Using Predicted Academic Performance to Identify At-Risk Students in Public Schools

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Measures of student disadvantage—or risk—are critical components of equity-focused education policies. However, the risk measures used in contemporary policies have significant limitations, and despite continued advances in data infrastructure and analytic capacity, there has been little innovation in these measures for decades. We develop a new measure of student risk for use in education policies, which we call Predicted Academic Performance (PAP). PAP is a flexible, data-rich indicator that identifies students at risk of poor academic outcomes. It blends concepts from emerging early warning systems with principles of incentive design to balance the competing priorities of accurate risk measurement and suitability for policy use. In proof-of-concept policy simulations using data from Missouri, we show PAP is more effective than common alternatives at identifying students who are at risk of poor academic outcomes and can be used to target resources toward these students—and students who belong to several other associated risk categories—more efficiently.

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1. Introduction

There have been substantial advances in education data infrastructure since the turn of the 21st century, and as of our writing this article, virtually every state in the U.S. has a state longitudinal data system (SLDS) supported by large investments from the federal government.[1] These data systems allow states to track students as they move through K-12 schools, monitoring their academic progress and providing rich information about their circumstances. Computing power has also increased rapidly during this same period of data infrastructure investment, so not only are rich data on K-12 students increasingly available, they are also increasingly usable.

However, these gains in data availability and useability have not translated into meaningful improvements in how states identify students in need of additional resources and supports, who are commonly described as being "disadvantaged" or "at risk" (we use these terms interchangeably throughout this article). Today, as has been the case for decades, states ubiquitously rely on blunt categorical indicators associated with disadvantage to identify these students. Examples of common indicators include free and reduced-price meal (FRM) enrollment, direct certification (DC), English language learner (ELL) status, individualized education program (IEP) status, and underrepresented minority (URM) status, among others.

The categorical approach to identifying at-risk students is limited in a number of ways. To illustrate with an example, consider California's Local Control Funding Formula (LCFF), which identifies at-risk students categorically based on whether they are FRM-enrolled or an ELL. But if FRM enrollment is a risk indicator, and ELL status is a risk indicator, what about a student who is both FRM-enrolled and an ELL? Or a student who is consistently FRM-enrolled versus one who is enrolled for just a single year? California's LCFF—like other categorical systems of which we are aware—does not allow for this type of differentiation to impact students' risk designations, despite its intuitive appeal and clear evidence from research that these differences matter (e.g., see Goldhaber et al., 2022; Michelmore and Dynarski, 2017). This

[1] New Mexico is the lone exception (see here: https://nces.ed.gov/programs/slds/stateinfo.asp, information retrieved 08.23.2021).

example illustrates two central problems with existing risk measurement systems. First, these systems are not precise about what it means for students to be "at risk"—i.e., there is not a conceptual framework guiding the dimension(s) of risk being measured. Second, they do not leverage the rich information available in state data systems to improve risk measurement.

With the limitations of existing measurement practices as motivating context, we develop a new measure of student risk, which we call "Predicted Academic Performance," or PAP. PAP is a singular indicator that draws on the many data elements available in state data systems to measure student risk, defined precisely as risk of poor academic performance. It blends concepts from emerging early warning systems with principles of incentive design to balance the competing priorities of accurate risk measurement and suitability for policy use.

In addition to developing a general measurement framework for PAP, we conduct an empirical proof-of-concept exercise using the Missouri SLDS to understand its potential value. We show that PAP is more effective than *status quo* measures at identifying students who are at risk of poor academic performance, which is by design. Furthermore, in a policy context, we show that PAP can be used to target resources toward low-performing students and their schools, or relatedly (and with lower stakes), as a diagnostic tool to help policymakers better understand how resources are distributed to students at the greatest risk of poor academic outcomes. We also show that PAP can be used to improve the targeting of resources to students across a broad range of traditional "categories of disadvantage"—namely, ELL, IEP, and URM students—compared to hypothetical systems based on poverty proxies (i.e., FRM and DC status) or a system modeled after California's LCFF.

We show that PAP is a useful measure of student risk and has a number of desirable properties, but it is not a panacea and has limitations that we elaborate on over the course of this article. While we are not claiming to be able to dislodge the entrenched reliance on simple categorical risk indicators in education policies with a single article, we hope that we can propel research forward on a more promising and modernized path toward the accurate and useful measurement of student risk. Improvements to risk measurement can bear fruit in the form of

more efficacious policies designed to narrow achievement gaps and promote better academic outcomes among disadvantaged student populations. Risk measurement is more than a question of measurement—it is a question of policy.

2. Motivation

Indicators of student risk are among the most consequential measures in education policy (along with measures of student performance). These indicators determine how billions of dollars of state funding are allocated to school districts each year through progressive funding formulas in most states.[2] And in conjunction with measures of academic performance, they are the primary tools we use to understand learning gaps and implement policies to mitigate these gaps.

But despite the critical role played by risk indicators in education policies, we know little about how effectively they measure student risk. Consider FRM enrollment, for instance, which is the predominant indicator of low-income status in consequential education policies.[3] There has long been a commonsense understanding that FRM enrollment is an imperfect proxy for family income (e.g., see Bass, 2010; Harwell and LeBeau, 2010). However, the research literature on the measurement properties of FRM data is thin, and it was not until recently, after decades of policy use, that definitive evidence of the gross inaccuracy of FRM-based income designations has been provided (Domina et al., 2018; Fazlul, Koedel, and Parsons, 2023). We are also not aware of any public policy documents expressing an urgency to understand the implications of

[2] For example, as of 2021, 44 states allocated at least some funds to school districts based on the enrollment of "low-income" students (Source: Education Commission of the States at link (retrieved 06.20.2022): https://reports.ecs.org/comparisons/k-12-and-special-education-funding-2021).

[3] Of the 44 states that allocate funding to school districts based on the enrollment of "low-income" students, 33 use FRM enrollment as at least part of the definition of "low income," and 23 use FRM enrollment exclusively (Source: Education Commission of the States at link (retrieved 06.20.2022): https://reports.ecs.org/comparisons/k-12-and-special-education-funding-2021).

using FRM data in policy applications.[4] And the issue is not limited to FRM data. Similar measurement concerns apply to other common risk measures.[5]

In addition to the dearth of research on the measurement properties of existing risk indicators, there has also been little innovation in the field of risk measurement, at least with respect to risk measures for use in education policies. Most policy measures have not changed meaningfully since the 20th century, despite substantial gains in our capacity to collect and analyze data. The long-standing use of a handful of blunt categories, as in current systems, is only preferable to a more holistic, data-driven approach if there is no marginal information to be extracted from the bevy of data available in state systems. This condition is intuitively implausible and has been refuted empirically for specific variables in recent studies by Goldhaber et al. (2022) and Michelmore and Dynarksi (2017).

There are two possible directions of research motivated by the current state of risk measurement. One is to expand our understanding of the properties of existing risk measures and the implications of using different measures in different education policies and contexts. The other is to develop new risk measures that address some of the limitations of existing measures to give policymakers (and researchers) more options and a broader understanding of what we can measure about student risk using modern data systems and analytic tools. Our contribution is along the lines of the latter.

3. A Framework for Constructing PAP

PAP measures the risk a student faces of poor academic performance. Academic performance can mean many things—achievement on standardized assessments, on-time grade

[4] It wasn't until the introduction of the community eligibility provision (CEP) to the NSLP in 2015 that policymakers began to seek out alternative income indicators, the most popular of which is direct certification (DC) status (Chingos, 2016; Greenberg, 2018). A plausible explanation is that the CEP changed the data in a highly visible and salient way. While there is reason to believe DC status is an improvement over FRM status as a measure of family income (Chingos, 2018; Fazlul, Koedel, and Parsons, 2023; Greenberg, 2018), like with FRM status, there is very little rigorous research on the properties of DC data and the implications of using DC status as a proxy for family income.

[5] Other indicators have received even less attention in research, in part because it is harder to quantify their limitations. For discussions and evidence on ELL and IEP misclassifications see Abedi (2004, 2008), Sullivan (2011), and Winters, Carpenter II, and Clayton (2017).

progression, school attendance, high-school graduation, college attendance, etc.—and in principle, PAP can be built around any of these concepts. However, for the bulk of our presentation, we anchor PAP to achievement on state assessments. State assessments are the most widespread and differentiated indicators of academic performance available in the education system. Moreover, research causally links test scores to consequential later-life outcomes such as college attendance and earnings.[6] In the extensions section below, we consider the potential for using alternative measures of academic performance to anchor PAP.

Moving forward with a test-based anchor in mind, the foundation of our framework is a predictive linear regression of student test scores on student attributes, which can be expressed as follows:

$$S_i = \beta_0 + \mathbf{X_i}\boldsymbol{\beta_1} + \varepsilon_i \quad (1)$$

In equation (1), S_i is a test score for student *i* and $\mathbf{X_i}$ is a vector of student attributes. $\mathbf{X_i}$ can be thought of as capturing information about students along a variety of dimensions and of a variety of types (e.g., contemporary and historical information, individual and school-level information, interactions of individual attributes within the vector, etc.). We discuss the considerations in selecting the variables for $\mathbf{X_i}$, and how we specify $\mathbf{X_i}$ in our empirical application using the Missouri SLDS, below.

Our decision to estimate equation (1) as a simple linear model is with an eye toward feasible policy use. As an academic exercise, it would be useful to extend this basic prediction framework using more complex methods to see if the quality of predictions can be meaningfully improved. However, we view this as a refinement of the framework rather than a core feature. Moreover, our findings below suggest that the quality of predictions plateaus quickly conditional on the variables that are generally available in state data systems, which suggests that the likely gains in predictive accuracy from increasing model complexity will be small.

[6] For a recent review of research and discussion on this point see Goldhaber and Özek (2019).

The predicted values from equation (1), $\hat{S}_i$, can be interpreted as measures of student risk. They are weighted averages of the attributes in the vector $\mathbf{X_i}$, where the weights—the coefficients in the vector $\boldsymbol{\beta_1}$—depend on the extent to which each attribute predicts student performance. Students with lower values of $\hat{S}_i$ are at greater risk of poor academic performance than their peers with higher values, as determined by their attributes.

An immediate question follows: If the aim is to define risk status based on S_i, why bother estimating $\hat{S}_i$ when S_i observed? There are two reasons, one practical and one conceptual. The practical reason is that S_i will be missing for some students—e.g., untested students when S_i is a test score. However, values of $\hat{S}_i$ can be calculated for all students as long as we observe $\mathbf{X_i}$. The extrapolation to students with missing S_i, such as those who miss tests or are outside of tested grades, requires assuming the attributes that predict performance for tested students are the same attributes that would predict performance for untested students, had they been tested. In our empirical application below, we provide evidence consistent with this assumption being upheld, at least to an approximation, by showing that we obtain similar values of $\hat{S}_i$ for individual students when we estimate equation (1) using different subsamples of tested grades (see Appendix Table A1).

