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Using Free Meal and Direct Certification Data to Proxy for Student Disadvantage in the Era of the Community Eligibility

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Free and reduced-price meal (FRM) data are used ubiquitously to proxy for student disadvantage in education research and policy applications. The Community Eligibility Provision (CEP)—a recently-implemented policy change to the federally-administered National School Lunch Program—allows schools serving low-income populations to identify all students as FRM-eligible regardless of individual circumstances. We study the CEP's effect on FRM eligibility as a proxy for student disadvantage, and relatedly, we examine the viability of direct certification (DC) status as an alternative disadvantage measure. Our findings on whether the CEP degrades the informational content of FRM data are mixed. At the individual level there is essentially no effect, but the CEP does meaningfully change the information conveyed by the FRM-eligible share of students in a school. Our comparison of FRM and DC data in the post-CEP era shows that these measures are similarly informative as proxies for disadvantage, despite the CEP-induced information loss in FRM data. Using both measures together can improve the identification of disadvantaged students, but only marginally.

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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 local, state, and federal levels have historically relied on FRM data in their efforts to monitor and regulate educational outcomes and interventions and allocate funding, including for the U.S. Department of Education's Title-I program (Camera, 2019; Massachusetts Department of Elementary and Secondary Education, 2017; Riddle, 2015). 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).

Research on the usefulness of FRM eligibility as a proxy for student disadvantage pre-dates the implementation of the Community Eligibility Provision (CEP), which allows all students in schools and districts serving low-income populations to receive free meals regardless of each student's individual circumstances. Setting aside the substantive impacts of the CEP, which have been studied elsewhere (e.g., see Gordon and Ruffini, 2018), our focus is on how the CEP affects the proxy value of FRM eligibility as a measure of student disadvantage. We also explore the viability of direct certification (DC) status as an alternative measure that could be used in the post-CEP era in addition to, or in place of, FRM eligibility.

We assess the CEP-induced change in the informational value of FRM data in terms of the ability of FRM data to predict key student outcomes—test scores and attendance. This approach builds on recent work by Domina et al. (2018) and Michelmore and Dynarski (2017) and is motivated by a measurement error framework in which the CEP can be viewed as increasing measurement error in the FRM-eligibility indicator. The nature of the measurement error is complicated and its substantive importance unclear *a priori*, prompting our empirical investigation.

Using administrative microdata from Missouri, we begin by estimating gaps in student outcomes by coded FRM status before and after the CEP was introduced. The pre/post models are a useful starting point for thinking about the influence of the CEP, but inference is confounded by changes to contextual factors over time that coincide with its introduction (e.g., changes to economic conditions, testing instruments, etc.). In order to separate the effect of the CEP from other factors, we rely on "pseudo-coded" scenarios in which we falsely code schools as CEP adopters prior to policy implementation. In our initial pseudo-coded scenario, we look forward in the data and identify Missouri schools that adopted the CEP in the first year it was available. We then go back in time and pseudo-code these schools as CEP adopters prior to the policy and estimate our models using the pre-CEP, pseudo-coded data. By comparing the results to results from models that use the actual pre-CEP data and FRM coding, we can assess how CEP-induced changes to which students are coded as FRM-eligible affect the informational content of FRM eligibility, holding all else constant. We expand on this idea to include schools that adopted the CEP within the first three years of its availability, then obtain an upper bound effect of the CEP by pseudo-coding all CEP-eligible schools in Missouri as adopters regardless of their future adoption decisions.

We find that the information contained by students' individual FRM designations is not noticeably influenced by the CEP. There are two mechanisms that account for this null finding. First, students whose coding status is changed by the CEP are not a random sample of students—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 where all eligible schools in Missouri adopt the CEP—is small. This result may be initially surprising but is easy to explain *ex post*. The reason is that schools eligible for the CEP already have high shares of FRM-eligible students (about 80 percent on average), so relatively few students switch status when a school adopts the CEP. At its maximum effect, we show that the CEP would increase the share of FRM-eligible students in the state of Missouri by just 5.3 percentage points, raising it from 51.2 percent to 56.5 percent.

In contrast, we show that the CEP has the potential to meaningfully affect the information conveyed by school-aggregated FRM measures. The reason is straightforward from a data perspective—while the CEP affects relatively few individual students, it affects a much larger fraction of schools. For example, in the maximum-effect scenario in which an extra 5.3 percent of the student population is coded as FRM-eligible due to the CEP (per the preceding paragraph), 30.7 percent of Missouri schools would adopt the CEP, thus flipping to 100% (coded) FRM-eligible.

After documenting the effect of the CEP on the informational content of FRM data, we go on to examine the potential for direct certification (DC) data to replace or augment FRM data in efforts to identify disadvantaged students in the post-CEP era. Students are directly certified for free meal receipt if they participate 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. Students can also be directly certified if they are classified as foster, migrant, homeless, or runaway. DC data have been suggested as an alternative to FRM data in recent policy reports and in the popular press (Blagg, 2019; Camera, 2019; Chingos, 2016; Greenberg, 2018). Despite the degradation of information in FRM data we document as a result of the CEP, the results from our comparative analysis show that FRM and DC data are similarly predictive of student outcomes in the post-CEP era.

In the discussion section we contextualize our findings for researchers and policymakers. From a policy perspective, the fact that the CEP affects the information contained by school-aggregated FRM data has implications for school accountability and finance policies at all levels of government. 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 in contemporary applications. Our comparative analysis of FRM- and DC-based measures makes clear that DC data are a viable substitute for FRM data in the post-CEP era but, somewhat surprisingly, also shows that DC data do not offer a meaningful improvement. We also discuss the policy tradeoffs associated with switching from FRM- to DC-based disadvantage metrics, which some states have done and others are considering (Chingos, 2018; Grich, 2019).

2. The Community Eligibility Provision

The CEP is a recent policy change in the eligibility criteria of the National School Lunch Program, which is administered by the United States Department of Agriculture (USDA). It allows high poverty schools and districts to provide free meals (breakfast and lunch) to all students without collecting individual household applications. School and district eligibility for the CEP is based on the fraction of students who are directly certified for free meal receipt. The income threshold for direct certification is lower than the threshold for eligibility for free and reduced price meals, thus a smaller sample is identified for direct certification (see Table 1; also Massachusetts Department of Elementary and Secondary Education, 2017).

Schools and districts choose whether to participate in the CEP, conditional on eligibility. Participating institutions are reimbursed for the free meals by the USDA using a kinked formula based on the share of DC students. The DC share must be at least 0.40 for baseline CEP eligibility. The USDA reimburses free meals at a rate of 1.6 times the DC share, and once the DC share reaches 0.625, the reimbursement rate plateaus at 100 percent. A notable feature of the program is that when a school or district is accepted, it can offer free meals 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 (see below)—therefore, schools that we observe implementing the CEP remain covered throughout the timeframe we study.[1]

We leverage CEP program rules for portions of our analysis to identify CEP-eligible schools. We define eligibility as meeting the 0.40 DC share, which is the broadest definition. In 2014, the year before any Missouri schools adopted the CEP (thus preserving the informational value of FRM data) schools with at least 40 percent of students identified as DC had, on average, 79 percent of students coded as FRM-eligible.

3. Data

Our analysis is based on student-level administrative microdata provided by the Missouri Department of Elementary and Secondary Education (DESE). The data panel covers the school years 2011-12 through 2016-17. The CEP was first adopted by Missouri schools during the 2014-15 school year (hereafter we refer to school years by the spring year; e.g., 2014-15 as 2015).

We assess the informational content of FRM and DC data using predictive models of student attendance and achievement in math and English language arts (ELA) in grades 3-8.[2] We define the attendance rate as the total number of days attended divided by the total number of days enrolled, on a 0-1 scale.[3] 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).

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[1] 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 suppressed for brevity we find no evidence that such changes are happening at a high enough rate to be detectable in our empirical analysis.
[2] 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.
[3] For students who are enrolled in more than one school in a year, attendance is calculated across all schools.

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.

Our data include race and gender information for each student, whether the student is an English language learner (ELL), and whether the student has an individualized education program (IEP). We also know each student's FRM eligibility status in each year; moreover, for the years 2013 to 2017, we know each student's DC status. As of the 2014-15 academic year, 96 percent of Missouri school districts were directly certifying categorically eligible students, which is slightly above the national average rate of 95 percent (Moore et al., 2016).

We aggregate FRM and DC data to the school level to provide contextual information about the school attended by each student. For example, the share of FRM-eligible students at the school gives information beyond what is conveyed by a student's own FRM-eligibility status (Ehlert et al., 2016). We also construct individual-level panel measures of FRM and DC status that we include in some models. These measures capture the fraction of years—up to and inclusive of the current year—that a student is coded as either FRM-eligible or directly certified in the Missouri data. Michelmore and Dynarksi (2017) show that panel measures provide more information about student disadvantage than contemporaneous measures alone.[4]

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[4] We use fractional measures for the panel variables, instead of counts of FRM- or DC-eligible years, to improve comparability of the panel variables across grade levels. For example, the meaning of a count variable for a grade-3 student will not be the same as for a grade-8 student, whereas the fractional measures are more comparable. Like with contemporaneous FRM, the panel FRM variable is influenced by the introduction of the CEP; and the panel DC variable is influenced by our truncated DC data panel. We examine the sensitivity of our findings to these data issues below and find that they do not affect our findings substantively.

