For example, without the CEP in place in column (2), a 50 percentage point increase in the FRM school share is associated with a lower math score of 0.479 student standard deviations, whereas in the upper-bound scenario in column (10) (pseudo-coding scenario 3), this same change corresponds to a lower math score of just 0.306 student standard deviations. This result substantively mirrors the CEP data effects documented in Table 3 using the binned model.[21]

Second, we estimate models that include the individual-student and school-aggregated student characteristics simultaneously. We estimate two versions of a combined model: (a) a model where we add the student-level characteristics to equation (2) as shown and (b) a model where we add them to the version of equation (2) that enters the FRM school share linearly. The results from the former are shown in Table 5, and the results for the latter are relegated to the appendix (Appendix Table A.5). Although the simultaneous inclusion of student- and school-level FRM information reduces the predictive impact of each data element individually in all models, the effect patterns of the CEP are similar to what we show above and reveal no new substantive insights. Specifically, given that the CEP has such a small, inconsequential effect on the information contained by the individual FRM indicator, the combined models primarily re-emphasize the point that the effect of the CEP is embodied in the FRM school share variable.[22]

6.2 Extensions

6.2.1 Free Versus Reduced Price Meals

Next we model the data effects of the CEP on separate "free meal" (FM) and "reduced-price meal" (RM) variables. Thus far, we have used the combined FRM variable to capture membership in

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[21] Like with our findings in Tables 2 and 3, our findings from the linear-FRM version of equation (2) are similar if we use ELA achievement or student attendance as the focal student outcomes (results omitted for brevity).

[22] In results omitted for brevity we estimate additional models that also include the original reference population set-up from equation (1a) for the individual FRM controls. The reference population is largely (but not exactly) redundant in the model that includes the FRM-share bins, but less so in the linear-FRM-share model. Nonetheless, whether we include this set up in the models has no bearing on our findings substantively because of the small effect of the CEP on the individual-student FRM indicator.

either group because the FRM variable is most policy relevant. The results in this section are meant to provide additional context.

The CEP converts all students in participating schools to FM-eligible. Thus, some students who were coded as RM-eligible at these schools are converted to FM-eligible (although the number of students impacted by this change is relatively small – see Table 1), in addition to previously FM *and* RM ineligible students being reclassified as FM-eligible.[23] At the individual level, we assess the data effects of the CEP on these variables by entering them separately into a model that otherwise matches the structure of equation (1a)—that is, we use equation (1a) but disaggregate the FRM indicator into separate FM and RM indicators. These results are shown in Table 6. At the school level we perform a similar disaggregation, shown in Table 7. We use the version of the school-aggregated model that enters the FM and RM school share variables linearly (as in Table 4), rather than as binned vectors, for presentational convenience.[24] In both tables we report results from models of math achievement.

Table 6 shows that RM students significantly outperform FM students in the pre-CEP period in math, by about 0.33 student standard deviations in column (1) and 0.21 standard deviations in the model with other controls (column (2)).[25] In terms of the effect of the CEP, the finding from Table 2 that the CEP has a very limited effect on the individual data carries over to Table 6 for both the individual FM and RM controls. This is easiest to see by comparing the baseline pre-CEP results in columns (1) and (2) to results from the upper bound CEP-adoption scenario in

[23] Only about 7-8 percent of all students, or 13-15 percent of FRM students, are RM students. It is not clear if this reflects the true income distribution or other factors. Domina et al. (2018) show that the mapping between income and FRM eligibility is not particularly strong, and it may be that some schools and districts generously award free meals.
[24] The translation of the binned model for this extension is complicated because it is not obvious how to set the bin ranges for the FM and RM variables separately and there will be a lot of overlap, clouding inference.
[25] The gap estimated in column (1) is somewhat larger than the FM/RM gap of 0.19 student standard deviations estimated by Domina et al. (2018) using data from a California school district. However, when we match their specification by adding school fixed effects to the model in column (1), the gap falls to 0.24 standard deviations, which is much closer to their estimate.

columns (9) and (10). The changes in the estimates of $\tilde{\psi}_1^{FM}$ and $\tilde{\psi}_1^{RM}$ across these scenarios are modest—in the full specification, the coefficients decrease by 0.047 and 0.013, respectively.

The results in Table 7 are more difficult to interpret. The trend in the coefficient on the linear free-meal share is similar to what we show for the linear FRM share in Table 4. The coefficient on the reduced-price meal share, in contrast, becomes more negative as the CEP takes stronger hold over the data. One reason is that the model is shifting weight that was falling on the free-meal share to the reduced-price-meal share as the information conveyed by the free-meal share becomes less informative. Interpreting the changes to the reduced-price-meal coefficient also comes with two other caveats: it is estimated much less precisely than the free-meal share coefficient, and its magnitude is misleading because a move from 0 to 1.0 in the RM share variable is much larger in the distribution than a 0-1.0 move in the FM share variable (per Table 1).

We conclude that no new, substantive insights emerge about the data effects of the CEP from the models that split free-meal and reduced-meal students.

6.2.2 District-Level CEP Adoptions

The upper bound condition in pseudo-coded scenario 3 is based on the CEP eligibility of individual schools. In this section we assess the sensitivity of our findings to reconstructing the upper-bound scenario to be based on district-level eligibility; i.e., rather than coding all eligible schools as CEP adopters, we code all eligible districts as CEP adopters. If a district is eligible, all schools in the district are coded as adopting the CEP regardless of individual eligibility (following CEP program rules). Allowing for district-level adoptions potentially increases the extent to which the CEP will degrade FRM information because within-district heterogeneity in income across schools could allow for some students who attend relatively wealthy schools (in generally high poverty districts) to change coded status.

We report the results from this exercise in Table 8, which are comparable to what we show under pseudo-coded scenario 3 in columns (9) and (10) of Tables 2 and 3. The comparison shows that our findings are similar regardless of whether we use district- or school-level eligibility to construct the upper-bound scenario. A caveat is that Missouri has a high ratio of districts to schools (i.e., Missouri is a "small district" state), and the lack of sensitivity of our findings may not generalize to states with large districts (e.g., Florida, Maryland). That said, we note that the results in columns (11) and (12) of Tables 2 and 3—where we randomly assign schools to CEP adoptions—will more than bound the effect of any additional heterogeneity among CEP schools owing to district-level adoptions, even in large-district states.

