Free and reduced-price meal eligibility does not measure student poverty: Evidence and policy significance

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Free and reduced-price meal (FRM) eligibility is commonly used in education research and policy applications as an indicator of student poverty. However, using multiple data sources external to the school system, we show that FRM status is a poor proxy for poverty, with eligibility rates far exceeding what would be expected based on stated income thresholds for program participation. This is true even without accounting for community eligibility for free meals, although community eligibility has exacerbated the problem in recent years. Over the course of showing the limitations of using FRM data to measure poverty, we provide promising validity evidence for a new, publicly-available measure of school poverty based on local-area family incomes.

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1. Introduction

Free and reduced-price meal (FRM) eligibility plays a central role in identifying high-poverty students in U.S. education policy. For example, under the federal Every Student Succeeds Act (ESSA), the accountability systems of all fifty states plus Washington DC track gaps in student achievement by poverty status, and 84 percent (all but six states and Washington DC) use FRM data to identify high-poverty students. FRM data are also used to allocate federal, state, and local funding with the goal of targeting resources toward schools serving low-income children.[1] The scholarly community is similarly reliant on FRM data to identify high-poverty students for a variety of research-based applications (Domina et al., 2018).

Noting that FRM data are commonly used in these roles, it is well-understood that FRM designations are error-prone, blunt indicators of poverty and obscure wide variation in income within FRM status bins (Domina et al., 2018; Harwell and LeBeau, 2010; Michelmore and Dynarski, 2017; Parsons, Koedel, and Tan, 2019). There is also evidence that FRM status is awarded to more students than income-eligibility thresholds would imply. For example, Bass (2010) shows disparate trends in youth poverty rates measured inside and outside of schools from the 1970s through the early 2000s. He also provides evidence from district audits showing that for many FRM-designated students, proof of income does not materialize when requested. Domina et al. (2018) link FRM data to IRS tax records and show that FRM status is awarded to more students than the income data suggest should be eligible.

We complement these previous studies by using two new data sources to evaluate the accuracy of FRM data for measuring student poverty. The first data source is administrative records on student direct certification (DC) status. DC data capture participation in social service programs outside of public schools. Participation in these programs is tied to income-eligibility and more carefully vetted than participation in the National School Lunch Program (NSLP). The direct certification criteria in our context—the state of Missouri—are such that in expectation,

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[1] According to data compiled by EdBuild, 33 states use FRM data to allocate increased funding toward students from low-income households (e.g., see here, retrieved on 04.11.2021: http://funded.edbuild.org/reports).

the share of DC students in a school should match the share of students at or below 130 percent of the poverty line, which is the stated threshold for *free*-meal eligibility under the NSLP. The second data source is recently-developed "school neighborhood poverty" (SNP) metrics made available by the National Center for Education Statistics (NCES). These metrics are estimated for schools using data on incomes of nearby households and were first made available by the NCES in 2016.

Both of these data sources are independently promising for measuring poverty. We increase our confidence in their reliability by successfully validating them against each other. After the validation, we use them to assess the accuracy of FRM-based povery designations. Our findings complement previous work by Bass (2010) and Domina et al. (2018) by showing that FRM data overstate poverty; moreover, we provide plausible estimates of the extent of the overstatement. Our data additionally allow us to look at free-meal and reduced-meal designations separately, which reveals the further insight that most of the oversubscription of students in the NSLP is in the "free meal" category. We also show that the oversubscription existed prior to the NSLP's community eligibility provision (CEP), although the CEP has made it worse.

We then extend our analysis to estimate oversubscription in the NSLP in 27 other states using our NCES-based poverty estimates, which can be constructed for most schools in the U.S. We combine these estimates with FRM eligibility rates from the Common Core of Data (CCD, also from NCES). On average, we estimate the rate of oversubscription in the NSLP nationally is similar to the rate in Missouri. We also uncover substantial heterogeneity across states. Some of the heterogeneity is surely real—presumably the product of differences across states in districts' FRM certification processes and/or the willingness of families to participate—but some may be attributable to errors in the CCD.

Our findings contribute to a thin literature on a topic of great importance for contemporary education research and policy in the United States. We provide the most comprehensive evidence to date that FRM data do not measure poverty in public schools accurately. There are several possible explanations for the persistent overstatement of poverty rates in FRM data, with a prominent one being that districts are incentivized to identify students as FRM-eligible but are not similarly incentivized to do so accurately.

A supplementary contribution of our study is that to the best of our knowledge, we provide the first external validity evidence for the NCES' school neighborhood poverty (SNP) estimates. These estimates are publicly available for most schools in the U.S. and estimated using a common methodology nationwide. Compared to FRM data, which in addition to being inaccurate are also subject to different administration and reporting processes across districts and states (see below), the SNP estimates are appealing measures of poverty.

It is also worth noting that although FRM data do not measure student poverty accurately, they do measure student disadvantage, broadly defined.[2] The distinction between measuring poverty and disadvantage is important in the modern context of U.S. education policy for two reasons. First, there are high stakes attached to poverty measurement in U.S. schools for funding and accountability, which implies that measurement ambiguity can have important consequences for districts, schools, and ultimately students. Second, the common acceptance of the idea that FRM data have historically served to measure poverty is hampering the use of modern data systems to develop new and more accurate poverty and disadvantage metrics. For example, old rates of FRM eligibility are being used as benchmarks for assessing the accuracy of new metrics (e.g., see Croninger, Rice, and Checovich, 2015; Grich, 2019; Massachusetts Department of Elementary and Secondary Education, 2017). Given that the old metrics are inaccurate, this type of benchmarking is impeding progress.[3] A related concern is that there is policy resistance to the idea of moving away from a basic poverty designation and toward the more nebulous—but arguably more useful—concept of disadvantage. However, as our results make clear, FRM data have always captured the more nebulous concept of student disadvantage, albeit under the guise

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[2] Domina et al. (2018) provide concrete evidence on this point; also see Harwell and LeBeau (2010) for additional discussion of the distinction between measuring poverty and disadvantage.

[3] In addition, in some states CEP-adopting schools and districts that no longer collect FRM data are being forced to report poverty using alternative metrics. If these schools and districts do not build measures that match (high) FRM-based poverty rates, it can put them at a disadvantage in state funding and other policies, even if the FRM-based rates are not correct. Gindling et al. (2018) provides a useful case study in Baltimore City Public Schools.

of measuring poverty and without the benefit of using all available information to measure disadvantage as effectively as possible.

2. Background

FRM eligibility for individual students under the NSLP is determined by school districts. Districts assess eligibility in two ways. First, students can be "directly certified" for free meals if they participate in a qualified federal assistance program such as the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), or the Food Distribution Program on Indian Reservations (FDPIR). In addition, foster, migrant, runaway, and homeless youth—and in some states, additional income groups like students eligible for Medicaid—can also be directly certified (Gindling et al., 2018; Greenberg, 2018; Greenberg, Blagg, and Rainer, 2019).

Second, school districts administer income surveys to parents, and students can be classified as eligible for free or reduced-price meals based on the survey responses. Students from families with incomes at or below 130 percent of the federal poverty line are eligible for free meals, and those from families with incomes between 130 and 185 percent of the poverty line are eligible for reduced-priced meals. Districts are incentivized to encourage and approve parent applications because they receive meal subsidies for FRM-eligible students and can gain access to additional federal, state, and local funding. Parents' incentives are also aligned—they benefit because participation in the NSLP lowers the cost of food for their children.

Only a very small fraction of NSLP applications go through an income verification process (Bass, 2009).[4] In fact, according to the USDA's Eligibility Manual for School Meals in 2017, attempting to verify more than three percent of applications without special cause is prohibited.[5] In instances where income-eligibility cannot be verified—which is quite common,

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[4] See NSLP Verification Toolkit from USDA. Retrieved on 03.30.2021 from https://www.fns.usda.gov/cn/verification-toolkit

[5] The 2017 version of USDA's Eligibility Manual for School Meals says, "With the exception of verification for cause, LEAs must not verify more or less than the standard sample size or the alternate sample size (when the alternate sample size is used). LEAs must not verify all (100 percent) of the applications." Verification for cause can

up to 50 percent of applications (Burghardt, Silva, and Hulsey, 2004)—FRM status is cancelled, but there are no other repercussions. The incentive structure clearly favors districts and parents stretching the boundaries of eligibility. We do not take a normative stance on whether this is good or bad from a policy perspective; but for the purpose of relying on FRM data to measure poverty, the incentive structure is cause for concern.[6]

Figure 1 updates a similar figure in Bass (2010) using data from the Digest of Education Statistics through 2018 (de Brey et al., 2021). It plots the national share of FRM-eligible students and the share of school-aged children living at or below the poverty line. The former data are collected by school districts as described above; the latter are based on data from the U.S. Census.[7] The income thresholds corresponding to these poverty definitions are different—i.e., the stated FRM-eligibility threshold is at 185 percent of the poverty line—which limits comparative inference to some degree. Still, the differential trends in the two poverty measures over the 2006-2018 period suggest a measurement problem. Most notably, whereas the share of children in poverty according to the Census moves with the business cycle as anticipated and increases by just 1.5 percentage points from 2006-2018, the FRM-eligible share rises throughout the sample period and increases by more than 10 percentage points over the same period.

The most closely-related study to our own is Domina et al. (2018), who merge FRM eligibility data from Oregon and a single school district in California with family income data from the IRS. These authors find disagreement in the data in both directions—i.e., seemingly FRM-eligible students based on income who are not enrolled in the program and income-ineligible students who are enrolled. However, consistent with our findings below, the latter are

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be performed if "the LEA is aware of additional income or persons in the household." This information was retrieved from the following address on 03.31.2021. https://fns-prod.azureedge.net/sites/default/files/cn/SP36_CACFP15_SFSP11-2017a1.pdf

[6] Research shows that providing all students in a school with free meal leads to better test scores (Ruffini, forthcoming; Schwartz and Rothbart, 2020), improved student discipline (Gordon and Ruffini, forthcoming), and higher wages later in life (Lundborg, Rooth, and Alex-Petersen, forthcoming). There is no evidence of increases in BMI or the probability of being obese or overweight (Davis and Musaddiq, 2019; Schwartz and Rothbart, 2020).