There is also a conceptual reason for using $\hat{S}_i$ instead of S_i: it creates a profile-based prediction of student performance that *does not depend on the student's actual performance*. This is appealing from an incentive-design perspective. To understand why, consider the intended use of the risk measures we aim to develop, which is to inform consequential state funding and accountability policies. Given this intended use, a principle of our framework is that the risk measures should be impervious to the activities of educational actors (i.e., districts, schools, and teachers) to the extent possible. To illustrate with a counterexample, consider a system where funding increases are provided to support at-risk students and risk status is defined

by observed performance, S_i. This would perversely incentivize the production of low test scores (and even if educational actors ignored their perverse incentives, schools and districts with low scores would receive more funding, which would feed into the perception that poor performance is rewarded). However, if risk status is defined by $\hat{S}_i$—that is, by how students are predicted to perform based on their attributes given statewide performance patterns—this undesirable design feature disappears.

A similar logic applies to the selection of the predictive attributes in the **X** vector. These variables should be chosen so that they cannot be manipulated by educational actors to the extent possible. We refer to this as the "non-manipulability principle" of the PAP framework and discuss it in more detail below.

4. Empirical Application

4.1 Data Overview

We use administrative microdata from the Missouri SLDS for our proof-of-concept empirical application. The Missouri SLDS is typical of other state systems nationwide. The foundation of the PAP framework is the cross-sectional regression described in broad terms by equation (1), which we estimate using the 2016-17 (hereafter: 2017) student cohort in Missouri. Before getting into the details of our empirical application, we remind the reader that this is a proof-of-concept exercise. Our goals are to (a) determine the feasibility of constructing PAP as a risk metric and (b) assess its potential value for use in policy. To do this, we must specify the precise variables that we will include in equation (1) and how they will be constructed. Although we try to make reasonable choices at each step of implementation, our proof-of-concept exercise is not meant to be prescriptive with regard to exactly how PAP should be constructed. Implementation in other contexts can deviate from our implementation depending on a wide range of factors including data availability, political and legal constraints, policymaker priorities, and so on. With this caveat in mind, we proceed with the details of our construction of PAP.

Equation (1) identifies two major components of the prediction framework: outcomes and

predictors. As noted above, for outcomes we use student test scores—specifically, scores on state assessments in math and ELA in grades 3-8. We standardize each test by subject-grade and define S_i for each student as the average standardized score across subjects. We restrict the analytic sample used to estimate equation (1) to students with test scores in both subjects. Recall that this does not influence our ability to produce estimates of $\hat{S}_i$ for all students because we apply the prediction model out-of-sample to untested students—i.e., we assume the model would predict their scores accurately if they were tested, on average. Under this assumption, we produce values of $\hat{S}_i$ for all students in Missouri in grades 3-12.[7]

Next, we turn to the predictors of academic performance. We include three broad types of predictor variables: (1) individual-level contemporaneous variables, (2) individual-level panel variables, or persistence variables, and (3) school-average variables. The list of available variables is in Table 1 and includes measures of student mobility (number of districts attended in year *t*, number of schools attended in year *t*), ELL status, IEP status, race-ethnicity category (where the categories are American Indian, Asian/Pacific Islander, Black, Hispanic, White, and Multi-race), gender category (male or female), FRM status, and DC status. We construct the panel variables as three-year averages of the individual-student variables taken over the current and two preceding years.[8,9] These variables capture the persistence of students' circumstances

[7] Values can also be assigned to students in grades K-2 using the same procedure. There are some technical implications with respect to variable construction for younger students that merit consideration—namely for the panel variables described below given that students' data histories do not begin until kindergarten—but in principle the predictions can be extended to earlier grades.

[8] We exclude variables that generally do not change over time, such as race-ethnicity and gender designations, from the panel variable list. For the mobility variables, we divide the total numbers of schools and districts attended by the number of years the student was enrolled in a Missouri school district in the last three years, then additionally control for the fraction of years the student was enrolled in Missouri. This three-variable set captures mobility between Missouri schools and districts and across state lines over the three-year period.

[9] Our use of three-year averages (as opposed to, say, count variables) allows us to use three years of data for students we observe for at least three years and fewer years for students new to Missouri or with missing data. For example, a student with two years of data who is FRM-enrolled in both years is coded as "100 percent" FRM-enrolled and similarly for a student with just one year of data. The alternative is to use count variables and drop students with insufficient histories (who would be treated as having incomplete **X** vectors). However, this is an inferior option from a policy perspective because we can estimate $\hat{S}_i$ for fewer students.

and are motivated by prior work on the predictive validity over academic outcomes of persistent poverty (Michelmore and Dynarski, 2017) and mobility (Goldhaber et al., 2022). The third set of variables includes school averages of the contemporaneous student variables. The school-average variables capture the predictive influence of schooling circumstances conditional on individual student circumstances.

Again, we do not wish to prescribe the precise set of variables that should be used in equation (1) or how the variables should be constructed. There are legal, political, and other considerations that will guide these decisions in different contexts (we elaborate on one example—the exclusion of racial/ethnic categories—in more detail in section 5.2). However, in order to proceed with our empirical application, we must implement decision rules that allow us to construct PAP.

4.2 Practical Issues

4.2.1 Excluded Variables and Adherence to the Non-Manipulability Principle

In the ideal implementation of our framework, the variables in the **X**-vector would be entirely non-manipulable. The non-manipulability principle leads us to exclude some information from the predictor set at the onset—examples include data on student attendance, behavioral incidents, course-taking, and grades. These and related variables are typically included in SLDS-based early warning systems (EWSs) designed to identify students at risk of poor academic outcomes, such as high school dropout (Li et al., 2016; Therriault et al., 2017). In fact, PAP can be viewed as a special case of an EWS indicator with the added constraint that the predictors are not manipulable. Manipulable variables are useful for the diagnostic purpose of early warning systems and are not problematic because EWS indicators are not high stakes, but they are a poor fit for PAP. Their inclusion in the PAP framework would create perverse incentives in the policy applications we have in mind, which have high-stakes funding and accountability consequences attached.[10]

[10] See Public Impact with Education Analytics (2021) for a related application and discussion.

While we omit some of the most manipulable variables from the prediction framework, we also acknowledge that not all of the remaining variables listed in Table 1 are entirely non-manipulable. For example, schools and districts can manipulate FRM status by adopting community eligibility, if eligible; and if not, they can manipulate individual student designations through other aspects of the NSLP (Bass, 2010). Schools and districts can also potentially manipulate other student categories including ELL and IEP status. Unfortunately, there are few strong predictors of student performance in the Missouri SLDS that are entirely non-manipulable, which suggests a tradeoff between the predictive validity of $\hat{S}_i$ and its manipulability. Ultimately, our preferred model includes all of the variables listed in Table 1 but uses DC status in place of FRM status as the measure of student poverty. We favor the use of DC status over FRM status because it is a more accurate measure and cannot be manipulated as easily (Fazlul, Koedel, and Parsons, 2023). After making this switch, students' ELL and IEP designations are the most manipulable prediction variables we use. The tradeoff between non-manipulability and predictive accuracy merits consideration in any policy application of our framework (or any other risk measurement framework).

4.2.2 Variable Weights

Even if the elements of **X** are selected to be non-manipulable, the weights—contained by the vector $\boldsymbol{\beta}_1$ in equation (1)—can still be influenced by school demographics and policies. The weights could be problematic if a particular district enrolls a disproportionate share of students with one or more of the predictive attributes. In that case, the district's own performance could meaningfully influence the weights on those attributes. For example, consider a case of extreme residential segregation by race-ethnicity in a system with two districts, A and B. If District A predominantly serves URM students and is also highly effective, the race-variable weights in equation (1) will partly reflect District A's effectiveness, leading to lower "risk" scores for URM students than would be implied by non-schooling conditions alone.

Fortunately, this concern can be circumvented by jackknifing, which is an estimation procedure that prevents individual schools and districts from influencing their own weights. In its purest form, a district-level jackknife with *J* districts involves estimating *J* "leave-one-out" versions of equation (1), where each version is estimated on *J-1* districts. The version estimated for district *j* includes data from all districts except *j* itself. The jackknifed fitted values for district *j* are a function of the characteristics of students in district *j*, **X**, and a set of weights, $\boldsymbol{\beta}_1^j$, unique to district *j* and estimated using data entirely outside of district *j*. Conceptually, these fitted values can be described as capturing the degree of risk faced by students in district *j* based on their attributes, as predicted by a statewide model outside of district *j*. The jackknifed estimates of the weighting parameters have the desirable feature that they cannot be influenced by district *j*'s own behavior.

Jackknifing is a common procedure in academic research, but at least in its purest form, it can be computationally intensive and may be unnecessarily complex for policy applications. Therefore, we explore the use of simpler variants of the jackknifing procedure. Our preferred jackknife is what we refer to as a "random-quarters" jackknife, where we randomly divide districts in Missouri into four equal-sized groups and estimate four "leave-one-group-out" jackknifed versions of equation (1). Each district's jackknifed values are from the regression that excludes the random quarter of the sample to which it belongs. In Appendix A, we confirm that other jackknifing approaches yield similar results—e.g., splitting the sample randomly into thirds, fifths, tenths, and a full jackknife (see Appendix Table A2). All of the results presented below use the random-quarters jackknife.

4.2.3 Risk Status Indicators

We use the values of $\hat{S}_i$ to divide students into high-risk and low-risk categories, mirroring the categorical structure of existing risk measurement systems. This facilitates apples-to-apples comparisons between PAP and competing measures of risk. Of course, an advantage of PAP over other risk metrics is that the underlying predicted values, $\hat{S}_i$, contain more

differentiated information about risk than is reflected in the binary categories—we return to this point in the extension section below. For now, we divide students into risk categories by specifying a threshold test value, $\tilde{S}$, that separates low-risk and high-risk students. We choose $\tilde{S}$ based on predicted test proficiency to align the categories with test policies, which are often proficiency-based, although this is not a prescriptive feature of PAP.

A full-scale policy implementation of PAP would likely match $\tilde{S}$ to proficiency targets on state tests, which are grade and subject specific. Mirroring this approach, but simplifying it and creating a degree of separation from the specific policy context in Missouri, we set a single threshold value of $\tilde{S}$ for all grades and subjects based on 2017 NAEP performance. Specifically, averaging across math and English Language Arts in grades 4 and 8, NAEP data show that 26.25 percent of Missouri students score below basic, and we use this percentile threshold—the 26.25th percentile—as $\tilde{S}$. We then apply this threshold to the Missouri state test (the Missouri Assessment Program, or MAP). That is, we assign students whose predicted test scores on the MAP are below the 26.25th percentile as high-risk students, and students with predicted scores above the 26.25th percentile as low-risk students.

Our simplified approach to setting $\tilde{S}$ based on NAEP data does not have any substantive bearing on how our framework operates. That said, an important feature of $\tilde{S}$ is that it is percentile-based rather than based on a raw test score value. This is necessary because the predicted scores, $\hat{S}_i$, are implicitly shrunken through the prediction process, and as a result, the distribution of $\hat{S}_i$ is tighter than the distribution of S_i. The use of a score-based value to set $\tilde{S}$ would result in a lower share of high-risk students identified than students whose actual test scores are below the threshold value.[11]

[11] Alternatively, a variance inflation procedure like the one discussed in Appendix G could be used to set score-based thresholds.

4.3 Statistical Summary of PAP

In this section we provide a high-level statistical summary of PAP as estimated using our preferred specification of equation (1). Our preferred specification includes all of the variables discussed above as individual, panel, and school-average variables, plus two-way interactions between these variables, and uses DC status instead of FRM status to capture economic disadvantage. We provide detailed statistical summary information for our preferred specification, along with numerous alternative specifications, in Appendices B and C.