6.2.3 High Schools

Next we extend the analysis to high school students using two outcomes—attendance and the English II end-of-course (EOC) test score. The attendance models include students in grades 9-12. The English II EOC models include students in the year they take the test, which for most students (about 90 percent) is grade-10.[26]

One reason that high schools merit separate attention is that high school students may be less likely to apply for free or reduced price meals. The mechanism argued in the popular press is that high school students are more sensitive to the social stigma associated with participation (Pogash, 2008; Sweeney, 2018). The implication is that the CEP may generate larger changes in coded FRM eligibility among the high school population.

To explore this possibility, we use the direct certification data from DESE to see if high school students are less likely to enroll in the NSLP conditional on the circumstances of their families. If they are, the translation between the direct certification share and the FRM share, prior to the CEP, should be weaker among high school students than students in lower grades. But this is

[26] We focus on the English II EOC because it is the EOC with the greatest coverage in high schools in Missouri.

not the case. As noted previously, in schools covering grades 3-8, we find that those with at least 40 percent directly certified students had an FRM share of 79 percent, on average, in 2014. Among Missouri high schools, the analogous FRM number is nearly the same—78 percent. Although social stigma has been shown to affect whether students actually *receive* their free meals when eligible (Schwartz and Rothbart, forthcoming); in terms of data on FRM eligibility, there is no indication of underreporting among high school students in Missouri when benchmarked against direct certification data.[27]

Noting this similarity across schooling levels, our investigation of high schools does uncover two notable contextual differences in the higher grades. First, a smaller fraction of high school students in total are FRM-eligible. Using data from the pre-CEP period, just 43.0 percent of students in Missouri high schools are FRM eligible (see Appendix Table A.8), compared to 51.2 percent of students in lower grades (per Table 2). A possible explanation for this result—conditional on the finding above that the mapping between direct certification and FRM status is similar in high school—is that families' circumstances improve as their children age.

The second distinguishing feature of the high school sample, which is related to the first, is that many fewer high schools are eligible for and adopt the CEP. This suggests a smaller scope for the CEP to affect the data. We calculate that only 15.2 percent of Missouri high schools are CEP-eligible based on their direct certification shares, compared to 30.7 percent of schools covering grades 3-8 (as in Table 2). This is because the distribution of the direct certification share among high schools has a lower mean, and a lower variance, than the distribution among schools serving lower grades. The lower mean reflects the point made above that high school students' families are

[27] The Schwartz and Rothbart (forthcoming) study is of middle school students in New York City. Per above, there are reasons to believe the social stigma effect of receiving free or reduced-price becomes more pronounced as students age, but our data cannot speak to this directly.

not as impoverished; the lower variance is intuitive given that high schools pool students from multiple lower-grade schools, shrinking the building-level variance of student characteristics.

Findings from our analysis of high schools are reported in Appendix Tables A.6, A.7, A.8, and A.9. Tables A.6 and A.7 show results using the English II EOC as the outcome, and Tables A.8 and A.9 show results for student attendance.[28] The tables are structured following Tables 2 and 3 in the main text. The general insights from our analysis of grades 3-8 carry over to the high school analysis. Specifically, the CEP has no substantive effect on the information contained by the individual FRM indicator regardless of which outcome we assess (Tables A.6 and A.8). It also meaningfully reduces the informational content of the school FRM share as measured by test scores (Tables A.7) but not as measured by attendance (Table A.9). In addition to the non-conforming finding for high school attendance, the pattern of estimates as CEP conditions strengthen is generally weaker in the high school analysis, suggesting more moderate CEP impacts on the information conveyed by FRM data. This is consistent with the scope of the CEP being smaller in the high school sample.[29]

7. Discussion

7.1 What have we learned?

Our analysis makes three main contributions to inform our understanding of the data effects of the CEP. First, we show that the effect of the CEP on the number of students identified as FRM-

[28] To ensure comparability across the analyses of the two high school outcomes, we restrict the sample of high schools in the attendance models to schools for which English II EOC scores are available during the pre-policy period (2012-14). This prevents changes to the composition of the school sample from driving differences in our findings across outcomes. This restriction results in us dropping a small number of non-standard schools from the attendance sample.
[29] Relatedly, the compression of the distribution of student disadvantage in high schools leads to a situation where FRM-share bins 1 and 2 are relatively sparsely populated, which causes some volatility in the estimates from the high-school analog to equation (2). If our primary goal was to investigate high schools, a different modeling structure (and/or binning structure) might be appropriate. We do not delve too deeply into this issue because the high school results are supplementary to our main analysis. However, we quickly explore the issue of specification sensitivity by also estimating linear FRM-share models using the high school data. The results from these models (omitted for brevity) are substantively similar to what we show for the linear FRM-share models in grades 3-8 in Table 4.

eligible in Missouri is modest. The primary reason is that schools with an ISP above 40, which is the minimum level for CEP-eligibility, already have many FRM-eligible students. Specifically, we estimate that 79 percent of students in these schools are FRM-eligible in the absence of the CEP, on average.[30] A 40-percent ISP corresponds to a much larger FRM-eligible share owing to the more stringent income threshold that primarily drives direct certification. This result should generalize broadly because it is driven by the mapping between the ISP and FRM-eligibility rate.

The limited impact of the CEP on the number of FRM-eligible students is not widely understood. In some instances the impact is directly misstated (e.g., Camera, 2019). A more common mistake is to imply that the entire student body at a CEP school gains access to free meals due to the CEP, without accounting for the substantial population of students who would receive free or reduced-price meals—but mostly free meals, per Table 1—even in its absence (e.g., see Neuberger et al., 2015 and Food Research & Action Center, 2017, 2019).

Our second contribution, which follows from the first, is to show that the effect of the CEP on the informational content of individual-student FRM status in state data is modest. We show that this is primarily driven by the small fraction of students who experience a status change because of the CEP. This result has direct implications for the use of individual FRM status to proxy for student disadvantage, which has been a widespread practice in research to date: if individual FRM status was a suitable proxy for disadvantage prior to the CEP (as suggested by Domina et al., 2018), there is no indication from our analysis that this has changed with the CEP in place.