[7] These data are reported across several issues of the Digest of Education Statistics, the most recent of which is de Bray et al. (2021).

much more prevalent than the former. A factor that differentiates our work is that Domina et al. (2018) use highly-restricted IRS data to examine the income-eligibility question. These are excellent data but unlikely to be commonly available to researchers or education agencies. A positive attribute of our study is that our key validation metrics—Direct Certification data and the NCES' SNP measures—are more readily accessible.

The data used by Domina et al. (2018) also pre-date the CEP, which is a provision of the NSLP that allows schools and districts to provide free meals to all students if the student body is sufficiently impoverished. The CEP was introduced beginning with the 2014-15 school year. In many states, FRM-eligibility data are overwritten for CEP schools to indicate that 100 percent of students are free-meal eligible (Chingos, 2016; Greenberg, Blagg, and Rainer, 2019; Koedel and Parsons, 2021). As a result, the CEP further degrades the link between student poverty and FRM eligibility. In the analysis that follows we show that the CEP contributes to the overstatement of poverty in modern FRM data, but it is not the primary driver and even in the absence of the CEP, FRM data still greatly overstate poverty.[8]

3. Data

3.1 Missouri administrative data

We conduct our primary analysis using administrative student records from the Missouri Department of Elementary and Secondary Education (DESE) for all students enrolled in public schools during the 2015-16 and 2016-17 school years (school years are hereafter identified by the spring year—e.g., 2016 for 2015-16). We restrict our analysis to schools with at least 25 students. The most important variables in the administrative data are students' "free" and "reduced-price" meal designations (FM and RM, respectively). Through DESE, we also have access to merged administrative data indicating whether each student is directly certified to receive free meals. We refer to this combined dataset as the Missouri administrative data.

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[8] One might worry about the impact of the CEP on the FRM trend in Figure 1, but no impact is visually apparent. There are two reasons for this: (1) some states have not overwritten their individual FRM-eligibility data, dulling its impact on a national scale, and (2) even in states in which the CEP has overwritten the data, only a small fraction of the total student population changes FRM designations due to the CEP (Koedel and Parsons, 2021).

Students from households that participate in SNAP, TANF, and FDPIR, and students classified as migrant, runaway, homeless, or in foster care are categorically eligible for free meals in Missouri.[9] DESE has an agreement with Missouri Department of Health and Senior Services to provide the program-participation information necessary to directly certify these students. All Missouri districts are required to download direct certification information for their students at least three times annually to make sure all students eligible for FM through direct certification are extended the benefit. Missouri's direct certification processes are above-average among states along several measurable dimensions (Koedel and Parsons, 2021).

A feature of the direct-certification landscape in Missouri that facilitates our analysis is that these criteria should identify students living at or below 130 percent of the poverty line. The most important direct-certification criterion in this regard is SNAP participation. SNAP is the largest program that leads to direct certification and it uses the 130-percent-of-the-poverty-line cutoff to determine eligibility. This is useful because it is the same income threshold used for free meal eligibility under the NSLP.

In most other states, these income thresholds are not aligned because of broad-based categorical eligibility (BBCE) policies that allow families with higher incomes to qualify for SNAP. Missouri is one of six states without a BBCE policy. As of January 2022, the gross income limit for BBCE across states ranged from 130-200 percent of the poverty line, with most states falling in the upper end of this range (United States Deparment of Agriculture, 2022). Below we discuss the generalizability of our analysis and findings outside of Missouri given the somewhat unique circumstance that Missouri lacks BBCE. For now, we note this contextual feature of Missouri is useful for our analysis in that it conceptually aligns the stated income thresholds for direct certification and free-meal receipt via the NSLP.

Although by its intent direct certification in Missouri should identify students at or below

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[9] Information retrieved from the following address on 04.04.2022: https://dese.mo.gov/media/pdf/free-and-reduced-application-and-direct-certification-information-and-procedures-2021.

130 percent of the poverty line, this may not happen in practice. We require the following assumptions to hold in order for the share of students who are directly certified in our data to reflect the share of students who are living at or below 130 percent of the poverty line:

1. Income-eligibility requirements for the social-service programs that lead to direct certification include students up to 130 percent of the poverty line and are strictly enforced.
2. All eligible families participate in social-service programs that lead to direct certification.

It seems implausible that these assumptions are never violated (e.g., surely eligible families exist who do not participate in a social-services program that leads to direct certification). However, if violations are uncommon, the DC data will effectively measure the fraction of students living at or below 130 percent of the poverty line. We rely on our validation exercise to assess these assumptions as described below.[10]

Table 1 provides descriptive information about our sample.

3.2 NCES School Neighborhood Poverty data

Beginning in 2016, the NCES began reporting school neighborhood poverty metrics for nearly every school in the United States.[11] These metrics are based on household income data from the U.S. Census Bureau's American Community Survey (ACS) and are reported as continuous variables that measure the average income-to-poverty ratio (IPR) in a school, multiplied by 100. A value of exactly 100, for instance, indicates that the average income value is at the poverty line. A value of 200 indicates the average income is double the poverty line, and so on. The IPR metrics are described in Geverdt (2019) as capturing "economic conditions of neighborhoods where schools are located," but to be more precise, they capture the income-to-

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[10] A notable contextual feature of Missouri is the share of the population that is Hispanic is small. Research suggests that assumption 2 is likely to be violated in states/locales with large Hispanic population shares (Lichter et al., 2015; Sandstrom, Huerta, and Loprest 2014; Williams, 2013; Zedlewski and Martinez-Schiferl, 2010), but empirically, the results that follow indicate that under-participation is not problematic in Missouri.

[11] For example, in Missouri, 2,172 out of the 2,215 public schools have corresponding SNP metrics from NCES in 2016 (98 percent). In 2017, SNP metrics are available for 2,186 out of 2,219 schools (99 percent).

poverty ratio in a household that would hypothetically be situated in the exact geographic location of the school.

We elaborate briefly on the construction of the SNP metrics here and refer interested readers to Geverdt (2019) for more information. The SNP metrics are estimated using a spatial estimation process called Kriging. This method uses the weighted sum of income values in measured locations to predict values in unmeasured locations (Cressie 1989; Cressie 1993). The predicted value in the unmeasured location is estimated by the following equation (Geverdt and Nixon, 2018):

$$\hat{Z}(s_0) = \sum_{i=1}^{\mathbb{N}} \lambda_i Z\,(s_i) \tag{1}$$

where $\hat{Z}(s_0)$ is the predicted income value in the unmeasured location, $Z(s_i)$ is the value at measured location i, and λ_i is a weighting parameter. The closer that measured location i is to the unmeasured location, the larger is λ_i. The value of λ_i also depends on the covariance structure of all measured locations; i.e., the relationship between distance and income elsewhere in the data, which is modelled using a semi-variogram when calculating SNP (Geverdt and Nixon, 2018). The NCES IPR estimates for each school are based on data from the 25 households closest to the school in the American Community Survey.[12]

While conceptually appealing, whether the SNP metrics are accurate measures of poverty is unclear. Sources of potential discrepancies include the following:

1. Not all students who live near a local public school will attend that school. This will be especially problematic if selection into a school is based on income—e.g., if wealthier families in an area with high income heterogeneity send their children to private schools, this would lead to a systematic discrepancy between the incomes of children who attend the public school and the IPR value.

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[12] An issue with basic Kriging is that it assumes the relationship between the variance of the measure and distance between locations is the same throughout the sample. But that may not be true for SNP estimation conducted on a national scale covering a variety of regions and regional contexts (e.g., urban versus rural areas). NCES's SNP metrics are estimated using empirical Bayesian Kriging which addresses this problem by dividing areas into smaller regions and developing models for each region (Geverdt and Nixon, 2018). The local models take into account differences in spatial dependence across regions.

2. Estimated poverty at the exact location of a school may not reflect poverty in a school's catchment area. If homes closer or farther from schools within catchment areas have higher incomes, the estimates could be biased.

Idiosyncratic circumstances such as these surely exist in the IPR data, but whether they are common or systematic is uncertain. Our validation of the IPR and DC data against each other provides a high-level test of the accuracy of the information in both data sources.

The bottom panel of Table 1 provides basic summary statistics using various poverty measures for Missouri schools in 2016 and 2017. This includes the IPR estimate, which is the direct value reported by NCES, along with two modified versions of the IPR estimate—IPR(130) and IPR(185)—that we construct as described in the next section.

4. Methods

We begin by validating the DC and SNP data against each other. To facilitate the validation—and again noting the DC share in each school is a plausible measure of the share of students living at or below 130 percent of the poverty line—the first step is to manipulate the continuously-measured IPR values to recover estimates of the share of students living at or below this threshold. Our manipulation of IPR values relies on the assumption that they are mean values from a normal distribution. Along with their standard errors (also reported by NCES), we can use the IPR estimates to construct the distribution of income in each school under the normality assumption. Then, the fraction of the student population with incomes at or below any threshold value can be calculated directly from the cumulative distribution function. Equation (2) gives an example at the focal value of 130 percent of the poverty line:

$$IPR\widehat{(130)}_{jt} = P\ (IPR_{jt} \leq 130) = \int_{-\infty}^{130} f(IPR)dIPR \tag{2}$$

In the equation, $IPR\widehat{(130)}_{jt}$ is the estimated fraction of students in school j and year *t* with family incomes at or below 130 percent of the poverty line, and $f(IPR)$ is the probability density function (pdf) of IPR. The general form of equation (2), where X indicates a generic income value as a percent of the poverty line, can be written as:

$$\widehat{IPR(X)}_{jt} = P\ (IPR_{jt} \leq X) = \int_{-\infty}^{X} f(IPR)dIPR \tag{3}$$

With estimated $IPR(130)_{jt}$ values in hand, we validate them against schools' DC shares using the following univariate regression, weighted by school enrollment:

$$DC_{jt} = \beta_0 + \widehat{IPR(130)}_{jt}\beta_1 + \varepsilon_{jt} \tag{4}$$

In equation (4), DC_{jt} is the share of directly-certified students in school j in year t, and $\widehat{IPR(130)}_{jt}$ is the estimated value from equation (2). If both variables in this regression are measuring the same construct, on average, then the expected value of β_1 is 1.0. Deviations from 1.0 would imply systematic differences in what the two variables measure. Note that the empirical Bayesian Kriging procedure used to construct the original IPR variables embeds shrinkage, so attenuation bias in β_1 is not a concern (Chetty, Friedman, and Rockoff, 2014; Jacob and Lefgren, 2008).