One notable result in Appendix B is that conditional on the first-order variables, there is only a marginal gain in explanatory power from adding the interaction variables to the models. The limited impact of the interaction variables does not mean that student assignments to multiple categories do not matter—the models without interactions still allow students who belong to multiple risk categories to have lower predicted performance. Rather, the limited impact of the interactions suggests that the predictive influence of multi-category assignment can be inferred (roughly) additively. It is for this reason that we do not pursue more complex models or additional interactions, although as noted above, future work could examine the potential to improve predictive accuracy via modeling and estimation adjustments more formally.

Table 2 summarizes our risk measures overall, and within traditional categories of disadvantage, by reporting means and standard deviations of $\hat{S}_i$. First, focusing on the group-average values of $\hat{S}_i$ in the first row of Table 2, the results reflect the well-understood achievement gaps that help to motivate the policy use of traditional categories of disadvantage. The gaps in average predicted achievement by DC status, FRM status, ELL status, URM status, and IEP status are 0.58, 0.52, 0.44, 0.59, and 0.94, respectively. These gaps are in standard deviation units of test scores and large by any reasonable standard.[12]

[12] We do not report values for the many coefficients from our prediction models because the multivariate regression framework makes their interpretation intractable, especially in our richer (and preferred) specifications. That said, the mean values of $\hat{S}_i$ across student categories in Table 2 permit inference about the net direction of the model predictions. More information about the performance of the prediction model can be found in Appendices B and C.

It is a useful (albeit predictable) validity check of our framework that it replicates well-established achievement gaps on average between the categories in Table 2. But the more important information is in the second row of the table, which reveals substantial heterogeneity in the risk for poor academic performance *within* traditional categories of disadvantage. To see this, first note that column (1) shows that across all students, the standard deviation of $\hat{S}_i$ is 0.50.[13] The subsequent columns show there is almost as much variation in $\hat{S}_i$ within several of the categories as in the full sample—e.g., the standard deviations within the FRM-enrolled category, non-ELL category, and URM category are all 0.49. Table 2 provides empirical support for the intuitive claim that traditional categories of disadvantage used in state policies are coarse and mask considerable variability in student risk of poor academic performance.

4.4 Policy Simulations

The technical information discussed above and provided in detail in Appendices B and C will be helpful for individuals interested in replicating our work or developing their own versions of PAP. However, it does not answer the question of whether PAP is "good enough to be useful", which depends both on its intended purpose and the quality of alternative options. To incorporate these dimensions, we use funding policy simulations to examine how the use of PAP affects resource allocations.

Our policy simulations are based on a generalized student-weighted funding formula. The formula allocates resources to "high risk" and "low risk" students as follows:

$$N_L + (1+Z)N_H = B \qquad (2)$$

In equation (2), N_L is the number of low-risk students, N_H is the number of high-risk students, and *B* is the total budget. The amount allocated to each low-risk student is normalized to 1.0, and *Z* is a positive multiplier that captures the additional per-pupil resources distributed to high-risk

[13] This value is below 1.0 due to shrinkage in the predictions. Table 1 (including the table notes) shows that the raw standardized scores have standard deviations of approximately 1.0, which is by construction.

students. N_L and N_H are choice variables that depend on how low-risk and high-risk students are defined. We consider policies that use $\hat{S}_i$ to assign students to low-risk and high-risk categories, and compare the allocations to policies that use FRM and DC status.

The values of N_L and N_H, determined by the definitions of "low risk" and "high risk" students, along with the fixed budget *B*, will yield different values of *Z*, as described by the following re-arrangement of equation (2):

$$Z = \frac{B - N_L}{N_H} - 1 \qquad (3)$$

We impose the constraint that $B > N = N_L + N_H$, which ensures that *Z* is positive. In other words, there is enough funding to provide more than one normalized resource unit for each high-risk student.

Table 3 shows results from our first set of policy simulations. We use PAP to identify high-risk students and allocate resources to students following equation (2), then compare the allocations to alternative allocations based on using DC or FRM status to identify high-risk students. We set $B = 1.25N$ in all of our simulations (where N is the total number of students). Our results are not directionally sensitive to the value of *B*, but all else equal, larger values of *B* generate larger resource gaps between high- and low-risk students.

Each column of Table 3 shows results from a different policy parameterization, defined by the first four rows. The subsequent rows show the average resource units accruing to students with different characteristics. It is these rows that show the policy impacts of our framework in the form of changes to the resource allocations compared to DC- and FRM-based alternatives.

We walk through how to read the table using the results in column (1) under the baseline PAP settings. First, we identify high-risk students as those below the 26.25th percentile in the distribution of predicted test scores, which gives a high-risk student share of 0.2625 (rounded to 0.263 in the table). From equation (3), with *B = 1.25N*, the third row of the table shows that *Z* is

0.952. Thus, the policy allocates 1.952 resource units to each high-risk student and 1.0 resource units to each low-risk student.

The bottom panel of the table in column (1) shows the tautological result that students identified as high risk based on a low value of $\hat{S}_i$ each receive 1.952 resource units. The other rows show the average resource units accruing to students with other characteristics. For example, students with actual test scores below the 26.25th percentile receive 1.537 resource units, on average. This is below the value for students identified by $\hat{S}_i$ because the model does not predict test performance perfectly. DC and FRM students receive 1.50 and 1.40 resource units on average, respectively, and the values accruing to ELL, IEP, and URM students are similarly shown. The resources accruing to students with these different characteristics derive from the association of these characteristics with low predicted performance (i.e., $\hat{S}_i$).

The normalization of resource units to 1.0 for low-risk students facilitates straightforward comparisons within and across columns in the table. The easiest way to compare the allocations is in percentage units relative to the normalized value of 1.0, which in a funding system would correspond to a foundational per-pupil dollar value. For example, in the baseline scenario in column (1), our framework allocates 1.621 resource units per URM student, on average, or an additional 62.1 percent of the foundational amount received by a low-risk student.

Next, we turn to the comparative analyses in Scenarios 2 and 3, where we compare PAP to alternative systems that define risk based on DC and FRM status, respectively. PAP will yield different allocations for two reasons: (1) it identifies students with different characteristics as high-risk, and (2) it sets a different threshold for high-risk status—in particular, 26.25 percent of students are identified as high-risk under our baseline PAP setting, compared to 27.3 and 50.3 percent of Missouri students who are directly certified and FRM-enrolled, respectively. To isolate the impact along the first dimension, holding the fraction of high-risk students fixed, we anchor our framework to the DC and FRM data, respectively, in the first columns for Scenarios 2 and 3. We do this by resetting $\tilde{S}$ to match the share of students identified as high-risk by the DC

and FRM designations (that is, we adjust $\tilde{S}$ so the bottom 27.3 and 50.3 percent of students based on $\hat{S}_i$ are identified as high-risk using PAP). The second columns for these scenarios use the DC and FRM data to identify high-risk students. Thus, within scenarios, we compare PAP to its alternatives holding fixed the fraction of high-risk students identified (and correspondingly, the value of *Z*). This can be seen in the top two rows of Table 3.

The results from Scenario 2 show that using a DC-based definition of risk results in more resources accruing to DC and FRM students, on average, compared to defining risk using PAP. The finding for DC students is again tautological—when we define risk using DC status, each DC student receives *1+Z* resource units. The finding for FRM students follows from the strong overlap between FRM enrollment and DC status. However, the targeting of resources directly to DC students comes at the cost of lower per-pupil resources for other types of students at risk of low academic performance. First, and unsurprisingly, the PAP-based system allocates more resources to students with low test scores and low predicted test scores (where the latter reflects the same tautology described above). This empirically confirms that PAP is more effective at identifying students at risk of poor academic performance than DC status. The PAP-based system also allocates more resources to ELL, IEP, and URM students, and by a substantial margin in all three cases.

A similar set of results unfolds in Scenario 3, which is anchored to FRM status. The magnitudes of the per-student allocations are smaller compared to Scenario 2 because *Z* is smaller (which, in turn, is because there are so many FRM-enrolled students). Still, the general pattern of findings holds. The FRM-based system is, by definition, better at targeting resources to FRM students, and similar or worse at targeting resources to every other category associated with student risk.

We can also compare our results across scenarios, which further allows PAP to influence student allocations by impacting the value of *Z*. We focus on the comparison of columns (1) and (5), which pits PAP against the FRM-based alternative, inclusive of the difference in the value of

Z. This comparison shows PAP allocates more resources per student along every measured dimension of risk except FRM status (including DC status, albeit marginally). For most non-FRM characteristics, PAP leads to substantially more resources per student, on average. This reflects both the broader targeting of resources in our framework based on the full vector of information, **X**, and the fact that we identify fewer high-risk students, which increases *Z*. A summary of this comparison is as follows: the PAP-based policy is more effective at targeting resources toward high-risk students both (a) as identified in terms of academic performance and (b) using most other common categorical definitions.

The comparisons in Table 3 are informative but generic. In Table 4, we make a complementary comparison grounded in a real-world policy by grafting the core features of California's high-profile Local Control Funding Formula (LCFF) onto the Missouri data—namely, LCFF's supplemental and concentration grants. This allows us to compare allocations based on PAP to what they would look like if LCFF were implemented in Missouri.

California's LCFF allocates additional resources to "targeted disadvantaged pupils" as identified by ELL status, FRM status, and foster youth. Students who belong to any category are counted, and students cannot be double-counted.[14] We implement a modified version of LCFF that ignores foster youth because we do not have access to data for this designation in Missouri.[15]

The LCFF also provides additional per-pupil funding to districts with concentrated need (Johnson and Tanner, 2018). Based on 2021 LCFF funding rules, we convert the LCFF formula to a student-level allocation model to fit within our analytic framework. The student-level version of the LCFF funding formula is as follows:

$$F_i = F_0 + (0.2 * F_0) * D_i + (0.65 * F_0) * \max[D_d - 0.55, 0] \quad (4)$$

[14] See here for more information about LCFF (link retrieved 11.01.2021): https://www.cde.ca.gov/fg/aa/lc/lcffoverview.asp; also Johnson and Tanner (2018).

[15] The implications of omitting foster youth should be small because few children in Missouri (1.4 percent in 2020) are in foster care (link retrieved 11.01.2021: https://www.stltoday.com/news/local/state-and-regional/missouri-foster-parents-get-help-from-legislature-but-why-are-more-children-coming-into-state/article_24fab000-d8ed-5ff7-a20d-eaa88a1ae21f.html), and of those that are, many are likely already FRM-enrolled.

In equation (4), F_i is the resource allocation for student *i*, F_0 is the base amount, D_i is an indicator equal to one if the student belongs to a "targeted-disadvantage" category (i.e., ELL or FRM), and D_d is the share of students in a targeted disadvantage category in district *d*. In words, the LCFF allocates an additional 20 percent of the base funding level for each targeted student, then an extra 65 percent of the base amount to districts for each targeted student in excess of 55 percent of enrollment. Following our analytic structure from above, we normalize F_0 to 1.0.[16]

We apply the "pseudo-LCFF" in Missouri and assign each student a value for F_i. To compare the subsequent student allocations to PAP-based allocations, we first use the sum of the F_i values across all students to calculate the total pseudo-LCFF budget in Missouri—i.e., the total amount allocated to students under the LCFF rules—which we set as *B* in equation (2). For notational convenience, we write the total budget in units of *N* as above (applying the LCFF rules in Missouri, B=1.152N—see Table 4). Then, using this budget, we allocate resources following equation (2) and set $\tilde{S}$ at the 26.25th percentile of test scores. This facilitates a fixed-budget comparison between the pseudo-LCFF and the PAP-based alternative.