We expect this result to generalize to other states with CEP take-up rates similar to Missouri. Moreover, the modest effect of the CEP in the upper-bound scenario in which all eligible schools—about 30 percent of schools in Missouri—hypothetically adopt the CEP further suggests this result

[30] Moreover, among eligible schools that choose to adopt the CEP, the FRM-eligible share is even higher because of the stronger participation incentives for more disadvantaged schools. The FRM-eligible share in 2014 of schools that we observe adopting the CEP in Missouri between 2015-2017 is 0.831, on average.

will generalize to most states. A caveat is that in states with very high CEP eligibility and take-up rates, the number of students who experience a status shift could be larger than even our upper-bound scenario, and our results may not generalize in these cases (as of 2019, the Urban Institute reports that 8 of the 50 states had a CEP school participation rate above 30 percent: DE, IL, KY, LA, NM, NY, TN, WV).[31]

Our third contribution is to quantify the degree of informational degradation of the school FRM share as a proxy for disadvantaged circumstances caused by the CEP. The information loss in this variable has implications for both researchers and policymakers. For researchers, the concern is that the school FRM share is a less useful proxy for contextual disadvantage in the post-CEP era. The fact that our analysis isolates this variable as the problem is instructive for developing an appropriate research response.[32] For policymakers our findings are of more immediate concern. With the CEP in place, resource and accountability policies based on FRM data—which have been ubiquitous in recent history—will not be as well-targeted toward disadvantaged students. To maintain or improve targeting moving forward, states will need to turn to other data sources to replace or augment FRM information in the post-CEP era.

7.2 The Policy Challenge and Next Steps for Research

A number of recent articles, policy reports, and government reports document the variety of ways that policymakers are responding to the new data environment in the post-CEP era (Blagg, 2019; Chingos, 2016; Gindling et al., 2018; Greenberg, 2018; Greenberg, Blagg, and Rainer, 2019; Grich, 2019; Massachusetts Department of Elementary and Secondary Education, 2017). Some

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[31] Data retrieved 12.30.2019 at: https://www.urban.org/features/measuring-student-poverty-dishing-alternatives-free-and-reduced-price-lunch?

[32] For example, a simple suggestion is to add an indicator variable to regression models for whether the school adopted the CEP. This will force identification of the coefficient on the school-average FRM share to rely on variation provided only from non-CEP schools. In results omitted for brevity we show that in Missouri, adding such an indicator meaningfully improves model performance. This approach could be combined with other improvements—e.g., incorporating external data to measure local-area disadvantage from a source like the U.S. Census—to further mitigate the adverse data effects of the CEP. An in-depth analysis of the range of potential solutions, and their efficacy, is beyond the scope of the current paper but should be pursued in future research.

states have made few changes and continue to rely on FRM data, while others continue to use FRM data but augment these data with other data sources. A growing number of states no longer use FRM data to identify student disadvantage at all, having entirely substituted into other metrics.

Unfortunately, states have little in the way of comprehensive research evidence to guide their responses to CEP data conditions. The most commonly-advocated alternative source for identifying student disadvantage is direct certification data, which are already being used in some states (Greenberg, Blagg, and Rainer, 2019). Direct certification data offer several advantages over post-CEP FRM data. Most notably, uncensored building-level values are accessible, and these data are cheaper and easier to collect because districts and states can essentially plug into data collected by other agencies (Grich, 2019).

However, direct certification data also have limitations. A basic concern is that the simple statistics used in state funding formulas, like the number of disadvantaged students, are affected by switching to direct certification data because of the more stringent poverty threshold.[33] There are also more substantive issues with using direct certification to identify disadvantaged students, such as the systematic undercounting of student populations that are less likely to participate in the social safety net programs that lead to direct certification, namely Hispanic students and undocumented immigrants (Massachusetts Department of Elementary and Secondary Education, 2017; Zedlewski and Martinez-Schiferl, 2010). Schools and districts in states with large Hispanic and immigrant populations have the potential for measured poverty to shift markedly in a transition from FRM-based to direct-certification-based metrics (Greenberg, Blagg, and Rainer, 2019; Massachusetts Department of Elementary and Secondary Education, 2017).

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[33] For example, in Missouri during the pre-CEP period, the average FRM school share was 0.51, whereas the direct certification share was just 0.28. Funding formulas based on counts of disadvantaged students must be adjusted to reflect the fact that the number of directly certified students corresponds to a larger number of disadvantaged students as measured by (pre-CEP) FRM. States moving to direct certification-based metrics have addressed this concern by multiplying the direct certification share by a constant above 1.0 (the value of which varies across states and has been subject to debate—e.g., Grich, 2019) to better align schools' percentages with their pre-CEP FRM percentages.

States and school districts are also considering other data sources to identify student disadvantage, sometimes in response to the limitations of direct certification data. One example is Medicaid data (Gindling et al., 2018; Greenberg, Blagg, and Rainer, 2019). States are also using national surveys, like the American Community Survey, to construct measures of district-level poverty (Greenberg, Blagg, and Rainer, 2019).

These different data sources all come with tradeoffs, some that are obvious and well-understood—like the undercounting of Hispanic students in direct certification data—and others that are less obvious and yet to be uncovered. There is an opportunity for researchers to contribute information to help policymakers during this time of uncertainty by rigorously evaluating alternative options for identifying high-need students. The changes states are currently pursuing, which we've summarized briefly above, all have conceptual merit. What is lacking is a comprehensive investigation of the costs and benefits of different approaches.

The next step for our work in Missouri is to adapt our analytic framework to compare the ability of different data sources, and combinations of data sources, to identify high-need students. Given the breadth of changes states are considering, and their potential implications for education finance and accountability policies nationwide, an expansive set of studies by a broad group of researchers to inform these changes would be desirable. The CEP has served as a shock to the long-standing data practices used in education to identify student disadvantage, breaking inertia in a way that would have been difficult to predict a decade ago. After the current period of change—the length of which is uncertain—it is likely that we will settle into a new inertial state in terms of how we use data to identify disadvantaged students. Research efforts that improve the next set of conditions into which policy settles can have far-reaching and long-lasting benefits.