A sufficient condition for recovering a value of $\beta_1 = 1.0$ is that the assumptions outlined in the previous section for each measure are satisfied; or, at least satisfied to a rough approximation. Given the assumptions for the two measures are very different substantively, it would be highly unlikely to recover an estimate of $\beta_1 = 1.0$ if either or both sets of assumptions are violated, in which case a divergence of the measures seems almost assured. Thus, the test from equation (4) of the null hypothesis that $\beta_1 = 1.0$ is a credible test of the plausibility of the assumptions that underlie *both* measures. If the measures agree, it is difficult to construct a story by which they are both wrong but the sources of errors just happen to align such that they are wrong in the same way and to the same degree.

Below we show that we fail to reject the null hypothesis that $\beta_1 = 1.0$ in equation (4) with a fairly precise confidence interval, which implies that both measures are accurate indicators of the share of students living at or below 130 percent of the poverty line, on average. Taking this as a point of departure, we then estimate the following univariate regressions, also weighted by school enrollment:

$$FM_{jt} = \delta_0 + \widehat{IPR(130)}_{jt}\delta_1 + e_{jt} \tag{5}$$

$$FM_{jt} = \gamma_0 + DC_{jt}\gamma_1 + u_{jt} \tag{6}$$

In equations (5) and (6), we regress the share of students eligible for FM in school *j* and year *t*, FM_{jt}, on the school's IPR(130) estimate and DC share, respectively. By rule, students identified as eligible for FM should include only those in households at or below 130 percent of the poverty line. Therefore, the same logic from equation (4) applies—we should anticipate that δ_1 and γ_1 have values of 1.0. Values above 1.0 would indicate that more students are designated for free-meal status than income-eligibility alone would dictate.[13] Our validation exercise suggests that these two equations are redundant—i.e., $\delta_1 \approx \gamma_1$—but we estimate both for completeness.

We also extend equation (5) to look at the threshold for free *and reduced-price* meal eligibility, which is at 185 percent of the poverty line, using equation (7):

$$FRM_{jt} = \lambda_0 + \widehat{IPR(185)}_{jt}\lambda_1 + \eta_{jt} \tag{7}$$

In equation (7), FRM_{jt} is the share of students eligible for free or reduced-price meals, and $\widehat{IPR(185)}$ is the income-aligned measure based on the SNP data. λ_1 takes on the same interpretation as δ_1 and γ_1 above—i.e., values above 1.0 indicate oversubscription of free and reduced-price meal eligibility. A caveat to this extension is that while it is motivated by the validation regression in equation (4), we do not have a comparable measure to directly validate our estimates of $IPR(185)$. We must assume that the validation of $IPR(130)$ also implies that $IPR(185)$ is an accurate measure of the fraction of families living at or below 185 percent of the poverty line. This assumption is reasonable, but we have no way of providing direct evidence to confirm or refute it.

Finally, we return to the point above that like many other states, the FRM data in Missouri are affected by the CEP. This means some high-poverty schools are coded as entirely comprised of FM students even when income-eligibility is below 100 percent. One could interpret the CEP as "biasing" upward the estimates in equations (5) and (7), although in our view the term "bias" is not appropriate because the CEP is a true source of inaccuracy in modern

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[13] There is some nuance to this interpretation—see Domina et al. (2018) for a discussion.

FRM data. Still, we assess the impact of the CEP on the estimates in equations (5) and (7) by imputing the FM_{jt} and FRM_{jt} values for CEP schools to their values during the last year prior to CEP implementation in Missouri, which was 2014. If the CEP was solely responsible for the oversubscription in FRM data, we would expect our models based on the data corrected to account for the CEP to recover coefficients on the key parameter of interest of 1.0.

5. Free and reduced-price meal eligibility does not measure student poverty

Table 2 shows results from our baseline regressions in equations (4), (5), (6), and (7). The column headers indicate the dependent variable in each model and the year for which the model is estimated (either 2016 or 2017, which are the first two years SNP metrics are available).

First, columns (1) and (2) report results from the validation regressions of the DC share on IPR(130). We cannot reject the null hypothesis that $\beta_1 = 1.0$ and our confidence intervals are precise. This is consistent with the aforementioned assumptions being upheld under which these two measures converge, at least on average.

Next, columns (3)-(6) show regressions of the FM share on IPR(130) and the DC share, respectively, as shown in equations (5) and (6). If we believe that student FM eligibility is allocated following income rules, we should also get coefficients of 1.0 in these regressions as well, but our estimates are much larger. The coefficients range from 1.37-1.51, implying an oversubscription rate in FM eligibility of 37-51 percent. In all cases in columns (3)-(6), we can comfortably reject the null hypothesis of a 1.0 coefficient.

We make two additional observations about the estimates in columns (3)-(6). First, the coefficients on the DC-share variables are somewhat smaller than on IPR(130). We do not explore this result in great detail, but note that modest differences along the lines of what we find are not ruled out by the results in columns (1) and (2). This is because those results show that DC and IPR(130) provide the same information about poverty on average, but they do not rule out distributional differences in the variables that could contribute to differences in the coefficients in columns (3)-(6). The second noteworthy observation is that the standard errors in the IPR(130) regressions are much larger, reflecting greater imprecision in these estimates relative to the DC shares based on the Missouri administrative data. We return to both of these issues below.

In the last two columns of Table 2, we extend the analysis to look at the 185-percent-of-poverty income threshold using the FRM share and our IPR(185) measure. These results also indicate program oversubscription, with coefficients in 2016 and 2017 of 1.385 and 1.396, respectively. These estimates are most comparable to the IPR(130) estimates in columns (3) and (4) because they use the same measurement mode. Inference based on both sets of estimates suggests the *free* meal oversubscription rate exceeds the *reduced-price* meal oversubscription rate. The lower oversubscription rate in RM data is alluded to in our descriptive statistics in Table 1, which show few students are listed as eligible for reduced-price but not free meals.

Next, we consider the possibility that the CEP is driving the oversubscription of FM and FRM eligibility in our data. We build a modified dataset in which the FM and FRM shares for non-CEP schools are left as reported in 2016 and 2017, but for CEP schools, these values are re-coded to the last pre-CEP year in Missouri: 2014. Table 3 shows results from estimating our models on this dataset. For brevity we show results using the IPR variables only (results using the DC share are similar following from Table 2).

The coefficients in Table 3 decline in both the FM and FRM regressions compared to their analogs in Table 2. Specifically, for the regressions of schools' FM shares, the coefficients on IPR(130) from 2016 and 2017 are 1.354 and 1.318, respectively, and for the regressions of schools' FRM shares, they are 1.293 and 1.308. These estimates still imply substantial and statistically significant oversubscription in the NSLP, although at a rate that is about 9-15 percentage points less than what is implied by the analogous estimates in Table 2. We conclude from these results that the CEP can explain some of the oversubscription in modern FRM data, but most of the oversubscription is not attributable to the CEP. This finding is consistent with Domina et al.'s (2018) pre-CEP analysis of FRM data and Koedel and Parsons's (2021) investigation of the scope for impact of the CEP.

Finally, we also test the accuracy of FM data using a framework based on the mean squared error (MSE). For this analysis we assume our administrative DC data from Missouri provide accurate measures of schools' poverty shares, then calculate the MSEs of IPR(130) and the FM share relative to the DC share. Table 4 shows that the MSE of the FM share is more than double the MSE of IPR(130). One reason this extra analysis is useful is that the MSE accounts for both bias and precision. The results in Table 2 indicate the IPR(130) estimates are noisier than their DC-share counterparts. But despite this, the MSE results make clear that IPR(130) tracks the DC share much more closely than its FM-based analog.

6. Additional results from Missouri

6.1 Using the student poverty metrics to predict student achievement

In this section we use the poverty metrics to predict student achievement in Missouri. Specifically, we estimate the following cross-sectional regression at the school level using data from 2017 (results from 2016 are very similar and omitted for brevity):

$$Y_j = \phi_0 + P_j\phi_1 + \varepsilon_j \qquad (8)$$

In equation (8), Y_j is the average standardized math test score for students in school j, and P_j is a measure of the share of students at 130 percent of the poverty line or below. We estimate this regression three times, where P_j represents either the DC share, IPR(130) estimate, or FM share. The sample includes all schools in Missouri with at least one grade in the 4-8 range (and we continue to impose the condition that enrollment is at or above 25).

It is well-documented that student poverty—despite its imprecise measurement using FRM data—is a strong predictor of low student achievement (Domina et al., 2018; Koedel and Parsons, 2021; Michelmore and Dynarski, 2017). Thus, we expect ϕ_1 to be negative in each version of equation (8). Moreover, if all three metrics are capturing the same information about poverty, we would expect ϕ_1 to be similar in magnitude in each regression.

Consistent with the preceding analysis, Table 5 shows that FM data do not convey the same information about student poverty as the other two metrics, which continue to track each other closely. Specifically, the table indicates that a one-percentage-point increase in IPR(130) corresponds to a reduction in student test scores of about 0.017 student standard deviations (noting that IPR(130) indicates the poverty share on a 0-1 scale). The analogous estimate based on the DC share is the same to the thousandth decimal place. In contrast, the same one-percentage-point increase in the FM share corresponds to a much smaller reduction in student achievement—just 0.011 standard deviations. This estimate is about 35 percent smaller in magnitude, reinforcing the finding from above that the FM-eligible share does not capture the same level of poverty as the DC share or IPR(130).[14]

6.2 Heterogeneity in the predictive validity of IPR(130)

Over the course of showing the limitations of using FRM data to measure poverty, we also provide the first external evidence of which we are aware on the accuracy of SNP-based poverty metrics, and IPR(130) in particular. In this section we expand on our finding that IPR(130) is an accurate measure of poverty, on average, by investigating its alignment with the DC share in more depth.

We begin by documenting the relationship between IPR(130) and the DC share throughout the income distribution. Figure 2 shows a binned scatterplot of IPR(130) and the DC share using 2017 data, along with the corresponding regression line from column (2) of Table 2. Theoretically, if IPR(130) and the DC share are measuring identical constructs and contain no error, we would expect the data points to form a precise line. Instead, the data plot is slightly convex—the relationship between the two variables is flatter at lower poverty values and steeper at higher poverty values. Because both metrics are estimates of poverty, and we do not observe true poverty values, it is difficult to identify the source of the modest nonlinearity in the figure.