Table 1 provides summary statistics for the data, which 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 1.8 million student-year observations (summed over the pre- and post-CEP years of the data panel). The share of directly certified students is lower than the share of students eligible for free and reduced-price meals in both the pre- and post-CEP periods. This is due to the difference in the poverty thresholds used to identify students by each measure (Blagg, 2019). On average during the post-CEP portion of the data panel, 11.6 percent of students in Missouri attended a CEP school.

4. Methodology

4.1 The Effect of CEP-Coding on the Informational Content of FRM Data

We begin with basic models designed to determine how much the CEP has degraded the proxy value of FRM-eligibility as an indicator of student disadvantage. Our initial models do not consider DC data. Instead, we focus on what in recent history has been the "business as usual" setting, which does not incorporate other disadvantage measures. We focus only on contemporaneous FRM information to begin with, which is consistent with how the information is typically used by researchers and policymakers. Later, we expand our analysis to include the panel measures of disadvantage.

The initial regressions take the following form:

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

$$Y_{igst} = \delta_0 + FRM_{it}\delta_1 + \overline{FRM}_{st}\delta_2 + \mathbf{X_{it}}\boldsymbol{\delta_3} + \overline{\mathbf{X}}_{\mathbf{st}}\boldsymbol{\delta_4} + \lambda_g + \phi_t + \varepsilon_{igst} \tag{2}$$

Equations (1) and (2) are nearly identical; the difference is that equation (2) includes school-average student characteristics as additional predictors of outcomes (to capture educational context). In both equations, Y_{igst} is the outcome of interest—either a math test score, an ELA test score, or the attendance rate (again, 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 in equation (2), $\overline{FRM}_{st}$ is the share of students attending school *s* in year *t* who are coded as FRM-eligible. X_{it} and $\overline{X}_{st}$ are analogous vectors of the other student and school-aggregated characteristics. The student characteristics are as shown in Table 1 and include student race/ethnicity and gender indicators, along with indicators for whether the student is learning English as a second language (ESL) and has an individualized education program (IEP). Conceptually the variables in the *X*-vectors are no different than the FRM-eligibility variables, but we separate out FRM_{it} and $\overline{FRM}_{st}$ visually because the coefficients ψ_1, δ_1, and δ_2 are focal to our analysis. Finally, ω_g / λ_g, ξ_t / ϕ_t and $e_{igst} / \varepsilon_{igst}$ are grade fixed effects, year fixed effects, and idiosyncratic errors clustered at the school level, respectively.[5]

We initially estimate these equations separately using data from the pre- and post-CEP periods for each student outcome. We report on changes to the coefficients ψ_1, δ_1, and δ_2, along with changes to the overall predictive power of the models. However, as noted above, a limitation of the simple pre-post analysis is that other factors may also be changing over the timespan during which the CEP has been adopted in Missouri, which could influence the results. Hence, in order to isolate the effect of the CEP, we estimate the models using data pseudo-coded as described above. In the first scenario, we identify all schools that adopted the CEP during its first year in Missouri (2015). Call these "group A" schools. We then estimate equations (1) and (2) using data from pre-CEP years only (i.e., 2012-14), but coded as if group-A schools had already adopted the CEP. Noting that no school had actually adopted the CEP during the pre-CEP years, group-A schools are "pseudo-coded" to have adopted the CEP prior to actual adoption.

[5] 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.

We estimate equations (1) and (2) using the same exact data, with and without the CEP pseudo-coding in place, to assess the data-quality consequences of the CEP holding all else equal. Like with the simple pre-post comparison, we focus our attention on the coefficients ψ_1, δ_1, and δ_2, and each model's overall predictive power. Given the basic statistics of measurement error, we expect ψ_1, δ_1, and δ_2 to attenuate and the overall predictive power of the model to decline with the CEP pseudo-coding, but the magnitudes of these changes are difficult to predict *a priori*.

We extend the above-described scenario with two more pronounced scenarios. In the first, we pseudo-code all schools that we observe adopting the CEP at any point during our data panel as CEP adopters in the pre-period. 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 school-level DC data to identify all CEP-eligible schools based on their DC student shares in 2016 (the middle year of the post-CEP portion of our data panel), then pseudo-code all of these CEP-eligible schools as adopters in the pre-CEP period. This scenario gives the upper-bound effect of the CEP in that it allows for the maximum number of schools to participate as permitted by program rules.[6]

Finally, recall from above that CEP adoptions can occur at the district or school levels. For districts, each individual school does not need to be eligible for the CEP as long as the district is eligible collectively.[7] The first two pseudo-coded scenarios capture real adoption decisions in

[6] The results from the upper-bound scenario where we code all eligible schools as CEP adopters make endogenous adoptions irrelevant, but there is selection into the CEP conditional on eligibility. In the other scenarios selection is relevant and the artificial coding is inclusive of selection, which is such that CEP adopters on average have a higher DC share than non-adopters among eligible schools. For example, the average DC share in 2014 at schools that adopted the CEP in 2015 is 0.61 versus 0.50 for eligible schools that did not adopt the CEP. This is consistent with program incentives, as meal costs are reimbursed at a higher rate for schools with a higher fraction of DC students (over the range of DC-share values of 0.400-0.625, per above). If students who attend schools with a higher DC share are more disadvantaged, this type of selection will reduce the amount of information degradation of FRM data because the students who experience a status change are disadvantaged relative to the eligible pool of non-FRM students.

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

Missouri and thus 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 schools based on district-level eligibility instead.

4.2 *The Effect of CEP-Coding on School Accountability*

We also consider the implications of CEP-induced data changes for school accountability policies based on value-added. We estimate school value-added to math and ELA achievement in grades 3-8 using a two-step model following Ehlert et al. (2016) and Parsons, Koedel, and Tan (2019).[8] Specifically, we estimate the following equations sequentially:

$$Y_{igst} = \gamma_0 + Y_{i(t-1)}\gamma_1 + FRM_{it}\gamma_2 + \overline{FRM}_{st}\gamma_3 + \mathbf{X_{it}}\boldsymbol{\gamma}_4 + \bar{\mathbf{X}}_{\mathbf{st}}\boldsymbol{\gamma}_5 + \pi_g + \varsigma_t + \eta_{igst} \quad (3)$$

$$\eta_{igst} = \boldsymbol{\theta}_{\mathbf{s}} + \tau_{igst} \quad (4)$$

The variables in equation (3) overlap entirely with the variables in equation (2) and are defined as above. The only change is that equation (2) models test-score levels (plus attendance, which we do not consider here), whereas equation (3) models test-score growth by including lagged achievement on the right-hand side. Lagged achievement is controlled for in a variety of ways in the literature (Koedel, Mihaly, and Rockoff, 2015); we use just the same-subject lagged score.[9] The estimates of school value-added are obtained from the vector $\hat{\boldsymbol{\theta}}_{\mathbf{s}}$ taken from equation (4), to which we apply *ex post* empirical Bayesian shrinkage as described by Koedel, Mihaly, and Rockoff (2015).

The CEP will affect school rankings based on value-added by changing which students are coded as FRM-eligible. This will factor into the regression adjustment in equation (3). We expect CEP coding to positively affect value-added rankings for affected schools. This is because the

[8] Missouri uses a structurally similar two-step model in its state accountability system, although the state model does not include all of the control variables we use (notably it does not include the FRM variables; see Ehlert et al., 2014).
[9] We have confirmed that our findings are similar if we include lagged off-subject test scores in the VAMs or use polynomials of the lagged scores, as has been done in some recent research (e.g., Chetty, Friedman, and Rockoff, 2014).

coefficients γ_2 and γ_3 in equation (3) are negative (despite CEP-induced attenuation) and some students who would not be FRM-eligible based on their own circumstances are coded as FRM-eligible under the CEP (which affects both individual student coding and the school share). The end result is that the model will predict students who attend CEP schools to score lower than it otherwise would, with the positive differential attributed to the school.[10]

4.3 Comparing FRM and DC Data

Next we explore the viability of using DC data either in place of, or in addition to, FRM data in the post-CEP era (i.e., for the years 2015 to 2017). We also expand our use of the disadvantage metrics to include the panel DC and FRM variables described above. Our full model for this portion of the analysis is as follows:

$$\begin{aligned} Y_{igst} &= \beta_0 + \mathbf{X_{it}\beta_1} + \overline{\mathbf{X}}_{\mathbf{st}}\boldsymbol{\beta_2} + FRM_{it}\beta_3 + \overline{FRM}_{st}\beta_4 + FRM_{it}^{P}\beta_5 + \\ & DC_{it}\beta_6 + \overline{DC}_{st}\beta_7 + DC_{it}^{P}\beta_8 + \rho_g + \tau_t + u_{igst} \end{aligned} \quad (5)$$

Equation (5) is structurally similar to equation (2) but contains more information. Y_{igst}, X_{it}, and $\overline{X}_{st}$ are as defined above. For each disadvantage measure, there are now three variables—for FRM these are FRM_{it}, $\overline{FRM}_{st}$, and FRM_{it}^{P}. The first two variables are as defined in equation (2), and the third is the panel measure. Analogous sets of variables are included for DC status. ρ_g and τ_t denote grade and year fixed effects, and u_{igst} is the idiosyncratic error clustered at the school level.