Before turning to the results, we note two key features of the pseudo-LCFF. First, it accounts for two categories of disadvantage simultaneously (FRM and ELL), albeit simply. Second, the "concentration" component of the formula allocates more resources to districts with concentrated need. Our PAP-based policy does not include a directly-analogous concentration component, but the use of the school-level variables in our prediction model is similarly-spirited. That is, to the extent that concentrated student risk is associated with lower test scores conditional on students' individual risk, our model will assign lower values of $\hat{S}_i$ to students in high-concentration schools. (We could also modify our policy structure to copy the LCFF by

[16] The way the student-level formula is written in equation (4), each student in a district with $D_d > 0.55$ receives a small positive increment, which is mathematically equivalent to identifying the fraction of students above 55 percent and providing the district with the full increment for each of these students.

allocating more resources to schools and districts with high proportions of low-$\hat{S}_i$ students, although we do not pursue this extension here.)

Table 4 shows the results comparing resource allocations based on PAP to the pseudo-LCFF in Missouri. Along most dimensions, including the key metrics of test performance and predicted test performance, PAP yields more resources per high-risk student than the pseudo-LCFF. The one exception is FRM students, who are explicitly targeted by LCFF and receive modestly higher resources. However, notably, this result does not translate to DC status under LCFF, which is a more accurate measure of poverty in Missouri (Fazlul, Koedel, and Parsons, 2023).[17]

PAP yields higher per-student allocations along most dimensions because it explicitly accounts for them in the test prediction model. Moreover, the funds targeted for high-risk students are less diluted under PAP, accruing to the bottom 26.25 percent of students. In contrast, under the pseudo-LCFF the excess budget is distributed across 51.1 percent of Missouri students (the unduplicated sum of FRM and ELL students).

Finally, Tables 3 and 4 focus on student-level allocations, but it is difficult to target resources to individual students differentially within a school. The extent to which student-level changes in resources will impact school-level allocations, whether in our framework or any other framework, depends on the distribution of student characteristics across schools. Residential sorting implies that changes to student-level allocations will translate at least partly to changes in school-level allocations. In Appendix D, we confirm this is true by examining correlations between school-level allocations and school characteristics.

In summary, our policy simulations incorporate realistic counterfactuals and show that using PAP to identify high-risk students in a funding policy would meaningfully change the allocation of resources. In terms of targeting students with low predicted or actual academic

[17] ELL students are also explicitly targeted by the pseudo-LCFF, but the effect on ELL students is overwhelmed by other factors. The ELL comparison is not especially useful due to the low ELL share in Missouri (in contrast to California).

performance, a PAP-based system is clearly preferred. Along other dimensions of measured risk, there are tradeoffs that depend on policy objectives. We conclude by noting our simulations only consider systems that fully substitute between risk metrics. PAP could also be used to augment existing systems as an additional dimension of funding consideration; or with lower stakes, for diagnostic purposes to help policymakers better understand the allocation of resources under current formulas.

5. Extensions & Policy Considerations

5.1 PAP Flexibility in Response to Changes in the Underlying Data

An advantage of PAP is that it can handle changes in the underlying variables used to measure student risk, or the information they contain, with greater flexibly and less disruption than systems that rely on categorical assignments. For example, consider a state that measures risk categorically using DC status. If the meaning of this variable were to change in a way that makes it less informative, perhaps due to a change to the state's Broad Based Categorical Eligibility policy, it could greatly influence measured risk.[18] In contrast, the effect of the same data change on PAP will be dulled because of the remaining predictors in the **X** vector. That is, to the extent the other predictors are correlated with DC status, their weights in the coefficient vector $\boldsymbol{\beta}_1$ will change to lessen the total impact on the predictions.

This is a clear theoretical benefit of our approach, but does it help in practice? We answer this question in two ways. We discuss the results here and show them in Appendix E. First, in Appendix Table E1, we report correlations of $\hat{S}_i$ as estimated by 12 different versions of equation (1) (the 12 versions are described in Appendix B). Column 1 of the table shows that compared to our primary specification, most alternative specifications yield similar risk values for students. For example, of the 11 correlations we produce between our preferred specification and its alternatives, only 3 are below 0.89, and the minimum value is 0.84, which is for a

[18] Broad-based categorical eligibility policies allow families with higher incomes to qualify for SNAP, the primary program that is used for direct certification.

particularly sparse version of equation (1). Broadly speaking, Appendix Table E1 confirms the risk metrics for individual students are fairly stable as we change the attributes included in **X**.

Next, in Appendix Table E2, we assess the implications of a hypothetical switch from using *free meal* (FM) status to using DC status to identify students from low-income families. A similar data substitution—i.e., from using FRM status to using DC status—is occurring or under consideration in many states today due to concerns about the accuracy of FRM data with the introduction of community eligibility for free meals in the NSLP (Chingos, 2018; Greenberg, 2018). The data switch we consider is conceptually more appealing because FM enrollment and DC status in Missouri are purported to identify students from families at the same income threshold—130 percent of the poverty line or below.[19]

The results in Appendix Table E2 make clear that the flexibility of our approach is a significant practical benefit in the event of this data substitution, as PAP is much less volatile than the categorical alternative. Specifically, using PAP, 4.4 percent of students experience a change in their risk status due to the change from using FM to DC data to identify students from low-income families, compared to 17.8 percent of students using the categorical system. Of course, from a statistical standpoint, the model-based approach that undergirds PAP must perform better than the categorical approach in this exercise. However, it is important to recognize the categorical comparison is a fundamentally accurate characterization of current systems, which helps to explain the policy consternation as states consider new definitions of low-income status in reaction to the introduction of community eligibility in the NSLP.

5.2 PAP Without Taking Explicit Account of Race-Ethnicity

The goal of equation (1) is to predict academic performance and race-ethnicity is a consistently strong predictor. Statistically, whether it is desirable to use information on race-ethnicity to improve the predictions is unambiguous: these data should be used. However, some have argued that race-ethnicity data should be omitted from prediction models such as ours based

[19] Although in practice FM enrollment is oversubscribed, partly due to community eligibility and partly for other reasons, as shown by Domina et al. (2018) and Fazlul, Koedel, and Parsons (2023).

on the concern that it sets different expectations for academic performance across racial-ethnic groups.[20] Moreover, depending on the intended use of PAP, it may not be legal to allow for a direct predictive impact of race-ethnicity. As such, we briefly consider how the omission of racial-ethnic information from the prediction model impacts PAP.

First, we re-estimate our preferred specification omitting all information about race-ethnicity. This produces estimates of $\hat{S}_i$ that are not directly influenced by race-ethnicity (though $\hat{S}_i$ will still be correlated with race-ethnicity given the inclusion of other predictors that are correlated with both race-ethnicity and student outcomes). Appendix Table F1 shows results from this model in the same format as Appendix Table B1. A comparison between the versions of our preferred model that do and do not include the race-ethnicity variables shows that the predictions from the latter are clearly worse. For example, the R-squared is 0.03 points lower, the MSE is 0.02 points higher, and the classification error rate is 1.5 percentage points higher.

Next, in Appendix Table F2 we use our policy simulation to show how resource allocations are affected if we use the values of $\hat{S}_i$ from the restricted model. The results are compared to the findings from our baseline scenario in Table 3. Most of the findings are similar regardless of whether we use the full or restricted versions of the model, which is as expected given the general robustness of the prediction framework shown in Appendix Table E1. However, there is one exception precisely where it is anticipated: using the model that is stripped of all racial-ethnic information results in less resources accruing to URM students. (Still, the amount allocated to URM students is larger than in funding formulas that rely on FRM or DC data, or the pseudo-LCFF.)

5.3 Augmentation for IEP Students

There are some aspects of current systems that PAP does not meaningfully improve upon. The most obvious example is students with severe IEPs, for whom both broad categorical

[20] Ehlert et al. (2016) provide a deeper discussion on this issue in the context of school accountability systems.

designations and PAP are insufficient to capture the extent of their needs. As a result, funding add-ons will be needed for IEP students to augment any general framework. For accountability policies, which we discuss briefly below, it may also be desirable to exclude these students, or at least those whose disabilities are deemed sufficiently severe, as is already common practice in many states.

5.4 Using Other Academic Outcomes to Anchor PAP

There is nothing that requires PAP to be anchored to student test scores. In this section we consider two alternative academic performance indicators—student attendance and high school graduation. Each of these possibilities has strengths and weaknesses compared to test scores. Our view is that test scores offer the best combination of properties, but again, the use of test scores as the anchor for PAP is not a prescriptive feature.

First, student attendance is an appealing anchor for PAP because it is linked to a broad swath of indicators of disadvantage (Ready, 2010). Of the available alternatives, it is also the one with the greatest data coverage—virtually every student should have an attendance record in every year. However, there are two main limitations of using attendance to anchor PAP. First, conceptually, attendance is not a pure measure of academic performance—it is arguably more of an input than outcome—and may be undesirable for this reason. Second, student attendance is a less-differentiated measure than student achievement on state tests, and relatedly, the data elements available in state data systems do not predict variation in attendance as well as they predict variation in test scores. For example, we estimated a version of our preferred model where the attendance rate was the dependent variable, and the R-squared was just 0.07, compared to 0.29 for test scores.[21] These technical limitations will lead to less differentiation in PAP anchored to attendance.

[21] One way to quickly convey the (relatively) limited variation in attendance is to note that a large fraction of students have very high attendance—e.g., in Missouri in 2017, 87 percent of students attended 90 percent of days or more).

High school graduation is another interesting alternative. It is appealing conceptually because graduation is an important goal of the education system. However, as an anchor for PAP, it is problematic for several reasons. First, graduation rates can be improved by increasing learning and improving student supports *or* by reducing standards, and it is difficult to disentangle these mechanisms. If different standards are applied to students who differ by their **X**-vector attributes, PAP would produce misleading risk gaps. Test scores do not have this limitation because all students take the same test. Another challenge of using high school graduation is that only 12th grade students graduate each year (with few exceptions). This means that PAP will always be backward looking—i.e., it will assign risk values based on how student attributes predicted graduation for an older cohort, and potentially a much older cohort (for early-grade students). Finally, a third limitation of using high school graduation is that it is a relatively undifferentiated performance measure. It splits students into just two blunt categories (graduated or not), and most students graduate.

In the context of these alternatives, test scores are appealing given their combination of (a) data coverage (a large fraction of the student population is tested), (b) both differentiation and predictable differentiation, and (c) conceptual alignment with one key purpose of schooling, which is to promote student achievement. That said, future research could consider combining PAP anchored to test scores and other student outcomes, including but not limited to attendance and high school graduation, in order to provide a more holistic indication of risk along multiple dimensions of academic performance.