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[14] While the findings in Table 5 are intuitive and follow from the preceding analysis, they are seemingly at odds with results from student-level regressions presented by Domina et al. (2018). To investigate the discrepancy, in Appendix A we estimate analogs to Domina et al.'s (2018) core specifications using the Missouri data. We replicate their empirical findings substantively, but our interpretation is different and we do not believe the results are at odds with our findings in Table 5. See Appendix A for discussion.

One possibility is that there are subpopulations of students who are underrepresented relative to their poverty shares in programs that lead to direct certification and these students are clustered in the income distribution. Previous research suggests that Hispanic students comprise one such group (Lichter, Sanders, and Johnson, 2015; Sandstrom, Huerta, and Loprest, 2014; Williams, 2013; Zedlewski and Martinez-Schiferl, 2010), but there may be other groups that are harder to identify.[15] There may also be heterogeneity within the income distribution in the efficacy of the Kriging procedure used by NCES or in the efficacy of the procedure we use to recover IPR(130) from the raw NCES estimates.

The nonlinearity in Figure 2 will lead to misalignment between the DC share and IPR(130) in income-clustered data subsamples. In addition, one could also imagine alignment heterogeneity along other substantive dimensions, of which we consider three: (1) schooling level (elementary/middle schools versus high schools), (2) urbanicity (rural versus urban/ suburban schools), and (3) school sector (charter versus traditional schools). Issues with the DC-share or IPR(130) metrics within these subsamples could arise for a variety of reasons. One example is that the sizes of school catchment can differ along all three dimensions, which would affect the geospatial SNP metrics. Another is that for the charter/traditional school split, there could be a greater disconnect between geographic residence and school attendance among charter schools. For the DC share, there could be students who are more or less likely to participate in social safety net programs in particular types of schools. Our finding that these two metrics validate against each other on average in Table 2 suggests that the scope for these types of issues within the data subsamples is modest, but does not rule them out.

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[15] The demographics of Missouri are such that the impact of Hispanic underrepresentation in programs that lead to direct certification will be limited (see Table 1).

We explore the extent of alignment heterogeneity in Tables 6 and 7, where we report on findings from validation regressions of the DC share on IPR(130) for the subsamples described above. For brevity, we show results for 2017 only, although results for 2016 are similar. First, Table 6 shows results from simple regressions estimated on each subsample, which reveal coefficients that we can reject from 1.0 in four of six cases.[16]

Next, Table 7 examines whether the failure of the validation regressions using the data subsamples is due to substantive reasons or simply due to bunching within the income distribution combined with the nonlinearity illustrated in Figure 2. To disentangle these mechanisms, we use simple matching tools to identify and compare groups of schools in each data split on a common support in the distribution of the DC share. We illustrate with the first split shown in Table 6, which divides schools based on whether they are elementary/middle or high schools. We run a simple, univariate probit predicting whether each school is a high school using the DC share. Next, we conduct a one-to-one match of high schools with elementary/middle schools using the predicted values (referred to as "propensity scores" in the matching literature). We impose a caliper of 0.01 and drop all schools without a match, which defines the common support. We then re-run the validation regressions using the matched samples of schools only. If the source of the subgroup failure of the validation regression is due to a substantive factor and not the nonlinearity, our findings will continue to differ between elementary/middle and high schools within the matched sample. Alternatively, if the findings are the same across school types in the matched sample, it would imply the nonlinearity—combined

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[16] The exceptions are for charter/traditional schools split. For charter schools we fail to reject the null, but the test is not very informative due to the large standard error. For traditional schools, the overwhelming majority of our sample consists of traditional schools, so our estimate for this subgroup essentially replicates our estimate from Table 2 for the full sample.

with the bunching of schools by type within the income distribution—is causing the failure of the validation regressions in the unmatched data.

Table 7 shows that when we use the matched samples within each data split, our findings are generally similar across subsamples. The coefficients still differ from one because in each setting we're pulling schools from only part of the income distribution—due to the nonlinearity, we should not expect a coefficient of 1.0 when we do this. However, the important comparisons are between the coefficients within the data splits over the common support. Of the three data splits we consider, the coefficients within the urbanicity split differ the most from each other in Table 7, but even then, the matched-sample coefficients are much closer together than those from the unmatched sample in Table 6.[17] Our findings in Table 7 indicate the primary source of failure of the validation regressions in the subsamples is the nonlinearity, and not substantive factors that one might be concerned about that differ across school types.

Still, regardless of the cause, Tables 6 and 7 show that IPR(130) does not align with the DC share in the data subsamples. We conclude by showing that despite this finding, IPR(130) still matches the DC share much more closely than does the FM share in all data subsamples. Following on Table 4, for each data subsample, Table 8 reports MSE's for IPR(130) and the FM share with respect to the DC share. The MSE's uniformly show that IPR(130) is the superior match to the DC share. Thus, even in subsamples where the correspondence between IPR(130) and the DC share is not as strong as in the full sample, IPR(130) is still a clear improvement over using FM data to measure poverty.

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[17] We also note that the urbanicity split has especially bad overlap because high-poverty schools are much more prevalent in the urban/suburban sample. This can be seen by the significant reduction in the total sample size in the matched data in Table 7 relative to Table 6—many schools are dropped even in the "treatment pool" of urban/suburban schools because there are no rural school matches.

7. Extensions beyond Missouri

We draw on data from the CCD to expand our analysis outside of Missouri. First, we construct IPR(130) and IPR(185) estimates for all schools in the U.S. using the baseline IPR values from the NCES. Next, we merge these variables with FM and FRM eligibility shares from the CCD.[18] We also attempted to replicate our validation regressions of the DC share on IPR(130) in other states but concluded that the DC data in the CCD are not reliable enough to support the validation regressions.[19]

A challenge with using FM and FRM eligibility shares from the CCD is that some states have changed how they report these categories due to the CEP and others have not, and there is no indicator in the data to distinguish them. It would cloud inference to evaluate a mix of states coding their data differently. To address this problem, we identify a subset of 27 states in the CCD that do not appear to have manipulated their FRM reporting due to the CEP as of 2017. The criteria we use to identify these states, based on Koedel and Parsons (2021), are (a) less than one percent of schools have a reported FRM share of 100 percent, and (b) there is less than a five-percentage-point increase in the share of schools with missing FRM data between 2014 and 2017. The latter condition reflects the fact that in response to the CEP, some states have begun to report FRM data as missing. The 27 states that satisfy these criteria are: AL, AR, CA, CO, CT, FL, HI, IA, ID, IL, IN KS, KY, ME, MI, NC, NH, NJ, NY, OR, RI, TX, VA, VT, WA, WI, and WV.

For each of these states, we run regressions of the FM share on IPR(130), and the FRM

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[18] For all 50 states and Washington DC, the CCD includes 105,862 schools in 2017. The SNP metrics from NCES has IPR estimates for 100,191 of these schools—i.e., the coverage rate is 95 percent.

[19] The CCD only recently began including DC data, and the currently-available data are problematic. There are two specific problems. First, the smaller problem is that DC data are missing for most schools in the CCD. Most states report either no DC information or very little DC information. More importantly, the data that are reported in the CCD, at least for Missouri, are not sufficiently accurate. To test this, we constructed shares of DC, FM, and FRM students in each school using the Missouri microdata and estimated separate univariate regressions of these shares on their data analogs in the CCD, using 2017 data. If the data elements in the Missouri microdata and the CCD data are the same, we should anticipate coefficients of 1.0 from each of these regressions. For the FM and FRM regressions, our coefficients are close to 1.0, at 0.97 and 0.96, respectively; but for the DC regression, the coefficient is just 0.48. We are not sure what is causing the discrepancy with the DC data, but our Missouri microdata are surely more reliable because they are based on a direct merge of administrative files between agencies.

share on IPR(185), as shown in equations (5) and (7). Like in our analysis of the Missouri microdata, coefficients of 1.0 on the IPR variables would indicate that students' FM and FRM designations are aligned with the stated income requirements of these programs. A caveat to this interpretation is that we cannot directly validate our IPR(130) measures outside of Missouri. Thus, an additional assumption in this portion of our analysis is that the IPR-based estimates continue to serve as accurate estimates of student poverty shares in other states. This is made more plausible by the fact that the methodologies used by NCES to produce the baseline IPR estimates, and by us to convert them into IPR(130) estimates, are consistent across schools in all states. That said, we acknowledge our validity results from Missouri may not generalize if school attendance patterns with respect to geography are different in Missouri and other states. A related concern is that school locations within their communities relative to the local-area income distribution are systematically different outside of Missouri. Neither of these threats to validity seems especially likely, but they are difficult to test empirically and thus difficult to rule out.[20]

Noting this caveat, we begin by establishing comparability between the results using the Missouri administrative microdata (from above) and the CCD. In columns (1) and (2) of Table 9, we show results from regressions of the FM share on IPR(130), and the FRM share on IPR(185), respectively, using the CCD data to populate the FM and FRM variables for Missouri. Note that the Missouri data in the CCD are inclusive of CEP coding, so these results should correspond closely to the results in columns (4) and (8) of Table 2. Table 9 shows that this is indeed the case, confirming that the administrative data and CCD yield similar results in Missouri.

Columns (3) and (4) show analogous regressions that pool data from the 27-state sample. We include state fixed effects in the pooled multi-state regressions to isolate within-state

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[20] Variation across states in the utilization of schools of choice, and private schools in particular, is an example of a specific factor that could affect the generalizability of our validity findings from Missouri. This is because the residences of private-school students are used to construct the SNP metrics (the households used by NCES include families with children but are not restricted to families with children in public schools), but these students will not affect poverty rates in public schools. All else equal, in states with larger private-school enrollment shares, there should be larger differences between IPR(130) and public-school poverty rates. Of note, Missouri's private-school enrollment share is above the national average, at 12.6 percent compared to 10.2 percent nationally as of 2017 (source: authors' calculations based on data as reported in de Brey et al., 2021).

variation for identification, although as a practical matter this has no substantive bearing on the findings. The estimates using the 27-state sample are a close match to the Missouri estimates—the coefficients from the FM regressions are very similar, and the coefficient from the FRM regression in the larger sample is slightly lower than the Missouri coefficient. This is broad evidence that FM and FRM data overstate poverty rates.