A complication with the panel FRM variable is that it is influenced by the timing of the introduction of the CEP because the CEP affects FRM coverage. In addition, the DC panel variable

[10] The issues described here are fundamentally similar if a one-step value-added model is used instead of the two-step model shown in equations (3) and (4). In the case of a one-step model, it would use within-school FRM-eligibility variation to identify the equivalents of γ_2 and γ_3 (for the latter, the only within-school variation is over time). Schools with fixed CEP status will contribute no variation to the identification of these parameters. The model will similarly "over-adjust" predicted achievement for incorrectly-coded FRM students at CEP schools, thereby benefiting these schools.

is influenced by the fact that DC data are first available to us in 2013 (we construct the panel DC variable as the share of years between 2013 and *t* in which a student is coded as DC). We examine the sensitivity of our findings to these data issues in the appendix (Appendix Table A.9) by analyzing just the last year of the data panel (2017) when the CEP effect would be most pronounced on the panel FRM variable and the DC panel variable is most complete. The results from 2017 are very similar to what we report in the text below using all three post-CEP years, indicating that these data issues do not affect our findings substantively.

5. Results

5.1 The Effect of the CEP

Table 2 shows results from the math-achievement version of equation (1) using pre- and post-CEP data and the pseudo-coded pre-CEP data. Models with and without the X-vector are included for each condition. The results for ELA and attendance are substantively similar to the results in Table 2 with respect to the implications of the CEP—although we can explain much less of the total variance in student outcomes in the attendance models—and thus for 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 various CEP conditions. The first two conditions in the table, for the pre- and post-CEP years of our data panel, show that the percent of CEP-adopting schools grew from 0 in the pre-CEP period to an average of 15.1 percent of schools 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 change) owing to program rules, and (2) the average CEP-adopting school is smaller than the average school in Missouri.

Turning to the pseudo-coded pre-CEP data, the first scenario also shows an increase in the FRM-eligible student share of 1.7 percentage points, to 52.9 percent.[11] The second scenario, in which we pseudo-code all schools that ever 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. While 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, rising just 5.3 percentage points to 56.5 percent.

Turning to the regression results, the top row of Table 2 shows estimates of ψ_1 for each CEP condition from models with and without the other control variables. The CEP has essentially no effect on the estimated achievement gap by student FRM status. For example, consider a comparison of the estimates of ψ_1 in columns (1) and (9), which captures the maximum scope for effect of the CEP. These models condition only on the grade and year, and thus the output can be interpreted as what is effectively raw differences in achievement by student FRM status. The results show that without CEP coding in column (1), FRM-eligible students score 0.623 standard deviations lower in math than ineligible students on average. With maximum CEP coding in column (9) the gap decreases, but only by a negligible 0.014 standard deviations, to 0.609.

When we include the other control variables, the model is better able to predict student outcomes (i.e., the R-squared increases substantially), but the story with respect to the predictive power of FRM status is essentially unchanged. The analogous comparison of columns (2) and (10) indicates that the CEP reduces the fully-conditioned FRM gap in math achievement by just 0.015

[11] 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 take-up rate).

standard deviations; from 0.442 to 0.427. The changes to the overall predictive power of the models paint a similar picture. Sticking with the comparison of columns (2) and (10), the reported R-squared values show that the CEP, at its upper bound effect, reduces the variance in math achievement explained by the model by just 0.4 percentage points (from 22.9 to 22.5 percent).

Table 3 follows the structure of Table 2 but shows output from equation (2) with the school-aggregated variables included. 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). Like in Table 2, in Table 3 there is no discernable change in the test-score gap between individual students who differ by FRM coding status across the pseudo-coded scenarios. In fact, $\hat{\delta}_1$ becomes nominally *more negative* as CEP coverage increases. In isolation this result is directionally inconsistent with the CEP inducing attenuation bias in $\hat{\delta}_1$. However, it is clear that there is an attenuating effect of the CEP loading onto $\hat{\delta}_2$. In both the sparse model (i.e., the model without the *X*-vectors) and the full model, $\hat{\delta}_2$ consistently declines as the influence of the CEP increases. Comparing columns (1) and (2) to columns (9) and (10) reveals a sharp change—the magnitude of $\hat{\delta}_2$ is reduced by roughly half.

We emphasize that this change occurs without any true changes in the world—the difference is driven entirely by whether we code FRM status as if the CEP were in place. The reason the attenuating effect of the CEP loads onto the school FRM share, rather than the individual FRM eligibility indicator, is that the school FRM share is impacted by the CEP at a much higher relative rate in the data. For example, in the first pseudo-coded scenario just 1.7 percent of students change individual eligibility status, whereas 13.2 percent of schools experience a change to the FRM share as a result of the CEP.

The effect of the CEP in Table 3—driven by the effect on the school-aggregated FRM variable—can be illustrated by comparing the predicted test score gap between two hypothetical students: (1) a student who is not coded as FRM-eligible attending a school where 25 percent of students are FRM-eligible, and (2) a student who is FRM-eligible attending a school where 75 percent of students are FRM-eligible. From Table 1, note that the 50 percentage point gap in the school FRM-eligible share corresponds to roughly two standard deviations in the distribution of this variable, so this comparison gives a large contrast. Based on the results from the model in column (1) of Table 3, the estimated gap between these students is 0.849 standard deviations of student achievement (0.467 + 0.5*0.763). But based on the model in column (9), the estimated gap is just 0.670 standard deviations (0.475+0.5*0.391). These results make clear that the CEP reduces the level of disadvantage conveyed by the share of FRM-eligible students at a school.

The evolution of the R-squared values in Table 3 is similar to what we find in Table 2, reinforcing the finding that the CEP has a modest effect on the ability of FRM data to explain variation in mathematics test scores on the whole. Focusing on the comparison between the sparse models in columns (1) and (9), the total change in the R-squared induced by the CEP is 0.021. The change is about half as large when we use the full model in columns (2) and (10) (0.012) because the X vectors (containing the individual and school-average variables) partially compensate for the information loss in FRM data due to the CEP.

Selected results highlighting the key findings from Tables 2 and 3 are presented visually in Figure 2.

We have suggested two mechanisms that dull the effect of the CEP on the informational content of FRM data. The first is that the CEP changes FRM status for students who attend high-poverty schools, which limits the substantive impact of technical inaccuracies. The second is that relatively few students experience a change in status as a result of the CEP. In Appendix Table A.5

we show results from a supplementary analysis in which we randomly assign schools CEP status *regardless of eligibility* while holding the total number of students affected by the policy constant at the upper bound of 5.3 percent. This allows us to disentangle the two mechanisms because the scope of the CEP is held constant in terms of the number of students who experience a change in FRM status, but the students who experience a change are no longer concentrated in high-poverty schools.

The analysis reveals that both mechanisms play a role in limiting the impact of the CEP. However, the more important factor is the small number of students who experience a status change. We draw this conclusion because when 5.3 percent of Missouri students are switched from FRM=0 to FRM=1 by randomly switching the CEP coding status of schools, the estimated achievement gaps by FRM status and the school FRM share remain fairly close to what we report in Tables 2 and 3 (see Appendix Table A.5 for details).

5.2 School Accountability

Table 4 shows results from school-level value-added models of math achievement for students in grades 3-8 (substantively similar results for ELA are available in Appendix Table A.6). To illustrate the effect of the CEP on value-added, we report the average percentile ranking of schools that actually adopted the CEP in the first year in Missouri under different data conditions. We also report the number of these schools that are in the top quintile of value-added rankings. Results are shown only for first-year CEP adopters (matching the first pseudo-coded scenario) because inference from Table 4 is confounded if the number of focal schools changes across columns. The table is structured so that in each column, ranking outcomes for the same set of schools are reported.

Table 4 shows that the CEP boosts estimated value-added for participating schools. As discussed in Section 4, the mechanism is that for a school that moves to CEP status, the predicted performance of their students declines per equation (3). This is because more of the students at the school are coded as FRM eligible and the FRM-eligible school share goes to 1.0. As a result, the actual performance of these students relative to predicted performance improves, which benefits the school in the value-added calculation. Comparing the results in the first and last columns of Table 4, where inference is cleanest, we find that the average percentile rank of year-1 CEP adopters in the actual pre-CEP data is 48.8. But with the CEP coding rules, the average percentile rank rises just over six points, to 54.9. Similarly, the number of CEP schools in the top quintile rises from 48 to 65.