5.5 Monitoring Achievement Gaps

Our policy simulations focus on school funding. However, PAP also has features that make it appealing for use in other policies, such as for monitoring achievement gaps within schools and districts.[22] PAP can consolidate accountability information across the multiple categories of risk tracked by many states (e.g., FRM, ELL, IEP, URM) into a singular indicator,

[22] Many states informally monitor within-school achievement gaps, and these gaps are incorporated into some states' formal accountability policies (Martin, Sargrad, and Batel, 2016).

which in turn can reduce information overload and mitigate type-I errors resulting from the multi-category approach (Davidson et al., 2015). Some states do something similar now with "super subgroups" that combine students from multiple risk categories, but current practice does not account for compositional differences in the super subgroup across schools and districts, clouding inference. Using PAP for accountability offers the simplifying benefit of the super-subgroup approach, while at the same time minimizing the potential for misleading comparisons due to differences in the composition of super subgroup across schools and districts. We elaborate on the potential use of PAP in accountability policies in Appendix G.

5.6 Uncoarsened PAP

In our policy simulations, we use binary risk categories to group students based on their underlying PAP values. This facilitates a straightforward comparison to *status quo* systems and lends policy relevance to our work given the strong cultural norm in education of grouping students categorically (and often in a binary fashion). However, by coarsening $\hat{S}_i$, we strip away much of the differentiating information about student risk it contains.

In the interest of brevity, we do not examine the potential for using uncoarsened $\hat{S}_i$ values to enhance policy practice here. A productive avenue of future research would be to consider how using multiple risk categories—e.g., moving from a two-category binary system to a three-, four-, or five-category system—could improve resource targeting by facilitating the allocation of additional resources to the highest-risk students. The ability to specify multiple risk categories may also be of use to researchers evaluating interventions designed to improve outcomes for at-risk students, as it would better allow for the analysis of heterogeneous program

impacts across the risk distribution.[23] In either application, the limiting case would involve using the fully uncoarsened $\hat{S}_i$ values.[24]

6. Conclusion

Motivated by the inadequacy of risk measures currently used in consequential educational policies, we develop and test a new measure of student risk, which we call Predicted Academic Performance, or PAP. PAP measures student risk in precise terms—it captures a student's risk of poor academic performance. It also modernizes the approach to risk measurement compared to available alternatives. PAP is a flexible measure and less sensitive to disruptions caused by changes to the data available for the purpose of risk measurement. The NSLP's Community Eligibility Provision is a recent example of such a disruption. Finally, PAP is designed for use in consequential education policies. Although the risk measures it produces are not perfectly non-manipulable, which is the theoretical ideal, the data and estimation procedures outlined in this article aim to minimize their manipulability.

We apply and test the policy value of PAP using the Missouri SLDS in a proof-of-concept exercise. Unsurprisingly, we find that PAP is more effective than common poverty-based risk indicators (FRM and DC status) at identifying students at risk of poor academic performance. This is a purposeful design feature of PAP and, as a result, should generalize broadly. We further show that in Missouri, PAP is more effective at identifying at-risk students defined by other traditional indicators of disadvantage including URM status, ELL status, and IEP status. Future research to explore the generalizability of these findings using SLDS data in other states would be valuable.

[23] Even in its coarsened state, the ability of PAP to better identify students at-risk of poor academic performance, as well as the added flexibility it provides in defining specific performance thresholds, should be of value in program evaluation research, particularly in comparison to currently available alternatives.

[24] Such an exercise may yield useful theoretical insights, although it would be less policy relevant (at least in the near term) given the predominant category-based policy infrastructure in education. In addition to being of less direct use in policy, there are also analytic challenges associated with developing a system based on the fully uncoarsened $\hat{S}_i$ values, which we discuss in Appendix C.

Once implemented, PAP is well-suited for continuous improvement, which is another advantage over current systems. For example, the set of predictor variables can be augmented in real time as new and higher-quality data become available. It will be important to monitor the potential for measurement disruptions from this kind of augmentation from year-to-year, but the basic diagnostics we present using Missouri data suggest that PAP will not change dramatically in response to most data changes. Moreover, some "drift" in PAP from year-to-year as new measures become available, and/or relationships between students' underlying risk indicators and academic performance change, may be desirable as it will allow PAP to adjust over time to evolving education circumstances. The alternative is a stagnant measurement system that changes less often but more disruptively.

We construct PAP based on student test performance, but the PAP framework can be used to measure risk along other dimensions of academic performance—e.g., in terms of attendance, graduation, and college matriculation. These metrics could replace test scores in the framework or, more likely, augment them. For instance, each student's total risk score could be a weighted average of risk assessed by different indicators of academic performance. PAP could also be applied to emerging measures of student well-being, such as social-emotional measures (Loeb et al., 2019). Extensions along these lines would require research to assess their costs and benefits, but the flexibility inherent to the framework allows for these kinds of continuous improvement efforts.

More broadly, PAP can be viewed as part of a larger effort to leverage SLDS data to improve the efficacy and equity of schooling. We developed PAP to be used as an input in consequential state funding and accountability policies, but in related applications, SLDS data can also be used to help schools and districts identify at-risk students in real time. As noted above, PAP is a special case of an EWS indicator with the added constraint that the included predictors are not manipulable. The non-manipulability principle is not necessary in states' early warning systems because there are no financial or accreditation stakes tied to EWS indicators. Most states do not have early warning systems in place, but the list of states with such systems is

growing (examples include Massachusetts, Montana, and Rhode Island). SLDS-based metrics designed for policy use—such as PAP—and related metrics designed for diagnostic purposes—such as EWS indicators—can be complementary in systems that both (a) allow states to incorporate the rich information available in SLDS data into the metrics they use to inform consequential policies and (b) provide schools and districts with regular, real-time, and low-stakes feedback about student risk.

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Table 1. Descriptive Statistics for Missouri Students, 2017.

	Mean	SD
Demographics		
Female	0.49	0.50
American Indian	0.00[a]	0.07
Asian/ Pacific Islander	0.02	0.15
Black	0.16	0.37
Hispanic	0.06	0.24
White	0.72	0.45
Multi-race	0.03	0.18
English Language Learner	0.04	0.20
Individualized Education Program	0.13	0.34
Poverty Measures		
Directly Certified	0.27	0.45
Free and Reduced-Price Lunch Enrolled	0.50	0.50
Free-Lunch Enrolled	*0.44*	*0.50*
Reduced-Price Lunch Enrolled	*0.06*	*0.24*
Mobility Measures		
Number of Districts Attended	1.04	0.22
Number of Schools Attended	1.05	0.24
Test Scores (Standardized)		
Average Math and English Language Arts	0.01	0.92[b]
N (students)	698,726	

Notes: This table shows the summary statistics for students in Missouri in the 2016-2017, restricted to students in schools with at least 25 students enrolled. Test scores are from a reduced sample of 387,317 students in grades 3-8 with math and communication arts tests.

[a]0.4 percent of Missouri students are American Indian

[b] The standard deviations of the standardized math and English Language Arts tests in the analytic sample are 0.99 separately; the standard deviation of students' averaged standardized scores is lower.

Table 2. Means and Standard Deviations of $\hat{S}_i$ Overall, and Within Traditional Categories of Disadvantage.

	All students	DC		FRM		ELL		URM		IEP	
		DC	Not DC	FRM	Not FRM	ELL	Not ELL	URM	Not URM	IEP	Not IEP
Average $\hat{S}_i$	0.03	-0.39	0.19	-0.23	0.29	-0.39	0.05	-0.43	0.16	-0.78	0.16
Standard deviation of $\hat{S}_i$ (with shrinkage)	0.50	0.44	0.42	0.49	0.35	0.44	0.49	0.49	0.42	0.38	0.39
Share of students in this category	1.0	0.27	0.73	0.50	0.50	0.04	0.96	0.22	0.78	0.13	0.87

Table 3. Resource Allocation Policy Simulations, Results Part I: Average per-Student Allocations Using PAP to Identify High-Risk Students, Versus DC Status and FRM Status.

	Scenario 1: $\tilde{S}$ set at basic/below basic achievement percentile (Baseline Scenario)	Scenario 2: $\tilde{S}$ set so the high-risk student share matches the DC share		Scenario 3: $\tilde{S}$ set so the high-risk student share matches the FRM share	
	Use $\hat{S}_i$ to define high risk	Use $\hat{S}_i$ to define high risk	Use DC to define high risk	Use $\hat{S}_i$ to define high risk	Use FRM to define high risk
N(H) Share	0.263	0.273	0.273	0.503	0.503
N(L) Share	0.737	0.727	0.727	0.497	0.497
Z	0.952	0.916	0.916	0.497	0.497
B	1.25*N	1.25*N	1.25*N	1.25*N	1.25*N
Average resource units per student, by type, where a value of 1.0 represents the normalized resource allocation to low-risk students:					
Actual Test Score (S_i) below 26.25th percentile	1.537	1.530	1.445	1.403	1.379
Predicted test score ($\hat{S}_i$) below 26.25th percentile	1+Z=1.952	1+Z=1.916	1.500	1+Z= 1.497	1.400
DC	1.500	1.500	1+Z=1.916	1.482	1.490
FRM	1.400	1.400	1.489	1.391	1+Z= 1.497
ELL	1.636	1.631	1.335	1.456	1.405
IEP	1.910	1.880	1.337	1.494	1.316
URM	1.621	1.618	1.432	1.455	1.404
N	698,726	698,726	698,726	698,726	698,726

Notes: Using different values of *B*, subject to the constraint $B > N$, does not affect the findings directionally, although it does increase the per-pupil dollar gaps for all student categories relative to 1.0.

Table 4. Resource Allocation Policy Simulations, Results Part II: Average per-Student Allocations Under our Framework versus Pseudo-LCFF, Holding the Budget Fixed Based on the Projected LCFF Amount.

	Our Framework	Pseudo-LCFF
N(H) Share	0.263	0.511
N(L) Share	0.737	0.489
Z	0.570	N/A
B	1.152*N	1.152*N
Average resource units per student, by type, where a value of 1.0 represents the normalized resource allocation to low-risk students:		
Actual Test Score (S_i) below 26.25th percentile	1.322	1.235
Predicted test score ($\hat{S}_i$) below 26.25th percentile	1+Z=1.570	1.271
DC	1.299	1.287
FRM	1.239	1.287
ELL	1.381	1.305
IEP	1.545	1.182
URM	1.372	1.295
N	698,726	698,726

Notes: *B* is determined based on the budget implied by the pseudo-LCFF, which we implement as described in the text. We convert the budget into units of *N* to facilitate comparability with other portions of our analysis. The high-risk group under pseudo-LCFF is as defined by that policy: the sum of ELL and FRM (unduplicated).

Appendices

Appendix A: Supplementary Tables

Appendix Table A1. Correlations of $\hat{S}_i$ in the Full Sample when the Predictive Regression is Estimated using Test Data from Grades 3-8, Grades 3-5 Only, and Grades 6-8 Only.

	$\hat{S}_i$ estimated using data from test takers in grades 3-8	$\hat{S}_i$ estimated using data from test takers in grades 3-5	$\hat{S}_i$ estimated using data from test takers in grades 6-8
$\hat{S}_i$ estimated using data from test takers in grades 3-8	1.0	--	--
$\hat{S}_i$ estimated using data from test takers in grades 3-5	0.977	1.0	--
$\hat{S}_i$ estimated using data from test takers in grades 6-8	0.974	0.930	1.0

Notes: We use our baseline jackknifing scenario that jackknifes the data into four equal-sized groups of districts to produce these correlations.