The pooled regressions in Table 9 obscure significant state-level heterogeneity in the estimated coefficients on IPR(130) and IPR(185). Figure 3 illustrates this heterogeneity by plotting all 27 state coefficients and their error bands. For ease of presentation, states are ordered in each panel from the largest to smallest coefficient values.

The range of estimates shown in Figure 3 is striking. For example, in the FM regressions, the coefficient on IPR(130) ranges from a minimum of 0.50 (Arkansas) to a maximum of 1.75 (Rhode Island). The range of coefficients in the FRM regressions is narrower, but still large, ranging from a minimum of 0.50 (Arkansas) to a maximum of 1.55 (North Carolina). This variability likely reflects a number of factors, including differences across states in districts' FM and FRM certification processes and measurement error in the CCD. The former could reflect, for example, differences in leniency across states in districts' income-verification processes and/or differences in families' willingness to apply to the NSLP. The latter would include all reporting errors between the point of data collection in individual districts to the point of entry into the CCD. In contrast, although we cannot rule out some cross-state variability in the meaning of the IPR(130) and IPR(185) variables per the above discussion, we expect any such variability to be small in comparison to variability in the NSLP data.

In addition to showing that FM and FRM data in the 27-state sample overstate poverty rates similarly to what we find in Missouri, on average, these results also raise larger concerns about using FM and FRM data from the CCD or other sources in multistate studies. Figure 3 suggests these variables have very different meanings with respect to poverty identification across states, implying that using them to control for the same conceptual factors (i.e., poverty or student disadvantage) in research spanning multiple states will be problematic.

Finally, we conduct an analog to the achievement-based analysis shown in Table 5 using the CCD data. Unlike in Missouri, we do not have access to administrative data on student test scores in the multi-state sample, so we use data from the Stanford Education Data Archive (SEDA). SEDA contains district-level estimates of average standardized test scores in Math and English Language Arts for students in grades 3-8 throughout the U.S. The comparability across states is facilitated by linking the state tests and the National Assessment of Educational Progress (NAEP) to develop a common scale (Fahle et al., 2018; Fahle, Shear, and Shores, 2019; Reardon, Kalogrides, and Ho, 2021).

SEDA reports achievement at the district level, so we aggregate our poverty data accordingly and estimate regressions of district-level achievement on district-level measures of poverty using the 27-state sample.[21] We construct the district-level poverty shares as enrollment-weighted averages of the school-level poverty shares. We also add state fixed effects to our regressions, similarly to above, which yields the following analog to equation (8) for this portion of our analysis:

$$Y_{ks} = \varsigma_0 + P_{ks}\varsigma_1 + \nu_s + \eta_{ks} \tag{9}$$

In equation (9), Y_{ks} is the average math achievement level in district *k* in state *s* from SEDA, P_{ks} is the poverty measure of interest, and ν_s is a state fixed effect. In the Missouri-specific version of this model we estimated it three times; defining P_{ks} as the DC share, IPR(130) estimate, and FM share. For the extended 27-state sample we do not observe the DC share, so we estimate the regression just twice—once defining P_{ks} by the IPR(130) estimate and once defining P_{ks} by the FM share.

The results are shown in Table 10. As in our preceding analysis of the Missouri data, the coefficient from the regression using IPR(130) is much larger than its analog from the regression using the FM share. Although both coefficients in Table 10 are larger than their comparison

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[21] SEDA includes district-level average standardized math scores for a national sample of 9,728 out of 10,921 districts in the CCD (89 percent) in 2017. For our selected sample of 27 states, 6,221 out of the 6,853 districts in the CCD (91 percent) have math scores in SEDA in 2017.

coefficients in the Missouri-specific analysis, the relative difference is similar.[22] That is, like in Table 5, the magnitude of ς_1 when we use FM data to measure student poverty is much smaller (by about 53 percent) than when we use IPR(130). This result further supports the conclusion that the FM share is not capturing the same level of poverty as IPR(130).

8. Implications for measuring poverty

The clearest finding from our analysis is that FRM data from the NSLP do not measure student poverty accurately. This is established in our deep-dive analysis in Missouri and replicated in our extension using national data. Given this finding, how should states respond? How should we measure poverty going forward?

The most obvious alternative to FRM data are data on direct certification, which are available at the student level and shown by our analysis to be effective at identifying students at or below 130 percent of the poverty line in Missouri. However, it is unclear how effectively DC data will measure poverty in other states. A concern is variation in states' BBCE policies, which result in a range of poverty thresholds for directly certified students from 130-200 percent of the poverty line across states (United States Department of Agriculture, 2022). Variability in states' BBCE policies has two implications. First, it means that DC status conveys different information about the level of poverty in different states, which matters for both internal state policies and broader federal policies impacting multiple states. Second, it is unclear whether program participation and income enforcement in BBCE states are such that DC status will measure the income thresholds intended by state rules. For example, in some states participation in Medicaid can lead to direct certification (Blagg, Rainer, and Waxman, 2019), but research shows that

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[22] The larger absolute values of the coefficients in Table 10 could be driven by a number of factors that differ in this portion of our analysis, including differences in state assessments that could differentially pick up differences in student poverty and disadvantage, factors related to SEDA's process for constructing comparable test scores across states, the impact of district aggregation on the estimates, and the related impact of variability in district size across states that leads to differential aggregation, among other possibilities. As this analysis is only supplementary to our main findings, and the absolute levels of the coefficients are not of first-order importance (it is their relative values that we care about), we did not thoroughly investigate the source(s) of the difference in coefficient magnitudes.

many Medicaid-eligible families do not participate (Sommers et al., 2012). Concerns have also been raised about the fidelity with which BBCE criteria are enforced (Blankley, 2019).

Our study suggests that IPR(*X*) estimates based on SNP data can be used to estimate the share of students living at or below *X* percent of the poverty line. This implies a strategy for assessing the information contained by DC data in other states: researchers with access to these data can essentially reverse-engineer the poverty level identified by DC status by finding the value of *X* that aligns the DC share, on average, with IPR(*X*). A natural starting point would be to set *X* to the stated income threshold for direct certification. Such work will not be conclusive because misalignment could reflect a myriad of problems, but it can offer a productive starting point for learning more about the information contained by DC data.[23]

This discussion also highlights the more general point that neither FRM nor DC data are viable options for generating consistent measures of poverty across states. At best, and pending future research, DC data may be shown to accurately identify students but at varying levels of povery across states; the picture for FRM data is more bleak—these data are not accurate within states or consistent across states. The informational content of both of these measures is also subject to continued change as policymakers target objectives other than poverty measurement. For instance, when the CEP was introduced to the NSLP to expand meal access, it diminished the informational content of FRM status with respect to poverty. Existing variability in states' BBCE policies, and likely any changes going forward, are similarly motivated by the need to meet policy objectives and not measure poverty *per se*. These issues point to a clear advantage of IPR(*X*) estimates based on the SNP data, which aim to capture a homogeneous measure of income at *X* percent of the poverty line and are policy invariant, by which we mean the measures are not subject to change in order to achieve other policy goals. Although a major drawback is

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[23] These tests are more informative when the estimates align than when they do not. This is because the assumptions required of the DC and SNP data for them to align are very different, which means that when they align, it is difficult to argue both sets of assumptions are violated such that they still align by happenstance. In contrast, if the estimates do not align, it opens up the possibility of violations of the assumptions undergirding one or both measures. Still, identifying the direction and magnitude of misalignment would be the first step toward identifying issues with the data.

that no student-level indicators can be constructed using the SNP data, it is easy to imagine research and policy applications where homogeneous and policy-invariant measures of poverty at the school level will be useful.

9. Conclusion

We validate two external measures of student poverty against each other, then use them to show that FRM data substantially overstate student poverty. This finding is established most clearly in our analysis of the rich administrative data from Missouri. Under the assumption that the validity evidence supporting the accuracy of our IPR(130) estimates in Missouri extends to other states—a plausible assumption but one that we are unable to test directly due to data limitations—it is also supported in a larger 27-state sample we construct using CCD data. Community eligibility for free meals within the NSLP has exacerbated the overstatement of poverty in FRM data, but the overstatement precedes the CEP.

We conclude with a brief discussion of why it matters for U.S. education policy that FRM data substantially overstate student poverty. There are two main reasons. First, behaviors of education administrators are a likely source of the oversubscription of students as FRM-eligible—in addition to applying for the CEP if eligible, administrators who are more aggressive in eliciting and approving parental applications can generate higher FRM shares. This should not be surprising based on how the program works, but it is at odds with the implicit notion that poverty is an objectively-measured attribute. To the extent that FRM-eligible students factor directly into states' funding and accountability policies, the process by which FRM data are generated, inclusive of potential heterogeneity across states and school districts, raises concerns about behavior that manipulates the underlying data (even if well-intentioned) and the resulting fairness of these systems.

Second, our findings refute the common misperception that at least prior to the CEP, FRM data measured poverty accurately. A consequence of this misperception is that pre-CEP FRM rates are being held up as benchmarks by which newer, alternative measures of poverty are judged. More concretely, there are efforts to calibrate newer poverty measures to match older

FRM-based measures in schools. The best example of this behavior is the practice of using a multiplier (above 1.0, with a commonly-advocated value of 1.6) to adjust new poverty rates based on direct-certification status to match older FRM-based poverty rates (e.g., see Croninger, Rice, and Checovich, 2015; Grich, 2019). Although accuracy is not the only motivation for this calibration—policy stability is another motivation, albeit one that we view as short-sighted once it is known the old measures are not accurate—policy documents suggest that it is not commonly understood that the original FRM-based poverty rates are not correct.[24]

In addition to being clear about what FRM data are not, it is also useful to be clear about what they are. While these data do not measure poverty accurately at the intended thresholds, they are useful measures of student disadvantage, broadly defined. This point is apparent in our regressions of school-average test scores on FRM shares in both Missouri and the larger 27-state sample and reinforced in a more detailed analysis on this point in Domina et al. (2018) (also see our Appendix A). Combining this evidence, we conclude that FRM status variables are best interpreted as hybrid indicators that combine information about income *per se* and the more nebulous concept of student disadvantage.