Whether the CEP-induced shift in rankings documented in Table 4 is desirable is an open question. On the one hand, at a fundamental level it is driven by a data inaccuracy, which makes it unappealing. However, in states with accountability systems that incorporate value-added, it gives a clear incentive for schools to adopt the CEP. This could be appealing to state education agencies for several reasons. First, the CEP is a federally funded program, so states interested in expanding access to federal aid for their schools should be supportive of increased take-up of the CEP. Second, the literature on universal free meals, although nascent, suggests that students benefit from these programs academically and otherwise (Dotter, 2013; Gordon and Ruffini, 2018; Schwartz and Rothbart, 2017). Finally, Parsons, Koedel, and Tan (2019) show that even value-added models that take great care to avoid bias favoring advantaged schools—like the two-step model described by equations (3) and (4)—remain at least marginally biased in favor of these schools under the most common estimation conditions. The CEP's positive effect on the rankings of low-income schools could offset some of this bias.

5.3 Comparing FRM and DC Data

Next we shift our focus to compare the predictive power of FRM and DC data in the post-CEP era (i.e., 2015-17). Because the income threshold for direct certification is lower than for FRM eligibility, the fraction of DC students is smaller (Table 1) and the population of DC students should be more impoverished than FRM-eligible students, on average.[12]

Table 5 shows results from versions of equation (5) where math achievement is the dependent variable and combinations of contemporaneous FRM and DC controls are included—we do not include the panel variables initially. The first two columns show models that include individual student FRM and DC variables separately, which allows for a clean comparison of the achievement gaps predicted by these measures without any other controls. Even in the post-CEP era, students coded as FRM-eligible have lower test scores on average than DC-coded students. Specifically, the test-score gap between FRM and non-FRM students is 0.651 student standard deviations, whereas the analogous gap between DC and non-DC students is 0.590.

Columns (3)-(4) add the school-average variables. Some of the weight on the individual measures shifts to the aggregate measures and the overall explanatory power of both models improves. The model using FRM data remains modestly more predictive of student achievement. One notable result is that the school-average DC share is a much stronger predictor of test scores than the school-average FRM share. This is driven in large part by the fact that there is less variation in the DC school share per Table 1. Specifically, the student-weighted standard deviation of the FRM school share from 2015-2017 is 0.256, whereas for the DC share it is 0.171. The implication is that the effect of a move from 0 to 100 percent coverage, which is what the coefficients capture, represents a lager change in the distribution of the DC share. This is much less of an issue with the individual FRM and DC controls, which have similar variances (see Table 1).

Finally, column (5) shows results from a model that includes the FRM and DC variables together (individual and aggregate), and column (6) further adds the other control variables in the individual and school-aggregated X vectors. The loading on the coefficients of interest in columns

[12] These differences between the measures are exacerbated by the CEP.

(5) and (6) follows from the previous columns of the table. That is, individual FRM is more predictive of student achievement than individual DC, but school-average DC is more predictive than school-average FRM. The total explanatory power of the model when both types of measures of disadvantage are included (R-squared: 0.143) is higher—but not markedly higher—than when either FRM (R-squared: 0.128) or DC (R-squared: 0.123) information is included in isolation.

As above, we also replicate the analysis in Table 5 for ELA achievement and obtain similar results, which are presented in Appendix Table A.7. However, unlike with the preceding analyses, the results from the attendance models differ somewhat from the achievement models and, as a result, are presented in Table 6. First, as is readily apparent from the R-squared values reported at the bottom of the table, FRM and DC data are less predictive of attendance than they are of student achievement.[13] In addition, Table 6 shows that whereas FRM data are marginally more predictive of achievement than DC data, the reverse is true for attendance.

Next, in Tables 7 and 8 we add the panel measures of disadvantage to the models. We continue to relegate the ELA results to the appendix (Appendix Table A.8) because of their similarity to the math results. We also suppress the coefficients for the contemporaneous individual and school-aggregated FRM and DC measures for presentational convenience. A notable feature of the models shown in Tables 7 and 8—which is not observable given this suppression—is that the panel variables, in addition to improving the predictive power of the models overall, predominantly take the weight off the contemporaneous individual FRM and DC variables. This is intuitive because with the panel variables in place, the contemporaneous individual measures capture only the marginal value of current status conditional on cumulative status.

[13] This is also the case in the earlier analyses and can be seen by comparing Tables 2 and 3 to their attendance analogs in the appendix.

While it is true that the panel variables increase the predictive power of the models, the increase is generally modest, especially in the richest specifications. For example, the full model in Table 5 explains 24.4 percent of the variance in math achievement, whereas the full model in Table 7—inclusive of the panel variables for each disadvantage measure—explains 25.3 percent of the variance, less than one percentage point more. A substantively similar pattern is present in Tables 6 and 8 for the attendance outcome. The limited gains in total explanatory power afforded by the panel variables is somewhat surprising but suggests that using multiple measures of disadvantage serves largely the same function as using panel variables of the same measure—i.e., to provide a more complete picture of student disadvantage.[14]

We conclude with an accounting of implied achievement and attendance gaps between students who differ in various ways by FRM and DC conditions based on the results in Tables 5-8. The most basic comparison—between students individually coded as either FRM or DC eligible—as in the first two columns of Tables 5 and 6, is straightforward. We also compare students who differ by measured gaps in the school aggregates and panel variables. Because the DC and FRM aggregate and panel variables differ in their distributions, we consider two types of comparisons: one based on absolute changes in these variables and another based on distributional changes. Specifically, we compare FRM- and DC-based gaps that differ by the individual student's own coded status, plus either a 0.50 change in the school-average share (i.e., reflecting a hypothetical move between schools that are 25 and 75 percent FRM or DC) or a one-standard deviation change in the school-average share (i.e., 0.256 for FRM and 0.171 for DC, per Table 1). We then further compare students who

[14] Recall that the informational content of the panel variables changes over time as the CEP persists and as we get further away from the first year of DC data (2013). We examine the sensitivity of our findings to this data issue in Appendix Table A.9 (focused on the math achievement models) by analyzing just the last year of our data panel (2017) when the CEP effect would be most pronounced and the DC data most complete. The results are very similar to what we report in the text using the entire post-CEP portion of the data panel.

differ by 1.0 in the FRM or DC panel variables and by one standard deviation of these variables, respectively (in this case the standard deviations are much closer per Table 1: 0.443 for FRM and 0.430 for DC).[15]

In summary, we make the following five comparisons between:

1. Students who differ by individual FRM or DC coded status (based on estimates from columns (1) and (2) of Tables 5 and 6).
2. Students who differ by individual FRM or DC coded status and by *0.50* in the share of students at the school coded as either FRM or DC (based on estimates from columns (3) and (4) of Tables 5 and 6).
3. Students who differ by individual FRM or DC coded status and by *one standard deviation* in the share of students at the school coded as either FRM or DC (based on estimates from columns (3) and (4) of Tables 5 and 6).
4. Students who differ by individual FRM or DC coded status, by *0.50* in the share of students at the school coded as either FRM or DC, and by *1.0* in the panel FRM or DC measure (based on estimates from columns (1) and (2) of Tables 7 and 8).
5. Students who differ by individual FRM or DC coded status, by *one standard deviation* in the share of students at the school coded as either FRM or DC, and by *one standard deviation* in the panel FRM or DC measure (based on estimates from columns (1) and (2) of Tables 7 and 8).

Results for these comparisons are shown in Figures 3 and 4 for mathematics achievement and attendance, respectively.[16] We also illustrate changes to the explanatory power of the models as we add more variables within each measure type (FRM or DC).

First, Figure 3 visualizes the broad point that FRM and DC data are similarly informative about student disadvantage with perhaps a slight edge going to the FRM data. Comparisons (2) and (4) from the list above suggest that the achievement gaps by DC are larger, but as is made clear in the analogous comparisons (3) and (5), this is due to the aforementioned differences in the distributions of the school-aggregate FRM and DC variables. In contrast, Figure 4 indicates that DC

[15] For the panel variables, an absolute change of 1.0 is most informative because, as noted previously, most of the weight on the contemporaneous individual FRM and DC variables shifts to the panel variables when they are both included in the specifications simultaneously (results available from the authors upon request).

[16] The comparisons in the figure can be reproduced based on the results shown in Tables 5-8, with the exception that several coefficients in Tables 7 and 8 are suppressed for ease of presentation. These coefficients are available from the authors upon request.

data are more effective in predicting gaps in student attendance. However, in terms of policy significance the differences in the predictive power of FRM and DC data in Figure 4 are modest—e.g., note the compressed scale on the vertical axes of the graphs.