8. Conclusion

Setting aside the substantive impacts of the CEP on student outcomes, there has been much consternation over how it affects the use of FRM data to identify student disadvantage in education research and policy applications. To the best of our knowledge, we present the first comprehensive analysis designed to explore this issue empirically. Our findings are mixed. While the CEP has essentially no effect on the level of disadvantage conveyed by individual FRM-eligibility, it does degrade the quality of information conveyed by the FRM-eligible share in a school. The implications of these results depend on the context in which FRM data are used.

We conclude with a brief note about the generalizability of our findings to other states. As indicated above, the first-order issues pertaining to generalizability are CEP eligibility and take-up rates. In states where eligibility and take-up rates are similar to Missouri, it seems likely that our substantive findings will generalize given the structure of the CEP program. As of 2019, the Urban Institute reports that just eight states had more than 30 percent of schools participating in the CEP, which is the participation rate in our (hypothetical) upper-bound evaluation scenario in Missouri.

The other contextual factor that may influence the generalizability of our findings is the education governance structure in a state. In Missouri, we find no substantive differences in the upper-bound effect of the CEP regardless of whether school- or district-level adoptions are considered. But this could be in part due to the "small district" structure in Missouri and may be less applicable to "large district" states. Noting this caveat, our analysis of the hypothetical case where schools are randomly assigned to CEP status, which surely generates more substantive miscodings in FRM-eligibility than would occur even in large districts that are CEP-eligible overall, should bound the effect of any additional heterogeneity introduced by large-district adoptions. In states where the generalizability of our findings is in question and pre-CEP data are available, our analytic approach provides researchers with a blueprint for assessing the implications of the CEP given their own local conditions.

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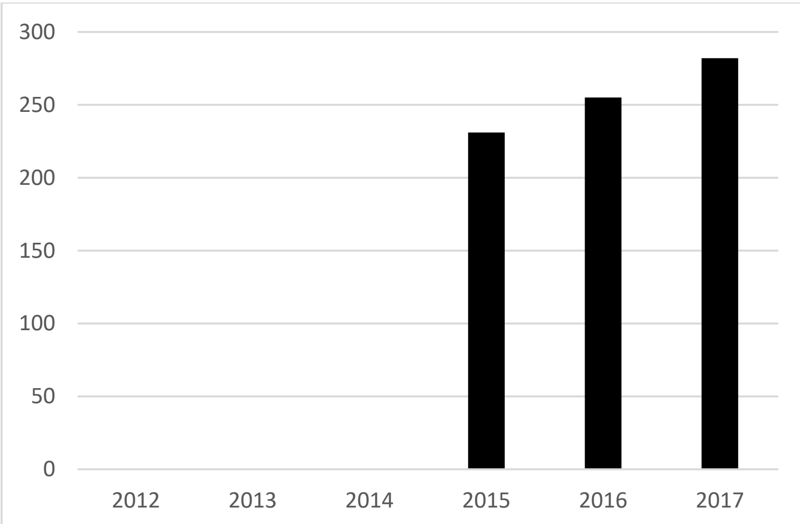
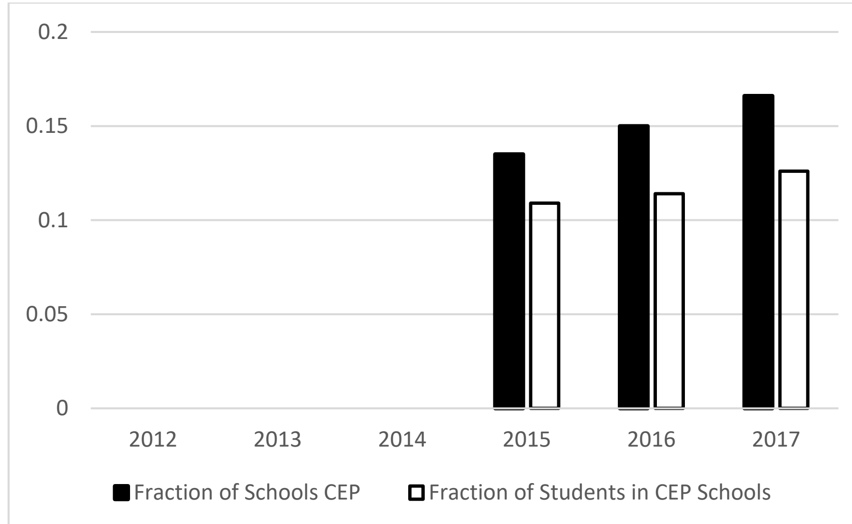
Figures and Tables

Figure 1. CEP School Counts, and CEP Coverage of Schools and Students, in Missouri Over Time for Schools with any Combination of Grades 3-8.





Notes: The graph on the left shows the number of schools with any combination of grades 3-8 (in our analytic sample) implementing the CEP in each year. The graph on the right shows CEP schools as a fraction of all eligible schools (with the same gradespan restriction) and the corresponding fraction of students in covered schools. All representations are cumulative—i.e., the numbers in 2016 reflect the cumulative effect of adoptions in 2015 and 2016. As in the main text, school years are indicated by the spring year.

Table 1. Means and Standard Deviations (in parentheses) of Key Data Elements.