Once this shift in interpretation has been accepted, it is reasonable to ask whether *student disadvantage* can be captured more accurately than with just FRM data. The answer to this question is almost surely "yes" because modern education data systems have rich information about students along many dimensions and over time. Examples include, but are not limited to, direct certification (in some states), geographic mobility, attendance patterns, test and other

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[24] The original source of the multiplier is the federal legislation that ushered in the CEP. The purpose is to approximate the share of FRM-eligible students in a CEP school based on the share who are directly certified. The multiplier is essentially what would be estimated from a regression very similar to the ones we run above of the FM share on DC share, but replacing the FM share with FRM share. The federal guidance establishing the multiplier explicitly references the link to the status quo of using FRM data to measure poverty: "using only the number of directly certified students would result in lower poverty percentages for Community Eligibility schools or LEAs" (U.S. Department of Education, 2014, p. 8). A conceptual reason for the multiplier is that the FRM-eligibility threshold—185 percent of the poverty line—is above the 130-percent level that is effectively enforced via direct certification (excluding some states BBCE policies), so it is intuitive that a multiplier of some sort would be needed to maintain a comparable poverty measure. Implicitly, this seems to be the rationale in the federal guidance, but there is no mention that FRM data overstate poverty in federal documents or subsequent state documents that discuss similar multipliers (e.g., Croninger, Rice, and Checovich, 2015).

school performance measures, participation in remedial programs, etc. If policymakers are not shackled by their perceived need to anchor to an objective concept like poverty—and encouraged by the false sense that they had been doing so in the past—measures of student disadvantage can be produced for use in policy that are much more robust and informative than FRM status.

This last insight is important because education administrators have been reluctant to make a shift in accountability and funding policies away from what they perceive as poverty measures toward measures of student disadvantage. But this hesitancy is based on the false premise that FRM data have been measuring poverty. While we do not purport to know what the correct decision is with respect to using information about poverty versus disadvantage to inform education policy, establishing a common set of facts is a first step toward productive discussions in this regard.

A final point is that historical FRM data have been used not only at the building level, as we have used them here, but also to track differences between groups of students within schools and districts. The most common application of within-district and -school data has been for the calculation of achievement gaps along the dimension of (presumed) poverty. Even if states and districts wanted to continue to use FRM data for this purpose, the CEP has made this impossible in many states. Exploring options for identifying high-poverty and/or high-disadvantage students at the individual level is an area of future expansion for this work.

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Table 1: Summary Statistics, Missouri Data

	2016		2017	
	Mean	Standard Deviation	Mean	Standard Deviation
Demographics				
Black	0.16	0.26	0.16	0.26
Hispanic	0.06	0.08	0.06	0.09
White	0.72	0.28	0.71	0.28
Multi-race	0.03	0.03	0.04	0.03
Asian/Indian/Pacific Islander	0.03	0.03	0.03	0.04
Female	0.49	0.03	0.49	0.03
IEP	0.13	0.08	0.14	0.08
ESL	0.04	0.08	0.05	0.09
Test Scores				
Standardized Math Score	0.00	0.45	0.00	0.44
Poverty Measures				
Share Free/Reduced-Price Meal Eligible	0.53	0.26	0.52	0.26
Share Free Meal Eligible	0.47	0.27	0.46	0.27
Share of Directly Certified	0.30	0.18	0.30	0.18
NCES IPR Estimate	284.11	137.88	289.84	138.81
IPR(130)	0.34	0.11	0.33	0.11
IPR(185)	0.42	0.13	0.41	0.13
Avg. Students Per School	423.47	341.14	421.47	340.71
N (Schools)	2,172		2,186	
N (Students)	919,786		921,335	

Notes: This table shows summary statistics for our analytic sample of schools in Missouri in the 2016 and 2017 school years with at least 25 students. The summary statistics are weighted by enrollment. Student demographics, test scores, free and reduced-price meal eligibility, and direct certification status are taken from Missouri administrative microdata. IPR estimates are taken from the NCES school neighborhood poverty (SNP) metrics. IPR(130) and IPR(185) are calculated from the reported IPR estimates and standard errors for each school as described in the text. Test scores are from a reduced sample of schools that have test-takers in grades 4-8. The test-taking school samples from 2016 and 2017 include 1,689 and 1,694 schools, respectively.

Data Source: DESE administrative data and SNP data from NCES, 2016 and 2017

Table 2: Univariate Alignment Regressions, Missouri Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	2016	2017	2016	2017	2016	2017	2016	2017
	Dependent variable: DC share	Dependent variable: DC share	Dependent variable: FM share	Dependent variable: FM share	Dependent variable: FM share	Dependent variable: FM share	Dependent variable: FRM share	Dependent variable: FRM share
IPR(130)	1.026	0.994	1.505***	1.469***				
	(0.033)	(0.034)	(0.048)	(0.051)				
DC share					1.372***	1.386***		
					(0.014)	(0.015)		
IPR(185)							1.385***	1.396***
							(0.034)	(0.037)
Constant	$-0.045^{\dagger\dagger\dagger}$	$-0.036^{\dagger\dagger\dagger}$	$-0.046^{\dagger\dagger\dagger}$	$-0.030^{\dagger}$	$0.049^{\dagger\dagger\dagger}$	$0.050^{\dagger\dagger\dagger}$	$-0.052^{\dagger\dagger\dagger}$	$-0.050^{\dagger\dagger\dagger}$
	(0.011)	(0.011)	(0.016)	(0.017)	(0.004)	(0.004)	(0.015)	(0.016)
Observations	2,172	2,186	2,172	2,186	2,172	2,186	2,172	2,186
R-squared	0.370	0.348	0.359	0.328	0.849	0.830	0.468	0.440

Notes: This table presents estimates from school level univariate regressions weighted by enrollment in each school year. In each regression, we test the null hypothesis that the poverty-measure coefficient is 1.0; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For completeness, we also report on the statistical significance of the constant term, where †††, ††, and † indicate the constant is statistically different from zero at the 1, 5, and 10 percent levels, respectively. Standard errors are in parentheses.

Data Source: DESE administrative data and SNP data from NCES, 2016 and 2017

Table 3: Univariate Alignment Regressions with Imputed FM and FRM Shares for CEP Schools, Missouri Data

VARIABLES	(1) 2016 Dependent variable: FM share	(2) 2017 Dependent variable: FM share	(3) 2016 Dependent variable: FRM share	(4) 2017 Dependent variable: FRM share
IPR(130)	1.354***	1.318***		
	(0.040)	(0.043)		
IPR(185)			1.293***	1.308***
			(0.030)	(0.032)
Constant	-0.020	-0.008	-0.033††	-0.035††
	(0.014)	(0.014)	(0.013)	(0.014)
Observations	2,160	2,168	2,160	2,168
R-squared	0.398	0.373	0.504	0.490

Notes: This table presents estimates from school level univariate regressions weighted by enrollment in each school year. For CEP schools, the FM and FRM shares are imputed at the 2014 level, the last year of non-CEP coded data in Missouri. In each regression, we test the null hypothesis that the poverty-measure coefficient is 1.0; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For completeness, we also report on the statistical significance of the constant term, where †††, ††, and † indicate the constant is statistically different from zero at the 1, 5, and 10 percent levels, respectively. Standard errors are in parentheses.
Data Source: DESE administrative data, 2016 and 2017, with imputed FM and FRM data from 2014 DESE administrative data for selected schools; and SNP data from NCES, 2016 and 2017

Table 4: Mean Squared Error Analysis

	2016	2017
FM share	0.051	0.054
IPR(130)	0.021	0.022
Observations	2,172	2,186

Notes: This table shows mean squared errors (MSEs) for the FM share and IPR(130). These MSE calculations assume the share of DC students reflects the true share of students at or below 130 percent of the poverty line, which is almost surely incorrect but is likely approximately accurate. Smaller values indicate less error.
Data Source: DESE administrative data and SNP data from NCES, 2016 and 2017

Table 5: Relationships between Student Test Scores and Measured Poverty, Missouri Data

VARIABLES	(1) 2017 Dependent Variable: School Avg. Test Score	(2) 2017 Dependent Variable: School Avg. Test Score	(3) 2017 Dependent Variable: School Avg. Test Score
IPR(130)	-1.732***		
	(0.167)		
DC share		-1.685***	
		(0.056)	
FM share			-1.087***
			(0.043)
Constant	0.577†††	0.535†††	0.527†††
	(0.058)	(0.020)	(0.021)
Observations	1,694	1,694	1,694
R-squared	0.172	0.490	0.454

Notes: This table presents estimates from school-level univariate regressions where the dependent variable is the school average standardized math test score, and the independent variables are three different measures of poverty–IPR(130), the DC share, and the FM share in the school. All regressions are weighted by enrollment. In each regression, we test the null hypothesis that the poverty-measure coefficient is zero; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For presentational consistency, we continue to denote statistical significance of the constant term at the 1, 5, and 10 percent levels using the same †††, ††, and † indicators from previous tables. Standard errors are in parentheses.

Data Source: DESE administrative data and SNP data from NCES, 2017

Table 6: Univariate Regressions of the DC share on IPR(130), School Subgroups, Unmatched Samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Elem/Middle Schools 2017 Dependent Variable: DC Share	High Schools 2017 Dependent Variable: DC Share	Urban/Suburban Schools 2017 Dependent Variable: DC Share	Rural Schools 2017 Dependent Variable: DC Share	Charter Schools 2017 Dependent Variable: DC Share	Traditional Schools 2017 Dependent Variable: DC Share
VARIABLES						
IPR(130)	1.118***	0.746***	1.141***	0.738***	1.298	0.968
	(0.036)	(0.063)	(0.051)	(0.042)	(0.289)	(0.034)
Constant	-0.046†††	-0.021	-0.072†††	0.040††	-0.062	-0.030†††
	(0.012)	(0.020)	(0.015)	(0.016)	(0.129)	(0.011)
Observations	1,612	574	877	1,309	64	2,122
R-squared	0.396	0.340	0.400	0.258	0.240	0.339

Notes: This table presents estimates from school level univariate regressions weighted by enrollment in 2017. In each regression, we test the null hypothesis that the poverty-measure coefficient is 1.0; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For completeness, we also report on the statistical significance of the constant term, where †††, ††, and † indicate the constant is statistically different from zero at the 1, 5, and 10 percent levels, respectively. Standard errors are in parentheses.