6. Extensions

6.1 District-Level CEP Adoptions

In our examination of the effect of the CEP on FRM data, the upper bound condition in pseudo-coded scenario 3 is established based on the eligibility of individual schools. In this section we assess the sensitivity of our findings to reconstructing the upper-bound scenario 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. However, the maximum effect of the increased heterogeneity facilitated by district-level adoptions is surely less than what we show in Appendix Table A.5, where we randomly assign CEP status to schools statewide.

We report the results from this exercise in Table 9, which are analogous to what we report for the full models under pseudo-coded scenario 3 in Tables 2 and 3. Table 9 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 as well to states with large districts (such as Florida and Maryland), although again we note that the results in

Appendix Table A.5 will more than bound the effect of extra heterogeneity among CEP schools owing to district-level adoptions, even in large-district states.

6.2 High Schools

Thus far we have focused on students in grades 3-8. Here we extend the analysis to students in high school 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.[17]

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 student population. However, in Missouri we see no evidence of this. For example, if high school students are less likely to enroll in meal programs conditional on the circumstances of their families, the translation between DC and FRM should be weaker in high school. But this is not what we see. Specifically, as noted previously, in schools covering grades 3-8 those with at least a 40 percent DC share 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. It may still be that social stigma affects whether students actually *receive* their free and reduced-price meals, but in terms of data quality there is no indication that FRM eligibility is underreported among high school students when benchmarked against DC status.

Noting this similarity across schooling levels, our investigation of high schools does uncover two substantive 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

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

in Missouri high schools are FRM eligible (Appendix Table A.11), compared to 51.2 percent of students in lower grades (per Table 2).[18] A possible explanation for this result—conditional on the finding above that the mapping between DC and FRM data is similar in high school—is that families' circumstances improve as their children age (it is beyond the scope of our paper to delve deeper into this finding).

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 thus adopt the CEP. Only 15.2 percent of Missouri high schools are CEP-eligible based on the DC share, compared to 30.7 percent of schools covering grades 3-8 (as in Table 2). This is because the distribution of the DC 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 in the previous paragraph that high school students' families do not seem to be as impoverished; the lower variance is intuitive given that high schools pool students from multiple lower-grade schools and through this process shrink the building-level variance in student characteristics.

Findings from our analysis of high schools are reported in Appendix Tables A.10, A.11, A.12, and A.13. Tables A.10 and A.11 investigate the effect of the CEP on FRM data quality and match the structure of Table 3, using the English II EOC and student attendance as outcomes, respectively. Tables A.12 and A.13 compare the predictive power of FRM and DC data in the post-CEP era and follow on Tables 5 and 6. To ensure comparability across the analyses of the two outcomes, we restrict the sample of high schools in the attendance models to schools for which

[18] Note that the 43 percent number is from our high school analytic sample, which imposes modest restrictions as described below. In an unrestricted sample that we do not use for analysis, the FRM-eligible share of high school students is similar but marginally higher (43.4 percent).

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.[19]

All of the general insights from our analysis above carry over to the high school sample. Specifically, Tables A.10 and A.11 show the CEP has essentially no effect on the informational content of the individual FRM indicator and in the upper bound scenario reduces the coefficient on the school FRM share by about half. In Table A.12, FRM data are more predictive than DC data of student performance on the English II EOC test in the post-CEP era, while Table A.13 shows that DC data are more predictive of attendance among high school students. Our conclusion that FRM and DC data are similarly informative about student disadvantage in the post-CEP period is upheld among the high school sample.

7. Discussion: Considerations for Policymakers and Researchers

The reduced informational value of school-aggregated FRM data resulting from the CEP is problematic for accountability and education finance policies at all levels of government, which rely on building-level data to assess performance gaps and funding needs. On the one hand, our findings show that policies that continue to rely on FRM data will be less effectively targeted toward high-need students with the CEP in place. However, on the other, our post-CEP comparative analysis of FRM and DC data gives no indication that one metric is clearly preferred.

Still, a case can be made for switching to DC data because they are cheaper to collect—districts and states can essentially plug into data collected by other agencies—and subject to rigorous accountability controls (Grich, 2019). But a takeaway from our analysis is that there is no reason to panic and rush into a new system. FRM data continue to perform relatively well in terms of identifying disadvantaged students, even with the CEP in place.

[19] We drop a small number of non-standard schools from the attendance sample due to this restriction. The post-CEP models use slightly different samples depending on the outcome variable because we do not impose this restriction after 2014.

For states wishing to make the switch to DC-based poverty metrics, the transition must be managed carefully given the prevalent historical use of FRM data for accountability and—perhaps more importantly—school funding. A basic concern is that the simple statistics used in state funding formulas, like the number of disadvantaged students, are affected by switching to DC data because of the more stringent poverty threshold (see Table 1). States moving to DC-based metrics have addressed this concern by multiplying the DC share by a constant (the value of which varies across states and has been subject to debate—e.g., Grich, 2019) to better align schools' DC percentages with their pre-CEP FRM percentages. Another possible adjustment in the same spirit of maintaining pre-CEP funding levels is to increase the amount of aid targeted per DC-identified student.

A more substantive issue with a data transition is that some student populations are systematically less likely to participate in the social safety net programs that lead to direct certification—most notably Hispanic students and undocumented immigrants (Massachusetts Department of Elementary and Secondary Education, 2017; Zedlewski and Martinez-Schiferl, 2010). The Hispanic population in Missouri is small and identifying undocumented students is challenging. This makes our study ill-suited to investigate this issue empirically, but 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- to DC-based metrics. A sensible solution to this type of problem would be to bring in outside data to buttress DC data. The Massachusetts Department of Elementary and Secondary Education (2017) discusses several possibilities within the larger context of reviewing the practical considerations that go into the transition from FRM to DC data.

For researchers, our comparative analysis of FRM and DC data suggests they are similarly effective in the most common application of these data in research—to control for student disadvantage. Regarding the use of FRM data in particular, which are still the most common type of data available to researchers, our results give reason for optimism about the continued use of these data with the CEP in place. Because (a) the students whose FRM status is changed by the CEP attend high poverty schools (which limits the substantive importance of CEP-induced data inaccuracies) and (b) the CEP does not result in status changes for a large number of students, the individual FRM control performs no worse with the CEP in place than without it.

The implications are different for researchers who are additionally interested in controlling for school context using the FRM school share. That said, the fact that our analysis isolates this variable as the problem is instructive for developing an appropriate response. For example, a simple suggestion is to add an indicator variable to regression models for whether the school adopted the CEP. This will offset the effect of over-representation of FRM-eligible students in CEP schools on average and force identification of the coefficient on school-average FRM to rely on variation provided only from non-CEP schools. Researchers could also supplement school-level FRM data with additional, related data from non-education sources, such as local area information about household incomes and education levels (e.g., from the U.S. Census), although at the potential cost of coverage gaps or misalignment between outside data and district and school boundaries.

Of course these suggestions apply only to the most common application of FRM data in research, which is to control for student disadvantage in models where there is another focal parameter (e.g., the effect of an intervention). Other applications of FRM data will be influenced more by the introduction of the CEP. For example, work on student sorting and segregation along the dimension of measured poverty (by FRM status) becomes much more difficult with the CEP in place (as in Clotfelter et al., 2018). The marginal value of access to DC or other related data for this type of research in the post-CEP era is much higher than before the CEP was introduced.

8. Conclusion

Setting aside the substantive implications of the CEP, 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, at least insofar as FRM eligibility relates to consequential student outcomes. 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 also perform a comparative analysis of FRM and DC data to determine their relative efficacy in proxying for student disadvantage in the post-CEP era. DC data have been advocated as a substitute for FRM data in several articles and reports that raise concerns over the potential data-quality consequences of the CEP (Camera, 2019; Chingos, 2018; Greenberg, 2018). Our comparative analysis shows that FRM and DC data are similarly informative about student disadvantage in the post-CEP era. We also show that little is gained by combining both types of information to improve the identification of disadvantaged students, as evidenced by the small increase in the explanatory power of our models when we include both types of measures at once, as opposed to either measure individually.

We conclude with a brief note about the generalizability of our findings to other states. With regard to the effect of the CEP on FRM data, 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. Thus, other states can quickly assess the likely applicability of our findings by producing these basic summary statistics. Other contextual factors that may influence the generalizability of our findings include (a) the education governance structure in a state and (b) differences in student demographics. Regarding the education governance structure, we find no substantive differences in the upper-bound effect of the CEP in Missouri regardless of whether school or district-level adoptions are considered. This could be in part due to the “small district” structure in Missouri and thus may be less applicable to “large district” states, although this concern is limited by the fact that our results are only modestly affected even in a hypothetical case where schools are randomly assigned to CEP status (in such a way that the total number of affected students is held constant). Turning to demographics, a notable feature of Missouri is the relatively small Hispanic population, which prevents us from empirically examining the issue that Hispanic students are less likely to participate in the means-tested programs used to directly certify students. In states where the generalizability of our findings is in question, our analytic plan provides researchers with a blueprint for assessing the implications of the CEP given their own local conditions.