Appendix Table A2. Correlations of $\hat{S}_i$ in the Full Sample Under Different Jackknifing Scenarios.

	"Leave-out-one-quarter" jackknife (baseline)	"Leave-out-one-third" jackknife	"Leave-out-one-fifth" jackknife	"Leave-out-one-tenth" jackknife	"Leave-out-one-district" (pure) jackknife
"Leave-out-one-quarter" jackknife (baseline)	1.0	--	--	--	--
"Leave-out-one-third" jackknife	0.988	1.0	--	--	--
"Leave-out-one-fifth" jackknife	0.987	0.987	1.0	--	--
"Leave-out-one-tenth" jackknife	0.995	0.988	0.990	1.0	--
"Leave-out-one-district" (pure) jackknife	0.983	0.976	0.987	0.984	1.0

Appendix B: Detailed Statistical Summary of PAP

Appendix Table B1 provides statistical summary information for variants of equation (1) that include different combinations of variables in **X**. Rows (*a*)-(*d*) include only student-level variables to predict test scores, rows (*e*)-(*h*) build on the models in rows (*a*)-(*d*) by adding corresponding panel variables, and rows (*i*)-(*l*) further add school-level variables. Within each set of rows, the models become increasingly rich moving down in the table. The last row within each horizontal panel (rows (*d*), (*h*), and (*l*)) also includes two-way variable interactions. The table notes give precise details about each specification. Our preferred specification is shown in row (*l*). It uses all available information, includes two-way interactions between the individual student, panel, and school-aggregate variables, and uses DC status instead of FRM status to capture economic disadvantage.

The columns provide statistical information about the models. In column (1), the R-squared values range from 0.21 in our sparsest specification to 0.29 in the models with the most predictive power (including our preferred model). Note the maximum possible R-squared value for each specification is below 1.0. This is because there is test measurement error in S_i and school effects explain some of the variance in student outcomes but are not accounted for in the model. This puts a ceiling on the maximum feasible R-squared; a rough estimate is that the maximum should fall in the range of 0.70-0.80.[25] Scaling the estimated R-squared from our preferred specification in row (*l*) by the center of this range—0.75—gives an *ad hoc* "effective R-squared" of about 0.39. This is our best estimate of the share of the explainable variation in student test scores accounted for by our preferred model.

Unfortunately, it is difficult to gain insight about the efficacy of our predictions from this

[25] First, measurement error attributable to the testing instruments accounts for about 10 percent of the variance in these tests (e.g., see Data Recognition Corporation, 2019), and following Boyd et al. (2013), if we use a broader definition of test measurement error it roughly doubles this value to 20 percent. In addition, based on Konstantopoulos and Borman (2011), unobserved factors across schools—inclusive of (and arguably primarily consisting of) school effects—can be estimated to account for up to an additional 10 percent the variance in scores. Subtracting these variance shares from the maximum R-squared value of 1.0 yields a feasible maximum in our application in the range of 0.70-0.80.

number. An R-squared value that is too low is undesirable because it would imply poor predictions, but an R-squared that is too high is also undesirable because some distance between S_i and $\hat{S}_i$ is appealing from an incentive-design perspective, per the discussion in the text. The R-squared values reported in Appendix Table B1 do not seem particularly "high" or "low" at a cursory glance, although it is a diagnostic limitation that there is no concrete way to judge the value of PAP based on the predictive power of the model (this motivates our policy simulations).

Complementing the R-squared values, column (2) shows mean squared errors (MSEs) for the predictions relative to observed test scores, and columns (3)-(5) show error rates for the binary predictors of which students score below $\tilde{S}$. For the results in these latter columns, we assign the lowest 26.25 percent of students based on $\hat{S}_i$ to the below-basic category then compare their predicted assignments to assignments based on their actual test scores. A false-positive is a student we assign as "high risk" based on $\hat{S}_i$ but who scores at or above the 26.25th percentile in reality; and vice-versa for a false negative. The MSE and error-rate numbers come with the same interpretive caveats as the R-squared values: numbers that are too high, or too low, are both of concern.

Although it is difficult to draw conclusions about the general efficacy of our framework from these numbers, Appendix Table B1 does provide several useful insights. One is that poverty status variables—whether FRM or DC—add substantial predictive value to the model. Relative to our specifications in rows (*a*), (*e*), and (*i*) that omit this information, the R-squared values increase by 3-5 percentage points in rows where we include it in various forms.[26] Another

[26] Between the two, FRM data are more predictive of test scores than DC data (this can be seen by comparing rows (*b*) and (*c*), (*f*) and (*g*), and (*j*) and (*k*)). In results omitted for brevity, we confirm DC status is a stronger predictor of low test scores than FRM status for individual students, but DC data contribute less explanatory power because there is more variance in FRM data (i.e., the FRM-enrolled share is closer to 0.50). Recall that our preference for using DC data is not based on maximizing predictive power, although it is helpful that there is not a major loss of predictive power in switching from FRM to DC data, especially in our richest specifications. Below we show that the predicted values, $\hat{S}_i$, are highly correlated in models that switch between using FRM and DC data.

insight, noted briefly in the text, is that conditional on the first-order variables, there is only a marginal gain in explanatory power from adding the interaction variables to the models.

Appendix Table B1. Statistical Output from Various Test Prediction Models. Row (*l*) Shows our Preferred Model.

	R-squared from predictive linear regression	MSE	Classification error rate percentage (i.e., predicted status ≠ actual status)		
	(1)	(2)	(3)	(4)	(5)
Predicting students' contemporary test scores using:			All	False positive	False negative
(a) Individual contemporary variables	0.213	0.67	23.56	8.92	14.64
(b) Individual contemporary variables with FRM	0.266	0.62	24.12	12.55	11.57
(c) Individual contemporary variables with DC	0.248	0.64	23.72	11.10	12.62
(d) Individual contemporary variables with DC and two-way interactions	0.251	0.63	24.32	12.79	11.53
(e) All individual variables in (a), plus corresponding panel variables	0.221	0.66	23.90	10.86	13.04
(f) All individual variables in (b), plus corresponding panel variables	0.277	0.61	23.93	12.18	11.75
(g) All individual variables in (c), plus corresponding panel variables	0.259	0.63	24.12	12.40	11.72
(h) All individual variables and two-way interactions in (d), plus corresponding panel variables and two-way panel interactions	0.263	0.62	24.09	12.53	11.56
(i) All individual and panel variables in (e), plus corresponding school-level aggregates	0.250	0.63	24.21	12.31	11.90
(j) All individual and panel variables in (f), plus corresponding school-level aggregates	0.290	0.60	23.81	12.20	11.61
(k) All individual and panel variables in (g), plus corresponding school-level aggregates	0.282	0.61	24.09	12.81	11.28
(l) All individual and panel variables and two-way interactions in (g), plus corresponding school-level aggregates and two-way school level interactions	**0.290**	**0.60**	**23.81**	**12.59**	**11.22**
N (Test Takers in Grades 3-8)	387,317				
N (Schools)	1,749				

Notes: Rows (a) – (d) include individual contemporary variables for students. Row (a) includes information about mobility, EL status, IEP status, sex, and race-ethnicity indicators. Row (b) adds FRM status to the variable list in row (a), and row (c) replaces FRM status with DC status. Row (d) includes all the variables in row (c) and adds all possible two -way interactions of these variables. Rows (e) to (h) include individual level panel variables corresponding to those in rows (a) – (d). Row (e) adds three-year averages of school and district mobility, share of years spent in a Missouri public school in the last three years as well as separate variables indicating the share of the last three years spent as an EL and IEP student. Model (f) adds the share of years as an FRM student, model (g) replaces that with DC status panel variable, and model (h) adds two-way interactions for all panel variables used in model (g), along with previous interactions of the individual variables. Finally, models (i) – (l) add school level aggregate variables to models (e) – (h) in the same fashion. The R-squared values indicate the share of the variance in the outcome—in this case, the student's year-t standardized test score averaged over math and communication arts that can be explained by the variables in each row. The binary classification error rates are calculated as the fraction of students whose predicted binary proficiency classification differs from their actual classification based on their observed test scores.

Appendix C: Supplementary Information about the Test Prediction Models

In this appendix, we provide additional details about the prediction models beyond what is shown in Appendix B. We do not report values for the individual coefficients in the prediction models because the multivariate regression framework makes it difficult to gain inference from them, especially in our richer (and preferred) specifications that include overlapping information (e.g., contemporary and panel measures of the same concepts, interactions of variables, etc.).[27] Instead, in Figure C1 we show the distributions of predicted scores, $\hat{S}_i$, for the different specifications shown in Appendix Table B1.

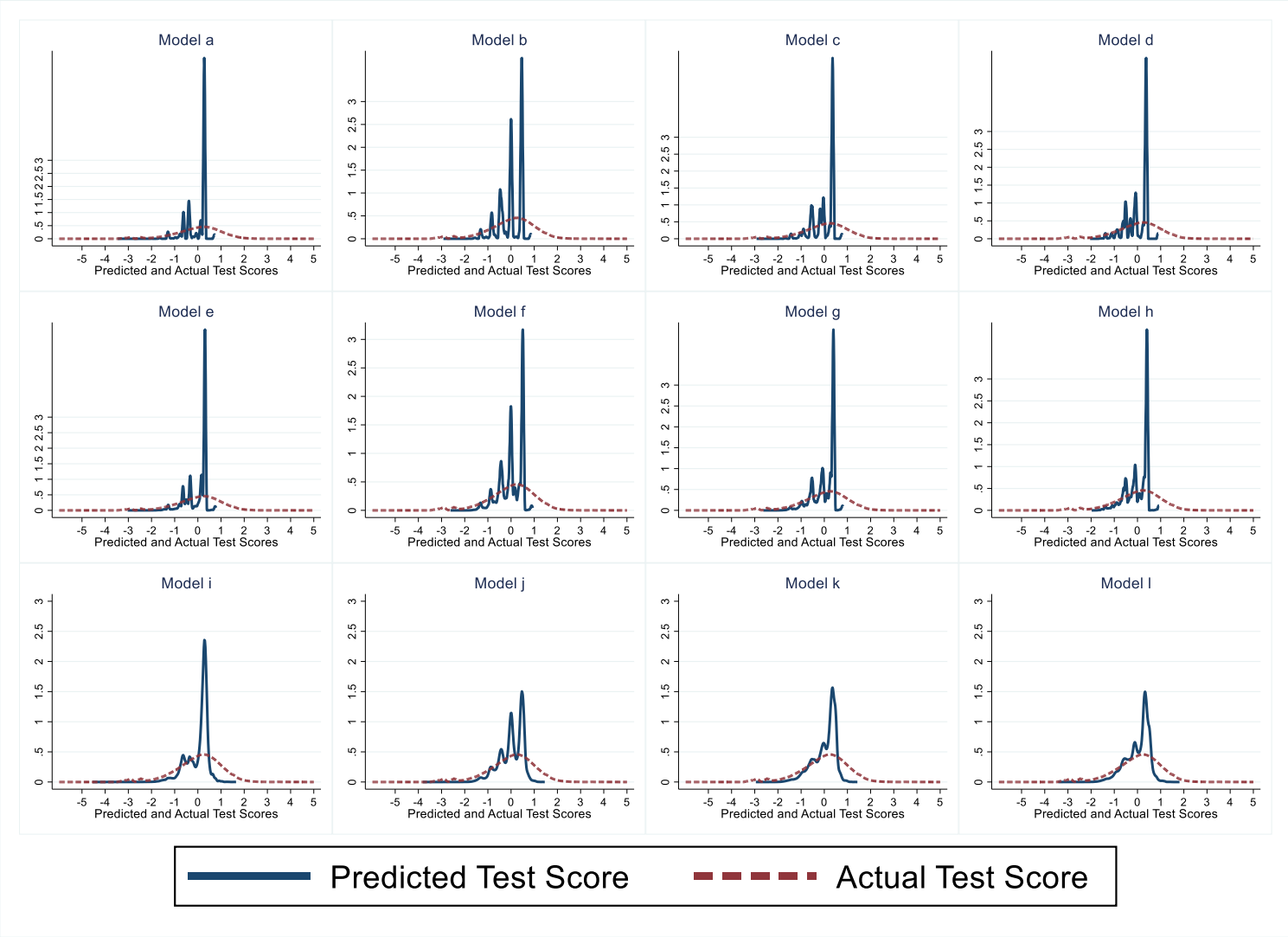
There are two reasons we show the distributions. The first is to highlight their "lumpiness," especially for the sparser versions of the prediction model. The lumpiness is not surprising because all of the student-level control variables in the models are binary or categorical indicators. When we add the panel versions of the variables to the prediction model it facilitates greater dispersion of the predicted values, and even more so when we add the school-average variables. This explains why the distributions of $\hat{S}_i$ are less lumpy going down the columns of graphs in Figure C1. Still, none of the distributions of $\hat{S}_i$ in the figure are smooth, which reflects the nature of the underlying data.