Data Source: DESE administrative data and SNP data from NCES, 2017

Table 7: Univariate Regressions of the DC share on IPR(130), School Subgroups, Matched Samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Matched Over Common Support Defined by High Schools		Matched Over Common Support Defined by Urban/Suburban Schools		Matched Over Common Support Defined by Traditional Schools	
	Elem/Middle Schools	High Schools	Urban/Suburban Schools	Rural Schools	Charter Schools	Traditional Schools
VARIABLES	2017 Dependent Variable: DC Share	2017 Dependent Variable: DC Share	2017 Dependent Variable: DC Share	2017 Dependent Variable: DC Share	2017 Dependent Variable: DC Share	2017 Dependent Variable: DC Share
IPR(130)	0.828***	0.746***	0.746***	0.940	1.298	1.256*
	(0.062)	(0.063)	(0.047)	(0.058)	(0.289)	(0.152)
Constant	-0.007	-0.021	0.018	-0.043††	-0.062	-0.020
	(0.018)	(0.020)	(0.013)	(0.020)	(0.129)	(0.055)
Observations	574	574	693	693	64	64
R-squared	0.297	0.340	0.276	0.313	0.240	0.434

Notes: This table presents estimates from school level univariate regressions weighted by enrollment in 2017. In each regression, we test the null hypothesis that the poverty-measure coefficient is 1.0; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For completeness, we also report on the statistical significance of the constant term, where †††, ††, and † indicate the constant is statistically different from zero at the 1, 5, and 10 percent levels, respectively. Standard errors are in parentheses.

Data Source: DESE administrative data and SNP data from NCES, 2017

Table 8: Mean Squared Error Analysis, School Subgroups

	Elem/Middle Schools 2017	High Schools 2017	Urban/Suburban Schools 2017	Rural Schools 2017	Charter Schools 2017	Non Charter Schools 2017
FM share	0.049	0.068	0.058	0.052	0.132	0.052
IPR(130)	0.021	0.023	0.031	0.015	0.034	0.021
Observations	1,612	574	877	1,309	64	2,122

Notes: This table shows mean squared errors (MSEs) for the FM share and IPR(130) values for schools in 2017. These MSE calculations assume the share of DC students reflects the true fraction of students at or below 130 percent of the poverty line, which is almost surely incorrect but is likely approximately accurate. Smaller values indicate less error.

Data Source: DESE administrative data and SNP data from NCES, 2017

Table 9: Univariate Alignment Regressions, Missouri and the 27-State Extended Sample Using the Common Core of Data

VARIABLES	(1) 2017 Dependent Variable: School FM Share in MO	(2) 2017 Dependent Variable: School FRM Share in MO	(3) 2017 Dependent Variable: School FM Share in 27 States, Not CEP-Coded	(4) 2017 Dependent Variable: School FRM Share in 27 States, Not CEP-Coded
IPR(130)	1.419***		1.397***	
	(0.052)		(0.008)	
IPR(185)		1.363***		1.253***
		(0.040)		(0.007)
Constant	-0.030†	-0.050†††	-0.045†††	-0.025†††
	(0.017)	(0.017)	(0.006)	(0.006)
State Fixed Effects	N/A	N/A	Yes	Yes
Observations	2,257	2,257	61,270	61,270
R-squared	0.306	0.417	0.477	0.525

Notes: This table presents estimates from school-level univariate regressions weighted by enrollment in each school in 2017 using CCD and SNP data from NCES. In each regression, we test the null hypothesis that the poverty-measure coefficient is 1.0; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For completeness, we also report on the statistical significance of the constant term, where †††, ††, and † indicate the constant is statistically different from zero at the 1, 5, and 10 percent levels, respectively. Standard errors are in parentheses.

Data Source: Common Core of Data and SNP data, both from NCES, 2017

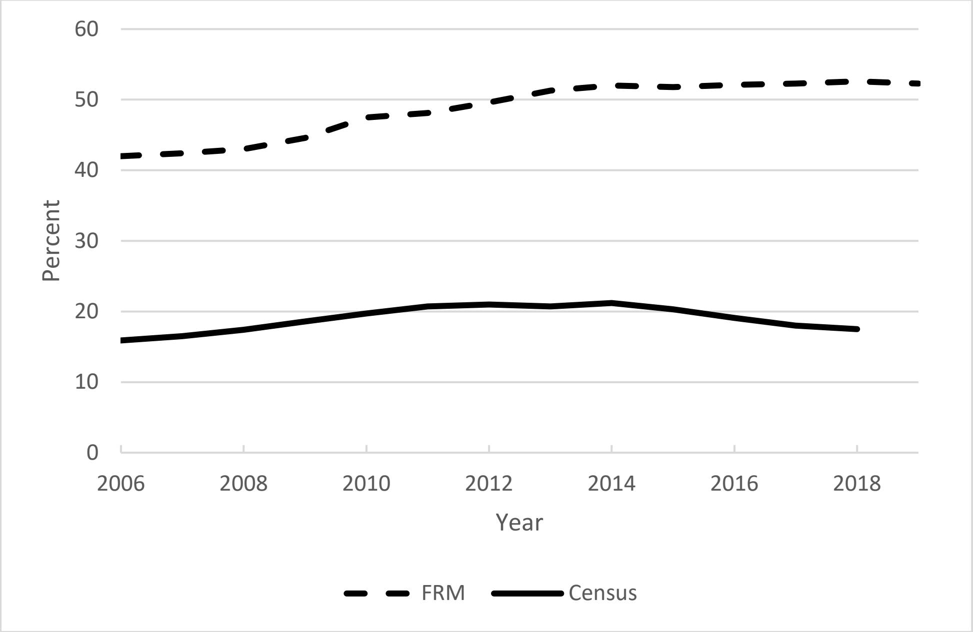
Table 10: Relationships between Student Test Scores and Measured Poverty, 27-State Extended Sample Using the Common Core of Data and Stanford Education Data Archive

VARIABLES	(1) 2017 Dependent Variable: District Avg. Test Score	(2) 2017 Dependent Variable: District Avg. Test Score
District IPR(130)	-3.020***	
	(0.081)	
District FM Share		-1.404***
		(0.047)
Constant	0.815†††	0.364†††
	(0.043)	(0.036)
State Fixed Effects	Yes	Yes
Observations	6,221	6,221
R-squared	0.579	0.692

Notes: This is a national-level analog to Table 5 using data from CCD, SEDA, and SNP data from NCES. This table presents estimates from district-level univariate regressions where the dependent variable is the district average standardized test score, and the independent variables are IPR(130) and FM share in the district, respectively. The regressions are weighted by enrollment in each district. In each regression, we test the null hypothesis that the poverty-measure coefficient is zero; rejection of this null hypothesis at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively. For presentational consistency, we continue to denote statistical significance of the constant term at the 1, 5, and 10 percent levels using the same †††, ††, and † indicators from previous tables. Standard errors are in parentheses.

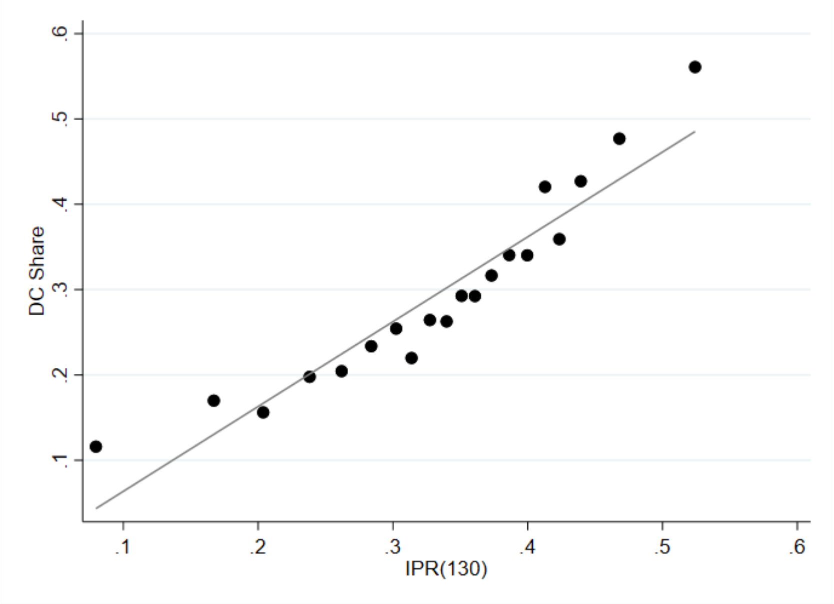
Data Source: Common Core of Data and SNP data, both from NCES, and SEDA data, 2017

Figure 1: Poverty Rates Among School-Aged Children Measured Using Different Data Sources



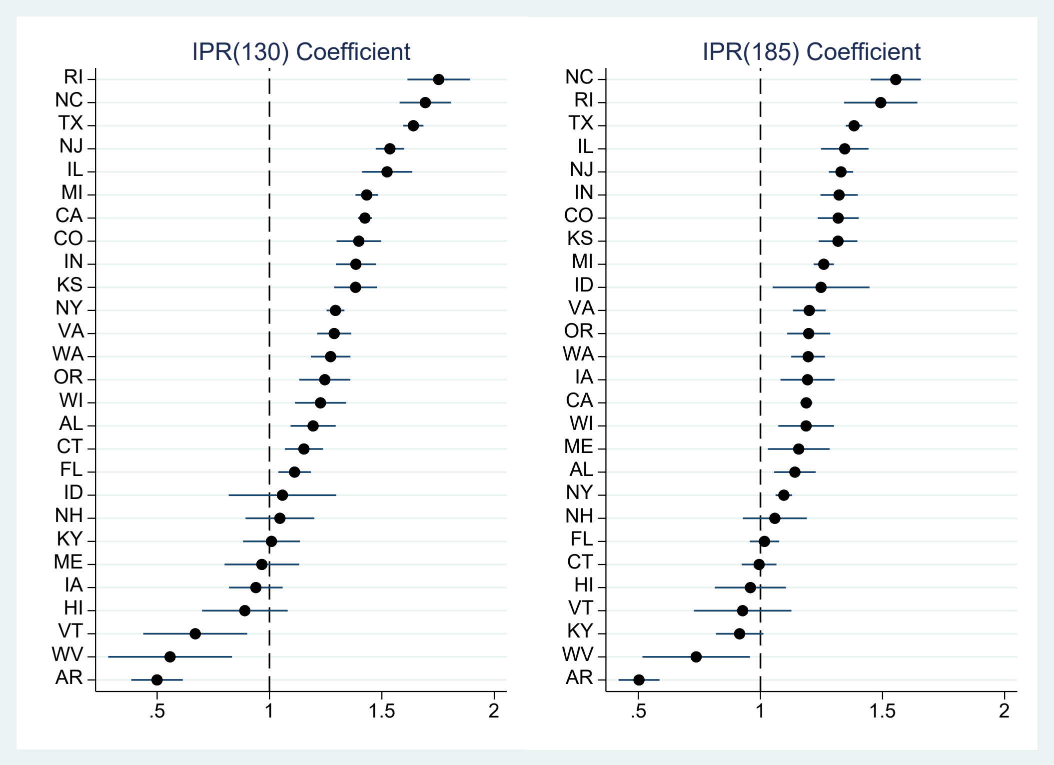
Notes: Trends are in the share of FRM-eligible students and the share of school-aged children living in families with incomes at or below the poverty line.
Data Source: NCES Digest of Education Statistics (de Brey et al., 2021).