	Pre-CEP Years 2012-14	Post-CEP Years 2015-17
Student Outcomes	Mean (stdev)	Mean (stdev)
Standardized Math Score	0.016 (0.989)	0.011 (0.991)
Standardized Reading Score	-0.006 (0.986)	-0.017 (0.987)
Attendance Rate	0.954 (0.046)	0.954 (0.044)
Student Characteristics		
Race/Ethnicity: White	0.743 (0.437)	0.726 (0.446)
Race/Ethnicity: Black	0.164 (0.370)	0.159 (0.366)
Race/Ethnicity: Hispanic	0.050 (0.218)	0.059 (0.236)
Race/Ethnicity: American Indian	0.004 (0.065)	0.004 (0.063)
Race/Ethnicity: Asian/Pacific Islander	0.020 (0.139)	0.020 (0.142)
Race/Ethnicity: Other	0.019 (0.137)	0.031 (0.173)
Female	0.488 (0.500)	0.488 (0.500)
English as Second Language (ESL)	0.030 (0.172)	0.040 (0.196)
Individual Education Program (IEP)	0.123 (0.328)	0.130 (0.336)
Measures of Disadvantage & CEP		
FRM Status (student level, a+b)	0.512 (0.500)	0.529 (0.499)
(a) Free Meal Status	0.434 (0.496)	0.460 (0.498)
(b) Reduced-Price Meal Status	0.078 (0.269)	0.069 (0.253)
FRM School Share (school weighted, a+b)	0.507 (0.227)	0.523 (0.256)
(a) Free Meal School Share	0.429 (0.219)	0.455 (0.260)
(b) Reduced-Price Meal School Share	0.078 (0.040)	0.068 (0.043)
FRM School Share Distribution		
FRM Share ≥ 0.90	0.064 (0.245)	0.122 (0.328)
75 ≤ FRM Share < 90	0.083 (0.276)	0.051 (0.221)
50 ≤ FRM Share < 75	0.379 (0.485)	0.360 (0.480)
25 ≤ FRM Share < 50	0.304 (0.460)	0.287 (0.452)
FRM Share < 0.25	0.170 (0.376)	0.180 (0.384)
Attends CEP School	0	0.116 (0.321)
N (Schools)	1748	1737
N (Student-Years)	920541	916760

Notes: The means of the standardized test scores differ slightly from zero because we standardize scores based on the full population of students but perform the analysis only for students in tested grades who are not held back. This data restriction is not substantively important for our analysis (only a small fraction of students are held back) but improves comparability of FRM and non-FRM students conceptually within grades.

Table 2. Estimates of the Math Achievement Gap in Grades 3-8 by Individual FRM Coding Status, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3		Pre-CEP Pseudo-Coding 3 (Random Assign.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
FRM ($\tilde{\psi}_1$)	-0.791	-0.604	-0.841	-0.652	-0.782	-0.596	-0.777	-0.592	-0.752	-0.572	-0.741	-0.561
	(0.021)***	(0.020)***	(0.024)***	(0.023)***	(0.021)***	(0.020)***	(0.021)***	(0.020)***	(0.021)***	(0.020)***	(0.023)***	(0.022)***
NFRM[75] ($\tilde{\psi}_2$)	-0.226	-0.215	-0.260	-0.251	-0.212	-0.211	-0.210	-0.209	-0.200	-0.203	-0.230	-0.219
	(0.020)***	(0.021)***	(0.024)***	(0.024)***	(0.020)***	(0.021)***	(0.020)***	(0.021)***	(0.020)***	(0.021)***	(0.022)***	(0.022)***
Other Controls		Y		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%		30.7%	
R-Squared	0.104	0.233	0.114	0.231	0.104	0.232	0.103	0.232	0.097	0.229	0.086	0.225
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 3. Estimates of the Math Achievement Gaps in Grades 3-8 by FRM School Share Bins, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3		Pre-CEP Pseudo-Coding 3 (Random Assign.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
FRM School Share Bin-1	-1.140	-0.873	-0.929	-0.663	-0.904	-0.613	-0.859	-0.579	-0.725	-0.502	-0.656	-0.443
(≥ 90 percent)	(0.038)***	(0.041)***	(0.034)***	(0.037)***	(0.037)***	(0.037)***	(0.035)***	(0.033)***	(0.027)***	(0.024)***	(0.041)***	(0.032)***
FRM School Share Bin-2	-0.713	-0.560	-0.738	-0.585	-0.685	-0.519	-0.679	-0.506	-0.640	-0.467	-0.693	-0.403
	(0.028)***	(0.029)***	(0.041)***	(0.043)***	(0.031)***	(0.033)***	(0.032)***	(0.035)***	(0.061)***	(0.055)***	(0.030)***	(0.031)***
FRM School Share Bin-3	-0.435	-0.416	-0.492	-0.458	-0.427	-0.407	-0.424	-0.405	-0.413	-0.397	-0.439	-0.403
	(0.018)***	(0.019)***	(0.022)***	(0.023)***	(0.019)***	(0.019)***	(0.019)***	(0.019)***	(0.020)***	(0.020)***	(0.020)***	(0.021)***
FRM School Share Bin-4	-0.224	-0.230	-0.255	-0.253	-0.227	-0.233	-0.227	-0.234	-0.226	-0.234	-0.228	-0.238
	(0.019)***	(0.020)***	(0.023)***	(0.024)***	(0.019)***	(0.020)***	(0.019)***	(0.020)***	(0.019)***	(0.020)***	(0.021)***	(0.023)***
Other Controls		Y		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%		30.7%	
R-Squared	0.081	0.088	0.079	0.089	0.072	0.083	0.069	0.083	0.062	0.082	0.050	0.078
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. The bin categories are for FRM school shares of (1) ≥ 0.90 (2) 0.75-0.89, (3) 0.50-0.74, (4) 0.25-0.49, (5) < 0.25, as reported in the text. Bin-5 is the omitted group. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 4. Estimates of the Math Achievement Gap in Grades 3-8 by the FRM School Share, Entered Linearly, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM School Share (linear)	-1.227	-0.958	-1.124	-0.904	-1.071	-0.811	-1.027	-0.763	-0.862	-0.611
	(0.036)***	(0.034)***	(0.034)***	(0.039)***	(0.036)***	(0.035)***	(0.035)***	(0.034)***	(0.031)***	(0.028)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.079	0.089	0.084	0.091	0.074	0.084	0.072	0.083	0.064	0.080
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 5. Estimates of the Math Achievement Gap in Grades 3-8 by the Binned FRM School Share and Individual FRM Status Simultaneously, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Indiv. FRM indicator	-0.477	-0.356	-0.497	-0.370	-0.482	-0.359	-0.482	-0.359	-0.483	-0.357
	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.007)***	(0.005)***
FRM School Share Bin-1 (≥ 90 percent)	-0.774	-0.597	-0.523	-0.357	-0.512	-0.311	-0.466	-0.277	-0.330	-0.202
	(0.037)***	(0.040)***	(0.033)***	(0.035)***	(0.036)***	(0.036)***	(0.034)***	(0.032)***	(0.027)***	(0.024)***
FRM School Share Bin-2	-0.407	-0.332	-0.420	-0.352	-0.381	-0.296	-0.374	-0.284	-0.342	-0.244
	(0.027)***	(0.029)***	(0.040)***	(0.042)***	(0.030)***	(0.032)***	(0.031)***	(0.034)***	(0.059)***	(0.054)***
FRM School Share Bin-3	-0.229	-0.258	-0.275	-0.293	-0.220	-0.248	-0.218	-0.246	-0.213	-0.245
	(0.018)***	(0.018)***	(0.021)***	(0.021)***	(0.018)***	(0.018)***	(0.018)***	(0.019)***	(0.019)***	(0.019)***
FRM School Share Bin-4	-0.123	-0.149	-0.148	-0.166	-0.125	-0.151	-0.125	-0.152	-0.125	-0.153
	(0.018)***	(0.019)***	(0.022)***	(0.022)***	(0.018)***	(0.019)***	(0.018)***	(0.019)***	(0.018)***	(0.019)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.128	0.242	0.126	0.236	0.117	0.237	0.114	0.236	0.102	0.233
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. The bin categories are for FRM school shares of (1) ≥ 0.90 (2) 0.75-0.89, (3) 0.50-0.74, (4) 0.25-0.49, (5) < 0.25, as reported in the text. Bin-5 is the omitted group. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 6. Estimates of the Math Achievement Gaps in Grades 3-8, Separately for "Free" and "Reduced-Price" Meal Students, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Free Meals ($\tilde{\psi}_1^{FM}$)	-0.841	-0.642	-0.886	-0.687	-0.827	-0.629	-0.820	-0.623	-0.785	-0.595
	(0.022)***	(0.020)***	(0.025)***	(0.023)***	(0.022)***	(0.020)***	(0.022)***	(0.020)***	(0.022)***	(0.020)***
Reduced-Price Meals ($\tilde{\psi}_1^{RM}$)	-0.512	-0.428	-0.541	-0.465	-0.495	-0.426	-0.492	-0.425	-0.478	-0.415
	(0.019)***	(0.020)***	(0.023)***	(0.023)***	(0.019)***	(0.020)***	(0.019)***	(0.020)***	(0.020)***	(0.020)***
NFRM[75] ($\tilde{\psi}_2$)	-0.226	-0.216	-0.260	-0.251	-0.212	-0.211	-0.210	-0.209	-0.200	-0.203
	(0.020)***	(0.021)***	(0.024)***	(0.024)***	(0.020)***	(0.021)***	(0.020)***	(0.021)***	(0.020)***	(0.021)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.111	0.236	0.121	0.234	0.111	0.235	0.109	0.234	0.102	0.231
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. These results match the results in Table 2, except that the FRM indicator is split into separate free meal (FM) and reduced-price meal (RM) indicators. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 7. Estimates of the Math Achievement Gaps in Grades 3-8, Separately by the School "Free" and "Reduced-Price" Meal Shares, Entered Linearly, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Free Meal School Share (linear)	-1.308	-1.031	-1.123	-0.880	-1.070	-0.783	-1.023	-0.738	-0.875	-0.638
	(0.036)***	(0.039)***	(0.034)***	(0.040)***	(0.035)***	(0.037)***	(0.034)***	(0.034)***	(0.030)***	(0.027)***
Reduced-Price Meal School Share (linear)	-0.353	-0.382	-0.622	-1.325	-0.411	-1.180	-0.598	-1.350	-1.003	-1.627
	(0.152)**	(0.144)***	(0.163)***	(0.180)***	(0.154)***	(0.149)***	(0.151)***	(0.145)***	(0.144)***	(0.146)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.083	0.089	0.084	0.091	0.074	0.084	0.071	0.083	0.064	0.082
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 8. Upper Bound Effects of the CEP Based on Hypothetical District-Level, Rather than School-Level, Adoptions.