The lumpiness is a limitation in the sense that it would be beneficial to have better-differentiated, consequential predictors of student test scores available in state data (i.e., continuous or near-continuous predictor variables of consequence). In the absence of such variables, the distributions of $\hat{S}_i$ are necessarily lumpy. That said, and following on the discussion in the text, it is still true that the degree of differentiation in $\hat{S}_i$ is far greater than the differentiation currently facilitated by states' categorical systems for identifying at-risk students. This is because the model allows students with different combinations of categorical assignments to have different values of $\hat{S}_i$. If useful predictors of student test scores become available in the future that are continuous or near-continuous, they could be incorporated into the framework in a straightforward manner to smooth the predictions further.

[27] Said another way, the "all else equal" interpretation typically ascribed to regression coefficients is not sensible in our models. However, a broad sense of how our predictions associate with key student characteristics is provided in Table 2 in the main text.

The second reason we show the predictions is to make clear that they do a poor job of differentiating students in the upper end of the distribution. This again reflects a feature of the underlying data: namely, that the data available in state systems are insufficient to differentiate high-achieving students. From the perspective of a generic predictive-modeling exercise, this is a serious limitation, but for our application it is not because we do not need to differentiate students in the upper end of the distribution to inform policies targeted toward high-risk students. The distributions from the richer specifications in particular show that the prediction model works well in lower tail of the distribution (including our preferred specification, model (*l*)), although this issue will make some potential expansions of our framework problematic—namely, expansions that greatly increase the threshold value for identifying at-risk students, $\tilde{S}$. More broadly, the poor distributional alignment in the upper tail between actual and predicted scores highlights a blind spot in state longitudinal data systems with respect to collecting data that permit the identification of high achievers.

Appendix Figure C1. Predicted Test-Score Distributions Using the Models Shown in Appendix Table B1. Model (*l*) is our preferred model.



Notes: The graph labels indicate the row in Appendix Table B1 to which the model corresponds. The actual test score distribution is the same in each graph.

Appendix D: School-Level Funding Allocations from the Policy Simulations

Our results from the policy simulations in Tables 3 and 4 of the main text focus on student-level allocations, but it is difficult to target resources to individual students differentially within a school. With this in mind, Appendix Tables D1 and D2 provide information complementary to Tables 3 and 4, but at the school level. We start with Table D1, which replicates the scenarios from the initial policy simulations shown in Table 3. Reflecting this, the first four rows of Table 3 and Table D1 are identical. The bottom rows of Table D1 differ in that they report correlations between school-average variables and school-average student allocations. Larger correlations, positive or negative, indicate resource allocations that are targeted more or less toward schools that serve students with the characteristics indicated by the rows. The findings in Table D1 are in the expected direction following on Table 3, although the magnitudes of the correlations vary depending on how students are distributed across schools.

We highlight two key takeaways from Table D1. First, the tautological aspects of the allocations from Table 3 remain: our framework is better at targeting resources to schools with lower average predicted test scores, and the DC- and FRM-based systems are better at targeting resources to schools with more DC and FRM students, respectively. Second, and also following from Table 3, our framework is more effective at targeting resources to schools with more at-risk students as defined by the non-test-score and non-poverty categories (ELL, IEP, and URM).

Table D2 reports the same school-level correlations under the pseudo-LCFF. Again, the general insights from the student-level resource allocations in Table 4 are reflected in the school-level correlations. The concentration portion of the pseudo-LCFF formula does not seem to greatly affect the correlations—as evidenced by the substantive similarity of the student-level and school-level results—although it does appear to put modest upward pressure on the correlation between resources and the FRM share.

Appendix Table D1. Resource Allocation Policy Simulations, Results Part I.A: Correlations between School-Level Allocations and School Characteristics.

	Baseline Scenario: $\tilde{S}$ set at basic/below basic achievement percentile	Scenario 2: $\tilde{S}$ set so the high-risk student share matches the DC share		Scenario 3: $\tilde{S}$ set so the high-risk student share matches the FRM share	
	Use $\hat{S}_i$ to define high risk	Use $\hat{S}_i$ to define high risk	Use DC to define high risk	Use $\hat{S}_i$ to define high risk	Use FRM to define high risk
N(H) Share	0.263	0.273	0.273	0.503	0.503
N(L) Share	0.737	0.727	0.727	0.497	0.497
Z	0.952	0.916	0.916	0.497	0.497
B	1.25*N	1.25*N	1.25*N	1.25*N	1.25*N
Correlations between average resources and school need as defined by:					
Average test score	-0.697	-0.695	-0.694	-0.659	-0.640
Average predicted test score	-0.829	-0.827	-0.678	-0.771	-0.617
DC share	0.786	0.793	0.994	0.842	0.864
FRM share	0.673	0.681	0.865	0.797	0.998
ELL share	0.227	0.229	0.143	0.200	0.200
IEP share	0.225	0.221	0.161	0.187	0.067
URM share	0.818	0.817	0.630	0.632	0.556
N (schools)	2,101	2,101	2,101	2,101	2,101

Notes: Using different values of *B*, subject to the constraint $B > N$, does not affect the findings directionally, although it does affect the strength of the correlations.

Appendix Table D2. Resource Allocation Policy Simulations, Results Part II.A: Correlations between School-Level Allocations and School Characteristics Under our Framework versus Pseudo-LCFF, Holding the Budget Fixed Based on the Projected LCFF Amount.

	Our Framework	Pseudo-LCFF
N(H) Share	0.263	0.511
N(L) Share	0.737	0.489
Z	0.570	N/A
B	1.152**N*	1.152**N*
Correlations between average resources and school need as defined by:		
Average test score	-0.697	-0.605
Average predicted test score	-0.829	-0.596
DC share	0.786	0.786
FRM share	0.673	0.907
ELL share	0.227	0.223
IEP share	0.225	0.024
URM share	0.818	0.658
N (schools)	2,101	2,101

Notes: *B* is determined based on the budget implied by the pseudo-LCFF, which we implement as described in the text. We convert the budget into units of *N* to facilitate comparability with other portions of our analysis. The high-risk group under pseudo-LCFF is as defined by that policy: the sum of ELL and FRM (unduplicated).

Appendix E: Implications of a Concrete Data Change for Student At-Risk Designations

Appendix Table E1 shows correlations of $\hat{S}_i$ as estimated using the 12 variants of equation (1) that we report on in Appendix B. The correlations are reported in reverse order in the table—from model (*l*) to model (*a*)—to emphasize correlations involving our preferred specification, model (*l*), and its closest analogs. The lower-lettered models are especially sparse, as shown in Appendix B. Based on the results in Appendix Table E1, we broadly conclude in the main text that "the risk metrics for individual students are fairly stable as we change the attributes included in **X.**"

In Appendix Table E2, we assess the measurement stability question using a more direct example. Specifically, we consider the implications of a hypothetical switch from using *free meal* (FM) status to using DC status to identify students from low-income families. Conceptually this is a reasonable substitution as both metrics are purported to identify students from families at 130 percent of the poverty line or below (although in practice FM enrollment is oversubscribed—see Domina et al., 2018; Fazlul, Koedel, and Parsons, 2021). In the categorical system, we recode students as high risk based on DC status instead of FM status. For PAP, we make the same data switch in the prediction model (i.e., in the model, we toggle between using DC and FM status for all poverty controls and interactions). We continue to identify high-risk students in our framework based on predicted achievement—i.e., a high-risk student has $\hat{S}_i < \tilde{S}$, where $\tilde{S}$ is set at the 26.25th percentile.

The results in Appendix Table E2 make clear that the flexibility of our approach is a significant practical benefit. PAP is much less volatile than the categorical alternative in response to the hypothetical data switch. Specifically, the data switch results in just 4.4 percent of students switching at-risk status as measured by PAP, compared to 17.8 percent of students under the categorical alternative.

The reason for the difference is that the weighting parameters in the prediction model adjust to reflect the informational content of the new variable, holding the share of at-risk

students fixed. We largely identify the same group of at-risk students regardless of whether we use FM or DC data. The change in the categorical designations is much more disruptive because of the large difference in the size of the FM and DC categories and the inherent inflexibility of the categorical approach.

Appendix Table E1. Correlations of $\hat{S}_i$ in the Full Sample When $\hat{S}_i$ is Estimated using Different Variables in the *X*-vector. Model Labels are from Appendix Table B1, and Models are Listed in Reverse Order (*l* to *a*) to Emphasize Correlations of $\hat{S}_i$ between our Preferred Specification (*l*) and the Alternatives (*k*-*a*).

	(l)	(k)	(j)	(i)	(h)	(g)	(f)	(e)	(d)	(c)	(b)	(a)
(l)	1.0	--	--	--	--	--	--	--	--	--	--	--
(k)	0.957	1.0	--	--	--	--	--	--	--	--	--	--
(j)	0.917	0.952	1.0	--	--	--	--	--	--	--	--	--
(i)	0.894	0.934	0.920	1.0	--	--	--	--	--	--	--	--
(h)	0.932	0.953	0.908	0.883	1.0	--	--	--	--	--	--	--
(g)	0.923	0.959	0.908	0.887	0.991	1.0	--	--	--	--	--	--
(f)	0.891	0.918	0.976	0.872	0.929	0.929	1.0	--	--	--	--	--
(e)	0.859	0.890	0.873	0.934	0.921	0.927	0.894	1.0	--	--	--	--
(d)	0.910	0.933	0.891	0.877	0.979	0.973	0.912	0.919	1.0	--	--	--
(c)	0.903	0.939	0.890	0.880	0.971	0.979	0.911	0.924	0.992	1.0	--	--
(b)	0.879	0.906	0.956	0.866	0.919	0.920	0.980	0.891	0.923	0.923	1.0	--
(a)	0.844	0.874	0.858	0.917	0.907	0.912	0.879	0.984	0.925	0.931	0.896	1.0

Notes: The row and column headers reference the rows of Appendix Table B1 that define the variable list used to estimate $\hat{S}_i$.