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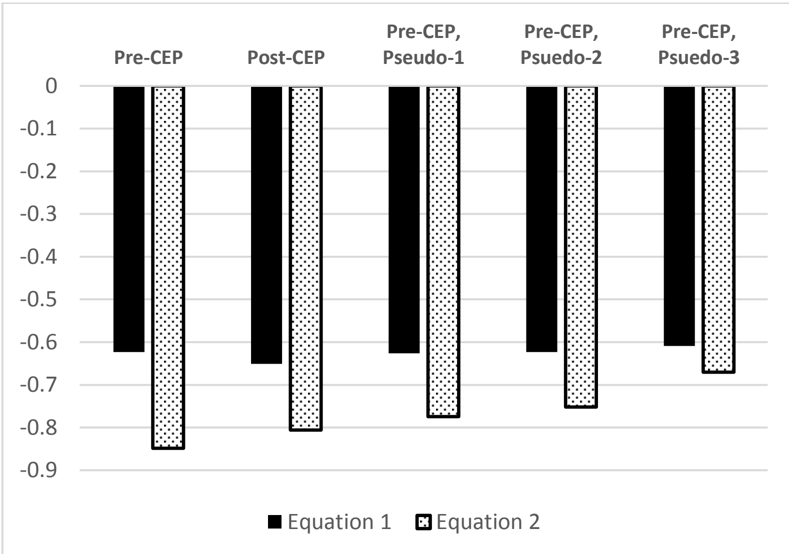
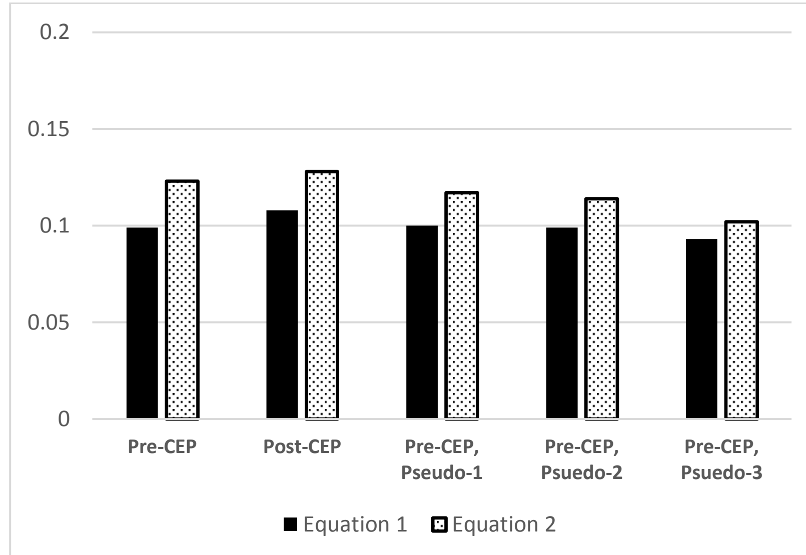
Figure 1. CEP School Counts, and CEP Coverage of Schools and Students, in Missouri Over Time for Schools with any Combination of Grades 3-8.

Notes: The graph on the left shows the number of schools with any combination of grades 3-8 (thus 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.

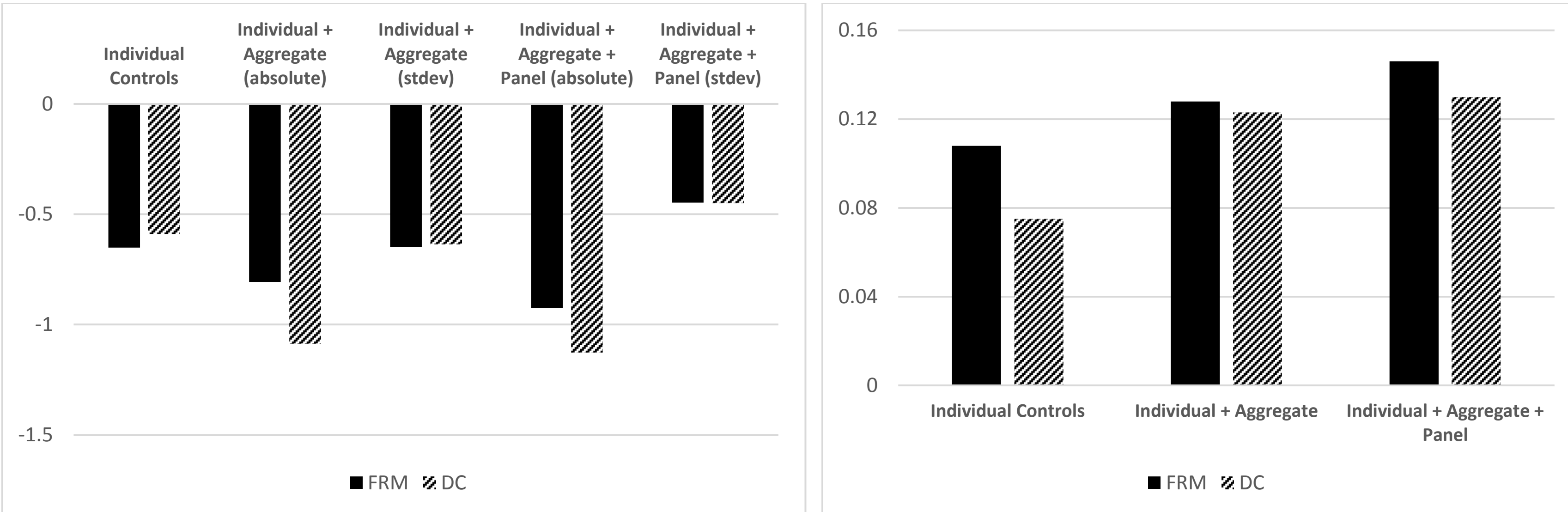
Figure 2. Predicted gaps in math achievement between students who differ by FRM status and FRM school conditions (left), and the overall predictive power of the sparse math achievement models shown by equations (1) and (2) (right), under various CEP conditions.





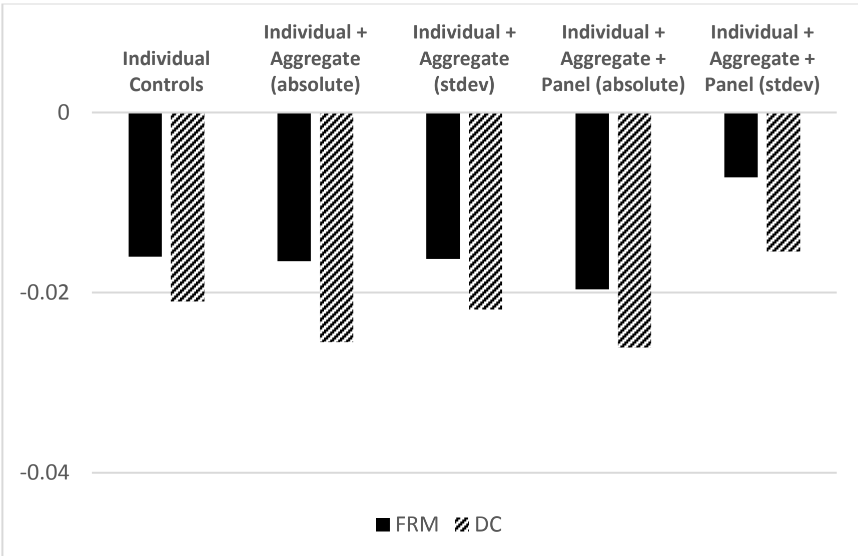
Notes: The graph on the left shows math achievement gaps as estimated by sparse versions of equations (1) and (2) (i.e., without the X-vector controls) under various CEP conditions. The gaps from equation (1) compare a FRM-eligible student to an ineligible student. The gaps from equation (2) compare students who differ by own FRM eligibility and have a 50 percentage point gap in the FRM eligibility shares at their schools (i.e., $\Delta\overline{FRM} = 0.50$, which is roughly a two-standard-deviation change in the distribution of the school FRM-eligible share). The graph on the right shows R-squared values from the sparse versions of equations (1) and (2), which indicate the overall predictive power of the models over math achievement under the various CEP conditions.