	Pseudo-Coded Adoptions are based on District Eligibility:			
	Pre-CEP Pseudo-Coding 3 (Comparable to Table 2)		Pre-CEP Pseudo-Coding 3 (Comparable to Table 3)	
	(1)	(2)	(3)	(4)
FRM ($\tilde{\psi}_1$)	-0.753	-0.570		
	(0.021)***	(0.020)***		
NFRM[75] ($\tilde{\psi}_2$)	-0.202	-0.211		
	(0.020)***	(0.021)***		
FRM School Share Bin-1 (> 90 percent)			-0.722	-0.446
			(0.029)***	(0.026)***
FRM School Share Bin-2			-0.679	-0.544
			(0.041)***	(0.042)***
FRM School Share Bin-3			-0.419	-0.414
			(0.019)***	(0.020)***
FRM School Share Bin-4			-0.225	-0.239
			(0.019)***	(0.021)***
Other Controls		Y		Y
Share of Students FRM	56.3%		56.3%	
Share of Schools CEP	25.3%		25.3%	
R-Squared	0.097	0.227	0.062	0.081
N(Students)	916461	916461	916461	916461

Notes: Columns (1) and (2) are comparable to columns (9) and (10) in Table 2, and columns (3) and (4) are comparable to columns (9) and (10) in Table 3. The notes to Tables 2 and 3 apply.

Appendix Tables
(for posting online)

Appendix Table A.1. Estimates of the English Language Arts Achievement Gap in Grades 3-8 by FRM Coding Status, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM ($\tilde{\psi}_1$)	-0.770	-0.593	-0.830	-0.651	-0.759	-0.584	-0.754	-0.580	-0.729	-0.558
	(0.018)***	(0.017)***	(0.022)***	(0.020)***	(0.019)***	(0.017)***	(0.019)***	(0.017)***	(0.019)***	(0.017)***
NFRM^{75} ($\tilde{\psi}_2$)	-0.219	-0.213	-0.240	-0.231	-0.208	-0.210	-0.206	-0.209	-0.199	-0.205
	(0.017)***	(0.018)***	(0.021)***	(0.021)***	(0.017)***	(0.018)***	(0.017)***	(0.018)***	(0.017)***	(0.018)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.102	0.264	0.120	0.257	0.101	0.262	0.100	0.262	0.093	0.259
N(Students)	918594	918594	914834	914834	918594	918594	918594	918594	918594	918594

Notes: This table replicates the analysis in Table 2 from the main text but using English language arts achievement as the outcome. The random assignment condition is not included in this table for brevity (corresponding to columns 11 and 12 in Table 2). The notes to Table 2 apply.