Figure 2: IPR(130)-DC Share Binned Scatterplot (2017)



Notes: We construct this chart by dividing IPR(130) into 20 equal-sized bins—each dot indicates the mean values of IPR(130) and the DC share within each bin. The full ranges of the IPR(130) and DC-share variables are 0-0.72 and 0.01-0.91, respectively. The regression line corresponding to the estimtes in column (2) of Table 2 is shown.
Data Source: DESE administrative data and SNP data from NCES, 2017

Figure 3: Heterogeneity of the FM and FRM Regression Coefficients in the 27-State Sample



Notes: The left panel shows the estimated coefficients from univariate regressions of the FM share on IPR(130) for the 27 states with non-CEP coded data, along with 95 percent confidence intervals. The right panel shows analogous coefficients and confidence intervals from univariate regressions of the FRM share on IPR(185). States are in descending order of the coefficient values in each panel.

Data Source: Common Core of Data and SNP data, both from NCES, 2017

Appendix A
Reconciling our findings with related findings from Domina et al. (2018)

Our regressions of test scores on school poverty show that more acute measures of poverty are more strongly associated with low achievement in schools (Tables 5 and 10 in the main text). This is an intuitive result but could be viewed as inconsistent with findings as presented by Domina et al. (2018). In this appendix we replicate the primary findings from Domina et al. (2018) using our Missouri data and explain why there is no real discrepancy. Our interpretation is that their findings are also consistent with the intuitive result that more acute measures of poverty are more strongly associated with low achievement.

Domina et al. (2018) examine the predictive power over test scores of FRM eligibility and income designations based on IRS data. They use the IRS data to identify students living at or below 130 or 185 percent of the poverty line to match stated NSLP thresholds. In Appendix Table A1, we show results as reported in columns (1), (2), and (3) of their Table 3. The results in columns (1) and (2) are from student-level univariate regressions of standardized test scores in English Language Arts in the eighth grade on FRM status and the IRS-based analog (i.e., income at or below 185 percent of the poverty line). Column (3) includes both of these variables simultaneously. These results are based on their analysis using Oregon data.[25]

Their results in columns (1) and (2) show the coefficient on FRM status is negative and significant, and more negative than the coefficient on the IRS analog. When both variables are included simultaneously in column (3) they find both are significant, but the coefficient on the FRM variable is larger.

In Appendix Table A2, we estimate analogous student-level models using our Missouri data. We cannot match their specifications exactly because at the student level, we do not have an analog to their IRS-based indicator for income at or below 185 percent of the poverty line. Instead, we estimate comparable regressions focusing on indicators at 130 percent of the poverty line (i.e., rather than using FRM eligibility and an income measure at 185 percent of the poverty line, we use *FM* elgibility and an income measure at *130* percent of the poverty line (DC

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[25] They also report substantively similar results from California using different specifications due to data availability.

status)).[26]

The key takeaway from Appendix Tables A1 and A2 is that the results are substantively similar between studies. In the univariate regressions the coefficient on the FM/FRM indicator is more negative than the coefficient on the DC/IRS variable. And when both types of variables are included together, the FM/FRM indicator has the more negative coefficient. Although our estimates are not identical to those from Domina et al., along the important dimensions we replicate their findings.

Where we differ from Domina et al. (2018) is in the interpretation of these findings. To illustrate, we begin by documenting how they describe their findings on page 543, where they write: "Perhaps surprisingly, the results of these analyses indicate that school-reported FRPL status variables are more closely associated with student achievement on standardized tests in ELA than parallel categories constructed using IRS-reported household income." It is easy to see how they arrived at this observation based on the regression results, but in this case the inference is erroneous.

First, following on our analysis and discussion in the main text of this article, an important contextual detail is that outside of a small number of observations that we attribute to data errors, all DC students are FM-eligible. However, not all FM-eligible students are directly certified. The implication is that almost all students in our data can be grouped into one of the following three categories, from lowest to highest income (again, outside of data errors): (1) DC and FM, (2) FM but not DC, and (3) neither DC nor FM. There is no meaningful population of "DC but not FM students." Similarly, while there is slightly more variability in the Domina et al. data, the vast majority of students with incomes below the threshold for free or reduced-price meal eligibility, as indicated by the IRS data, are coded as FRM-eligible; whereas many students who are coded as FRM-eligible have incomes well above the stated income threshold (see Figure 1b in their paper).

This contextual detail provides insight about the results in column (3) of Appendix Tables A1 and A2. In our models, because all DC students are also FM-eligible, the predicted score for the average DC student is the sum of the two coefficients since both indicator variables

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[26] We estimate associations between these student designations and standardized math test scores, not ELA scores as in Domina et al. (2018), although this has no substantive bearing on our findings. In addition, we have estimated these models with and without other covariates, and like in Domina et al. (2018), the pattern of estimates on the poverty variables is similar regardless of whether we include additional controls.

will equal one for these students. DC students clearly perform worse on tests than FM-only students (for whom only the FM indicator is equal to one) and students who are neither FM nor DC (for whom both indicators are equal to zero), on average. A similar interpretation applies to Domina et al.'s results.

Next we turn to the univariate regressions in columns (1) and (2), which are again substantively similar for us and Domina et al. (2018). At first glance, these results seem to refute the interpretation in the preceding paragraph because the coefficients on the IRS/DC variables are not more negative. However, the univariate regressions cannot be compared due to Simpson's paradox (Simpson, 1951). This can be seen most clearly if we make a simplifying assumption that student test performance is monotonically increasing in income—i.e., income and test scores move together such that lower income students always have lower scores. Under this assumption, consider 100 students ranked by income and test scores from lowest to highest. Based on the proportions of students shown in Table 1 in the main text, suppose the first 30 of these students are directly certified and FM-eligible (30 percent of the sample), the next 17 students are FM-eligible but not directly certified (summing to 47 percent total FM-eligible, per Table 1), and the remaining 53 students are neither directly certified nor FM-eligible. The regression coefficient in column (2) of Appendix Table A2 would correspond to the difference in test scores, on average, between the 30 DC students and remaining 70 non-DC students. The regression coefficient in column (1) would correspond to the average difference between FM students, inclusive of DC students, and non-FM students. These two coefficients differ only because of how the 17 students who are FM, but not DC, are coded. The following two things are true about these 17 students: (1) among the non-DC students in the sample, they are the lowest performers, and (2) among FM students, they are the highest performers. The implication is that shifting these 17 students between groups has an ambiguous interpretation on the coefficient from the univariate regression—it can go up or down, or remain unchanged. Of course, in reality, the simplifying assumption of strict monotinicty between income and test scores will not hold exactly, but the key insight remains.

To be clear, none of this changes the bottom-line conclusion from Domina et al. (2018), which is that FRM data are a valid measure of educational disadvantage. Their findings show, and we confirm through our replication, that even conditional on accurate indicators of student income, FRM status is still informative about student performance. The explanation is straightforward: FM and FRM data identify a lesser but still meaningful tier of disadvantage.

The reason for this appendix is not to refute this central claim of Domina et al. (2018), but rather to buttress our finding that school poverty shares are more predictive of low test performance when they measure more acute poverty. This is an intuitive finding, but some of the discussion in Domina et al. (2018) could lead to confusion about whether it is consistent with their results, which we believe it is.

Appendix Table A1. Results from Columns (1), (2), and (3) as reported in Table 3 of Domina et al. (2018).

VARIABLES	Model 1 Coefficient SE	Model 2 Coefficient SE	Model 3 Coefficient SE
National School Lunch Program free/reduced-price lunch	-0.362*** (0.003)		-0.305*** (0.004)
IRS free/reduced-price lunch		-0.263*** (0.003)	-0.115*** (0.004)
School Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
R-squared (Adjusted)	0.124	0.111	0.127

Notes: "IRS free/reduced-price lunch" is a variable for whether the student's family's IRS data indicate she is eligible for free/reduced-price lunch based on income. The dependent variable in these regressions is standardized achievement in English Language Arts in the eighth grade. Standard errors in parentheses.
*** p<0.001, ** p<0.01
Data Source: Results taken directly from Table 3 in Domina et al. (2018)

Appendix Table A2. Results from our substantive replication of the results in columns (1), (2), and (3) as reported in Table 3 of Domina et al. (2018). 2017 data.

VARIABLES	Model 1 Coefficient SE	Model 2 Coefficient SE	Model 3 Coefficient SE
National School Lunch Program free lunch	-0.480*** (0.004)		-0.382*** (0.005)
Directly certified for free meals		-0.400*** (0.004)	-0.155*** (0.005)
School Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	N/A	N/A	N/A
R-squared (Adjusted)	0.195	0.182	0.198

Notes: Coefficients on the school fixed effects are suppressed. Domina et al. also include year fixed effects but these are unnecessary in our cross-sectional regressions using 2017 data. The dependent variable in these regressions is standardized achievement in math in grades 4-8. Standard errors in parentheses.
*** p<0.001, ** p<0.01
Data Source: DESE administrative data, 2017