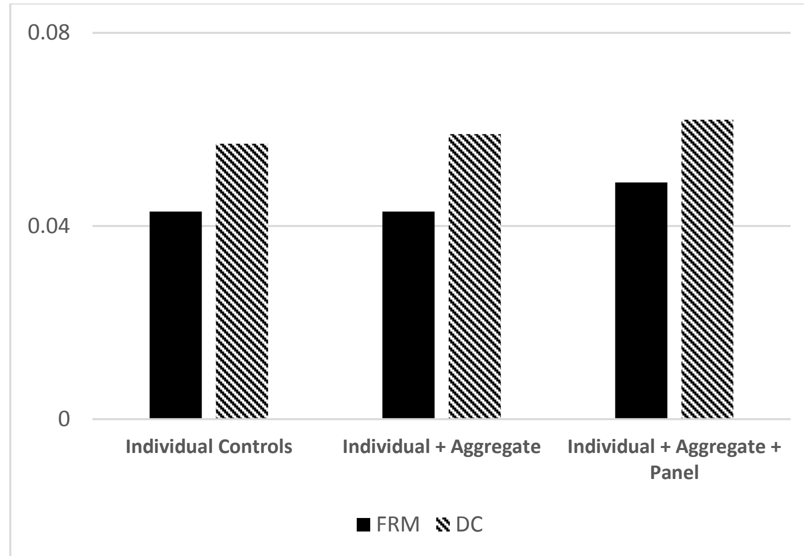
Figure 3. Predicted gaps in math achievement between students who differ by various measures of FRM and DC (left), and the overall predictive power of versions of the math achievement model shown in equation (5) using FRM versus DC data (right), during the post-CEP era.



Notes: The graph on the left shows math achievement gaps estimated using FRM or DC information from versions of equation (5). We make five comparisons as described by Section 5.3 in the text. These are between students who: (1) differ by individual FRM or DC coded status, (2) differ by the individual FRM or DC coded status and by *0.50* in the share of students at the school coded as either FRM or DC, (3) differ by the individual FRM or DC coded status and by *one standard deviation* in the share of students at the school coded as either FRM or DC, (4) differ by the individual FRM or DC coded status, by *0.50* in the share of students at the school coded as either FRM or DC, and by *1.0* in the panel FRM or DC measure, and (5) differ by the individual FRM or DC coded status, by *one standard deviation* in the share of students at the school coded as either FRM or DC, and by *one standard deviation* in the panel FRM or DC measure. The graph on the right shows R-squared values from versions of equation (5) that include grade and year fixed effects and either the FRM or DC information indicated by the labels on the horizontal axis.

Figure 4. Predicted gaps in attendance rates between students who differ by various measures of FRM and DC (left) and the overall predictive power of versions of the attendance model shown in equation (5) using FRM versus DC information (right), during the post-CEP era.





Notes: These graphs are analogs to the graphs in Figure 3 but based on the models of attendance rates instead of math achievement. See notes to Figure 3.

Table 1. Descriptive Statistics

	Pre-CEP Years 2012-14	Post-CEP Years 2015-17
Student Outcomes	Mean (stdev)	Mean (stdev)
Standardized Math Score	0.016 (0.989)	0.010 (0.991)
Standardized Reading Score	-0.006 (0.986)	-0.018 (0.988)
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)	0.512 (0.500)	0.529 (0.499)
FRM School Share (student weighted)	0.507 (0.227)	0.523 (0.256)
Attends CEP School	0	0.116 (0.321)
Direct Certification Status*	0.279 (0.449)	0.300 (0.458)
Direct Certification School Share* (student weighted)	0.275 (0.165)	0.295 (0.171)
Panel FRM	-	0.528 (0.443)
Panel DC	-	0.319 (0.430)
N (Schools)	1748	1737
N (Student-Years)	920541	916760

Notes: Data on direct certification status are only available—and thus only reported in the table—for the school years 2012-13 to 2016-17. For the analysis, we use the panel variables in the post-CEP years only and thus report descriptive statistics for these variables in just these years.

Table 2. Estimates of the Math 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	-0.623	-0.442	-0.651	-0.469	-0.626	-0.441	-0.623	-0.438	-0.609	-0.427
	(0.011)***	(0.007)***	(0.013)***	(0.009)***	(0.012)***	(0.008)***	(0.012)***	(0.008)***	(0.013)***	(0.009)***
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	0.099	0.229	0.108	0.225	0.100	0.228	0.099	0.228	0.093	0.225
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.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 3. Estimates of the Math Achievement Gap in Grades 3-8 by FRM Coding Status of Individual Students and Schools, 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	-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	-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.
***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 4. The Effect of CEP Coding on School Accountability Based on Value-Added to Test Scores in Mathematics, Grades 3-8.

	Pre-CEP	Post-CEP	Pre-CEP with Pseudo-Coding 1
Average percentile ranking in the school distribution of year-1 CEP adopters (in 2014-2015) using VAM for all students	48.8	51.3	54.9
Number of year-1 CEP adopters (in 2014-2015) ranked in the top quintile using VAM for all students	48	56	65

Notes: Ranking outcomes reported for 231 schools that adopted the CEP in 2015 (per Figure 1).

Table 5. Estimated Math Achievement Gaps in Grades 3-8 Using FRM and DC Information During the Post-CEP Period (Years 2015-17).

	(1)	(2)	(3)	(4)	(5)	(6)
Individual FRM	-0.651		-0.484		-0.380	-0.273
	(0.013)***		(0.006)***		(0.006)***	(0.004)***
Individual DC		-0.590		-0.402	-0.197	-0.164
		(0.011)***		(0.006)***	(0.004)***	(0.003)***
FRM School Share			-0.644		0.096	-0.052
			(0.034)***		(0.068)	(0.075)
DC School Share				-1.369	-1.189	-0.892
				(0.041)***	(0.097)***	(0.114)***
Other Controls						Y
R-Squared	0.108	0.075	0.128	0.123	0.143	0.244
N (Students)	909974	909974	909974	909974	909974	909974

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. ***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 6. Estimated Attendance Rate Gaps in Grades 3-8 Using FRM and DC Information During the Post-CEP Period (Years 2015-17).

	(1)	(2)	(3)	(4)	(5)	(6)
Individual FRM	-0.016		-0.016		-0.007	-0.008
	(0.000)***		(0.000)***		(0.000)***	(0.000)***
Individual DC		-0.021		-0.020	-0.016	-0.015
		(0.000)***		(0.000)***	(0.000)***	(0.000)***
FRM School Share			-0.001		0.0117	0.0170
			(0.001)		(0.002)***	(0.002)***
DC School Share				-0.011	-0.028	-0.019
				(0.002)***	(0.004)***	(0.004)***
Other Controls						Y
R-Squared	0.043	0.057	0.043	0.059	0.063	0.072
N (Students)	916760	916760	916760	916760	916760	916760

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. ***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 7. Estimated Math Achievement Gaps in Grades 3-8 Using FRM and DC Information During the Post-CEP Period (Years 2015-17), Inclusive of Panel Measures.

	(1)	(2)	(3)	(4)
Panel FRM	-0.616		-0.511	-0.415
	(0.013)***		(0.011)***	(0.009)***
Panel DC		-0.445	-0.108	-0.092
		(0.009)***	(0.007)***	(0.006)***
Contemporary Individual and School FRM Share	Y		Y	Y
Contemporary Individual and School DC Share		Y	Y	Y
Other Controls				Y
R-squared	0.146	0.130	0.156	0.253
N (students)	909974	909974	909974	909974

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. ***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 8. Estimated Attendance Rate Gaps in Grades 3-8 Using FRM and DC Information During the Post-CEP Period (Years 2015-17), Inclusive of Panel Measures.

	(1)	(2)	(3)	(4)
Panel FRM	-0.017		-0.010	-0.011
	(0.000)***		(0.000)***	(0.000)***
Panel DC		-0.014	-0.008	-0.008
		(0.000)***	(0.000)***	(0.000)***
Contemporary Individual and School FRM Share	Y		Y	Y
Contemporary Individual and School DC Share		Y	Y	Y
Other Controls				Y
R-squared	0.049	0.062	0.067	0.077
N (students)	916760	916760	916760	916760

Notes: All models include grade and year fixed effects. Standard errors clustered by school reported in parentheses. ***/**/* indicates statistical significance at the 1/5/10 percent level.

Table 9. Upper Bound Effect 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 (Matches Table 2)		Pre-CEP Pseudo-Coding 3 (Matches Table 3)	
	(1)	(2)	(3)	(4)
FRM	-0.609	-0.420	-0.477	-0.352
	(0.013)***	(0.009)***	(0.006)***	(0.004)***
FRM School Share			-0.397	-0.237***
			(0.034)***	(0.031)
Other Controls		Y		Y
Share of Students FRM	56.3%		56.3%	
Share of Schools CEP	25.3%		25.3%	
R-Squared	0.093	0.223	0.102	0.227
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	-0.607	-0.433	-0.656	-0.482	-0.607	-0.429	-0.604	-0.426	-0.588	-0.412
	(0.010)***	(0.007)***	(0.012)***	(0.009)***	(0.011)***	(0.008)***	(0.011)***	(0.008)***	(0.012)***	(0.008)***
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	0.097	0.260	0.115	0.252	0.097	0.258	0.096	0.258	0.090	0.255
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 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	-0.018	-0.017	-0.016	-0.015	-0.018	-0.017	-0.017	-0.016	-0.017	-0.016
	(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 notes to Table 2 apply.