Appendix Table A.2. Estimates of the Attendance Rate Gap in Grades 3-8 by FRM Coding Status, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM ($\tilde{\psi}_1$)	-0.019	-0.017	-0.016	-0.015	-0.018	-0.017	-0.018	-0.017	-0.017	-0.016
	(0.001)***	(0.001)***	(0.001)***	(0.000)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
NFRM^{75} ($\tilde{\psi}_2$)	-0.001	-0.001	0.001	0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.000
	(0.000)**	(0.000)	(0.000)*	(0.000)**	(0.000)*	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.050	0.058	0.043	0.051	0.049	0.056	0.049	0.056	0.045	0.053
N(Students)	920541	920541	916760	916760	920541	920541	920541	920541	920541	920541

Notes: This table replicates the analysis in Table 2 from the main text but using the attendance rate as the outcome. The random assignment condition is not included in this table for brevity (corresponding to columns 11 and 12 in Table 2). The notes to Table 2 apply.

Appendix Table A.3. Estimates of the English Language Arts Achievement Gaps in Grades 3-8 by FRM School Share Bins, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM School Share Bin-1 (≥ 90 percent)	-1.096	-0.876	-0.899	-0.676	-0.864	-0.607	-0.821	-0.571	-0.693	-0.489
	(0.029)***	(0.032)***	(0.033)***	(0.034)***	(0.032)***	(0.032)***	(0.031)***	(0.029)***	(0.024)***	(0.021)***
FRM School Share Bin-2	-0.688	-0.562	-0.684	-0.556	-0.662	-0.513	-0.652	-0.495	-0.588	-0.430
	(0.024)***	(0.023)***	(0.029)***	(0.033)***	(0.026)***	(0.026)***	(0.027)***	(0.027)***	(0.053)***	(0.044)***
FRM School Share Bin-3	-0.420	-0.402	-0.479	-0.449	-0.413	-0.394	-0.411	-0.392	-0.403	-0.386
	(0.015)***	(0.016)***	(0.020)***	(0.021)***	(0.015)***	(0.016)***	(0.015)***	(0.016)***	(0.016)***	(0.017)***
FRM School Share Bin-4	-0.226	-0.230	-0.258	-0.255	-0.230	-0.234	-0.230	-0.235	-0.229	-0.235
	(0.016)***	(0.017)***	(0.020)***	(0.022)***	(0.016)***	(0.017)***	(0.016)***	(0.017)***	(0.016)***	(0.017)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.077	0.082	0.078	0.086	0.068	0.077	0.066	0.076	0.059	0.075
N(Students)	918594	918594	914834	914834	918594	918594	918594	918594	918594	918594

Notes: This table replicates the analysis in Table 3 from the main text but using English language arts achievement as the outcome. The random assignment condition is not included in this table for brevity (corresponding to columns 11 and 12 in Table 3). The notes to Table 3 apply.

Appendix Table A.4. Estimates of the Attendance Rate Gaps in Grades 3-8 by FRM School Share Bins, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM School Share Bin-1 (≥ 90 percent)	-0.027	-0.022	-0.014	-0.008	-0.019	-0.011	-0.018	-0.011	-0.014	-0.009
	(0.002)***	(0.002)***	(0.001)***	(0.001)***	(0.002)***	(0.001)***	(0.002)***	(0.001)***	(0.001)***	(0.001)***
FRM School Share Bin-2	-0.014	-0.011	-0.012	-0.009	-0.014	-0.010	-0.014	-0.009	-0.011	-0.007
	(0.001)***	(0.001)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.004)***	(0.003)**
FRM School Share Bin-3	-0.007	-0.007	-0.005	-0.004	-0.007	-0.007	-0.007	-0.007	-0.007	-0.006
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
FRM School Share Bin-4	-0.004	-0.004	-0.002	-0.002	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.032	0.033	0.020	0.023	0.027	0.030	0.027	0.031	0.025	0.030
N(Students)	920541	920541	916760	916760	920541	920541	920541	920541	920541	920541

Notes: This table replicates the analysis in Table 3 from the main text but using English language arts achievement as the outcome. The random assignment condition is not included in this table for brevity (corresponding to columns 11 and 12 in Table 3). The notes to Table 3 apply.

Appendix Table A.5. Estimates of the Math Achievement Gap in Grades 3-8 controlling for the FRM School Share, Entered Linearly, and Individual FRM status, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Indiv. FRM Indicator	-0.467	-0.346	-0.484	-0.358	-0.474	-0.351	-0.474	-0.351	-0.475	-0.349
	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.007)***	(0.004)***
FRM School Share (linear)	-0.763	-0.603	-0.644	-0.539	-0.601	-0.449	-0.556	-0.402	-0.391	-0.255
	(0.036)***	(0.033)***	(0.034)***	(0.038)***	(0.036)***	(0.034)***	(0.036)***	(0.034)***	(0.032)***	(0.028)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	51.2%		52.9%		52.9%		53.5%		56.5%	
Share of Schools CEP	0		15.1%		13.2%		16.4%		30.7%	
R-Squared	0.123	0.242	0.128	0.236	0.117	0.237	0.114	0.235	0.102	0.230
N(Students)	916461	916461	909974	909974	916461	916461	916461	916461	916461	916461