Appendix Table E2. Changes in Student Categorizations as At Risk in the Hypothetical Condition where DC Data are Used in Place of FM data to Identify Students from Low-Income Families.

	PAP-Based Framework: $\hat{S}_i$ is predicted with DC data using the model shown in row (*l*) of Appendix Table B1, and again using the same model but with FM data in place of DC data; high-risk status in both scenarios is assigned if $\hat{S}_i < \tilde{S}$	Categorical System: At-risk status is initially assigned categorically based on FM status, then by DC status
Share of high-risk students using FM	0.263	0.441
Share of high-risk students using DC	0.263	0.273
Share of students who have a change in risk status (high to low, or low to high) due to the data change	0.044	0.178

Notes: The DC scenario within our framework corresponds to row (*l*) of Appendix Table B1; the FM scenario is identical except we replace any DC-based information with FM-based information in the prediction model.

Appendix F: Omitting Information about Race-Ethnicity from the Prediction Model

In this appendix, we briefly report on our findings if we omit information about student race-ethnicity entirely from the prediction model. There is not a strong statistical rationale for omitting race-ethnicity information from the model. Nonetheless, we provide this analysis for completeness and in recognition of the fact that there may be political, legal, or other reasons for its exclusion.

The results from our analysis omitting all racial-ethnic information from the prediction model are provided in Appendix Tables F1 and F2. Table F1 corresponds to Appendix Table B1, and Table F2 corresponds to Table 3 in the main text. In very brief summary, Table F1 shows that the prediction model performs worse if we omit racial-ethnic information. This is readily apparent in the output from the predictive regression. The R-squared is lower, the MSE is higher, and the classification error rate is higher. Table F2 shows that for the most part, the average student allocations in the policy simulation are not meaningfully affected by omitting racial-ethnic information from the prediction model. This result follows from Appendix Table E1, which shows that using different predictors, and combinations of predictors, generally does not have large effects on students' risk-status rankings within our framework. The one exception is with regard to URM status—URM students have much lower average allocations in Table F2 compared to Table 3. This result reflects the fact that if we do not allow for racial-ethnic differences in student performance in the model, it does not recognize race-ethnicity as an independent indicator of risk status; unsurprisingly, this corresponds to fewer URM students being identified as high-risk.

Appendix Table F1. Statistical Output from Primary Test Prediction Model, Omitting All Race-Ethnicity Information.

	R-squared from predictive linear regression	MSE	Classification error rate percentage (i.e., predicted status ≠ actual status)		
	(1)	(2)	(3)	(4)	(5)
Predicting students' contemporary test scores using:			All	False positive	False negative
(*l*) Replication of the results from the model in row (*l*) in Appendix Table B1 (our preferred specification)	0.290	0.60	23.81	12.59	11.22
(*l'*) All variables included in row (*l*) of Appendix Table B1, except any variables and interactions involving race-ethnicity	0.260	0.62	25.31	14.09	11.23
N (Test Takers in Grades 3-8)	387,317				
N (Schools)	1,749				

Appendix Table F2. Comparison of Primary Policy-Simulation Findings from Table 3 Using Test Prediction Models with and without Race-Ethnicity Information.

	Baseline Scenario: $\tilde{S}$ set at basic/below basic achievement percentile (repeated from Table 3)	$\tilde{S}$ set at basic/below basic achievement percentile (test prediction model does not include any race-ethnicity information)
	Use $\hat{S}_i$ to define high risk	Use $\hat{S}_i$ to define high risk
N(H) Share	0.263	0.263
N(L) Share	0.737	0.737
Z	0.952	0.952
B	1.25*N	1.25*N
Average resource units per student, by type, where a value of 1.0 represents the normalized resource allocation to low-risk students:		
Actual Test Score (S_i) below 26.25th percentile	1.537	1.544
Predicted test score ($\hat{S}_i$) below 26.25th percentile	1+Z=1.952	1+Z=1.952
DC	1.500	1.558
FRM	1.400	1.404
ELL	1.636	1.606
IEP	1.910	1.924
URM	**1.621**	**1.466**
N	698,726	698,726

Notes: Using different values of *B*, subject to the constraint *B* > *N*, does not affect the findings directionally, although it does increase the per-pupil dollar gaps for all student categories relative to 1.0.

Appendix G: Using PAP for Accountability (Within School Achievement Gaps)

In this appendix, we discuss some of the drawbacks of how data are used to track achievement gaps across student groups within schools and describe how PAP can be used to improve upon current practice.

Based on their plans submitted to the federal government as part of the Every Student Succeeds Act (ESSA), states currently track achievement gaps in one of two ways. The first is to specify multiple categories of student risk (e.g., FRM, ELL, IEP, URM) and track gaps for each category separately. The second is to combine the categories into one "super subgroup" and track the achievement gap between students who do and do not belong to the super subgroup.

Each approach has strengths and weaknesses. The former follows from the structure of the predecessor to ESSA—No Child Left Behind (NCLB). On the one hand, it is useful because it provides detailed information about achievement gaps along a variety of dimensions. But on the other hand, it can be misleading because of heterogeneity in expected student performance within the categories across schools. For example, if schools A and B both have ELL students, but the ELL students at school A are also at relatively greater risk along other dimensions (e.g., if they come from lower-income families), the ELL-based gap will be higher in school A than in school B due to compositional difference, all else equal.

Other problems with the multi-category approach include that it can (a) cause information overload (Sutcliffe and Weick, 2009) and (b) lead to type-I errors because as the number of groups tracked for accountability increases within a school, the likelihood of bad outcomes for some groups increases statistically (Davidson et al., 2015). The super-subgroup approach is meant to solve these problems by reducing the achievement gap within a school to a single number comparing students who do and do not belong to the super subgroup. However, its limitation is that there will be compositional differences in the super subgroup across schools, which exacerbates the problem raised in the preceding paragraph of group heterogeneity in expected student performance.

PAP facilitates the single-comparison simplicity of the super-subgroup approach while minimizing the potential for misleading comparisons due to differences in the composition of the super subgroup across schools. The basic idea is to compare schools' predicted achievement gaps between high-risk and low-risk students to their actual gaps. Schools with actual gaps that are smaller than the predicted gaps have less inequity than would be implied by the characteristics of their student bodies, and vice versa for schools with actual gaps that are larger than their predicted gaps.

To illustrate, we begin by identifying all students with $\hat{S}_i \geq \tilde{S}$ as low risk and all students with $\hat{S}_i < \tilde{S}$ as high risk. We continue with the 26.25th percentile in mind as the threshold for $\tilde{S}$, although this choice is not substantively important in what follows.

Consider the following representation of the observed achievement gap in school *k* between low-risk and high-risk students:

$$\frac{1}{N_{L,k}}\sum_{i=1}^{N_{L,k}} S_{ik} - \frac{1}{N_{H,k}}\sum_{i=1}^{N_{H,k}} S_{ik} \qquad \text{(G1)}$$

In equation (G1), the subscript *k* is added to each student's score, S_{ik}, to denote the school assignment. Next consider the predicted achievement gap based on our framework, where the only change is that we replace students' actual scores, S_{ik}, with their predicted scores, $\hat{S}_{ik}$:

$$\frac{1}{N_{L,k}}\sum_{i=1}^{N_{L,k}} \hat{S}_{ik} - \frac{1}{N_{H,k}}\sum_{i=1}^{N_{H,k}} \hat{S}_{ik} \qquad \text{(G2)}$$

The observed and predicted achievement gaps in equations (G1) and (G2) can be used to determine how the actual achievement gap at school *k* compares to the predicted gap based on the **X**-vector attributes of students who attend school *k*. A useful metric for school *k* can be expressed as the difference between equations (G1) and (G2):

$$\{\frac{1}{N_{L,k}}\sum_{i=1}^{N_{L,k}} S_{ik} - \frac{1}{N_{H,k}}\sum_{i=1}^{N_{H,k}} S_{ik}\} - \{\frac{1}{N_{L,k}}\sum_{i=1}^{N_{L,k}} \hat{S}_{ik} - \frac{1}{N_{H,k}}\sum_{i=1}^{N_{H,k}} \hat{S}_{ik}\} \qquad \text{(G3)}$$

Momentarily suppressing discussion of one technical caveat, equation (G3) has a straightforward interpretation. If the value is positive, the actual achievement gap between low-risk and high-risk students at school *k* exceeds the predicted gap based on the attributes of low-risk and high-risk

students; and vice-versa if equation (G3) is negative. Said another way, schools with negative values of equation (G3) have achievement gaps that are smaller than what would be predicted based on their compositions of low-risk and high-risk students.

Equation (G3) provides a single, summary indication of how the achievement gap in school *k* compares to what is predicted. States can quickly identify schools that have narrower achievement gaps than predicted, and larger gaps than predicted, based on this single number. The potential for equation (G3) to be misleading about the school's gap is much less than in the simple systems states currently use. This is because the composition of high-risk and low-risk students along many dimensions is accounted for by the rich specification from which the $\hat{S}_{ik}$ values are estimated.

The one technical caveat to this simple interpretation is that the fitted values in equation (G2)—i.e., the $\hat{S}_{ik}$ values—are implicitly shrunken through the predictive regression. As noted in the main text, shrinkage is inherent to the prediction process. Due to the shrinkage, the average gap between the test score predictions in equation (G2) will be attenuated relative to the gap in observed scores in equation (G1), resulting in disproportionately positive values for the difference in equation (G3).

Fortunately, as in the allocation-policy context, there are straightforward solutions to address the shrinkage problem. One solution, following from our preceding analysis, is to calculate the values from equation (G3) using percentiles rather than actual and predicted scores. The interpretation of equation (G3) would be as follows: for each school, it would indicate the difference in the actual versus predicted percentile gap between high-risk and low-risk students. If equation (G3) is calculated in percentiles, the simple interpretation of positive and negative values would hold from above.

However, it may be undesirable from a presentational standpoint for states to report achievement gaps in percentiles. If states wish to report the difference in equation (G3) in test-based units and not percentiles, a mathematically-equivalent solution is to inflate the variance of

$\hat{S}_i$ to match the variance of S_i by multiplying the $\hat{S}_i$ values by a constant.[28] This inflation should occur after the predictions are made using equation (1) in the main text, but before constructing the average predicted values in equation (G2). Using the variance-inflated $\hat{S}_i$ values, equation (G3) can be interpreted in test-based units, and the same inference can be drawn for positive and negative values as described above.

[28] Specifically, if each value of $\hat{S}_i$ is multiplied by the ratio of standard deviations of S_i and $\hat{S}_i$, it will inflate the variance so the variance of the modified $\hat{S}_i$ values matches the variance of S_i. This will preserve students' rankings in the distribution of fitted values and allow for appropriate interpretation of equation (G3).