Appendix Table A.3. Estimates of the English Language Arts Achievement Gap in Grades 3-8 by FRM Coding Status of Individual Students and Schools, 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	-0.462	-0.340	-0.508	-0.381	-0.467	-0.342	-0.467	-0.342	-0.466	-0.338
	(0.005)***	(0.003)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***	(0.006)***	(0.004)***
FRM School Share	-0.709	-0.586	-0.569	-0.505	-0.552	-0.436	-0.510	-0.390	-0.353	-0.247
	(0.030)***	(0.027)***	(0.031)***	(0.035)***	(0.031)***	(0.030)***	(0.031)***	(0.030)***	(0.028)***	(0.024)***
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	0.118	0.272	0.131	0.262	0.112	0.266	0.109	0.265	0.097	0.259
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 notes to Table 3 apply.

Appendix Table A.4. Estimates of the Attendance Rate Gap in Grades 3-8 by FRM Coding Status of Individual Students and Schools, 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	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016	-0.017
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
FRM School Share	-0.010	-0.001	-0.001	0.006	-0.006	0.003	-0.005	0.004	-0.001	0.006
	(0.002)***	(0.001)	(0.001)	(0.001)***	(0.002)***	(0.001)***	(0.002)***	(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	0.052	0.064	0.043	0.055	0.050	0.062	0.050	0.061	0.045	0.059
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 notes to Table 3 apply.

Appendix Table A.5. Estimates of the Math Achievement Gap in Grades 3-8 by FRM Coding Status, Pseudo-Coded Scenario-3, but with Random Assignment of Implementation of the CEP across Schools.

	Pre-CEP Pseudo-Coding 3, Random Pseudo-Coding, Equation 1		Pre-CEP Pseudo-Coding 3, Random Pseudo-Coding, Equation 2	
	(1)	(2)	(3)	(4)
FRM	-0.570	-0.398	-0.468	-0.345
	(0.013)***	(0.010)***	(0.006)***	(0.004)***
FRM School Share			-0.364	-0.197
			(0.045)***	(0.039)***
Other Controls		Y		Y
Share of Students FRM	56.5%		56.5%	
Share of Schools CEP	30.7%		30.7%	
R-Squared	0.082	0.221	0.088	0.225
N(Students)	916461	916461	916461	916461

Notes: This table replicates the results in columns (9) and (10) of Tables 2 and 3, except schools are randomly assigned as CEP switchers, rather than using CEP eligibility rules. The results can be compared to the results in Tables 2 and 3 to assess the extent to which the concentration of miscoded students at high-poverty schools reduces the impact of the CEP on model performance. The notes to Tables 2 and 3 apply.

Appendix Table A.6. The Effect of CEP Coding on School Accountability Based on Value-Added to Test Scores in English Language Arts, Grades 3-8.

	Pre-CEP	Post-CEP	Pre-CEP with Pseudo-Coding 1
Average percentile ranking in the school distribution of year-1 CEP adopters (in 2014-2015) using VAM for all students	49.3	51.3	55.5
Number of year-1 CEP adopters (in 2014-2015) ranked in the top quintile using VAM for all students	49	49	69

Notes: This table replicates the results reported in Table 4 in the main text, but using value-added to English language arts achievement. The notes to Table 4 apply.

Appendix Table A.7. Estimated English Language Arts Achievement Gaps in Grades 3-8 Using FRM and DC Information During the Post-CEP Period (Years 2015-17).

	(1)	(2)	(3)	(4)	(5)	(6)
Individual FRM	-0.656		-0.508		-0.405	-0.293
	(0.012)***		(0.006)***		(0.006)***	(0.004)***
Individual DC		-0.591		-0.414	-0.195	-0.170
		(0.010)***		(0.006)***	(0.004)***	(0.003)***
FRM School Share			-0.569		0.159	0.026
			(0.031)***		(0.070)**	(0.074)
DC School Share				-1.285	-1.171	-0.972
				(0.035)***	(0.099)***	(0.110)***
Other Controls						Y
R-Squared	0.115	0.080	0.131	0.123	0.145	0.271
N (Students)	914834	914834	914834	914834	914834	914834

Notes: This table replicates the analysis in Table 5 from the main text but using English language arts achievement as the outcome. The notes to Table 5 apply.

Appendix Table A.8. Estimated English Language Arts Achievement Gaps in Grades 3-8 Using FRM and DC Information During the Post-CEP Period (Years 2015-17), Inclusive of Panel Measures.

	(1)	(2)	(3)	(4)
Panel FRM	-0.641		-0.537	-0.443
	(0.014)***		(0.011)***	(0.009)***
Panel DC		-0.466	-0.109	-0.100
		(0.009)***	(0.007)***	(0.006)***
Contemporary Individual and School FRM Share	Y		Y	Y
Contemporary Individual and School DC Share		Y	Y	Y
Other Controls				Y
R-squared	0.151	0.130	0.160	0.281
N (students)	914834	914834	914834	914834

Notes: This table replicates the analysis in Table 7 from the main text but using English language arts achievement as the outcome. The notes to Table 7 apply.

Appendix Table A.9. Replication of Full-Model Math Achievement Results in Table 7 Using Only the Final Year of the Data Panel (2017).

	(1)	(2)
Panel FRM	-0.500	-0.405
	(0.013)***	(0.012)***
Panel DC	-0.126	-0.106
	(0.010)***	(0.009)***
Contemporary Individual and School FRM Share	Y	Y
Contemporary Individual and School DC Share	Y	Y
Other Controls		Y
R-squared	0.154	0.255
N (students)	306646	306646

Notes: This table extends the analysis in Table 7 in the main text. The notes to Table 7 apply.

Table A.10. Estimates of the Achievement Gap in High School on the English End of Course Test by FRM Coding Status of Individual Students and Schools, 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	-0.438	-0.322	-0.443	-0.317	-0.447	-0.328	-0.448	-0.329	-0.452	-0.332
	(0.013)***	(0.009)***	(0.013)***	(0.009)***	(0.013)***	(0.010)***	(0.014)***	(0.010)***	(0.013)***	(0.009)***
FRM School Share	-0.707	-0.890	-0.454	-0.668	-0.423	-0.603	-0.398	-0.575	-0.371	-0.549
	(0.063)***	(0.064)***	(0.098)***	(0.094)***	(0.076)***	(0.074)***	(0.072)***	(0.072)***	(0.066)***	(0.067)***
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	0.167	0.286	0.164	0.280	0.156	0.276	0.156	0.276	0.156	0.276
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.

Table A.11. Estimates of the Attendance Gap in High School by FRM Coding Status of Individual Students and Schools, 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	-0.034	-0.034	-0.034	-0.034	-0.036	-0.035	-0.036	-0.036	-0.036	-0.036
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
FRM School Share	-0.028	0.005	-0.011	0.014	-0.016	0.013	-0.014	0.014	-0.013	0.015
	(0.008)***	(0.006)	(0.006)*	(0.004)***	(0.007)**	(0.006)**	(0.007)**	(0.006)**	(0.007)*	(0.006)***
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	0.055	0.072	0.056	0.070	0.055	0.072	0.055	0.072	0.055	0.072
N(Students)	795723	795723	795260	795260	795723	795723	795723	795723	795723	795723

Notes: This table is an analog to Appendix Table A.4 above. The notes to Table A.4 apply.

Table A.12. Estimates of the Achievement Gap in High School on the English End of Course Test Using FRM and DC Information During the Post-CEP Period (Years 2015-16).

	(1)	(2)	(3)	(4)	(5)	(6)
Individual FRM	-0.539		-0.443		-0.365	-0.251
	(0.025)***		(0.013)***		(0.013)***	(0.009)***
Individual DC		-0.500		-0.374	-0.164	-0.140
		(0.020)***		(0.013)***	(0.011)***	(0.009)***
FRM School Share			-0.454		0.374	0.117
			(0.098)***		(0.248)	(0.183)
DC School Share				-1.373	-1.605	-1.682
				(0.146)***	(0.427)***	(0.378)***
Other Controls						Y
R-Squared	0.155	0.128	0.164	0.157	0.175	0.289
N (Students)	128245	128245	128245	128245	128245	128245

Notes: This table is an analog to Table 5 in the main text. The notes to Table 5 apply.

Table A.13. Estimated Attendance Gap in High School Using FRM and DC Information During the Post-CEP Period (Years 2015-17).

	(1)	(2)	(3)	(4)	(5)	(6)
Individual FRM	-0.037		-0.034		-0.020	-0.020
	(0.001)***		(0.001)***		(0.001)***	(0.001)***
Individual DC		-0.047		-0.041	-0.030	-0.030
		(0.001)***		(0.001)***	(0.001)***	(0.001)***
FRM School Share			-0.011		0.051	0.054
			(0.006)*		(0.008)***	(0.008)***
DC School Share				-0.054	-0.115	-0.085
				(0.010)***	(0.019)***	(0.018)***
Other Controls						Y
R-Squared	0.055	0.063	0.056	0.070	0.079	0.089
N (Students)	795260	795260	795260	795260	795260	795260

Notes: This table is an analog to Table 6 in the main text. The notes to Table 6 apply.