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. The share of students coded as FRM reported at the bottom of the table is calculated using data from the full sample shown in Table 1, regardless of test score availability.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table A.6. Estimates of the Achievement Gap in High School on the English End of Course Test by FRM Coding Status of Individual Students, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM ($\tilde{\psi}_1$)	-0.719	-0.612	-0.722	-0.603	-0.705	-0.599	-0.703	-0.597	-0.703	-0.598
	(0.029)***	(0.028)***	(0.033)***	(0.030)***	(0.029)***	(0.028)***	(0.029)***	(0.028)***	(0.029)***	(0.029)***
NFRM^{75} ($\tilde{\psi}_2$)	-0.265	-0.277	-0.281	-0.287	-0.262	-0.280	-0.260	-0.279	-0.255	-0.276
	(0.028)***	(0.030)***	(0.028)***	(0.029)***	(0.028)***	(0.030)***	(0.028)***	(0.030)***	(0.028)***	(0.030)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	41.9%		44.5%		43.9%		44.3%		44.8%	
Share of Schools CEP	0		11.0%		10.6%		12.9%		15.2%	
R-Squared	0.160	0.277	0.165	0.273	0.158	0.274	0.158	0.274	0.158	0.274
N(Students)	192738	192738	128245	128245	192738	192738	192738	192738	192738	192738

Notes: This table is an analog to Table 2 in the main text. The notes to Table 2 apply with one exception. Because the gap between students with attendance data and students with English EOC test scores is so large (because the English EOC is given primarily to 10th graders), we report the share of FRM students based on the test sample in this table. The full-sample numbers for high schools can be found in the analog attendance table (Table A.8).

Table A.7. Estimates of the Achievement Gaps in High School on the English End of Course Test by FRM School Share Bins, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM School Share Bin-1 (≥ 90 percent)	-1.043	-1.041	-0.651	-0.587	-0.620	-0.503	-0.616	-0.503	-0.625	-0.525
	(0.186)***	(0.203)***	(0.115)***	(0.129)***	(0.086)***	(0.088)***	(0.076)***	(0.078)***	(0.066)***	(0.066)***
FRM School Share Bin-2	-0.742	-0.713	-0.687	-0.634	-0.644	-0.552	-0.639	-0.573	-0.710	-0.553
	(0.076)***	(0.100)***	(0.094)***	(0.094)***	(0.067)***	(0.075)***	(0.091)***	(0.085)***	(0.280)**	(0.242)**
FRM School Share Bin-3	-0.437	-0.427	-0.481	-0.456	-0.447	-0.425	-0.443	-0.423	-0.432	-0.419
	(0.030)***	(0.028)***	(0.026)***	(0.029)***	(0.027)***	(0.028)***	(0.027)***	(0.028)***	(0.027)***	(0.028)***
FRM School Share Bin-4	-0.282	-0.283	-0.306	-0.298	-0.286	-0.288	-0.286	-0.287	-0.286	-0.287
	(0.029)***	(0.030)***	(0.027)***	(0.028)***	(0.029)***	(0.030)***	(0.029)***	(0.030)***	(0.029)***	(0.030)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	41.9%		44.5%		43.9%		44.3%		44.8%	
Share of Schools CEP	0		11.0%		10.6%		12.9%		15.2%	
R-Squared	0.123	0.125	0.128	0.131	0.117	0.120	0.118	0.120	0.118	0.121
N(Students)	192738	192738	128245	128245	192738	192738	192738	192738	192738	192738

Notes: This table is an analog to Table 3 in the main text. The notes to Table 3 apply with one exception. Because the gap between students with attendance data and students with English EOC test scores is so large (because the English EOC is given primarily to 10th graders), we report the share of FRM students based on the test sample in this table. The full-sample numbers for high schools can be found in the analog attendance table (Table A.9).

Table A.8. Estimates of the Attendance Rate Gap in High School by FRM Coding Status of Individual Students, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM ($\tilde{\psi}_1$)	-0.040	-0.035	-0.035	-0.032	-0.040	-0.035	-0.040	-0.035	-0.040	-0.035
	(0.002)***	(0.001)***	(0.002)***	(0.001)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
NFRM^{75} ($\tilde{\psi}_2$)	-0.002	-0.002	0.002	0.002	-0.001	-0.000	-0.000	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)*	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	43.0%		45.0%		45.1%		45.5%		45.9%	
Share of Schools CEP	0		11.5%		10.6%		12.9%		15.2%	
R-Squared	0.052	0.060	0.055	0.062	0.054	0.061	0.054	0.062	0.054	0.061
N(Students)	795723	795723	795260	795260	795723	795723	795723	795723	795723	795723

Notes: This table is an analog to Table 2 in the main text. The notes to Table 2 apply.

Table A.9. Estimates of Attendance Rate Gaps in High School by FRM School Share Bins, Various CEP Conditions.

	Pre-CEP		Post-CEP		Pre-CEP Pseudo-Coding 1		Pre-CEP Pseudo-Coding 2		Pre-CEP Pseudo-Coding 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FRM School Share Bin-1 (≥ 90 percent)	-0.037	-0.007	-0.035	-0.011	-0.045	-0.016	-0.043	-0.015	-0.042	-0.015
	(0.015)**	(0.018)	(0.006)***	(0.005)**	(0.007)***	(0.007)**	(0.007)***	(0.007)**	(0.006)***	(0.007)**
FRM School Share Bin-2	-0.059	-0.031	-0.036	-0.021	-0.040	-0.019	-0.044	-0.034	0.004	0.022
	(0.008)***	(0.009)***	(0.013)***	(0.014)	(0.016)**	(0.019)	(0.021)**	(0.019)*	(0.002)	(0.009)**
FRM School Share Bin-3	-0.014	-0.009	-0.013	-0.008	-0.014	-0.009	-0.013	-0.009	-0.012	-0.009
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
FRM School Share Bin-4	-0.008	-0.007	-0.003	-0.003	-0.008	-0.008	-0.008	-0.008	-0.008	-0.008
	(0.002)***	(0.002)***	(0.002)**	(0.002)**	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Other Controls		Y		Y		Y		Y		Y
Share of Students FRM	43.0%		45.0%		45.1%		45.5%		45.9%	
Share of Schools CEP	0		11.5%		10.6%		12.9%		15.2%	
R-Squared	0.030	0.038	0.024	0.034	0.024	0.036	0.024	0.036	0.024	0.036
N(Students)	795723	795723	795260	795260	795723	795723	795723	795723	795723	795723

Notes: This table is an analog to Table 3 in the main text. The notes to Table 3 apply.